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#### FORMAT

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

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The editor wishes to express his thanks and appreciation to John Ryle who is resigning his post as editor. Thanks are due to Paul Dresch, Martin Cantor and all those who have helped with the production of this issue.

### Notes for a Study of Fertility

There is a play in the idea of fertility that is of tremendous importance. First it is a capability to produce: children, ideas, crops, life of all kinds. But it is also performance: actual numbers of babies born, a slogan for the work of an author, a known attribute of the soil. Fertility as it extends over time is a process in which all living beings participate. But it is also a subject of assessment; the value of fertility is not everywhere conventionalized in the same way.

We have only the most haphazard idea of why this is, even though questions of this sort have been of considerable and indeed polemical interest almost perpetually. The demographic aspect was separated off very early, and put in its modern form by Malthus (1798) as a relation between social ideas and practices and material constraints. Malthus was also prescient in the elaborate efforts he made to get numerical information about population. Neither of these contributions were original, nor was the dubious class interpretation he built upon them. Nonetheless, the Malthusian model in which social mores decide the numbers of people, and in which these numbers, every increasing, approach a point at which the exhaustion of resources intervenes, is still the most widely accepted description. Productivity carries its dangers; fertility wants control. As a statement of general possibility this is trivially true, but the power of the idea is evident in its direct contribution to two defining features of our era. The first, which does not directly concern us, is Darwin's theory of natural selection; the second which includes some influence of Darwin, is the conceptualization of human populations in numerical terms in which social influences are included solely for their material, in this case, biological consequences.

The use of numerical methods in studying populations has a very long history; it cannot be said that Malthus contributed much to this, he was mostly just awake to its possibilities. By the time these methods had truly become statistical at the turn of this century the metaphors of evolution had pervaded the study of society, so that the writings we recognize today as the first formulation of fertility in the demographic sense were made as mathematical contributions to biology. The gradual sociologizing of these metaphors took place, as it did in anthropology, in the period up to about 1940. Sociologists of fertility since that time have chosen to concentrate on a statistical method parallel to demography; the categories of these statistics are a thoroughly ad hoc mixture consisting of remnants of the biological glosses, stock categories of academic sociology, and those items required by the statistical method itself. The assessments of prior periods are included in these categories in some scattered part; but there is to the demographic and sociological study of fertility little of the vital force of the idea of fertility itself.

It is well known that, aside from the occasional statistical advocacy, anthropologists applied their socio-biological metaphors to aspects of society in which the advantages of enumeration and statistics were not immediately apparent. The censuses taken by ethnographers are more in the way of initial reconnaissance than a major influence upon subsequent description. Mere survival is not an issue for most societies anthropologists have studied unless this was a matter of the encroachment of neighbouring or colonial groups. Malthus and even later writers who included primitive peoples in their population studies, such as Carr-Saunders and Krzwicki, have never had an anthropological following. This did not leave anthropologists free to take up their own approach. As it turned out the attachment of anthropology to colonialism, the pseudo-biological idea of functional integration, and the correlate inattention to language and native representation

united to remove the importance of fertility in the self-definition of groups from the ethnographers' attention. Perhaps the study of 'kinship' and 'marriage', had it been left a greater element of self-definition, might have given the 'play' of fertility explicit attention. As it stands, it is an open question whether these institutions and their terminological and ritual expression embody anything like the range of ideas in English surrounding 'fertility', 'conception', 'creation', 'germination', and the like. The same is true if we ask what the influence of the range of activities so described has over changes in the numerical composition of groups. That is, if we ask the inevitable question of the relation of ideas and infrastructure, of classification and action.

The particular importance of fertility is the 'play' between the fact of the process in time and the conventional assessments which are made an object of study as if they were outside of time. The 'play' encapsulates a current problematic, that is, the definitions that we ordinarily go by and the ranges of experience we thereby shut out. We would like to reinstate time, not knowing altogether what is meant by such a grandiose phrase. And we would wish, thereby, to do away with the painful hyperstasis of phrases such as 'ideas and infra-structures' and 'classification and action'.

The centrality of fertility is not just its evocativeness, as tends to be the case with a similar term, viz 'generative'. Rather, it provides us with something of a course to follow, at least in the initial stages. The 'play' is equally inaccessible to demography and anthropology: to show that the situation of these two subjects is essentially the same is at least of polemical value; and insofar as this refers the major method of study in this century (statistics and formalisms generally) to a subject which considers itself a defender of the informal and semantic, we would be tackling a case of general importance. Inevitably this would say something of the capabilities of the methods of each for the problem at hand. The two subjects seem particularly suited for such a critique: demography, of all the social studies, is remarkably conscious of the artificiality of its method; the anthropology with the greatest implication for fertility, the study of prescription, marriage, and related symbolism, is among the most highly developed in the subject.

A critique does not offer a way out. One is inclined to agree with those who argue that the next steps await an ethnography we do not as yet have. At times this seems particularly damning, as if those who could go into the field if they merely wished do not, and those who would like to find they cannot get the most simple help. The history of these two subjects, which makes up a kind of ethnography of a certain scientific problem of our period, at least permits us to show the extent to which the current problematic may be stretched.

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It is a remarkable impasse that we are unable to account for the influence of collective representations upon changes in population size and composition. A glance at history does tell us something about the demographic situation. At present we possess a remarkable calculus for expressing changes in relative numbers of people considered in the abstract; but there is no comparable analytical framework which conceptualizes these changes as they follow from native representations, considered for their own abstract structures. The sociological study of fertility, which has attempted to account for these changes statistically, without attending to the structure of native representations, has yet to produce anything like a theory. All of this can be said to follow from the historical situation at the beginning of this century: basically, that a certain conception of the use of formal methods was widely accepted, and that anthropologists while also accepting it generally chose to study situations in which such methods seemed pointless or impracticable.

While there has always been a certain disdain for statistics or formalism, anthropologists have never bothered to produce a thorough-going critique. Some obvious problems, such as the inappropriateness of standard demographic categories and schedules to particular ethnographic situations have been noted many times; but these have become rather pat criticisms which are merely a folk-lore within anthropology. Nor have anthropologists applied themselves to semantical analyses of the representations that might be responsible for changes in population structure in particular societies; this in spite of the fact that most of the societies they have been studying have been going through the most radical displacements imaginable.

There is a good scattering of ethnographic information in the vicinity of the topic, some of it very interesting: these range over anecdotal information on sexual practices, historical and demographical accounts, physical and cosmological representations as they enter into systems of exchange, recent discussion of ethnic definition, and simple passing references. It would be an interesting if quaint exercise to assemble these materials, for the similarity in native manners of expression of fertility might well make up a kind of natural resemblance. However, previous experience in assembling these tangential writings in accord with the interests of other academic periods, has shown them to be very suggestive but inconclusive.<sup>1</sup> This is likely to be all that can be said.

A history of the separation of anthropology and demography, of the missed critique on the one hand and the missed ethnography on the other, would not be without interest. Needless to say, demographers are doing something quite different in their study than are anthropologists; the point of such a history would not be to suggest that they fail to take up the problem of the influence of native representations, for they never intended to; rather, it would show some of the consequences of setting such questions aside. These are of some interest as they are part and parcel of the statistical method generally. Demographic analyses, because they are in this way incomplete, have been susceptible to the wildest interpretations and, accordingly, have been used unintentionally to misrepresent the very relations they are intended to show. Such an account would not startle demographers at all, for they are accustomed to the mistakes their method engenders; but it also would not help them with this problem, nor give us a hold on the semiotics of fertility. However, a historical stretch of the successive interpretations of demographic statistics does provide us with a good set of examples of the semiotics.

A collation of anthropological part-references to fertility would only remind us of certain familiar limitations in the methods of interpretation of different periods of anthropology. It is not possible to consider these as part of a semiotics of fertility since anthropologists have never really conceptualized them in anything like that way. There is no tradition of study to be ferretted out here. But the recent experience of anthropologists in 'rethinking' the short-comings of earlier accounts has led them to regard questions of idea and infra-structure such as posed by fertility as left outstanding by traditional descriptive methods. Fertility is the kind of problem whose current fragmented state of formulation can be recognized as more than a consequence of preferred methods at the inception of these subjects and a subsequent division of labour. We can, instead, invoke that heavy word 'epistemological' to describe certain features of the thinking at that time which continue on into the present.

Obviously one such feature was introduced by the conception of formal methods: the requirements of a notational system, notably the total unambiguity of its characters and their relations, means that the manner in which it specifies events is remarkably different than that of ordinary language. The consequences of this difference are very far reaching. They

include not only the tendency to recast ethnographic situations in an alien form, but something of the rationale behind the division of academic subjects such as anthropology and demography. When we speak in passing of 'levels' of analyses, we are invoking an ideal in which the clarity and precision of mathematical and geometrical analyses is never far away. When anthropologists bicker about the status of formal methods in their subject, as I will go on to do in this paper, they are, for whatever their disagreements, basically just reasserting these familiar divisions.

A further epistemological issue is the way in which ethnographic situations, of which that of the analyst can only be another example, seem to present themselves. This is really a matter of our own inarticulateness. Fertility may serve as the case in point, considered 'just' with reference to its central aspect of human procreation. We might take this, as is often done, as a question asked by some hypothetical couple as to whether and when they should have a child. Of course familiar collective sentiments weigh in very rapidly. These may be on quite a different scale, such as the state of the economy in a particular sector, a totalitarian character of government, or a tightly-knit ethnic or religious community. All of these may be rendered locally as, for example, the social pressures on working mothers, the number of children one can expect to get into the Party, or the threat of assimilation to a small community.

The definitions over-ride even the unpredictable physiology of conception. Take, for example, the experience of those women 'on the pill'. Quite a number of births and abortions seem to follow from misgivings about its physiological effects - misgivings which lead to sporadic use. There is good cause for agonizing here, whether it is really unknown possibilities of clotting or cancer, or the daily physical discomforts. Some women put up with all of these and some women finally refuse, but the incidence of all of the symptoms is scattered through the full range of users. Who would say that their problems and self-diagnoses are merely either physiological or 'psychosomatic'?

To take just the pregnancies which seem related to this; the availability of abortion marks some change in the view of women and men and pregnancy; this seems to have lessened the reality of lumbered marriages and self-induced or clandestine abortions, if only by adding possibilities. One may note that this owed to social redefinition as well as technology; the technology has not removed the physiological indeterminacy, nor made contraception and abortion popular, although it has in some way participated in the changing ideas people have about what to do when unexpectedly pregnant. Plainly this is a part of a much larger and continuing change. Although we may consider the control of fertility as an axis along which the relative positions of men and women are conventionalized, there is much more to these situations than any simple linguistic statement can convey.

The epistemological puzzle posed by situations such as these is that, on the one hand, they expand to take in very large ranges of society; on the other, they reduce to a tenuous interpretation of infra-structure. No one characterization seems adequate. Nonetheless, when we sometimes refer to 'the pressures' on people who happened to be procreating (as well as all sorts of other activities similarly influenced) we are acknowledging the relatedness of all of this, and people in these situations do see themselves as 'pressurized'. The problem is not unlike the one, in an overlapping area, which led Edmund Leach to argue that there could be no simple definition of marriage; marriage is at best 'a bundle of rights'. All we have are these awkward, short-shrift phrases. No one will think, then, that I am trying to substitute 'fertility' for 'marriage', 'kinship', and the rest.

There is the graceful option to consider only those sets of definitions which cluster around recurrent events. Bundles of rights, kin terminologies, colour terms, are all examples. Particular rituals or myths also suggest themselves as encapsulations of basic social themes. One can imagine an attempt to take some situation in which the 'play' of fertility enters, such as the situation of young unmarried pregnant women in 'family planning' clinics in our own society, and try to trace the themes expressed in these regularly occurring situations through to the wider ranges of social representations that are of influence. Perhaps such events can provide a kind of text in the manner, for example, of Gregory Bateson's Naven.

There are many problems here, even setting to one side that we have no such accounts, and whatever might be the problems of the midst of such an ethnography. Taken as an idea of how to go about studying such situations, we might criticize the 'ritual' or 'terminological' approach in two ways. First, while such an ethnography would tremendously improve our understanding, there is nothing in the formulation which would allow us to monitor shifts, e.g. in attitudes toward abortion, or in control exercised by men and women, or in the very difficult questions of diagnoses. Such a description gives us valuable information about the current state of conventions, not of continuing process. Second, the status of such terminologies and rituals seems rather idealized. It is presumptuous to proceed as if important terms and routines will everywhere take up coherent sets of terms and actions; if approached as sets anyway, we should expect such sets to be loosely structured, full of 'hollow' categories, and impossible to interpret without a diachronic sequence of changes. The idealization is both a fixation into forms whose distinctiveness may be endlessly debatable, and a fixation of time.

We began this section by remarking on our inability to connect collective representations and population changes in a convincing way. Somewhere between the two we have insinuated young unmarried pregnant women and their men in situations somewhat like those in which the control of their fertility evolves. Anthropological descriptions, which might be very welcome additions to our knowledge about these people, do not seem suited to showing how the major changes in social definition of their situations occur, nor the consequences for demographic structures. Our description of these has been quite summary; however, the static quality of anthropological descriptions, and the monographic method in which the no doubt very plausible relations are filled out by illustration and anecdote seem to be sufficiently long-standing subjects of criticism within anthropology as to not require restatement. There is no question that recent work on classification marks a major improvement; the replacement of pseudo-biological analogies by pseudo-grammatical ones has not proceeded without an awareness that such changes are of the same kind as the ones anthropologists study; but insofar as these improvements are addressed to understanding ostensibly 'new' sets of classifications rather than attending to their modes of derivation or production - and the tendency to stereotype changes in time as 'evolutionist', 'functionalist', 'structuralist', 'post-structuralist' is one obvious example - all of these developments serve to obscure the very sort of problem we are trying to get at.

We have also begun to give some idea of the background of the particular forms, anthropological and demographic, through which the 'play' of fertility has been fixed. We identified two epistemological aspects of this, without however, relating them; the separation of formality from language; and the range of implications of particular instances of 'play' which resist formulation either in an englobing way or cluster by cluster. The potential of their linkage seems obvious enough: the 'play' which is both meaning and action, is in essential aspects non-linguistic, and our frustration in formulating the range and movement of these situations comes no doubt from our attempt to force them into language anyway; formal notations are non-

linguistic expressions of connectedness and suggest themselves, therefore, as ways of reaching beyond the language. However, insofar as notational systems have their own rules of specification, which have nothing to do with ranges of social events, there is at first glance no reason to believe that they can be any more attentive to non-linguistic specifications than language. What does the use of formal ideas entail?

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The adoption of formal methods, whether in analogy to various schools of mathematics or linguistics, generally resolves upon a form which allows a tremendous multiplicity of events to be expressed through a few, concise relations. Even the use of general formal ideas such as opposition, homology, and symmetry on a piecemeal basis retains a form which insists upon the crisp connectedness of theoretical formulations, in contrast to the informal and vacillating character of the experience of reading, writing, conversing, and so on. Formal methods generally resolve upon notational systems or schemes which insure the unambiguity of the items and relations. A formal method thus involves a set of relations in which the connecting operations are quite different from those which order social events. The correspondence of formal schemes to the conventional assessments of the events is thus far from immediately apparent.

The question 'to what do the elements of formal systems refer?' is resolved by the institution of 'data'. That is, a substitute reality is constituted which purports to be an accurate selection of information from a local setting. The implications of this in the statistical case are well known: the categories of the data follow the interest of the collecting agent and not of the local setting, although there is often a great deal in common. Statisticians such as demographers generally consider the gathering and condensing of information as a separate problem from the theoretical manipulations of their notation; the inferences and assumptions that make up a statistician's handling of materials, before or after they are accorded the status of data, usually remain unanalysed; and insofar as writers tend to refer to 'collection of data' rather than of information - i.e. the data is reality - the solution to the question of reference can amount simply to banishing both the processes and assessments of the peoples studied.

Anthropology counts a partial improvement on this. There is a tendency, particularly in formal analyses, to consider the written ethnography as data, that is, as an adequate account of a particular people. This in spite of the fact that the formal analyst is almost invariably asking a different set of questions than did the ethnographer; the situation would seem to be improved only when the analyst and ethnographer are the same person, and the account includes a description of how the formal rendering of native representations was decided upon. The work on terminological sets (with its incumbent limitations) alluded to earlier is a case in point.

Analysis of published ethnography has depended upon the generality of certain aspects of communication which lend themselves to formal expression. These ideas owe their entry into anthropology to Levi-Strauss's fitful explorations of linguistics and mathematics between 1945 and 1955, and their clarification to Needham's studies of prescription and lateral symbolism between 1958 and 1969. The basic distinction is that between prescription and preference, i.e. between self-defining categories and those for which there is a considerable element of choice. At a very general level there seems to be a close fit between the idea of a prescriptive rule and the categorical practice of native peoples. Thus, when Needham joins Leach in stating that 'prescriptive marriage is not merely (an) ideal type but actual'<sup>2</sup> he is asserting a one-to-one correspondence between theoretical relations formally expressed and the relations carried in certain native classifications. However, this applies only to the few categories that may be considered prescriptive: thus, while knowledge of a rule of pre-



scriptive marriage indicates what categories of people are allowed to marry, it does not say which individuals in those categories will marry, whether and how the categories may change, and it does not preclude that prescribed individuals may be reclassified as marriageable. For a working out of the practice, an intimate knowledge of personality, etiquette, tastes, manners of speech, local background - in short, of preferences - is necessary. Prescriptions arise out of preferences, both in the course of the investigator's understanding, and in the course of events generally. As Levi-Strauss notes, all prescriptions are preferences from a certain point of view:<sup>3</sup> it is the assumption of a system of classification which in both cases turns the definition of certain preferences into the definition of a situation. Hence the quality of self-definition.

Thus, anthropology improves upon the use of formal methods insofar as the analyst first has some familiarity with native classification; and even then, the formal renderings are restricted to a few general conventions. The distinction between prescription and preference makes a slight but significant realignment in the usual attitude of anthropology which keeps formality separate from semantic interests. Formal ideas map selected ranges of representation rather well, and are an important aid in their exploration; but because this range is so limited, the direct applicability of formal systems - group theory, statistics, matrices and networks, etc. - as systems seems to imply an inevitable forcing of native classifications into some wholly alien mode.

We may class this clarification 'slight' in the sense that its main effect is to better articulate a long standing anthropological view. For example, although passing positive reference to statistical formalisms has been a part of anthropology practically from the beginning, there have been few attempts to give these methods a more than secondary role. These now tend to be identified with a certain period of the subject:

Certain members of the Central African/Manchester school of anthropologists did set out to improve the observational methods of fieldwork. Barnes, Mitchell and others made it possible to apply advanced statistical methods where they had been previously regarded as impracticable. The result was unexpected: such studies were not much welcomed even by avowed empiricists. The more 'statistically rigorous' seemed to mean, in some way, the less 'anthropological'. We may not necessarily deny the soundness of this instinct.<sup>4</sup>

No doubt the same instinct has participated in the misinterpretations of Needham's prescriptive studies; and these have, in turn, stimulated on his part several recent statements of method regarding the proper place of formal analyses.<sup>5</sup> He plainly wishes to differentiate his work from the increasing mass of formal studies of all sorts; and where the question is of the nature of reference between the formal and some social reality, his comments turn specifically on prescription. The use of formal methods may be extended to cover prescriptive categories provided that the formal method is not a full-blown system but an opposite selection of formal relations. The situation for preferences, however, remains unchanged from Levi-Strauss's distinction between mechanical and statistical models: the formal approach to prescription is not suitable for preferences due to their multiplicity and changeability; formal methods such as statistics, while applicable, still do not follow the preferences as they are implied by native classification.

The problems posed by preferences are further confounded, as Leach noted several years ago, in that there is no necessary connection between collective and individual representations.<sup>6</sup> These vagaries of preference no doubt account for the turn in some of Needham's current writings from the publication of formal analyses to an advocacy of conceptual analysis ac-

according to 'a combination of the approaches of Benveniste and Wittgenstein'.<sup>7</sup> For this programme he states the limits of formal methods quite succinctly:

...the mode of analysis necessarily remains subject to two main critical qualifications. First, that the formal constructs themselves call ultimately for a validation that is independent of the ideological tradition in which they are framed; and this cannot be done either by meta-formal analysis or by reliance on the traditional concepts that the abstractions are supposed to rectify. Second, that however abstract or purely logical the formal notions may be, they are useful only to the extent that they mediate between the concepts of natural languages; and as soon as these are brought into any connection there rearise all of the stock hazards, of grammar and social circumstance, that attend any attempt to convey meaning from one form of life into the categories proper to another.<sup>8</sup>

This is, I think, an elegant clarification of the long-standing view that formal analyses and anthropological attention to the native point of view don't mix. In this conception, as in the definition of anthropology in contra-distinction to statistics, it is the relation which gives the respective sides much of their significance. Together they make up a common view, a seemingly inevitable division in the understanding of society. With the aid of the clarity Needham has brought to this relation we can make two points.

The first is that the relation as phrased is solely between formality and language. All of the argument above regarding our inability to formulate extensive ranges of social relations in language as well as the movement of these relations over time, weighs-in here. Insofar as these ranges influence our use of language, we can expect any accounting of concepts confined to their linguistic aspects to be frustrated. This is equally true for any accounting of the use of formal ideas without reference to the constant interdigitation of formal abstractions and their semantic counterparts. The application of formal ideas will have to be taken not merely in terms of their notational relations, but according to their use in the midst of reading, writing, arguing and other ways of understanding. This will vary considerably according to the situation of the notation. Needham rightly considers this as an inter-relation with language where mathematical notation and his own use of general formal ideas are concerned; but we cannot expect this to be the case for musical notation, for notations of human movement such as the Laban system, and for whatever schemes might be of use for ritual and other events in multi-dimensions.

The second point is that the distinction between prescriptive and preferential rules, insofar as it marks an overlap of formal and collective representations, does make the first step toward a consideration of the formal as used in combination with other semiotics. However, insofar as the distinction leaves unchanged our inability to model preferences in any other than statistical way, it makes no real advance. Examination of this inability allows us to elaborate upon the commonality of certain anthropological and statistical methods.

Levi-Strauss's distinction between mechanical and statistical models was drawn from Wiener: there are models expressible in the language of classical mechanics - ordinary language - and there are models in which the components are so many and various that they can only be considered in the aggregate. That is, the individuals of statistical mechanics are classes of individuals. However, both the classical and statistical models are mechanical explanations, and the logic of analysis of the individuals of one is true for the classes of individuals of the other. A statistical model is merely a mechanical model in which the operations which follow-out the assumptions of the system are probabilistic. We can carry the analogy on to refer to prescription and preference in the following way. The logic of

both statistical and mechanical explanations consists of prescriptions which rule the elements of the notation. Where the latter is concerned, the prescriptions state the relations of individuals - in the case of marriage, of groups in alliance and even of particular marriages. It so happens that prescriptive rules are characteristic of societies of moderate size so there would be little point to their statistical specification. However, in mass societies the definition of groups and the significance of particular marriages is much less clear, statements about the marriage practices of mass societies will still be based on a logic consisting of prescriptions; but insofar as particular marriages and groups could only be identified tediously, a statistician usually defines his own classes which, in the analogy to individual intermarrying groups, stand as collections of very large number of alliance groups.

The point I wish to make is that statistical analyses are not different from formal prescriptive analyses in their logic; their difference, as Levi-Strauss noted,<sup>9</sup> is a matter of scale. A statistical analysis could be carried out within the logic of a prescriptive marriage system, although there would be little point to this other than confirmation in certain cases. Statistical analyses becomes suitable for preferences due to the considerable scale of possibilities they admit; but what such an analysis does, in effect, is to reproduce a prescriptive analysis, a mechanical model, in which the details are settled in the aggregate. There is no analysis of preferences as preferences; there are only prescriptive formal analyses, some of which are statistical; any of these may attend to the nature of reference between the analyst's prescriptions and the data, or the data and the ongoing events.

The similar consequences of formal analyses of preferences, whether carried out statistically or verbally, can be seen by a brief consideration of the results of the papers on lateral symbolism. Fortunately, the status of these results have recently received explicit statement.<sup>10</sup> The elements drawn from the ethnography in these analyses are taken from reports of particular situations which show clear evidence of dual classification; the elements are then listed in columns, but the placing of an element in one or the other column is not indicative of any common property among the elements: the only common factor is that they enter into the same kind of relation, and that their distribution seems to accord to some very widely applied distinctions, such as right and left. Needham takes up two questions regarding the theoretical status of this scheme and its components: first, the question of the relation of the analysis to the peoples concerned; and, second, the extent to which such analyses may be objectively validated or refuted. The notational scheme is not, of course, in the minds of the natives. However, having accorded the ethnography the status of data, the elements in relation are regarded as one-to-one with collective representations as used in native situations. The listing of these relations together does not indicate that either the situations or the dual symbolizations are in any way connected. That is, analysis says nothing further about a context than that it exhibits dualism; and nothing is said of the relation of contexts. Finally, presence or absence of dual classification says nothing necessary about the presence, absence, or relative importance of other principles of classification for these isolated situations.

Plainly, any similarity of such an analysis to one performed by someone using a statistical method, such as a demographer, must be at the level of the underlying logic, the prescriptions of method, rather than in the statistical elaboration. We have already noted the similar attitude toward information which accords its written presentation the status of reality. A demographer expects certain general principles to be operating in the data; we could say that the counterpart to dualism in a statistical analysis would be the regular characteristics of aggregates, such as the tendency of elements to cluster around a mean, or their asymptotic properties. The demographer would, of course, choose a principle to which the data seemed suited; and

the notational scheme is, of course, in his mind and not in those of the peoples studied. The situations in the data in which the principle is exhibited are organized according to the usual practices of the discipline: the settings in which the data are constituted, both in collection and writing-up, are no more apparent in demographic tables than they are for the situations described verbally in anthropological monographs. In both cases the reader must try to decide the general plausibility of the relations for himself. In our analogy then, each demographic table is the equivalent of each situation for which a dual relation is listed in a table of dual classification. A demographic table is, after all, a collective representation. And, as in the case of the table of oppositions, there is no necessary connection between the contexts or the symbolizations represented by a table; similarly, a table is subject to analysis according to many principles, without specifying their relation.

In sum, the careful limitations Needham places upon the analysis of lateral symbolism, particularly the way relations are shown to operate in the data, and the extreme generality and yet isolated specificity of these relations, are very much in the character of ordinary demographic analysis. We would expect this to be the case insofar as both utilize a mechanical model consisting of a few descriptive injunctions which do seem appropriate to the data; and both models convey the impression of producing relations which go past the data and have some hold upon actual situations. The methods differ only in that, once the model is in place, a demographer will confirm its assumptions statistically; the additional difference, that demographers tend to draw their prescriptions in line with what they regard as infra-structure rather than with the structure of native representation, is simply an academic convention and is not a necessary or essential characteristic of this kind of analysis. However, we can now readily understand why the demographer is inclined to do this: insofar as formal anthropological analyses are subject to the same restrictions with regard to preference as demographic analyses, there is no readily available structure to native representations for the demographer to accommodate his analyses to. It is not so much that demographers fail to attend to differences in classification, as that the information and analyses of those who specialize in such classification have never been suited to the assessment of changes in population structure.

It is perhaps not surprising that Needham concludes his paper with the nagging question of the validity of such analyses:

...it is still an unavoidable concern to ask how, or in what degree, oppositional analysis can ever be said to be right."

This kind of problem is a long-standing one for demographic analyses. Indeed, demographers are continually reminded of the consequences of the removal of information in space and time from ongoing social settings, for they are concerned to project future population structures, and thus are regularly confronted with the possibility of factual refutation. The kind of formal analysis we are describing renders the multiplicity of native preferences according to prescriptive principles and academic conventions agreed upon beforehand; it does not attend to the flow of preferences, and it cannot connect the particular principles it identifies in the data with the wider range of conventions that may be current in a society. The example of demography shows that the problems faced by this method are of two kinds: a tendency to state the obvious, in part because all statements repeat the initial assumptions; and, a tendency for results to be wrong because they assume an absence of change. It is well known, for example, that the most highly regarded actuaries of the 1920s and 1930s believed that western Europe and North America faced a dire threat of depopulation; or, that the extraordinary decline in American fertility which began in the 1960s, due in large part to changing opinions about contraception, was not anticipated. The Knowledge, Attitudes, and Practices surveys of the 1960s,

which were intended to provide information on the realities of contraceptive use, but were conceived with little or no attention to native representation, indicated, for example, that people will say they do not want too many children.

This last example reminds us that anthropologists, insofar as they are able to make direct and sometimes remarkably continuous contact with native representations may not be as susceptible to a reading-in of their own cultural posits as are demographers. Indeed, the potential contribution of anthropology to population studies has always been for this reason tremendous. Anthropological study, nonetheless, is particularly vulnerable to changes in time owing to the short duration of field studies. Formal analysis accentuates this, and it is remarkable that the effects of the removal of events as data from time are not considered in Needham's introduction. In the case of the Nyoro, there is, as he notes, a century of published ethnography in several languages and of varying quality; there is no comment on the effects of this upon analysis, nor on how the effects might be accounted for. In his Nyoro article the various references are cited one after the other, as if the information of the periods was equivalent. Thus, in a consideration of colour symbolism<sup>12</sup> the sequence of published examples runs: 1964; 1922, 1911, 1953; 1879; 1938; 1911; 1920; 1960; 1911; 1867; 1911; 1895; 1911; 1922; 1920; 1911; 1938; 1867; 1893, and so on. The ethnography is variously English, French and German. Even though the 'colours' under examination are white, black and, in passing, red, which seem to have some general significance,<sup>13</sup> it is presumptuous to assume that their ranges would remain identical for a century in four languages. At least the terminological approach could be brought to bear here.<sup>14</sup>

The definition of the Nyoro in space is also not considered. Although I do not have an extensive command of the literature, this may very well be because the written ethnography does not include an account of how the Nyoro define themselves, particularly with reference to neighbouring groups and dialects. There is also some variation in the locations to which the existing accounts refer. But again, as long as the point of analysis is to show the presence of certain general principles, and to illustrate their operation, the definition of the social units may be assumed and moreover, assumed to have no effect upon analysis. The situation is much the same for the demographer, who chooses the social units under consideration to suit his own convenience. It is as if the Nyoro exist in a pure space, much in the way they exist outside of time.

Thus, although anthropologists are not in the habit of trying to make practical use of their limited methods in the way demographers, rightly or wrongly, have, they thereby miss a certain critical edge which gives demographers a good idea of the applicability of their methods. We may surmise that, given the similarities in the situation of anthropology and demography and, indeed, in all of the social studies, anthropological analyses would be subject to a similar fate.

Needham notes three other paths to confirmation which are closed to formal analysis. There is no final recourse to the traditional concepts as expressed by participants; nor is the very general incidence of relations such as dualism, and the comparison this facilitates, a basis upon which formal expressions may be completely justified. Quite so. Finally, confirmation is precluded even if a particular formal analysis, based upon written materials collated by an author otherwise unfamiliar with the society, is reviewed affirmatively by the ethnographers of that society. Once such an analysis is a part of the written record it may set the terms in which the society is viewed, and thus influences whatever criticism it may receive; thus a negative review does not erase either the influence or the possible validity of such an analysis. There is not only no confirmation, there is no refutation.

This last argument acknowledges that the criteria of the validity are set by academic discourse; as academic discourse cannot be a subject of analysis without further recourse to itself, the problem of validity is intractable. This is not a very interesting situation, and it can be said to follow from the initial mistaken belief that the course of analyses or the course of events in society can and should be separated from the analysis of that society. We are in the habit of considering formal schemes as if they were wholly alien to language, while insisting at the same time that they are in important senses dependent upon language. And there is now a dangerous possibility that this specious separation will be extended to semiotics generally.

The separation of formality and language, of theory and observation, of observer and participant are all of the legacy of the separation of putative objective and subjective realities. While compelling and necessary to an idea of theory which involves a separation of levels of discourse, these distinctions are widely recognized as incomplete descriptions of analysis. Specifically, they exclude the possibility of understanding and following the influences of the analyst. This fixation is unintentionally extended into anthropology by the break which is posited between the mechanical, the prescriptive, the paradigmatic, and the myriad, the preferential, the syntagmatic. There is no rendering the preferential for itself, precisely because it is an ideal, created by the success, one might say by the naturally imperialistic tendency, of the paradigmatic tendency of thought.

Plainly this tendency will participate in any attempt we make to conceive of the flow of events. We need not fear, then, that we will lose hold of this faculty if we return, for example, to the position of Levi-Strauss, and say that all prescriptions are really preferences. We know they are of a special kind, but that is not all that interests us here. Equally we may return from the view that formal theory is essentially reductionist; there is no doubting that its use has been; but the applied use of formal ideas - here I have to bracket aside pure mathematics - is always embedded in linguistic practice, not to exclude semiotics generally.

The interest of semiotics is that at least it gives us a way of talking about non-linguistic and para-linguistic phenomena. Ardener's papers<sup>15</sup> show that the advantages of the distinction between prescription and preference may be subsumed in the Saussurian paradigm; and this gives us some idea of the way in which the congeries of events, such as in any 'play' of fertility, are determined. It does, however, leave the question of movement 'outstanding'. And while some place for formality has always been secure in the Saussurian tradition, the question of the manner in which formal methods are to be explored seems completely open.

The idea of semiotics originates, it could be said, in the hopeful anticipation that those aspects of experience for which linguistic description is inadequate may nonetheless be said to be 'related' or 'integrated' or 'systematic' or in some sense orderly. Semiotics are not completely articulated or articulable in language, and there is no reason to expect them to be. This poses the interesting possibility that insofar as these ranges of 'meaning' cannot be expressed in language without fundamentally changing them, anthropologists may need to develop other-than-linguistic modes for their interpretation. This is not to revert to some argument that, for example, to understand mimes and clowning anthropologists will have to become clowns - though that argument is not so silly. Rather, the theoretical rendering of semiotics cannot be entirely in language, though language inevitably participates; and it seems, through sheer want of other possibilities, that we are thrown back upon formal methods.

This is not to suggest that events that defy linguistic description are any more susceptible to, say, mathematical expression. There would be

little advantage to substituting the hyperstatis introduced by one for the other. The same is true for any idealized consideration of non-linguistic expression in itself, whether a notation is suggested for it or not. There is a possible danger of the assimilation of non-linguistic forms to linguistic ones, as an effect of semiotics. We may, to begin with, place inverted commas around statements that refer to say ritual or musical 'signs', 'languages', 'grammars', 'events' and so on: but we can expect these to fall away on all sides in a short time. It can only be hoped that the simply unsatisfactory quality of linguistic expressions of the non-linguistic will on the whole mitigate this. There is already a tendency to delegate the questions of use such as asked in this paper to rather unoccupied and, as we have shown, unoccupiable spaces. It seems important to insist upon the obvious fact that these ranges of experience to which semiotics are supposed to refer are not pure but composite: they are tangled mixtures of language (i.e. speech, writing, reading etc.), physical movement, machines and artifacts, of unstated and unstateable definitions of state.

If this is the case, then we are more or less in the position of the particle physicist: even if we can develop a formal notation to express events fundamentally different in kind to those of the language of classical mechanics i.e. ordinary language, we are still left the problem of needing some at least partial translation of these entities into language.<sup>16</sup>

This puts anthropologists in a fine quandary. They have for some years been aware that formal methods cannot hope and do not try to account for the subtlety and nuance of the images surrounding situations such as those of fertility. Anthropology, of all the sciences, has retained a hold on the fact that explanation is in language; formal analyses are satisfactory to the extent that they can be translated, for it is by their effects upon ordinary description that we usually judge their plausibility. Now there is this reminder of what was known all along: much (how much?) of what is experienced in thinking, believing, feeling, expecting and so on seems to resist depiction in language. Care and attention to language, essential as it is, is not merely futile but misleading insofar as it expects to be complete. The very questions anthropologists seek to answer, which concern the envelope of representation and physical action in which events are experienced, seem to fall very much at the edge of what can be said. Anthropology appears stuck between conceptual analysis it knows to be partially inappropriate and formal analysis in which it has no confidence.

We can already see that the walls of this predicament are paper-thin. Our tendency to speak of language as separate, as if linguistic expression were privileged and isolable, is really quite abstract and ideal. The metaphor of 'grammar' was apt because it helped to explore behaviour as if it were 'ruled'; we had the habit of speaking that way any way, even if it was not always grammar we had in mind and the social facts expressed in language seemed peculiarly accessible, at least when compared, e.g. to the expression of power by a charismatic leader or a dancer. A subtle change in our use of language is introduced as the more schematic and less immediately apposite aspects of the metaphor become acceptable: thus we have become accustomed to speak of 'behaviour' and even 'ruled behaviour'; we are not likely to be aware that we are wedding what we think to be a grammatical analogy to one that was chemical.<sup>17</sup> Formal schematizations are no different, even if more indecently exposed; still, that American families average 2.53 children scarcely raises an eyebrow. In both cases, however, there are operations in the background of the analogy whose schematic import is not so plausible: the linear form of speech and writing is hardly suited as a model for events which occur simultaneously in several dimensions; and no one would expect American birth rates to be fixed at their present level for the next 50 years. This description will not be too far from the facts: specialists accept schematizations as limiting cases, but as they further develop the analogies, their use, for several reasons, becomes more lax; usage passes into a wider

public, and the specialist will only rarely deny the acclaim for his image and likeness. What is true with the elaborate metaphor of a 'grammar' is true for analogies or schematizations generally, whether formal or informal: they are imperialistic. They are capable of replacing and reducing other classifications in language, as well as those manners of expression which do not fit into language. The elements displaced and the early stages of displacement may be erased; and the implications of the analogy are inevitably traced partially.

It would seem more fruitful to examine formal methods in the context of their use, that is, in the midst of linguistic analogies and institutional incentives, rather than to consider them only for their alienating effects in particular analyses. If semiotics are composite, we can expect the effects of language and formal methods in composing some aspect of the unexpressed much in the way the physicist uses mathematics to circumscribe sub-atomic phenomena. There would be somewhat less of a problem of assimilating these experiences to language given the less familiar and even peculiar sense of mathematical expression. And to understand such a rendition of events would require, as in the reading of most any mathematical text, a careful, step by step working-through of the relations. That is, it involves a reconstruction of the relations by the reader, which no doubt would raise many of the options and preferences taken up or set aside by the analyst in his own particular presentation. The greater emphasis this would place on reading would be welcome, and could turn it more into a simulation. Such a reading could only be a part of method, and it is to these questions of the relation of formal schemes to semiotics generally that we should now turn our attention.

Phil Kreager



Notes.

1. Lorimer, F. (ed.) Culture and Human Fertility 1954; Nag, M. Factors Affecting Human Fertility in Non-Industrial Societies: a cross cultural study 1962. The first of these includes writings by Fortes and others which attempt to relate ideas about the function of lineages and marriage practices such as polygamy to the categories of demography. All of the subsequent criticisms by Leach (Rethinking Anthropology 1961) regarding the irrelevance of matrilineal classes of societies and of universal definitions of marriage apply to these papers. Some problems of reconciling native expression with the narrow demographic view of fertility are described by Ardener, E. Divorce and Fertility, 1962. Nag's study was based upon the Human Relations Area Files; again the basic criticism of this kind of study is by Leach. The criticisms which might be levelled against yet another try with these materials would no doubt include the following: 1. the extreme variance in the reliability of these accounts, and of their location and the location of the societies to which they refer in history, precludes any point of reference for such a study other than some association with mankind in general; 2. the interest of this kind of general attribute of human expression for the understanding of particular situations would seem confined to some distant future, that is, to a time in which there will be more useful accounts directed to the specific purposes; 3. such an accounting, for its attention to representations, is very suited to the aspect of assessment, but as it considers process to be an inaccessible, internal aspect of individual mental operations, it removes essential aspects of fertility we wish to consider here. (cf. Needham, R. Belief, Language and Experience 1972: 156-9)
2. Needham, R. 'Introduction', Rethinking Kinship and Marriage 1971: lxviii.
3. Levi-Strauss, C. 'Preface to the Second Edition', The Elementary Structures of Kinship 1969: xxxi-xxxii.
4. Ardener, E. 'The New Anthropology and its Critics' Man 6:3 (1971): 450-1.
5. The misunderstandings of the various critics of his early papers on the Purum attempt to assimilate his use of formal ideas to more typical bases such as ideal types or statistical frequency; alternately, because these bases are what formal analysis is believed to imply, some critics seem to assume that if he is using formal methods at all he must be 'grossly manipulating' native representations. The main citations can be found in Needham, R. op. cit. 1971: lxvi-lxxx. A similar argument is made by Beattie in his criticism of Needham's interpretation of his and related ethnography on the Nyoro ('Aspects of Nyoro Symbolism' Africa 38 (1968): 413-42). Unfortunately, because Beattie evidently misunderstood the lack of necessity implied by the dual preferences listed by Needham we still lack a critical assessment of this way of handling ethnography as data by an ethnographer of the people concerned. The assumptions of method, however, were not stated until 1973 ('Introduction', Right and Left). Indeed, the materials on lateral symbolism have thus far not received critical attention. Korn and Needham (Man 5(1970): 393-420) critique recent mathematical analyses of kinship with specific reference to their inability to accord to native classification, particularly regarding prescription. This paper develops the opposition between the application of formal systems developed outside of anthropology to anthropological materials, and the use of a few particular formal ideas in the midst of analysis in which attention to representations has already begun. Some of the possible consequences of anthropological use of other

systems, with glancing reference to economics, are contained in his 'The Future of Anthropology: Disintegration or Metamorphosis?' in Anniversary Contributions to Anthropology 1970.

6. Leach, E. 'Magical Hair' JRAI LXXXVIII (1958):147-64.
7. Needham, R. op. cit. 1972:209.
8. Needham, R. op. cit. 1972:222.
9. Levi-Strauss, C. 'Social Structure', in Structural Anthropology 1963:283-9.
10. Needham, R. op. cit. 1973. I am concerned particularly with this most recent assessment; however, an example has also been drawn from his 'Right and Left in Nyoro Symbolic Classification', originally published in Africa 37 (1967): 425-51, but reprinted in Right and Left 1973: 299-341.
11. Needham, R. op.cit. 1973:xxxi.
12. Needham, R. op. cit. 1973:310-312.
13. Berlin, B. and Kay, P. Basic Color Terms, their Universality and Evolution 1969.
14. This is not to return to the mistaken attempt to make formal analyses of preferences refer to the character of events in a society as a whole. It is important, however, to realize the implications of the limits of this method which Needham so carefully articulates. Anthropologists appear to be merely reinventing the logic of demographic methods, without at least the benefits of validation which give such methods some value. If we are interested in the question of the rightness of such analyses, then it is necessary to consider the nature of their reference; insofar as such an analysis cannot be said to refer to anything, except a collection of isolated instances, we may wonder about the significance of 'Nyoro'. One may wonder, given the seeming shift from formal to conceptual analyses, why Needham did not take the opportunity of the republication of the papers on left and right to comment upon the nature of opposition which separates in the same way pairs such as these: normal, esteemed/hated; wealth/poverty; joy/sorrow; good/evil; good omen/bad omen; etc. (1973:328). Surely the conceptual ranges of each of these are very different; one wonders whether these brisk, formal glosses on the isolated situations he considers would stand up to the strictures of Belief, Language and Experience.
15. op. cit. 1971; also, 'Some Outstanding Problems in the Analysis of Events', 1973.
16. There is, of course, the additional option of not insisting upon some separation of levels of analysis and observation; this, as anthropologists have long been aware, is tantamount to doing something other than 'science'.
17. Ardener, E. "Behaviour": A Social Anthropological Criticism. JASO IV:3:152-4.

Conceptions of Women in Classical Athens

Women have always been fashionable in some quarters and even within Classics no-one could claim that their study has been a totally neglected one. Even before the present outbreak of interest in all things feminine, 'The Position of Women in Athens' had become a hackneyed, if minor, topic claiming its obligatory chapter in all general surveys of Greek civilisation, and not a few works devoted specifically to the subject. Now, of course, since it touches in its own small way on our current concern with the general issue of woman's place in society, it is fast becoming within Classics a new obsession.<sup>1</sup> For all that, I do not think that we have got very far; nor, to be frank, do I think that we shall ever know very much about Athenian women -- for a simple reason: we lack the right sort of evidence.

At first sight this situation might seem to have little in common with the treatment of women in anthropological writings. It could be argued, with some justification, that the minor role usually attributed to women in most ethnographies is the result of certain prejudices, or at least presuppositions, about the essentially 'masculine' nature of society on the part of the ethnographer (whether the ethnographer happened to be male or female). Now, I would certainly not argue that the classicist or historian was any the less prone to making sexist assumptions than the ethnographer; but the classicist or historian is not entirely at liberty to gather his data from wheresoever he chooses to look. He is always at the mercy of the biases of his evidence -- and of its omissions. In this sense, at least, he cannot exercise his own prejudices with quite the same ease as the ethnographer who, in a way, fashions the material he has to study. Thus it is scarcely the historian's personal responsibility that the evidence from antiquity largely neglects women. On the other hand, what the historian must continually confront, and take account of as a legitimate part of his material, is precisely the prejudices of those peoples who have chosen to record themselves for posterity, and which have endured in the written authority of their texts.

But, in the context of the study of women, this may mean that the historian's situation is not so different from the anthropologist's. Something of the ethnographer's traditional blindness to women has been explained by the simple fact that any presuppositions he might have held about the comparative social unimportance of women were likely to have been shared and reinforced by the views of the males of the society with which he was dealing and from whom he gleaned his information. If attention turned to women, both ethnographer and 'his people' were likely to have been engaged in a very similar process of 'bird-watching' (Ardener 1972; I hold no brief for the pun). Consequently, what the anthropologist ought to be accused of is not so much a failure to have recorded the social truth about women, but a failure to have seen beyond a social truth about women located in a reality constructed by men. We might feel some sympathy for him. For the ethnographer to have given women their due would probably have necessitated almost a refusal to participate in the observations of the society he was studying; it would have involved

an attempt to dispel precisely what he was trying to assimilate.

Now, whatever the correctness of the hypothesis that a society is not one, but two -- a male and a female -- and that beyond the dominant ideology of the male, which purports to account for the society in its totality, there exists another 'social reality' constructed by women, in which not only their own role but also the role of men might be significantly different, it is still manifestly the case that when we look to determine 'the Position of Women in Athens' we can claim to be determining only what Athenian men thought about women, how Athenian men represented women, and how rules and regulations constructed by men sought to define and locate the position of women within the male conception of society. It is for this reason that I have not called this essay 'the Position of Women in classical Athens', but rather 'Conceptions of Women in Classical Athens'. For all of what we do know about Athenian women comes from the representations and ordinances of men. And that, in certain areas, it is so very little, becomes in this respect a salient fact. Our evidence will not allow us to discover the whole truth about Athenian women. Nevertheless, like most ethnographers, though more honestly, and with less choice, we can still record a quite valid, but in every sense of the word 'partial' truth about them.

I have not mentioned all this simply to make nice distinctions about possible titles -- or to defend myself in advance for recording a view of women which, in our terms, might appear more than a little sexist. The difference between the naive view of social reality as a set of objective phenomena to be recorded, examined, and even judged, and social reality as a construct already replete with meanings given it by those who are both its substance and its essence, lies at the heart of any attempt to discuss what Athenian women were, and, unfortunately, of the confusions that have resulted from most attempts thus far.

Those who have written over the years about the position of women in Athens have tended to polarize into two groups which, for the sake of convenience, I shall call the 'pessimists' and the 'optimists'. The nineteenth-century orthodoxy, which still has its adherents, and which probably prevails, holds that in classical Athens women lived lives of cloistered confinement, that they were legally, politically, economically and socially restricted, subjugated, and suppressed, and that they were considered natural inferiors and generally held in contempt. The 'optimist' challenge, which started with an essay by A.W. Gomme in 1925, whose views were largely followed by Kitto (1951), Seltman (1956), and now by an increasing number of contemporary scholars, holds, to the contrary, that Athenian women were cherished and honoured members of the community. I hasten to add that the polarization is a tendency: scholars who have painted a bleak enough picture of Athenian women's lives have at times felt compelled to add that no doubt many Athenian men truly loved their wives, or something of that sort. And the 'optimists' could not deny that at least from a legal point of view women's position was a markedly inferior one. But in essence, a dichotomy of opinion remains. And it is perhaps worth noting that among recent writers, whether men or women, whose

interest in the subject has been aroused fairly obviously by a commitment to the present women's movement, this same dichotomy continues. Those who have an axe to grind are still a little uncertain as to whether it would be more profitable to their cause to show that, contrary to the general opinion, Athenian women played an important and recognised role in society, thereby, if not exactly awakening the ghost of primitive matriarchy,<sup>2</sup> at least proving a conspiracy of male scholarship: or whether, by adding an element of further indignation to the traditional view that women were suppressed, they should demonstrate yet again man's inhumanity to woman.

The real basis of the divergence of opinion is, however, an evidential one. It depends on just what sort of evidence they are willing to give weight to.<sup>3</sup> The pessimist view is largely based on a reading of the legal and forensic material, philosophical moralist writings, and what little can be pieced together from various sources about Athenian daily life and social organisation. The optimist view springs from a consideration of Athenian art, tragic drama, and 'myth'. I shall let Gomme speak for himself: "There is, in fact, no literature, no art of any country, in which women are more prominent, more carefully studied and with more interest, than in the tragedy, sculpture, and painting of fifth-century Athens."<sup>4</sup> Adherence to one or other of the opposed views then necessitates the mutual charge that the evidence on which the oppositions claims are based is either unimportant or unrepresentative. Thus Gomme would argue that the legally defined position of Athenian women has, a priori, little or nothing to do with the 'respect' and 'honour' in which women might have been held. Lacey, (1968) in a recent book about the Athenian family writes, on the other hand, that "Among the intentional omissions of this book are large-scale references to Greek Tragedy .... What the characters say (in tragedy) has no independent value for telling us about society, though very often it will support what we know from other sources to be true."<sup>5</sup>

Now, it seems to me that in general two sorts of things have gone wrong, and I shall deal with them in turn. Firstly, questions about the position of women in Athens have usually been posed from the outset, either implicitly, more often quite explicitly, in moral or evaluative terms. We are asked to decide whether women were, on the one hand, 'despised' and suppressed', or whether, on the other hand, they were 'honoured' and 'cherished'. To put it bluntly, the question is usually: "Did the Athenians treat their women decently or not?" Now this is an impossible question to answer; moreover, it is the wrong question to ask. One cannot read through Athenian literature and substantiate, in any empirical fashion; from direct statements about the matter, whether Athenians 'liked' or 'disliked' women; whether they went round 'honouring' or 'despising' them. There is a body of very misogynistic literature; but for every explicitly misogynistic statement, one can find another to the effect that there is no greater joy than a good woman.<sup>6</sup> This being so, the recourse has obviously been to evidence other than direct expressions of affection or contempt, from which classicists have deduced whether the Athenians honoured or despised women. But such deductions are, of course, based on a series of a priori judgements

about what sort of behaviour towards women, and indeed, what sort of characterisations of women, constituted an attitude of 'honour' or 'contempt'. Needless to say, the trading of opinion has been interminable. The same, of course, applies to 'suppression'. We can certainly say that in Athens, from the available evidence, a woman's life appear to have been a very much more restricted one than a man's; that she was not allowed to do, or did not do, many thing that a man did. And, by the way, I am not claiming that women were not suppressed. But, surely before we can talk of 'suppression', we must know whether the restrictions imposed on women contravened or frustrated their own desires. And this we most certainly do not know. What we have instead are the classicists' opinions as to how one ought to treat women -- and they show their differences.

"I can say all I have to say (for the women) in one short word of advice. Your great glory is not to be inferior to the way nature made you; and the greatest glory is hers, who is least talked about by men, whether in praise or in blame."<sup>7</sup>

Richter's comments on this famous passage (1971) exemplify the sort of confusions currently produced. Richter, an optimist, is intent on proving that this passage cannot be taken as evidence that the Athenians despised women. He argues (1) that the sentiments in this speech should be attributed to Thucydides the historian rather than to Perikles himself, and that they reflect a Thucydidean prejudice, since Thucydides regularly ignores women in the rest of his History; (2) that they can hardly be taken to carry a genuine misogynistic connotation, since clearly Perikles was no woman-hater. Richter reminds us of Perikles' notorious relationship with the courtesan Aspasia, and of the anecdote that on his death-bed Perikles sheepishly admitted to having kept all through the years an amulet some woman had given him; and (3) that Perikles' advice is just another expression of that characteristic Hellenic ideal of sophrosyne (discretion, prudence), on the grounds that any reminder to the effect that public familiarity with a respectable woman's private life might only compromise her would not be out of place in such an oration.

But while the substance of what Richter says is quite probably true, it functions only within the framework of a quite illusory argument. Exactly why a 'prejudice' on Perikles' part would be important, but, if expressed by Thucydides, can be ignored, is unclear. More to the point, the fact that Thucydides ignores women in his History seems to be quite in accord with the general Athenian exclusion of women from the public domain. But, that this should constitute a 'prejudice against women' rests on Richter's own ethnocentric assumptions. That Perikles was no 'woman-hater' seems quite reasonable, but whether he loved or hated women is not the question; the real question is what, in the male Athenian mind, was 'woman' which Perikles as an individual was at liberty either to love or to despise and what, for Perikles - or for Thucydides, or for Athenian men in general - constituted a good woman whom they could honour and respect if they so desired. Perikles' answer is clear enough; "the greatest glory is hers, who is least talked about by men, whether in praise or in blame." But there is no reason to

deduce from this that Perikles - or Thucydides, or the Athenians - hated women and there is consequently no need to waste time proving that in fact Perikles rather enjoyed them. The real question, and the only worthwhile one, is what for the Athenians constituted 'a woman'. We cannot presume that we know what 'a woman' is, and then proceed to judge whether the Athenians appreciated them or not. 'Woman' is both a cultural product, and ideological formation. What we must attempt to do is to situate the concept of 'woman' within the semantic field formed by Athenian society. Perikles' advice is another example of that most characteristic Hellenic ideal of sophrosyne -- what we must find out is how that ideal applied to women.

The second mistake is more troublesome. As we have already remarked, the real cause for the divergence of opinion about the position of women in Athens does stem from the contradictory nature of our evidence. How is one to reconcile the sheer prominence of women in art, imaginative literature, and 'myth' with the picture usually derived from the 'social' and legal evidence of their restricted role in other areas of Athenian public life? The Lacey approach is to say that tragedy, for example, has 'no independent value for telling us about society', and to simply rule it out of court. But surely Athenian art, the public performance of a dramatic festival, is just as much a part of Athenian social reality as a haggle about an inheritance before a court of law, or someone's wife sitting spinning in the women's quarters. The other approach is to say that since the representations of women in Athenian art do not accord with what we know of social practice, then obviously our knowledge of social practice is incomplete, biased, or unrepresentative. What follows from this is a continual attempt to explain away almost all the evidence we do have. Thus, according to Richter again, Ischomachos' painful instruction to his newly-wed wife<sup>8</sup> on the ways of the world as he sees them, since she knows nothing having been kept in careful ignorance by her family, has nothing to do with the Athenian attitude towards women, but stems from the fact that she is only a twelve year old girl and he is probably thirty. Yes -- but surely it is significant that, at least among the upper classes, girls were trained to know nothing and given in marriage to men twice their age. And surely it is significant -- not just an irrelevant legalism -- that throughout her whole life a woman was a perpetual minor to be represented in her every undertaking by a male guardian, her kurios, -- her father, her brother, her husband, perhaps finally her son, or their appointee. Independence of any sort was a legal impossibility -- not quite our conception of Klytemnestra, or Medea, surely.

Now, it seems to me that both the 'optimists' and the 'pessimists' are really making the same sort of implicit assumption: viz. that ideally all the evidence concerning women ought to be integrated on exactly the same level. That one ought to be able to arrive at an aggregate picture of women from all references to them. Manifestly this is not the case, and so they dismiss one or other half of the evidence as being either 'fictional' and hence irrelevant, or unrepresentative and incomplete. What I would argue -- and very simply -- is (1) that all the evidence must be taken into account;

(2) that we should expect, or at least hypothesize, that evidence about women which comes from the same society ought in some way to form a relatively coherent whole; but (3) that we should not expect that the evidence concerning women can be correlated by analysing it as if it all related to exactly the same level of social reality; as if we could place the evidence from 'myth' and Tragedy and art side by side with what we know about women from other sources. What we must attempt to do is to trace through various systems of thought, and behaviour, and representations and institutions in which women had a place, and to see how these systems relate to each other, and if there is any particular underlying concept of 'woman' which is articulated throughout. We do not have to fret because such figures as Klytemnestra or Medea or even Antigone do not seem to conform in their status and actions to the women of fifth and fourth-century Athens as we know them from other evidence, for there is no necessity to presume that what Athenian art is doing, or what Athenian drama is doing, or what the myths upon which both were based are doing is to describe the social conditions of fifth and fourth century Athens -- not, that is, unless we work on the naive assumption that the only purpose of all artistic expression is to realistically recreate the conditions of the society which created it. There is, however, a very real necessity to take account of the 'artistic' evidence: for, as an expression of the ideas, beliefs and values of Athenian society, it is itself most certainly a part of the social reality which we are attempting to understand and describe. Indeed, an exploration of visual art, drama, 'myth' may be our only way of gaining access to the semantic field within which the behaviour of Athenian men towards their women starts to make sense. It may lead us to comprehend what, for Athenian men, a woman was. It may indeed be invaluable for 'telling us about society', as Lacey puts it.

Very roughly, then, I intend to look at 'the Position of Women in Athens' from three different points of view; to analyse it on three different levels. Although each point of view will tend to concentrate on a certain type of evidence, it should be stressed that no particular piece of evidence is by definition ear-marked for allocation to any particular 'level'. We shall look at (1) 'social organisation' -- woman's incorporation or lack of incorporation into the official divisions and bodies of the state, the polis, and her role within the family structure. Under this heading we shall also have to include not only the legal rules which defined her capabilities and incapacities, but also her economic status and the degree of her participation in the less formally defined areas of social life; (2) what we might loosely call 'popular morality' -- the sort of explicit characterisations made in the writings of fifth and fourth century Athens about the nature, or 'personality' of women. Here we shall have to include some attempt to present certain of the characteristics, 'psychological' and behavioural, thought ideally to be the prerogative of, or fitting to, men; for it will be necessary to see in what way women were thought to be different from men in order to appreciate the meaning of those characteristics which men attributed to women; (3) finally, we shall look at what, with grave misgivings, I am calling 'myth'. This last heading requires a few immediate explanatory comments.



'Myth' is becoming a dubious word in Anthropology, so great are the confusions its mention immediately generates; and within Classics Kirk's two recent books (1970, 1974) have done their best to undermine the utility of the term. But I am not particularly interested in trying to 'decode' or 'interpret' or 'decipher' myths in the sense of trying to find out 'what they really mean'. Nor am I interested in trying to set apart a form of communication which is qualitatively different to other forms of expression -- to make distinctions between 'myth' and folk-tale or legend, or even history. Within the Greek context such distinctions are particularly unhelpful. The working definition of myth which I am using, and which is satisfactory for my purposes, is that myths are stories which, in the Durkheimian sense, are 'collective representations' -- stories which exist independently of, and prior to, their any particular telling by any particular individual; or, perhaps more importantly, that whatever their origins, they have passed into the collective possession of a whole society. This latter qualification allows me, I hope, to include Homer and even Hesiod; for if their works were individual creations, they became public possessions. What I am interested in is looking at the presentation of a series of standardised or fixed situations and events which, if not reflections of reality, were reflections on reality -- a series of imaginative orderings and shapings of experience which might expressed in concrete situational form attitudes and values operative in the mundane world of Athenian society. Such are not, of course, the prerogative of any special category of expressions which we might call 'myth'. The same can be found in certain fixed expressions of every-day speech, in a body of culturally standardised metaphors, or on the other hand in what we might want to pigeon-hole off as 'ritual'. In other words, what I am interested in is the symbolic expression of a series of cultural assumptions.

One further point must unfortunately be dealt with. Classicists never tire of reminding us that we do not possess a mythological corpus as such. Within the Greek context, myth, as we have it, is always a literary phenomenon -- for the most part the tragedies of fifth-century Athens. If this is the material we must work on, will it fit even our working definition? Are we not dealing with the products of individual, not to say individualistic, minds? In a sense this is true. Obviously the individual playwrights had their individual concerns -- political, moral, theological, not least aesthetic -- which they expressed through their works. They may even, like the modern writer, have been setting their ideas in opposition to public opinion, rather than celebrating it. Such is the concern of the literary critic. Nevertheless, with very few exceptions, the same body of stories is the constant recourse of all the dramatists to supply their basic situation, their common plots. And it is these recurrent situations which interest us -- at least under the heading of 'myth'.

I made the point earlier that no particular piece of evidence or category of evidence was by definition ear-marked for allocation to one or other level of our analysis. This is particularly true of tragedy. Here we must attempt a multiple reading of the texts. On the one hand we have the basic situation, the plot, the 'myth'

and it is here that we encounter those massive female figures whose prominence we must account for, and which must have some bearing on the Greek, or Athenian, conception of women, but which does not accord with what else we know of women in Athenian society. On the other hand, although no-one would claim that, as a genre, Greek tragedy was 'realist', nevertheless the tragedies do to some extent attempt to situate themselves within the actual social mores of contemporary Athens. The myths, transformed on stage into drama, into a context which demands a certain recreation of the mundane world, do make their gestures towards 'realism'. And here we do have some conformity 'with the evidence we know from other sources to be true' (Lacey) -- not in terms of the basic characters, or the major actions, but in terms of the humbler detail. Thus Elektra, whose actions certainly do not seem to conform to the habits of Athenian girls, who is about to avenge her father, Agamemnon, and help murder her mother, Klytemnestra, can still be upbraided by her peasant husband for talking to strange males outside the houses. As Gould pointed out, in this sense it is just not true to say, as Gomme did, that in tragedy women are free to come and go as they like. A description of reality intrudes and glosses the 'myth' where this is possible -- though sometimes it is not so easily done. Aeschylus' Agamemnon is an interesting case. Klytemnestra cannot, by virtue of the role she must play in the structure of the plot, conform to the realistic representation of an Athenian wife. But what Aeschylus does, is to translate this necessary non-conformity into a peculiar and individualistic character-trait of his heroine. Thus the chorus and characters keep remarking that Klytemnestra 'has the mind of a man', that she 'acts like a man'. And the chorus informs us, and thereby explains to the audience as if by way of an historical note that 'when the man is absent and the male throne empty, it is right to honour the woman'.<sup>9</sup> In drama, the myths do make their accommodations with the description of social reality; but both that which is 'mythic' and that which is 'descriptive' do, in their different ways, allow us to see something of the male conception of the position of women in Athens.

If we look at the available social, legal and economic evidence, and at what we can reconstruct of women's daily lives from the writings of fifth and fourth century Athens, then I think we can largely support the traditional view that women did live quite extraordinarily restricted lives. We shall leave well alone, however, any attempt to determine whether this meant that men 'despised' women.

To all intents and purposes, women were excluded from public life, -- with the major exception of their ritual and religious role, which deserves special attention. And one should bear in mind the emphasis that the Athenians put on participation in public life. Athens's economy was slave-based; a substantial liberation from the necessities of toil allowed the ideal that a citizen's first duty, and indeed his fulfilment as a human being, lay in his involvement with the life of the city. The word for private in Greek is idios; for what it is worth one might note the direction of its shift in meaning. But women, whatever their status in other terms, were not citizens, politai. They were not members of the citizen body; they had no right to vote, to speak, or even to be present in its

congregation, the ekklesia, the sovereign legislative and executive body of the state. Obviously exclusion of women from the political sphere is scarcely unique to Athenian society, and it is not the fact of their exclusion itself which is interesting. But given the degree of importance placed on political membership of the state within this radical and participatory democracy, then we might at least suggest that the exclusion of women qua women had a correspondingly more important place in their social definition. For the most part, they are not given even nominal membership. The feminine form politis is occasionally found, but in all official contexts where it was necessary to distinguish the mother, wife, sister or daughter of a citizen from other women resident in Athens (as it frequently was), then the term aste, 'city-woman', is employed. If man could be ideally defined as a 'political animal', woman definitely could not.

The anonymity of women is maintained even in law court speeches directly concerned with their claims to inheritance, or male claims through them. They are referred to as so-and-so's mother, so-and-so's wife, so-and-so's sister -- their definition as individuals is formed only by their situation within the network of their male relationships. Very rarely indeed are we told their actual names -- and then the context is usually derogatory. In fact, in a number of cases, it was quite possible for one party to flatly deny the existence of a certain woman who had lived only two or three generations back.

Their exclusion from the rights (and duties) of citizenship extends, of course, to more than 'politics' in our rather narrow sense of the word. Their legal status was that of perpetual minors. From birth to death they were under the constant guardianship of a male -- whether father, brother, husband, son or appointee -- whose presence was necessary for their every undertaking. By law, they could not personally engage in any contract whose value exceeded one medimnos of barley -- that is, they were limited to buying and selling only the smallest of personal items. Nor, I think, as in the case of male minors, did the presence of the guardian merely legalize contracts and sales which women made; whatever property a female had was under the jurisdiction of her guardian. Her consent was not necessary for any arrangements he might make. In a court of law a woman again could not give evidence -- or least, not directly. It was given by her kurios in her name. Needless to say, no woman could hold any administrative position in the secular organisation of the state.

In marriage also, a woman was totally subject to her guardian. Her father, or her brother, or even her deceased husband by will, i.e. the head of the household which she was resident, married her to the head of another household. The contract was made between the two men. The woman is transferred from the authority of the one to the other. No doubt a girl was able on purely personal grounds to influence the choice of a husband for her -- but certainly there is no legal provision for her to exercise any choice. And the expressions one continually encounters are that so-and-so married his sister with a dowry of so much to so-and-so. Of particular interest is the epikleros, the girl who is her father's heir in the

absence of male descendants. She is with-the-property, and claimable by her father's next of kin with the property. Although this is disputed, it seems that this was the case even if the girl was already married. Her marriage would simply be dissolved, and she would pass with the property to her father's closest collateral. This has upset some scholars, but in fact the position of the epikleros is no worse than any other girl's. In the one case she is married by her guardian to whomsoever he pleases; in the other, as an epikleros, she is adjudged by a court of law to her father's closest kin.

As far as property ownership goes, we have no certain case of any Athenian woman in the classical period owning land; and any such personal property as she did have would always be under the jurisdiction of her kurios. This applies even to the dowry. In fact it does not become her husband's property, and is not merged with his property; but while she is living with him it is completely under his control. In the event of divorce, or the death of her husband, the dowry must return with her to her natal household, and she will be remarried with it. Alternatively, it would pass into the control of her sons who must support her. Such seemingly wealthy and independent women as we do encounter are courtesans, in most cases non-Athenians. Even here the exact title of their property is dubious -- it might well have been exercised through the nominal ownership of their lovers.

The state, the polis, of Athens was not conceived of as an autonomous body -- that is, it was but the highest order of collectivity of what still might best be considered as 'descent groups'. Membership of the state was not determined at the level of the state, but at the level of these descent groups. Citizenship was an hereditary privilege. And it was patrilineal. One belonged to the same deme, and the same phratry as one's father. Now, the matter is slightly controversial, but I think that the evidence points towards the fact that women were not members of the phratries or demes; that just as they were not 'citizens', so neither did they belong to those descent groups whose membership defined membership of the state. A little caution is due here, however, because, apart from acting as basic organizational units of the state in a number of contexts, the main functions of the phratries, at least, appear to have been 'religious'. Cult worship organized at the level of the phratries would then in many cases involve women, affiliated to a phratry via their kurios, their guardian, whether father or brother, or alternatively husband in the case of married women. Nevertheless, I do not believe that women were ever considered in their own right to be members of phratries or demes.

In short, we could say that as far as the state is concerned, women are non-participating members. They are virtually non-persons, in this context. They are protected by the law, but they have no positive rights which they can exercise independently and on their own behalf.

If we try to look at the private life of Athenians -- and this is difficult to do -- then at least we can say that the bulk of the evidence does point towards the seclusion of women. Due allowance

has to be made for differences of economic status. There were ribbon-sellers and bread-sellers trading in the market. One would presume that peasant-farmers could not afford to keep their women out of sight. But as an ideal, and an ideal which could be put in practice by the upper economic groups, women were not to be seen in public unless accompanied by their kin, or by a slave. Religious festivals, funerals etc. seem to have been their major escape from confinement within the house. It is a very vexed point as to whether they actually attended the dramatic festivals. Within the house itself there were separate women's quarters; it appears that in some cases the husband and wife would sleep separately if she had a young child. At all events, men had plenty of other sexual outlets. Certainly no woman who was not a hired entertainer would ever be present at the dinner-parties and symposia which formed a vital part of male entertainment. Eating and drinking together with males who were not close kind could be used in court as partial proof that a woman was not a legitimate wife (who could beget legitimate children), but a prostitute or a courtesan. In Demosthenes 47., a delightfully sordid tale, the speaker can contrast two incidents, one designed to provoke the outrage of the (male) court -- that his opponent had at one stage burst into his houses in his absence and there confronted face to face his wife, children and old Nurse, but a neighbour, Hagnophilos, hearing the commotion will not enter because he has not the right in the absence of the kurios; the other to show his own decency -- for on a previous occasion, in the heat of argument, he had burst into his opponent's house, but, he adds, he knew that his opponent was a bachelor.<sup>10</sup> Finally, I will take an example from Kenephon's Oikonomikos. Ischomachos is explaining to Sokrates what a happy marriage and a wonderful wife he has. Indeed, he trained her himself, and he relates his instructions to his newly-wed wife in which he explains the relative roles of husband and wife, and the separate areas of their activities. It is for him to be always outside, organising the running of his estate, shopping in the city, conducting his affairs in the agora, both private and public, indeed, talking to Sokrates to improve his mind. It is for her to remain always indoors, supervising the household, receiving and caring for the bounty he will bring into it from his exertions in the outside world. If she needs anything, she is to tell him, or to send out a slave. Ischomachos doesn't exactly tell her about the birds, but he does go into great detail about bees -- the perfect model for his wife, the queen in the hive. And here we might note an interesting point: within the traditions of Greek mythology, bees are sexless, reproduced by spontaneous generation.

The distinction between slave and free in Athens obviously represents a major social division. But I rather suspect that the distinction involved more than simply a contrast between those who were legally slaves, douloi, and those who were legally free, eleutheroi. The opposition between slave and free was seen to apply not only to social categories of people, but to psychological or behavioural characteristics. A free man was someone who in every sense of the word was autonomous, in control of himself; a slave was anybody who had lost his self-control. The very strong aversion to any form of hired labour can be understood in these terms. A hired man had lost his integrity. It was not the work itself which was resented, but the external compulsion. And to call a

hired man a slave, a doulos, was I think, rather more than a metaphorical association with a class of people who by law were 'slaves'. A man not under his own command was a slave.

Now, when the Athenians talked of emotions, or passions, or physical indulgences in any pleasures, and when they talked of them in a disparaging fashion as they usually did, then the phrases they used were continually variations on the following: 'he has become a slave to his desires', 'he has been mastered and overcome by pleasures', 'he is enslaved by passion' etc. The frequency of these is quite remarkable.<sup>11</sup> The hostility to emotion, passion, pleasure, physical indulgence, is never on the grounds that such things are per se wrong -- but that they threaten integrity and self-control. They allowed the possibility that one might end up no longer master of oneself. We arrive at what, by some contemporary standards, might even appear paradoxical. For the man whose actions were unrestrained, was unfree - since obviously he had been enslaved by desires. The man who was truly free, was so by the command he rationally exercised over himself. It would seem that when the Athenians spoke of emotions, passions, etc., they conceived of them as something outside of and separate from their true selves, and which represented a potential threat to their true selves.<sup>12</sup> It would seem further, that they conceived of their true selves in terms of their 'rationality', and that it was their rationality, their logismos which guaranteed them freedom and self-control. Enkratia, self-control, was seen as a primary virtue upon which all other virtues necessarily depended. And enkratia was based on rationality, for passions and emotions are exterior forces which will enslave the self and deprive one of self-control.

It is of course possible that this was all only a 'façon de parler'. But I am inclined to think that a 'façon de parler' is always in some degree a 'façon de penser'. At all events, if we go back to the Homeric epics, then, as Dodds showed ~~some~~ years ago (1951), emotions and passions were clearly portrayed as external agencies inflicted on men by the gods. If we look at the drama of fifth century Athens, emotional forces are still conventionally portrayed in concrete form as external agents which attack the self. Again, to some extent we are obviously looking at elaborate poetic representations, which were even queried by the characters themselves when the moral implications of 'responsibility' or its lack were discussed. Nevertheless, the conventions were operative, And even in Plato and Aristotle, whose models of the psyche are comprehensive and include the emotions and passions, a very clear distinction is still maintained between the 'rational' and the 'irrational' faculties of the psyche.<sup>13</sup>

Now if we were to draw up a list of the characteristics conventionally attributed to women in Athenian literature of all types, then that list would pretty well appear as a list of the antitheses of the male virtues. There is nothing particularly note-worthy in this fact itself. But, what is interesting is that these female characteristics all cluster around incontinence -- a total subjection to emotion passion or desire; a complete inability to rationally contain these, ---or, perhaps more correctly, to ward them off. Woman, in short, was psychologically

incapable of self-control. She could not, by nature, be free. Such a characterisation was not necessarily derogatory it was simply a fact of life. A woman was irrationally jealous, vindictive, unforgiving -- but she was also irrationally loyal and loving. In Aristophanes she is regularly a drunkard and a glutton, incapable of resisting any physical pleasures. Almost everywhere she is sexually avid - and much more so than man, who could, if needs be, always resist. In fact, she was considered to derive by far the greater pleasure from intercourse. Woman was uncontrolled by herself, for she was irrational. And Aristotle bluntly states that the rational faculty of the psyche is, in woman, akurion, 'not capable'.<sup>14</sup> In other words, she lacked not only those masculine virtues compatible with civilised life, but rationality itself from which, for the Athenians, both those virtues and civilisation derived. Permanently under the sway of the irrational, she could even pose a danger to the society of men. And here we might note an interesting law around which the whole of *Isaia* 2 revolves. No man's will was valid if it could be proved that he was not of sound mind. And he was not of sound mind if he was attacked by madness, if he was senile, if he was under the influence of alcohol or drugs, and if he was under the undue influence of a woman.

Now, if all this was so, then I would argue that two options were open: Either woman could remain outside the bounds of civilisations and society to wander in the wilderness of her passions; or, she could be incorporated into society by being put under the control of those more rational than herself. Both options, I think, were taken. The latter is precisely the situation that Athenian law envisaged and that Athenian social organisation portrayed. Woman, whose rational faculty was a-kurion, 'in-capable', was placed under the permanent supervision of someone who was capable for her - her kurios. But, it is the former situation which, I think, we see displayed in myth, and in certain of the rituals of Athens.

Let us start with the most extreme cases: the Amazons and the Maenads. The Amazons are amongst the oldest and most well established of Greek mythical figures. Although there is no specific fifth-century drama which deals exclusively with an Amazonian story, there are frequent references to them, and they appear in Homer and are one of the most popular subjects of Greek vase-painting. Their appearance in other poetry is plentiful. Their general characteristics are well enough known: they are a totally independent female society, either keeping a subject and crippled male population for breeding, or having a working arrangement with neighbouring tribes; they are indomitable warriors, and they are exclusively horse-riders. Now, the actual geographical location of the Amazons varies considerably according to the version of the myth we are following, and according to the period when it was set down, and the familiarity of the Greeks with their geographical surroundings. But one thing remains constant: wherever the Amazons come from, it is from somewhere beyond the bounds of the civilised world -- from the extreme north, from the north-east around the Caucasians, from Asia, or from the south in Libya or near the Atlas mountains. Conceptual space is translated into geographical space. Women, whose nature places them outside of civilisation are, as an independent society, placed geographically outside the borders of the civilised world.

And the places which they are said to inhabit are not only distanced from civilisation, but are also infamous for their inhospitality and wildness.

I am fairly unashamedly employing Levi-Strauss' nature/culture dichotomy. Man is the rational 'political' being; woman is from the beyond, the inhabitant and representative of what lies outside civilisation. Here it is perhaps worth taking note of the fact that the Amazons are always horse-riders. So indeed were some Greeks. But cavalry by the classical period was an aristocratic anachronism. At all events, the Amazonian association with horses is almost symbiotic. And in at least one reference, they are characterised as eaters-of-raw-flesh.<sup>15</sup> We could, if we had time, trace through a whole complex of associations between horses, the eating of raw flesh, and uncivilised savagery. But to stay close to the Amazons themselves, it is interesting to note how frequently they appear in conjunction with those other creatures situated somewhere in between humanity and bestiality, the centaurs -- half horse, half man, perhaps semi-divine, and, with the notable exception of Achilles' mentor Khiron, the models of the savage and the wild.

The Greeks' most popular culture hero, Herakles, has dealings with the Amazons. His ninth labour is to steal and bring back Queen Hippolyte's girdle -- virtually, of course, sexual assault and subordination. But the Athenians' own culture hero, Theseus, is also involved in this expedition. The variations of the myth are complex, but at all events Hippolyte's sister, Oreithyia invaded Attica in revenge. In other words, the very first threat posed to Attica, newly federated and given political form by its founder Theseus, comes from the invasion of its territory by a horde of wild and vengeful horse-riding women, allied, we might note, with those traditional barbarians of the north, the Skythians. Athens won.

The Maenads, or Bacchantes, are of course the historicist mythographer's favourite. Again, their representations are manifold, but Euripides' magnificent Bacchae has secured their place in everyone's memory. Maenad, of course, is a reflex of mainesthai, to be frenzied, to rave, and, given the traditional Greek view of the matter, to be possessed or 'en-thused'. They are the worshippers of Dionysos, or Bacchos -- the god of wine, certainly; but more correctly the god of inebriation, of 'liberation' in general. His origins are mythically in the East. Euripides' Bacchae relates his establishment in Greece, his subjugation of Thebes. And the historicists would take this to have been in fact the case -- the importation onto Greek soil of a foreign Asiatic cult. But let us remember the place of the irrational in Athenian society. We do not have to go to Asia to find that it is outside civilization.

The Maenads, or Bacchantes, themselves revelled, according to myth, with Dionysos on the wild and rocky slopes of Mount Kithaeron. It is the women who, to King Pentheos' disgust, become enthused. Naked or dressed in skins, drunk with wine and with the god, the women tear wild animals from limb to limb and devour their raw flesh. Both the threat that this offers to civilisation and the accommodation that civilisation must make with the irrational is the theme of Euripides' version of the myth. Pentheos resists



Dionysos and suppresses his worship. Dionysos transmogrifies to a bull before Pentheos' eyes, drives him insane, dresses him in womens' clothes to spy on the Maenads, and leads him out of the city where, mistaken for a lion by the Maenads in their frenzy, he is ripped limb from limb, with his own mother, Agave, tossing and exhibiting in triumph the bloody remains of his wrenched off head.

Let us now look very briefly at some of the tragic and mythic heroines. Medea is one of the most awesomely evil figure of Greek mythology -- despite her relatively sympathetic treatment in Euripides' play. What is perhaps briefly worth noting is that this sorceress again comes from beyond the civilised Greek world. She is brought back by Jason on the Argo from far-off Kolchis by the Black Sea. She is introduced into the Greek world, married to Jason, integrated into society as the mother of his children -- except that it doesn't work. Mad with jealousy she murders her rival for Jason's love, Glauke, with a poisoned robe before killing her own children and fleeing on her chariot drawn by winged serpents. The story is interesting because it touches on certain themes presented elsewhere in a less exotic context.

The introduction of a woman who is from outside, who is a foreigner and who introduces uncontrolled and uncontrollable emotions and passions into the city where they rip apart the fabric of ordered male society is present even in so basic a set of Greek stories as those concerning the Trojan war. Helen, the most beautiful woman in the world, comes to Sparta, and then to Troy, -- Menelaus and Paris, both infatuated, destroy both their cities as a result. Helen, as the cause of the Trojan war, a creature of wondrous beauty introduced into both the House of Atreus and the House of Priam to their mutual destruction, is magnificently dwelt on by the chorus of Aeschylus Agamemnon. And there, she is also the lion-cub whose savage nature cannot be suppressed, and the bird whom the luckless boy, Paris, cannot hold.<sup>16</sup> Aeschylus' is a sophisticated rendition of the tale, but the imagery with which he supplements the myth tells the same story -- the fusion of the wild, the beautiful, the destructive, the passionate, and the female.

And if we turn to Sophokles' Trachiniae, where we meet Deianeira, perhaps the closest we come to a portrait of the self-effacing, loyal, and dutiful little Athenian wife, the same element that we find in the Medea story is still present. For Deianeira, pining at home for her promiscuous husband Herakles, sends him a cloak she has woven impregnated with what she believes, in all innocence, to be a love potion. But it is a poison which devours Herakles and eats his flesh away. And it is a poison concocted from the blood of the dying centaur who once tried to rape her. The myth re-asserts itself even through Sophokles' humanist and realist rendition of the tale. Deianeira, 'man-destroyer' as the name implies, kills Herakles through her passion with a centaur's poison blood. She gives the same 'don-fatal' as Medea -- and significantly a piece of woman's work, a woven cloak, like the cloak with which Klytemnestra ensnared Agamemnon, and the garment by whose weaving Penelope destroyed her luckless suitors. We might suspect that even the product of woman in her domestic role had

its dangers. And the domestication of women finds its symbolic representations also.

Even with common speech, women, or rather young girls, are wild horses to be yoked, saddled, mounted and broken in by marriage. Those are the clichés of Athenian talk -- they may also be clichés of Athenian thought. Girls are given in marriage for ploughing, and the sowing of legitimate seed. And if such goddesses as Demeter and Hera are clearly on the side of culture, then it is because they represent the appropriation, exploitation and domestication of nature for the purposes of civilisation. The Thesmophoria is exclusively a woman's festival in honour of Demeter; and it is an official and state-sanctioned celebration. But it is also the exclusive preserve, sanctioned by heavy penalties, of women who were legitimate married wives and matrons, of women whose purpose had been defined by the state and whose role was celebrated by it. In contrast we have the Adonia -- whose participants are in the main prostitutes, concubines, courtesans; which is open to all, whether slave or free, legitimate or illegitimate, and which was marked by the indulgence of sexual and sensual licence. Not sanctioned by the state, it also involved the temporary dissolution of civic roles and divisions and the return to promiscuity which was thought to be woman's natural inclination.

Finally, it may seem odd that if something like the nature/culture dichotomy is an integral part of the Athenians' conceptualization of women, that Athens' own patron goddess, and the patroness of civilisation itself, should have been a female, Athena. But let us note her peculiarities that, like the bees, she is sexless. She is Athena Parthenos, Athena the Virgin. And unlike that other virgin, Artemis, her virginity does not spring from an opposition to marriage, to domestication, and from a compensatory over-indulgence in the wild untamed world of the animals and nature; but from a genuinely androgynous and asexual and purely rational existence. She is transvestite. She is virgin. But she also sprang fully armed and fully formed, a parthenogenetic creation, from the head of Zeus -- to the intense annoyance, we might add, of his wife, Hera.

From 'myth' to social organisation, from the dramas of Athens to its laws, the male conception of woman is coherent. Its manifestations differ, as do its contexts -- and this is what must be recognized -- but if we have the patience to trace them through, then I think we can finally arrive at what, for the Athenians, a woman was.

I shall conclude with Kreon's words from Sophokles' Antigone, which draw together and identify more concisely than I could woman's position in myth and society.

"Anarchy, it ruins states, it dissipates the host, while discipline preserves the ordered ranks: therefore we must maintain authority, and yield no title to a woman's will."

(672-8)

Not Sophokles' own sentiments perhaps -- but the expression of a widely held conviction.

Roger Just.

Notes

1. I would like to acknowledge my debt to Prof. John Gould, whose Hellenic Society Lecture, Law, Custom and Myth: Some Aspects of the Social Position of Women in Classical Athens, June 1974, has greatly influenced this paper throughout, and whose opening remarks I follow here.
2. It still lurks in remoter regions, however. See C.G. Thomas' article 'Matriarchy in Early Greece, the Bronze Age'. Arethusa Vol. 6 (1973) 2.
3. Cf. Pomeroy 1973 p.141.
4. Gomme A.W. 1925, p.4.
5. Lacey W.K. 1968, p.10.
6. Gomme made this point well, but used it to discount the evidence of misogyny in order to argue that women were 'honoured' and 'respected'.
7. Thucydides II 45.
8. Xen. Oec. 7-11.
9. Aesch. Ag. 259-60.
10. These passages were admirably discussed by Prof. Gould of Lysias III. 6. where a man's sister and nieces are so nicely brought up that they were embarrassed even to be seen by their male kin. This passage, too, comes from an account of a house breaking.
11. Dover 1974, p.208.
12. Ibid. p.125.
13. See Fortenburgh 1975.
14. Arist. Pol. 1260<sup>a</sup>.
15. Aesch.. Supp. 287.
16. Aesch. Ag. 399-455 and 681-749.

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### The Self and Scientism

In this paper I examine some of the background to the debate about subjectivity in participant observation; the primary methodological technique in empirical anthropological research. Earlier versions of this paper given at seminars included more detail of my own field work experience. This I have had to leave out for more elaborate analysis in future writing. Meanwhile I have presented some of my field work as examples in the approaches explored and suggested for participant observation. There is a need for more explicit recognition of field work as personal experience instead of sacrificing it to a false notion of scientific objectivity.

The problem of subjectivity in research is recognised by most contemporary social scientists mainly to forestall criticism and further argument. The traditional response is to refine the 'objective' methodology by formally eradicating the direct link between observer and observed. For example, the questionnaire method in much empirical sociological research predetermines the subject matter and questions arising and information transmitted is selective and curtailed. This premeditation and control over interaction is presented as proof of objectivity. The questionnaires are administered by assistants (nameless and usually female) and the 'hard data' written up by (named and usually male) research lecturers. The larger the sample and the more random the selection, the more 'scientific' the findings. The people interviewed are usually willing to volunteer fundamental and unpredictable insights which are merely jotted down under supplementary 'remarks'. Even in more informal unstructured interviews the inquisitor never abandons his dominant role. Other information acquired in less formal contexts is referred to as 'impressions', and 'soft' data to be tested by the hard data. (See Young and Willmott 1962: Appendix). The method is inherently authoritarian.

In anthropological participant observation there is greater reciprocity in the exchange of information. Here the problem of subjectivity becomes explicit. The field worker, as opposed to those who analyse other peoples' material, has a peculiarly individualistic and personal confrontation with 'living' data. This close contact has made anthropologists feel vulnerable to criticism from those who employ formal techniques of distancing between subject and object. Hence the peculiar coyness which anthropologists have shown in discussing their relationship with the various people they have studied.

The participant observer does not deliberately impose preconceived notions of relevancy and ready worked hypotheses on the data to which he has access. Despite criticisms from the formalists, this absence of filtering is the source of strength. The individual is open to a complete range of information and not merely what people say they do.<sup>1</sup> This material is of course analysed in the

light of existing anthropological theory but not prejudicially at the field work stage. In distinguishing the methods of sociology and anthropology, Maquet (1964) has justified the use of participant observation on purely technical grounds. In non literate societies, written sources, written questions and answers were not feasible and the totality of customs largely unknown to the observer, thus requiring long stays and 'indirect' observation. Such methods were unnecessary in the study of 'one's own' literate society ... 'where the whole culture is taken for granted'. The broad difference in techniques which Maquet describes might indeed have this historical foundation, but their merits cannot be judged solely in terms of their subject matter. Participant observation is equally valid in 'one's own society'. To take the whole culture 'for granted' is also to be guilty of subjectivity, more insidious because it goes unrecognised. Both the study of the observer's and another society involve subjectivity, but of a different order. I wonder if the belief in objectivity attained by studying another society is unconsciously explained by geographical not theoretical distance.

As in dry research methodology, the participant observer does have a problem of subjectivity. This cannot be resolved by distancing, repression and short cuts to abstractions. Objectivity is an ideal model to work with, not a fact. In the study of human being by another human being, (and what better medium is there?), the specificity and individuality of the observer are ever present and must therefore be acknowledged, explored and put to creative use. It is fashionable now for authors from a variety of disciplines to give an apologia or acknowledgement of his or her ideological stance as Marxist, liberal structuralist etc. in the preface. This kind of confession is no substitute for the continuing and conscious working through of these implications. Similarly in psychoanalysis, it is not enough for the patient to be labelled; his situation has to be examined and understood through hundreds of hours of analysis. Political interests are also now made more explicit or better understood. This applies especially to anthropologists looking back on the colonial era. (Macquet 1964). Less attention is devoted to the individual characteristics of the observer as important subjective factors conditioning knowledge. In any case in anthropological research, few analytic tools or categories have been developed to explore the various forms of subjectivity.

So far, the remedies suggested by anthropologists have mainly involved greater external control rather than any creative use of the observers' individual resources. To deal with what he called 'the personal equation', Nadel considered the selection of anthropologists on the basis of 'psychological testing' (1951: 50). I wonder by what culturally loaded criteria would candidates be deemed suitable, and for which culture? As another means of 'overcoming the limitations of the personality' Nadel suggested teamwork.

While considering Fortes and Evans Pritchard's awareness that the 'passing through a single mind' of the data is peculiarly valuable, Nadel asserts: 'once more we must face the issue that science cannot in any respect, be private' (1951). Again, as is the case with many others, impersonal verification is confused with objectivity. (Macquet 1964).

Given this distaste for privacy in science, it seems all the more extraordinary that the anthropologist's private and personal experience of field work is not exposed to view. Since almost nothing about the people studied is dismissed as private, taboo or improper for investigation, the same should apply to the investigator. I am not suggesting that everything be then put into print or theses for public scrutiny. Already many things in field notes must remain confidential, are later dismissed as trivia or disguised. The problem is that the fieldworker's personal reactions and experience are suppressed or dismissed from the outset. A certain personal exposure may in time be seen not as professional disaster but intellectual growth. Revelation of the 'humanistic and experiential' elements of field work has been advocated on moral grounds; as an exploration of moral relativism (Swallow 1974: 58). My reasons for advocating this aspect of field work are not rooted in morality, but directly to the epistemological problem of subjectivity which is perhaps implicit in Swallow's discussion. Too often the personal is represented in opposition to the objective, when the latter merely conceals the personal in pretentiousness. This dichotomy of knowledge is reflected in the sexist division of labour and knowledge in our society. Women are less inhibited about exploring and expressing the personal element, although they are expected to apologise for this in academic debate. Women are more likely to comprehend a theory through an example or image whereas men will grasp a theory through generalisation; given the different upbringings of males and females and the exclusion of women from direct economic and political power, only indirectly obtainable through personal relations with individual men, it is not surprising that the consequences are expressed in mode of thought. Arais Nin describes the polarity in her diary:-

'Now analysis is revealing how little objectivity there is in man's thinking... Man generalizes from experience and denies the source of his generalizations. Women individualizes and personalizes, but ultimately analysis will reveal that the rationalizations of man are a disguise to his personal bias, and that woman's intuition was nothing more than a recognition of the influence of the personal in all thought'.

(1967 Vol. II: 23-24)

In this quotation I interpret the concept intuition as culturally loaded, not as something inherent in all females. If as anthropologists we accept and explore different modes of

thought in different societies, we should not be averse to finding them within one society and in neither case does it follow that these variations are genetically determined.<sup>2</sup> Actually women lose out on several accounts. In a context where the specific is described as 'hard', scientific and objective fact, it's opposite is 'airy fairy' speculation, emotional and soft - womens' domain.<sup>3</sup> In another context where fact is equated with 'vulgar empiricism' and it's opposite is theory, women are seen to be the fact gatherers and men the theoreticians.<sup>4</sup>

Rodney Needham in his discussion of lateral symbolism points out that the dualism in this debate 'is a central issue in any humane discipline and as an essentially philosophical problem it may not admit any definite resolution. It has to do with the variable meanings attached to such abstractions as "fact" and "theory"' (1973: xxxi). In the case of our own society the 'fact'/'theory' dualism is transposed to the female/male division which corresponds to a political and economic actuality and is reinforced by self-fulfilling ideologies. Whatever 'female thought' may be, it is the one which is undervalued. The same goes for so called 'primitive thought'. There are parallels between the kind of thought which Arais Nin associates with women and 'the science of the concrete' discussed by Levi-Strauss (1966: 15-22). The participant observer is not at once removed from his material. His method involves working through images and anecdote.

Nevertheless whether through scientific or sexist bias, the personal is often denigrated in anthropological monographs. The 'I' of the observer sometimes disappears altogether as though the material was acquired by impersonal procedures. The classical handbook 'Notes and Queries' (1967) tells the fieldworker that really only amateurs suffer from 'bias'. 'Scientific' training successfully obliterates cultural and personal history and presumably the self (1967: 27).<sup>5</sup>

Evans-Pritchard's considerable insight into both his methods and personal form of 'Nuerosis' (1940: 9-15) and indeed Malinowski's first public account of field work (1922: 2-25) have not always stimulated anthropologists to give more or even as much information about their research.<sup>6</sup> From the 1960's a few anthropologists have presented more autobiographical accounts. (Turnbull, 1961; 1973; Maybury-Lewis, 1965; Read, 1965; Mead, 1972; Chagnon, 1974). Earlier in the most explorative and sensitive account of the relationship between the outsider and people encountered in fieldwork, Smith-Bowen (1954), felt obliged to fictionalise events and persons and publish under a pseudonym, so reaffirming the tradition of separating the 'subjective' from the public body of academic work.

In some of the publications, the anthropologist emerges as narrator (Conrad style) and actor along side other



characters in the exotic setting. Detailed descriptions, normally reduced to the opening pages of monographs, are given of scenery and personalities. The stream of events is action packed. Often the only structural unity of the narrative appears to be the chronology of the stranger's visit. This is a new kind of ethnography based on 'true story', subject to limitations of which literature is free. Such techniques may be a welcome rebellion from the depersonalised monographs of the past, but they have not yet resolved the problems of subjectivity in participant observation research.

Take Colin Turnbull's study of the *Ik* (1974) which on the basis of long term observation, is an informed rival to the genre of travelogues so despised by Levi-Strauss in his own autobiographical account 1955 and 1963: 17-18). Turnbull considers it right that 'any description of another people ... is bound to be subjective'. Since he had no access to previous accounts of the *Ik*, he believes that he started with 'a clean slate', without 'a preconceived notion', just 'clinical observation' (1974: 13). He omits to mention his preconceived notions about all human societies which he brought in his own head and landrover. His personal asides are presented as universals which we readers are supposed to support. They are no more than his own common-places; our intellectual journalist reporting. His subjective exposure lacks any self analysis and he certainly can't analyse his companions. For instance, he interprets laughter as merriment never as hysterical distancing or catharsis. Smith-Bowen never made that mistake, perhaps because she let herself experience the same.

Napoleon Chagnon has attempted to preserve 'an intimate relationship between ethnography, methodology and theory' (1974: x) and in two chapters (1 and 5) gives a personal account of his fieldwork. There is virtually no relationship between these chapters and his main work which consists largely of data gleaned by the latest technological gadgets. Details of his fieldwork are justified more in terms of technical/precedural problems of data collection; i.e. which villages he could stay longest in, why he had to leave others, rather than any theoretical link between the self and others. Discussion of the observer's experience is described as 'the non quantifiable aspects of fieldwork' (162), thereby conveying the feeling that if something can't be counted, it demands no alternative analysis description. If, as the preface claims, Chagnon is trying to show how the exotic becomes commonplace (page: viii), he fails. His form of personal revelations exploits on every page the exotic or bizarre as would be understood by the North American or European general reader. The chapters make exciting and sensational reading. We are rarely presented with the range of Chagnon's inner feelings, instead his escapades and heroism in the pursuit of science. One night the natives were going to crush his skull, another time he caught a 'raging' fungal infection of the genitals,

after borrowing a man's loin cloth. The opening paragraph titillates the reader's fantasies about Shaman and savages. Familiarity in the exotic is conveyed not by his description of the people around him, but by the 'commercial breaks' for his IBM printout and the Tri-x for his Rentax. Our Napoleon from Pennsylvania is better than Biggles or the Boy's Own Annual.

Having been so unkind about others, at this point I should show myself amenable to self examination especially in relation to the kind of part an anthropologist imagines he or she is playing in the field. Among the Pygmies, Turnball was romantic, among the Ik a cynic, with the Yanomamo Chagnon appeared hero; what was I, a lone woman living with the gypsies in southern England? I could tell some stories exotic precisely because they are set not in rain forests nor deserts, but on the edge of greater London just up the M.1. My tutor, on reading my notes exclaimed, 'They're more violent than the Dinka!' I plan to describe the strangeness; but not telescoped and wrested from the commonplace and imponderabilia of everyday. For the moment let's dissect what I thought I was at, in fantasy not ethnography.

Early in my anthropological studies I was impressed by a common assertion among both men and women anthropologists that the female anthropologist in the field is not 'hampered' by her sex because she is treated as an 'honorary male'. I didn't realise at the time how deeply this appealed to deep contradictions in my own history. This belief is confirmed in 'Notes and Queries': 'Among very unsophisticated natives ... a woman may find that she is regarded primarily as a stranger and is given the status of male' (1967: 30). To my surprise and perhaps disappointment, when I entered the field I did not find this so. I had to be extremely cautious in talking to men, usually making sure that a woman was present since non-gypsy women are regarded disapprovingly as licentious and immoral, just like the non-gypsy (gorgio) stereotype of gypsy women. Women, not men, had to be my main informants and allies. The rigid male-female segregation meant that any woman seen talking alone to a gypsy man who was neither kin nor husband, was accused of sexual infidelity. I had to be careful and more conscious of myself as female and began to wonder if this was so different in some other field work situations.

Increasingly, I suspect that women anthropologists are given ambiguous status in the field, not as 'honorary males', but as members of an alien race. So where did this 'honorary male' come from? The idea has its roots instead in the anthropologists' own society where the ideals of male and female behaviour are as marked. It requires some nerve among women of my society to travel alone without plans and timetables.

"Adventure is pulling me out. When a man feels this, it is no crime, but let a woman feel this and there is an outcry."

Anais Nin (1974 Vol. 2: 51)

Now for self investigation:- In the single sex boarding school where I was educated from the age of nine to eighteen, the 'world' was divided into four 'houses'. These cosmological institutions were not represented by any buildings; they were groups of girls competing for cups in field sports, conduct, deportment and drama. Most significant to this discussion; the four houses were named Rhodes, Livingstone, Shackleton and Scott. I belonged to the last. So our models were all white colonisers or explorers. The trouble was they were all males. These heroes, not heroines set an example to which we, as Penelope was to Ulysses, could never, nor indeed, should ever aspire.

Brought up only to marry or beget a Scott or Livingstone perhaps I found my journey into Gypsydom especially exhilarating, although I rejected entirely the idea of being a coloniser. I hurtled down motorways in my fifteen hundred weight van, I loaded heavy scrap iron and dwelt with strangers and nomads, so escaping the strictures of the domestic role allotted the females of my own kind. In addition, this rite de passage made me an honorary male among those back home. By this experience, the female anthropologist not only achieves equality in her own society, she might unconsciously feel a confident separation from the domestically burdened wives and mothers in the society she is studying.

Whereas the female anthropologist, not accompanying a husband, is rejecting her conventional destiny by the act of fieldwork, the white male anthropologist is completing his (See also Levi-Strauss (1963: 42) for a discussion of the function of travel among young French men). That is why a little more self awareness in motives might be helpful to both male and female anthropologists in the presentation of the self in their fieldwork confessions.

It is ironic that the man who first developed and gave scientific status to participant observation in anthropology also kept a personal diary, which has caused only embarrassment or been overlooked as an invaluable adjunct to field work by his colleagues and academic descendants.<sup>7</sup> Malinowski's diary (1967) is a remarkable case study of the concealed subjectivity in field work method and general ideology, distorting both evidence and theory. Still today personal and cultural conflict in the field are relegated to the anecdotal and oral traditions of faculty gossip (Swallow, 1974). Thanks to the consent of Malinowski's widow we have a record of his on the spot reactions. Among other anthropologists their reactions are usually only recalled after field work and therefore changed in their retrospective autobiographies.

Unfortunately Malinowski did not use his diary in the way which I shall advocate - as a means of exposing and exploring subjectivity. Instead he used it as a privatised escape from the fieldwork situation to maintain a 'sane' and familiar internal order in an alien land, and as a punitive stimulant to his rigid work ethic. His dependence on the norms and values of his own culture were at risk; for example he was worried he might forget about academic commitments and the obligations of sexual fidelity. The diary became an internal dialogue with a culture-bound conscience where the private and subjective were artificially separated from the professional and objective. On the one hand he was publicising and eliciting appreciation for the Trobriander's institutions in his official writing, on the other, he was contending with his hostility to an alien race in terms similar to those of his colonial contemporaries.

There are four aspects which it would be useful to isolate in Malinowski's diary.

- 1) Attitudes to women and sex (the two for him were rarely separated).
- 2) Personal feelings about the Trobrianders.
- 3) Interaction with local white men.
- 4) Ideas about keeping a diary.

1) He expresses a longing for a white woman, in particular his future wife whom he considers has 'the miraculous power to absolve sins'. Raymond Firth cites this as proof of the depth and sincerity of his love (1967: xviii). This woman is identified with white civilisation (Malinowski 1967: 148). But he also has lustful feelings for another white woman with whom he cannot break contact. His conflict seems to be the classical one in western civilisation between the pure woman (wife) and the sexual fiend (whore). On a day to day level, Malinowski is confronted with his feelings towards the black women who became the victims of his projections and concepts of the whore. Sexual relations with them are seen as 'whoring' and 'sloshing in the mud' (181). Sometimes he confesses to his sense of their beauty (255) but bitterly regrets having 'pawed' one (256). His diary serves to goad his conscience, and control any deviation from his self imposed sexual code and that of his own civilisation. This can sometimes only be achieved by negating sexuality in women: 'Moral tenets: I must never let myself become aware of the fact that other women have bodies, that they copulate' (1967: 249). The tendency for women to be seen mainly as sexual objects may well have encouraged anthropologists to avoid or underestimate them as persons and informants (see Ardener's discussion 1972: 137-138).

For a long time I was guilty of a kind of sexism in my own field work. At first I considered my segregation among

gypsy women only as disadvantage. I was always trying to get through them to the men where I presumed all the action was. It was only gradually that I actually became aware of the important political and economic role of the gypsy women in their own right and precisely because they were women. My initial prejudice I have tried to rectify (Ohely 1975). My field work mistakes arose partly because of a belief in the separation of my 'personal' political views on feminism and my 'objective' role as researcher in another culture. After the London Womens' Anthropology Workshop in 1973, I suddenly saw that the two were interconnected.

## 2) Malinowski's feelings about the Trobrianders

In its strongest aspects, he might now be accused of racialism because of his use of the word 'nigger', just as he could be accused of sexism in his indiscriminate use of the word 'whore'. The first time 'nigger' appears in the published text (1967: 154) there is an evasive footnote giving Webster's definition, and designed rather naively to take away the full impact. If as the editors claim, the word was non pejorative for Malinowski, one wonders why he should have so assiduously excluded it from his public texts.

Malinowski's use of the word in a private context is interesting to the reader because it indicates that he was a carrier of the stereotypes and underlying values of his own culture, even though he wanted to take the Trobriand culture seriously. Obviously the strain and stress of field work, for example, the personal isolation and 'alien' life style are bound to bring out the 'worst' in anyone, but that does not explain away the cultural form which the 'worst' takes. Irritation with members of one's own race and one's own sex would be expressed differently; the peculiar faults of the individual rather than his or her social category would be exaggerated and deframed. In examining the dilemmas faced by field workers, a psychologist, Wintrob (1969) tends to examine problems of ambivalence, racialism and questioning of motives more as symptoms of stress rather than as valid problems in themselves. Malinowski's 'racialism' cannot be described simply as stress, it must also be explained in terms of white man's 'culture'.

The contrast between Malinowski's professional or intellectual aims and his private feelings, which reflect his own culture, appears in a single page. He considers composing a memoir on 'the value of Ethnographic Studies for the Administration ... above all the knowledge of a people's customs allows one to be in sympathy with them, and to guide them according to their ideas' (note there is the presumption of 'guidance'). Then after been misinformed about a kula expedition, Malinowski expresses 'hatred for the niggers' (1967: 238). He is not sufficiently self conscious to set his annoyance at the natives' independence in the context of white/black relations.

As a role whose country suffered Austrian rule, Malinowski was, as Lucy Mair suggests (1957: 232), able to identify with oppressed minorities. However this may have blinded him to his status in Melanesia. Deracine he became a cosmopolitan communicating with Europeans, Americans and Australians and speaking their language. His identification was limited mainly to white men of the industrial world, so his racial status and origin became more important than his nationality. In the field therefore, he was identified by the Trobrianders as a colonial and they obviously concealed information from him which might prejudice their position. Elsewhere I have examined how the Trobrianders' apparent ignorance of paternity, as told to Malinowski, might be explained by his status as white man unwittingly associated with the missionary's decrees on sexual behaviour (Oxford Womens' Anthropology Symposium 1975).

Malinowski makes an explicit connection between his personal reactions to a Trobriand individual and the policies of a colonial oppressor (1967: 279). This is followed in a manner comparable to free association, by a reference to sex between the races, more importantly his dismay at a white woman's relations with a black man. Racism is thus interwoven with sex and sexism, and all are symptoms of white male chauvinism. Whereas white male/black female sexual relations may be 'sloshing in the mud'; a regrettable past-time, black male/white female sexual relations, in any analysis of white man's racialism, is the ultimate taboo, since it undermines the presumption of white male 'supremacy' and white female 'purity'. In this racist and sexist system, where the male is considered superior to the female, it also gives alarming superiority to the black man over the white woman. This passage in the diary of an individual indicates the links explicit or unconscious between the particular or subjective and the general.

### 3) Interaction with local white men.

Malinowski's diary reveals a similar ambivalence or hostility to white men in the Trobriands as he does towards women of any race and the native Trobrianders. To the lieutenant governor he is obliged to be deferential for the sake of his visa, but regrets that 'paying attention to this crew simply banalises my work' (1967: 128). Firth praises Malinowski's thumbnail sketch of this 'legendary figure' (1967: Introduction), but I am left dissatisfied. This man at the 'apex of the official pyramid' could have been considered as worthy of as the Trobrianders as a subject of study. Malinowski accepts the white administrators' 'power over the natives' (1967: 167). Yet he is always trying to eradicate them; 'What is terrible is that I am unable to free myself from the atmosphere created by foreign bodies: their presence takes away the scientific value and personal value of my work' (1967: 163). On the contrary, his amnesia towards the white administrators had considerable repercussions on the 'scientific' value of his analysis. The

Trobriand society was over represented as a functional whole, with economic and political self-sufficiency.

The Trobriand Islands were inhabited not only by white administrators but also missionaries and traders. Instead of pursuing the consequences of these immigrants' invasions for Trobriand society, Malinowski sees his relationships with the white men as personal intrusions on his objective research. He continually tries to avoid intensive conversations with whites, especially with the trader Raffad whom he finds so intelligent and sympathetic that he fears the man might become his 'main subject of study' (1967: 264). The ideal model of the isolated, simple society didn't exist, even at the outset of intensive anthropological field work.

Like Malinowski, I found myself, at the beginning of field work, trying to blot non-gypsies or gorgio administrators from the landscape. I saw them as useful sources of background information, a way in to the gypsies, rather than as important constraints within gypsy society. If I had more self-consciously analysed my personal desire to disassociate myself from these petty gorgios, I might have recorded everything about them and treated their words and actions as equally if not more 'exotic' than those of the gypsies. Again I have tried to rectify this in later analysis (Okely 1975a: Ch. 2).

#### 4) Malinowski's ideas about keeping a diary.

As I have elaborated above, Malinowski used his diary more as an escape from the field than as an intellectual tool in research, yet in one astonishing passage he recognises its potential:-

"a diary is a 'history' of events which are entirely accessible to the observer, and yet writing a diary requires profound knowledge and thorough training; change from theoretical point of view; experience in writing leads to entirely different results even if the observer remains the same - let alone if there are different observers! Consequently we cannot speak of objectively existing facts: theory creates facts. Consequently there is no such thing as 'history' as an independent science."

(1967: 114).

It is regrettable that he did not extend the diary's function as the link between subjectivity and 'scientific' participant observation.

As in social anthropology, the discipline of psychoanalysis is exploring the problem of the analyst's subjectivity, 'notably because the treatment has come more and more to be understood and described as a relationship' (Laplanche and

Pontalis 1973: 92). The technical term 'Counter-Transference' refers to the analyst's unconscious reactions to the individual patient. Freud stresses that 'no psycho-analyst goes further than his own complexes and internal resistances permit' (1910 cited in Laplanche & Pontalis). Hence every analyst has first to undergo analysis. Techniques of dealing with counter-transference take several forms: to reduce it as far as possible by personal analysis; to exploit it in controlled fashion as a guide to interpretation. The analyst's unconscious is seen as the ideal means to understanding the patients' unconscious (Laplanche and Pontalis 1973: 92-93).

Social anthropology might explore analogous methods. Subjectivity as influenced by individual personality, cultural history and gender should be analysed not repressed, and exploited for finer observation and interpretation. It would be of additional value if the anthropologist had undergone personal psychoanalysis, but this is not the core of my suggestion. Whereas the patient is battling largely with his personal history, the anthropologist is also battling with his cultural and social history. And here psychoanalysis has something to learn from anthropology. The anthropologist as participant observer is, like the psychoanalyst, involved in a relationship; this time between the self and many others; between two cultures. There is a problem of cultural counter transference.

The methods which I tentatively advocate for confronting and making creative and theoretical use of this relationship are as follows:-

- (a) Self-analysis
- (b) The Diary
- (c) Autobiography.

(a) Whereas a diary is usually a record of conscious thoughts and experiences known to the author but concealed from others, the kind of self analysis recommended by Karen Horney (1962) demands the discovery of unconscious links in thought and experience. 'This fundamental disinterest in the self is one of the great difficulties in self analysis (1962: 144) and 'the real difficulty is not that of intellectual understanding but that of dealing with resistances' (1962: 146). In this context I would describe both Malinowski's and my own dis-interest in administrators, white men or gorgios as a cultural resistance.

Since thought moves faster than the pen, it would be useful to jot down key words, concepts and images, as well as apparently disjunctive free associations. All these may bring insight at the time or at a later date; a structural analysis of the self.

(b) The Diary as a means of self exploration should be regarded as an essential part of field work methodology.



It could be the place for the key words and jottings of self analysis, but in addition the place for more conscious thoughts and experience. No anthropologist would consider writing a retrospective monograph about the people studied if he had taken no notes at the time of participant observation. Details of conversations and events are lost with each day of delay in recording, so that the fieldwork risks falling back on pre-field work presumptions. The same goes for any description of the self in the field. Moreover the very act of recording stimulates and develops ideas at the time.

In 'Notes and Queries' the use of a journal is suggested merely as a supplement to note taking, its junction being mainly to record the chronology of events and seasons. There is a single mention of 'self', (no other concession to subjectivity), but no indication that it's description be in other than medico-spatial terms; it is lost in the 'weather' and 'special events' (1967). Audrey Richards (1971) recommended the use of a diary along similar lines but her additional comments hinted at its potential for self examination.

In my own field work I recorded all my data in diary form, thereby avoiding the problem of pre-conceived categories. Several copies were made of each typed page and my material categorised in ways which often appeared relevant long afterwards. I bitterly regret that I rarely thought it 'professionally relevant' to record or analyse at length my personal reactions and dilemmas as they occurred. In my postgraduate training examination performance was assessed mainly in the Malinowski paper. I was encourage to read virtually all his articles and books except the diary.

There was another reason for my dedication to a science which excluded the self: my notes were to be examined by my employers, a research organisation, some of whose members had expressed grave doubts about the 'reliability and 'objectivity' of anthropological methods. At the first stage of field work I had to conceal the fact that I was taking notes at all. Instead everything was supposed to be recorded in massive questionnaires. These I hid in a suitcase under my bunk and later sabotaged by giving them to social workers to administer. The Gypsies gave brilliant and ambiguous answers which I was told despairingly couldn't possibly be coded.

During some three months follow up field work I kept a diary, which of course could never recapture the details of earlier responses. A proper development of simultaneous self-analysis awaits my next project. However, I can explore to some extent the third and final method:-

c) Autobiography

Here the writer attempts to describe and recreate the stages of past experience (Abbs 1974:6-7). The presentation of the past will vary in accordance with the present time chosen to examine it. Retrospective analysis of field work will give another dimension to contemporary analysis and the diary. Perhaps the final product should consist of an analytic combination of all three, the aim being that self analysis should have influenced and enriched the research at all stages. It is debatable how far the autobiographical exploration should be a self contained section; at the very least it should be recognised as an integral part of published research. Pocock's valuable 'Idea of a Personal Anthropology' (1973) recognises and explores a person's assumptions about his own society, embedded in written texts and recorded interaction with another people. In this paper, I have chosen to concentrate more on the refinement of self consciousness in the field situation, the actual process of interaction.

In the creative use of autobiography, anthropologists can learn from literature. The greatest writers have often had to work through most explicitly their youthful autobiographical experience:- Tolstoy in Childhood, Boyhood and Youth; George Eliot in 'The Mill on the Floss'; James Joyce 'In Portrait of the Artist'; and D.H. Lawrence in 'Sons and Lovers'. Philosophers have felt compelled to write autobiographies in addition to, and separate from their main work:- Rousseau, J.S. Mill, Sartre and De Beauvoir. For others the autobiography has stood as their single product for example; O'Sullivan (1933).

So far I have emphasised the methodological advantages of self analysis and autobiography in anthropology. The experience and a full and creative record of it are valuable in themselves. The anthropologist, entering another society crosses also a boundary of self definition. Some novelists have dealt with this experience most successfully in recording the passage between youth and adulthood. (Balzac, Stendhal, Flaubert and Hesse).<sup>8</sup> Unfortunately the anthropologist's rite de passage between two cultures has largely been defined only in the context of his natal culture. That is, he is said to undergo a painful and isolating experience in a criminal area before he or she returns as a full member of the academic club. This witticism thus disposes of the experience. The anthropologist is then said to enter the field in order to return, he or she is not said to be in anthropology in order to enter the field. Both Malinowski (1967:161) and Levi-Strauss (1963:17,43) tried to play down field work as a life experience, although their own evidence contradicts this pose. Perhaps Castenada has aroused such interest precisely because he is prepared to abandon a formal objective purpose for new and personal knowledge on the other side. (1970).

Field work is a dramatic contrast to the private, sedentary and academic demands of university existence. Practical and manual skills may be greatly valued, also the ability to interact with a wide range of people. In participant observation in a non literate society, my usual manner of dress, accent, past education were sources of stigma. Details of my past, important

to me, were irrelevant to the gypsies, other details to which I felt indifferent were to them most meaningful. All this can be both shattering and exhilarating.

My main field work has been within the geography of my own society. I was travelling through or camping in towns I'd known before, but in this different context the landscape was transformed. When I knocked on doors asking for 'any old iron, batteries or rags', I often came face to face with people of my own 'background' and social class, but they were aliens and they treated me as one. Often I suffered a profound alienation. After crossing an ethnic boundary it seemed I belonged nowhere. (See also Lawrence 1935 end of Chapter one). This cannot be eradicated by self-analysis although better understood and used imaginatively. If you let go you see aspects of yourself as mere props. You are made aware of your 'personal anthropology', its flaws and its virtues not just through retrospective nor even verbal analysis, but through action. Dedication to objectivity is exposed as the ego of your own history.

Judith Okely.

#### Notes

1. Willmott and Young (1962) in their 'classic' conceded: 'For the most part we can only report what people say they do, which is not necessarily the same as what they actually do' (1962:14).
2. Not in one's own society at least is anyone obliged to accept and encourage this dichotomy as politically permanent. I would agree with Levi-Strauss that 'the anthropologist who is critic at home and conformist elsewhere is therefore in a contradictory position' (1963:384). But I disagree with his suggestion that the anthropologist should take no action in his own society for fear of 'adopting a partisan position' elsewhere (p385). Levi-Strauss' ideal objectivity is falsified since acceptance of the status quo is as much a subjective stance as intervention.
3. It is not coincidental that Swallow's paper appears in the special issue on 'Women in Anthropology' (Cambridge Anthropology 1974), and that the earliest draft of my paper was first given to the Women's Seminar at Oxford.
4. Even in the discipline I get a sneaking feeling that obscure abstractions are considered among some contemporaries as the sole proof of intellectual power as opposed to the infinite mental intricacies of field work problems. Is field work destined to be another female occupation like social work?

5. As it turns out, the ensuing pages of Notes & Queries occasionally reveal some amusing examples of colonial paternalism and wholesale generalisations about 'other' peoples, which social anthropology was itself trying to discredit:- 'The unsophisticated native is often suspicious of all strangers' (1967:29); 'patriotic flattery may be useful' (1967:33); 'women can be just as offended by the offer of (to them) unsuitable beads as are European girls' if given presents suitable for elderly women' (p.33).
6. Raymond Firth (1936 republished 1963:10) in his very discreet description of himself and his methods yet feels obliged to apologise for 'this somewhat egoistic recital'.
7. Raymond Firth (1967 Introduction) considers that the diary 'in it's purely ethnographic sense cannot be ranked as more than a footnote to anthropological history'.

For Geertz (1974) the diary exposes any previous claim that anthropologist's had some 'unique form of psychological closeness, a sort of transcultural identification with our subjects'. He neglects both self-analysis and biography as techniques for understanding the interaction.

8. Here I refer specifically to Balzac's 'Les Illusions Perdues'; Stendhal's 'Le Rouge et Le Noir'; Flaubert's 'L'Education Sentimentale'; and Hesse's 'Demian'.

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Formulating a Linguistic Analogy for Society

It has become somewhat fashionable in recent years for those engaged in social studies to make frequent allusions to linguistics, to the study of language, to language itself. The three principal articles in the last issue of this journal were devoted to one aspect or other of the significance of language; there does seem to be a feeling in the air that whatever society 'is', it is something 'like' language'. One gets the impression that scholars tend to think that once a social process or structure has been equated with language in this way, some sort of 'explanation' for that phenomenon has been given, or perhaps at least that the phrase 'like language' is a metaphor appropriate to convey the mystery, the depth, the importance, of the matter under consideration. Edwin Ardener, who has written extensively on the subject of language and its significance for social anthropologists, had this to say in his penultimate paragraph of an article which appeared in this journal three years ago with the title 'Language, Ethnicity and Population':

'Ultimately, among the things that society 'is' or 'is like', it 'is' or 'is like' identification. The entities set up may be based upon divisions in empirical reality, or may be set up on reality by the structuring process of the human mind in society. In such statements 'reality' is, however, frequently only a compendium of 'positivistic' measures and approximations. We experience the structures themselves as reality: they generate events, not merely our experience of events. Anthropologists would argue I think that this process is analogous to language, possibly subsuming language, rather than a process of language. But all agree that language acquires a position of critical empirical importance in its study.<sup>1</sup>

The subject of this paper is to try to explore this language analogy, to try to ascertain the implications of regarding the 'structuring processes of the human mind in society' as being 'analogous to language', to try, with the aid of some reference to ethnographic aspects of the city of Jerusalem and the contemporary Palestine conflict, to simply comment on the nature of language itself as a result of its now fashionable use as a model for understanding society.

\* \* \*

It would seem at first glance that just about anything can be explained as being 'like language', 'analogous to language'. Just as many things were explained in the 19th century with reference to the concept of 'evolution' - everything then had an 'evolution' - so today nothing can be denied a priori as not being somehow a mirror of something linguistic. The obvious truism here would be to say that language today explains

everything, but in so doing it explains nothing. I love humanity, but I love nobody in particular.

In a sense it could be said that if this is the great weakness of the humanities, it is also its great strength - anything can be said about anything. This is perhaps what freedom means for the poet. And indeed if we were to see our job here at the university as a matter of writing poetry, then there would be little further left to say why the analogy of language may be problematic. But in fact what I am concerned with could be described as the problem of testing the innocence of a relationship between two disciplines. Within the confines of one discipline there is clearly a great amount of freedom concerning the way in which new light can be cast on old problems; it is really at the moment of inter-disciplinary contact that the possible absurdities become all too apparent.

What sort of absurdities do I mean? There are a number of absurdities: one kind of absurdity arises when it becomes clear that the analogy is being simply pushed too far, in other words when the author really believes that a given social form is so much 'like language' that he (or she) pushes the 'fit' so tight that the evidence itself may eventually become distorted for the benefit of this 'fit'. Another kind of absurdity arises when it becomes clear that the possibility of creating a parallel or homologous terminology simply leads to confusing what should be different levels of analysis, such as for example the celebrated case of phonemes in kinship or myth. Another kind of absurdity concerns the reversability of the analogy. An example of this, drawn from the Palestine material, is the story about the two great guide books to Palestine printed before the First World War, Murray's Guide Book and Baedeker's Guide Book: the frontispiece of the one bore the aphorism 'Palestine is the best guide-book to the Bible'; the other, with equal truth, declared 'The Bible is the best guide-book to Palestine'.<sup>2</sup>

Perhaps the most interesting evidence for the view that X is analogous to language comes from what one might call the extreme view, which simply argues that problem X is nothing but an essentially linguistic problem. Such a view is well-known in philosophy, in linguistic philosophy ('all philosophical problems are nothing but problems of language'); but it also appears in a lot of recent work done in the sociology of education ('Many teachers in schools and in colleges of Further Education see... that educational failure is primarily linguistic failure')<sup>3</sup>; teachers have thus been turning to Linguistic Science for some kind of practical guidance. But,

"We were conscious of the wide divergence between the aims of the linguist, primarily interested in language as a system for organising 'meanings', and the needs of those who now wanted to gain access to the insights that resulted from that interest. In particular, we were aware of the wide gap that separated the literature



of academic linguistics from the majority of those who wished to find out what Linguistic Science might have to say about language and the use of language."<sup>4</sup>

Naturally there is a 'wide gap', a 'wide divergence': there are two separate disciplines involved. But what is the proposal here? The proposal is to publish a sort of linguistics for the layman, linguistics for the teacher in the polytechnic, linguistics for the non-linguist. In short, what is proposed is to set up a model of linguistics, a model of language, which could be 'useful' to those who would need it for their own purposes. The important point here is that the wide gap is recognized at the outset even despite the fact that it is argued that educational failure is linguistic failure. Even in this extreme case the proposal is to set up a model of what language is. Anthropologists who do not argue that society is language, merely that it is like language, similarly rely on setting up a model of what language is. It is to these models of language that we now turn. In the course of my argument below I wish to suggest why language is the fashionable analogy today, but for the moment it is perhaps worth noting that whenever Society as such is under discussion the one thing which virtually all commentators agree on is that apart from rules of kinship perhaps, it is language which is common and basic. When Malcolm Crick, in an article published in the last issue of this journal, attempts to pinpoint what is characteristic about Society that makes the ethological analogy inappropriate, he rightly says 'Language is really the crucial test here', and that human language, containing possibilities for meta-language, is on another level altogether from animal communication.<sup>5</sup> And George Steiner puts the same point too, with his characteristic turn of phrase:

"It may well be that our love-making does not differ very much from the great apes. But this is to say little. Through its verbalized imaginings, through the rich context of pre-physical and para-physical erotic exchange in which it takes place, human intercourse (a term obviously akin to 'discourse') has a profoundly linguistic character."<sup>6</sup>

And,

"Nothing destroys us more surely than the silence of another human being."<sup>7</sup>

I agree that language is the crucial test, and I propose to use language as a model for describing the society of Jerusalem, in particular the kind of Jerusalem as presented here:

"Few scenes in the East remain more distinctly printed in the memory than do those connected with life in Jerusalem. The motley crowd in its lanes, where every race of Europe and of Western Asia meets; the gloomy churches; the beauty of the Arab chapel of the Rock;

the strange fanaticism of the Greek Festival of the Holy Fire; the dervish processions issuing from the old Temple area; the pathetic wailing at the Temple wall; the Jewish Passover; the horns blown at the feast of Tabernacles; Russian, Armenian, Greek and Georgian pilgrims; the Christ crucified by Franciscan monks in the gilded chapel of Calvary; the poor whose feet are washed by a crowned bishop - all remain in the memory with the mighty ramparts of the city as seen by Christ and His disciples, and the blue goggles of the tourist from the West. No other town presents such an epitome of history, or gathers such a crowd so representative of East and West"<sup>8</sup>

The image of Jerusalem as a highly heterogeneous city is perhaps further emphasized by contemporary Arab propaganda that seeks to condemn the government of Israel for trying to 'judaize' it: there are many linguistic, religious and ethnic realities there, many paths to God in His Holy City. It is this diversity of Jerusalem onto which I wish to pose the question Is it 'like language'? Would an emphasis on the multi-lingual character of Jerusalem be appropriate at all?

\* \* \*

But first it is necessary to look at those models of language itself which are to be found in the literature. Mary Douglas, in her book Rules and Meanings, includes an excerpt from an essay on the novelist William Golding by Michael Halliday, and blithely entitles it 'Syntax Enunciates the Theme' - and this, in the section of the book she calls 'Formal Correspondences'. The notion that culture possesses a 'syntax' seems quite common, yet syntax is merely one part of grammar and may have little meaning as a concept outside a theory which would describe the part it plays in the grammar. This is not simply a case of wrenching terms from linguistics; rather, it seems to create a travesty of language itself. If there is no attempt to think through the notion that culture is 'like language', to follow out that language and linguistic meta-language have their own internal relations, then what we are left with is a spoof of language and a spoof of linguistics.

It is thus with a certain amount of reserve that one must greet the efforts of Lacan to link psycho-analysis with language, as expounded by Martin Thom in the last issue of this journal.<sup>9</sup> It is indeed proper that psychology should cross-reference with language - I would find it hard to visualise a psychology which could be separate from how a specific linguistic world-image conditions the life of the mind - and to that extent Lacan is surely on the right track. Interestingly, he emphasises the minimal aspect of language that Malcolm Crick also emphasises in his article in the same issue, namely the capacity that human beings, as distinct from animals, possess in creating and using metaphor. But this is a minimum of language, it represents a starting-point from which one begins a study of language. It must be therefore quite inadequate as a model of what language is.

Moreover Martin Thom in his article concedes that the distinction between metaphor and metonymy, a distinction which we learn Lacan leans on so heavily, is not even specific to language. This he concedes; he says it is 'undeniable'.<sup>10</sup> But if so, how can it be acceptable to study phenomena at a level below which the specific meaning resides? How can Lacan meaningfully speak about the Unconscious structured 'like a language' when he apparently relies on a model of language as deficient as this? And as for the idea that the Signifié does not undergo change, Thom himself says that Lacan's reading of de Saussure is 'highly idiosyncratic'.<sup>11</sup> But still, Lacan speaks about the Unconscious being structured 'like a language'.<sup>12</sup>

Examples of this sort of approach could be multiplied, but for the sake of a comparison let us look briefly at some models of language that linguists themselves have. The school of transformational-generative grammar (TG) associated with the name of Chomsky has come under a good deal of criticism for being too ethnocentric, for forcing all languages into the mould of English just as 17th-century grammar tried to enclose all speech into the mould of classical Latin, for pontificating about the existence and nature of universals in language when only a few dozen have been studied out of the thousands that exist even today, let alone all the thousands that are now dead. George Steiner criticises TG just because it is a formal model and not a representation of actual living language. Chomsky cuts out of his formal model how people actually speak; his model of linguistic competence shows how language;

would work optimally, given the kind of frictionless, homogeneous, perfectly measurable reality in which the laws of physics, such as we learn them in school books, are said to operate. But it is the language *donnée* in which we conduct our lives, whether as ordinary human beings or as linguists. We have no other.<sup>13</sup>

The fascination that Chomsky has with the universal attributes of language seems to Steiner to be a modern version of the *Ursprache* myth, the language of all mankind before the great event at the Tower of Babel. Indeed, he says TG reflects a profound bias towards mono-lingualism. But empirically linguistic reality is quite different. 'Most of language begins where abstract universals leave off.'<sup>14</sup> If I criticise the anthropologist for his non-proven claim that his subject-matter is 'like language', it is also true to say that one may criticise Chomskyan linguistics for claiming that its 'deep structures' are 'like language'. Some feel, indeed, that they are not 'like language' at all.

For George Steiner the crucial fact about language is its huge multiplicity. Chomsky's model might well be acceptable if the whole world spoke one language - but why are there 1000 times more languages than blood-groups, asks Steiner.

The model of language for linguistic philosophy is a question of words, the arrangements of words, and their truth or falsity - but words, says Steiner, cannot be about truth at all, for if they were there would not be so many languages. Steiner's model of what language is he calls 'alternities': each human being is a post-Babel universe is miniature, carrying in his head all sorts of alternative worlds, worlds other than the world, and inventing new worlds at the time. Language is for Steiner a theory of translation between these worlds.<sup>15</sup>

Steiner's work is undoubtedly highly imaginative and stimulating, but I am not convinced that the details of his debate with Chomsky<sup>16</sup> need detain us here; I find it significant that there is one feature of language which they both regard as its minimum feature and which they both use as their starting-point for analysis, and that feature is the creativity of language, its inventiveness, its open-endedness. Chomsky has rightly been praised for stressing the human being's capacity for infinite linguistic creativity, and I believe Steiner will be praised for re-opening the question, Why Babel? And indeed it is this model of language which I think is the crucial one for anthropologists, for *prima facie* it is linguistic variation that encodes, encapsulates, crystallises, generates cultural differentiation, whether between cultures or within a single culture. Hence it is through the perspective of linguistic alternities that I shall be looking at the Jerusalem material.

There are a number of implications that follow from this position. The first point concerns the question of style. Information theory, or at least a model of language that treats language as being essentially the transmitting of information or the expressing of propositions, does not generally take account of the fact that languages possess a multiplicity of styles. But part of the meaning of a linguistic utterance is contained in the way in which it is said. If language is information, why is there such an anti-economic multiplicity of different ways of speaking? This fallacy is found in many places: Lacan speaks about the language spoken by the mass of human beings; the translators of the New English Bible tried to 'render the Greek, as we understood it, into the English of the present day, that is, into the natural vocabulary, constructions and rhythms of contemporary speech',<sup>17</sup> and, it seems, appointed a panel of literary advisors. But this is to show a blank unawareness that different modes of discourse require different styles: how can 'English of the present day' be the same as 'contemporary speech'? And how can they arrive at the idea that style is some sort of decorative addition, not essentially concerned with the meaning? They appointed their panel of literary advisors because, as they explained, 'sound scholarship does not always carry with it a delicate sense of style.'<sup>18</sup> But surely sound scholarship here would be precisely the saying in English with all the possible delicacy what the original says in Greek. If the scholarship does not emerge in the

translation itself, where is it? This is a very common position - in the acknowledgements to the Jerusalem Bible there is also a separation between 'translation' and 'revision'. And in the anthropological literature it is unfortunately all too common to come across apologies for a poor translation, as if anthropology was about something else instead.

Another implication of a model of language which uses alternatives as its integrating theory is to raise the problem of context. The notion of context has had a somewhat vexed history: in linguistic anthropology it took its first substantial roots in the writings of Malinowski, who seemed to be reducing linguistic meaning totally to its context of utterance. In many important ways, important for anthropologists, this is a very fertile idea, but of course it can lead to absurdities. If I say, for example, 'Mary did it for John's sake', an unsophisticated theory of context - and there are enough of them about - might argue that from this we can infer that John has a sake. However if I say, 'She did it for his sake', the presence of the pronouns may indicate a context, and we might wish therefore to distinguish these two sentences.

We have seen, albeit briefly, some of the difficulties which attend the use of the expression or model 'English of the present day', in that it overlooks the multiplicity of alternatives that exist at a given moment in time. But the phrase 'English of the present day' also implies a decision on time, on its diachronic relations into the past. The notion that certain linguistic styles, such as religious English, become 'outmoded' or 'out of date' is somewhat misleading: it concerns not some mysteriously inevitable process of linguistic development but rather a question of usage which is conceptualised in terms of a diachronic image. Part of the context of a use of language is its setting in the history of itself. Language is capable of patina, as we can see by the commonly used device of translators to make use of archaic forms of the language to gain special effects. The use of religious English, of the type generally associated with the 1611 Authorized Version of the Bible, has often been described by Christians in this country as a form of identification with the generations of Christians who expressed their lives through that form of language. The model 'English of the present day' does not capture this kind of context; it does not take alternatives into account, just as the model 'ordinary speech' fails in this respect.

We use the word 'cliche' to refer to a use of language that is repeatedly ripped from a context and appears to survive without context at all. It is the classic example of 'dead stretch' in language. A dead stretch is the slaughtering of the experience, sometimes done in order to analyse it, and it is something increasingly common in our own culture, like for example the reduction of the experience of going to an art gallery to flipping over the photographs of a glossy album, or the reluctance to go to a concert but rather listen to the

gramophone record. An appeal for context, therefore, whether context of a diachronic or synchronic kind, is an appeal to re-create the experience in order to understand it. And even studies of the myth are, as Levi-Strauss reminds us, part of the myth itself. Without context, we are left with a dead stretch. A model of language, especially when it will be used to help us in understanding society, should search after all the contextual rhythms it can find, and rebuild their patina. In other words, what is needed is a theory of context. Parallel with that, what is needed is a theory for anthropologists which should concern itself with what a model of language should look like. My criticisms till now have tried to show that only certain selected aspects of language or linguistics have been selected by anthropologists, often in a highly idiosyncratic and arbitrary way, and then we have been told that the phenomenon under discussion is 'like language'.

But anthropologists who use the old term from linguistics, such as transformation, rule, lexicon, syntax, etc. are saying as little about language as an anthropologist who merely lists the ingredients - or perhaps even only some of them - of a soup. Language itself can be approached in a number of ways - to extrapolate one aspect and hence argue that the society or social form thus functions 'like language' is surely a case of a syllogism that is arguing from minor to major. Analysis of language seems possible on an indefinite number of levels; consider, for example, etymological, the functional, the structural, the synchronic, the diachronic, the phonetic, the phonemic, the morphological, the syntactic, the sociolinguistic, the psycholinguistic, the metalinguistic, even the grammatical. To what is the reference to 'language' made? Some might argue that it should be to all these things together, other might prefer to see a conscious selection or shaping of aspects in order to describe language, similar perhaps to the shaping of a historiographic approach as practised by the historian. What I am arguing for is a consciousness by anthropologists in constructing the linguistic analogy, that one may learn from the process of model-building itself.

Consider the following passage from Michael Halliday in a discussion on language acquisition by children:

"The question 'what is language?', in whatever guise it appears, is as diffuse and, at times, disingenuous as other formulations of its kind, for example 'what is literature?' Such questions, which are wisely excluded from examinations, demand the privilege of a qualified and perhaps circuitous answer.

"In a sense the only satisfactory response is 'why do you want to know?' since unless we know what lies beneath the question we cannot hope to answer it in a way which will suit the questioner. Is he interested in language planning in multilingual communities? Or in aphasia and language disorders? Or in words and their histories? Or in dialects and those who speak

them? Or in how one language differs from another? Or in the formal properties of language as a system? Or in the functions of language and the demands that we make on it? Or in language as an art medium? Or in the information and redundancy of writing systems? Each one of these and other such questions is a possible context for a definition of language. In each case language 'is' something different.

"The criterion is one of relevance; we want to understand, and to highlight, those facets of language which bear on the investigation or the task in hand. In an educational context the problem for linguistics is to elaborate some account of language that is relevant to the work of the English teacher. What constitutes a relevant notion of language from his point of view, and by what criteria can this be decided? Much of what has recently been objected to, among the attitudes and approaches to language that are current in the profession, arouses criticism not so much because it is false as because it is irrelevant. When, for example, the authors of *The Linguistic Sciences and Language Teaching* suggested that teaching the do's and don'ts of grammar to a child who is linguistically unsuccessful is like teaching a starving man how to hold a knife and fork, they were not denying that there is a ritual element in our use of language, with rules of conduct to which everyone is expected to conform; they were simply asserting that the view of language as primarily good manners was of little relevance to educational needs."<sup>19</sup>

Hence Halliday argues that what is relevant to the teacher is that model of language that a child has. He says the child has a number of models of language: An Instrumental model, a Regulatory model, an Interactional model, a Personal model, a Heuristic model, an Imaginative model, and a Representational model. His comments on the last one of these, the Representational model, are worth quoting:

"So we come finally to the REPRESENTATIONAL model. Language (in this model) is, in addition to all its other guises, a means of communicating about something, of expressing propositions. The child is aware that he can convey a message in language, a message which has specific reference to the processes, persons, objects, abstractions, qualities, states and relations of the real world around him.

"This is the model of language that many adults have; and a very inadequate model it is, from the point of view of the child. There is no need to go so far as to suggest that the transmission of content is, for the child, the least important function of language; we have no way of evaluating the various functions relatively to one another. It is certainly

not, however, one of the earliest to come into prominence; and it does not become a dominant function until a much later stage in the development towards maturity. Perhaps it never become in any real sense the dominant function; but it does, in later years, tend to become the dominant model. It is very easy for the adult, when he attempts to formulate his ideas about the nature of language, to be simply unaware of most of what language means to the child; this is not because he no longer uses language in the same variety of different functions (one or two may have atrophied, but not all), but because only one of these functions, in general, is the subject of conscious attention, so that the corresponding model is the only one to be externalized. But this presents what is, for the child, a quite unrealistic picture of language, since it accounts for only a small fragment of his total awareness of what language is about."<sup>20</sup>

Halliday is here perfectly explicit in distinguishing between what language is, and what language is for the child. He is clear on what he is leaving out from the former in order to describe the latter. He specifies the heuristic purpose of his model. But in addition to its interest as an example of conscious model-building of the nature of language, this text also shows a crucial point for anthropologists, namely that there is no a priori reason whatsoever to suppose that another culture will have the same model of language as we have of ours, just as a child does not possess the same model of language that an adult has.

In other words, a theory of context which would be part of a model of language for anthropologists would have to include how different cultures use language, and how we use language too. A theory is needed, for without a theory there is no way to assess the status of any one particular analysis or any one reference to the linguistic analogy.

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In an important introduction to a discussion of the problem of ethnicity, Fredrik Barth has outlined for us some of the limitations of taxonomic approaches.<sup>21</sup> Indeed, ethnic identity may very well be a contextual matter and not a matter for a priori judgment, despite a long tradition of the latter on the part of the colonial or republican administrator as also on the part of the anthropologist. Rather, in order to understand ethnic perceptions of inclusion and exclusion, it is necessary to make ad hoc analyses of the world structure or the total way of thinking of the particular society under discussion.

In many ways language is remarkably similar to these problems of ethnicity: it is even possible to read whole paragraphs of Barth's text substituting the word 'language' for 'ethnic group' and 'dialect' for 'sub-culture', and the argument



would remain valid and indeed strong. A question then that I would pose about language could also be posed about ethnicity, and the question is this: if context is the issue at stake, how do people preserve in their heads the complexity of their shifting identities, styles or languages? We are all multi-lingual, in the narrow sense of the term - what theory of context do we have in our heads, quite apart from the theory of context that the anthropologist should have in his head? Is there not a situation of entropy, of conflicting energies, latent here? Why do we look for explanations when violence erupts, instead of wondering all the time when it does not? I think there is something here that needs explanation, that a theory of context must explain, namely how states of entropy are avoided. If I may for a moment recall the story of Maxwell's Demon, there was just such a similar situation: how was it possible for entropy to be avoided? What was this Demon that could prevent entropy? And the answer which was found was this: in order to keep the hot gas and the cold gas apart, the Demon had to be itself consuming energy. The analogy is this: I should like to posit a linguistic form perhaps unseen or unobserved which itself represents the native theory of context by virtue of its singular capacity to prevent linguistic entropy. That energy is the sense of the human being of his own linguistic wholeness, whatever his multi-faceted capacity for making alternities. It is a native theory of unity. I shall come back to it again under the name *lingua franca*.

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At this stage it might perhaps be desirable to see where we have got to. Let me summarise the argument so far: models of language are frequently used to illustrate social phenomena, but generally these models reveal only a certain aspect of language within a certain context, and also are not sufficiently self-conscious in an analysis; however the truth about language is that it does operate in a variety of contexts, multiplicities and histories simultaneously - and the question that arises is whether a certain entropy is not generated internally as a result, to which I have suggested that there is an inherent *lingua franca* machinery that welds the alternities together.

However an enormous problem confronts us here immediately. If, as I have argued earlier, the way in which things are said is in fact part of the meaning of the thing said, then we are faced with the difficulty that it is through language that we are in fact talking about language. This human capacity for meta-language (language about language) is, as Malcolm Crick points out in his article in this journal mentioned above, central to the question of what human society rests on, but meta-language can also be used falsely, as Crick also points out. He says that ethologists speak - incorrectly - of the 'language' of animals, and he complains that their use of the term 'language' is to 'semantically violate' and to involve a 'linguistic confusion'.<sup>22</sup> I agree totally that the word 'language' is being used differently by ethologists, but

although Crick sees that language can be used by humans to lie, he does not draw out the idea that language can also be used by humans to create false meta-languages. It is right to complain of the eclectic use of notions such as transformation, deep structure, surface structure, etc; he is right to criticise the ethologist model of animal communication being 'like language' or even being 'language'. But the problem is more complex than that. We know how to talk about the 'language of love' or the 'language of music' - indeed, one of our alternatives is to talk about the language of anything or the syntax of a bumble bee. The capacity for meta-language may be universal - but specific meta-languages are, on the other hand, deeply rooted in culture. George Steiner puts it tellingly - that meta-languages have no extra-territorial immunity; and he would wonder whether the implication of that is that a genuine science of language is thereby rendered impossible.<sup>23</sup> Robinson's recent book on TG puts a similar point, though in somewhat more polemical fashion:<sup>24</sup> TG postulates a scientific, culture-free, universal meta-language but this is arrogant nonsense - 'All the efforts to show us what underlies natural language ... are themselves language-dependent';<sup>25</sup> Chomsky's TG meta-language would 'attribute to the child a quite advanced theoretical knowledge';<sup>26</sup> 'Chomsky is confusing what the grammarian does with what the speaker does';<sup>27</sup> just as syntax is only one part of grammar, so 'sentences are a rather small part of language. Chomsky never gives any account of paragraphs, chapters, books or any other of the larger units of which sentences are a kind of atom. But it is the larger unit which decides what the sentence is doing in language, not vice versa',<sup>28</sup> yet 'There is no reason to suppose that speakers of English have acquired a concept 'sentence of English'.'<sup>29</sup>

In other words, the assumption that it is possible to set up a meta-language for linguistics that is not itself ethnocentric may well turn out to be suspect. Certainly a good deal of 'objective observations' about language may stem from our own perception of our own language, especially in the absence of a contemporary neuro-chemical theory of human language. Moreover a perfect theory, a perfect meta-language, a perfect translation, is something we would never know about, even if it were possible - there is no way of proving a perfect fit when it comes to the question of the nature of understanding itself.

So what of our own meta-languages? The problem of the false meta-language extends beyond simply a criticism of work in ethology or TG. George Steiner has argued in a number of well-known books and articles how we live today in what he calls a 'post-culture' the chief distinguishing characteristic of which is what he calls 'the retreat from the word'. Our contemporary English language is simply debased. Robinson (in an earlier book)<sup>30</sup> describes how the New English Bible in no way gives the sense, the strange and savage sense, of the original; the miracles of the new Bible all seem gross

impostures, superstitions as reported by the modern journalist. One now encounters attempts to judge moral questions according to common sense or utility rather than according to Christian or any other absolute standards. The Ten Commandments are now glossed in the new Anglican liturgy: 'You shall not commit adultery. Know that your body is a temple of the Holy Spirit', it now says. But the commandment was clear and not dependent on this gloss: 'thou shall not commit adultery', it said, namely, regardless of your beliefs and opinions. Robinson is convinced that our language today reveals that we are not concerned with the meaningfulness of our actions, and he discusses extensively texts from books about the so-called 'science of sex' to demonstrate his point: 'Turn to page 55 for an assessment of your love-making talents' - sexologists are proud to tell us that they exclude subjective criteria, but surely as Robinson says,

The sense, the reality of the 'same' sexual act varies with the language, and context in which it takes place. This is what the biologist qua biologist cannot observe... The sexologists are up to the old trick of trying to get the event without the meaning, just like the old-fashioned linguists trying to understand languages by concentrating only on sound.<sup>31</sup>

This kind of writing about sex is what Robinson would call pornography - but the failure of the New English Bible as a translation is evidence for him of a lack of sincerity in the use of language: 'translators who cannot show the Bible to be the word of God cannot produce a sincere translation'.<sup>32</sup> And he quotes Collingwood: 'To express it badly is not one way of expressing it,... it is failing to express it.'<sup>33</sup>

The substance of Robinson's argument is that if, to put the matter crudely, style is an integral part of content, then the style of contemporary influential texts yields evidence for the debasement of our language and by inference of the status and scope of our meta-languages. The implications for anthropologists are very important, if it is true that

Ours is a time when... the capacities of English-speaking people to contemplate the mysterious and metaphysical through the word are weakened and unexercised...<sup>34</sup>

Or consider this passage from Wittgenstein, talking about Frazer's 'Golden Bough':

What narrowness of spiritual life we find in Frazer; And as a result: how impossible for him to understand a different way of life from the English one of his time! ...Frazer is much more savage than most of his savages, for these savages will not be so far from any understanding of spiritual matters as an Englishman of the twentieth century. His explanations

of the primitive observances are much cruder than the sense of the observances themselves.<sup>35</sup>

These are all controversial questions, which could be argued at length. To argue that even on the basis of the evidence that our language is 'debased' implies a value judgement which does not have meaning outside a theory of culture, such as that proposed by Toynbee or Spengler. But the relevance of these arguments here is this: traditional socio-linguistics, and the traditional view that society or culture may be occasionally 'like language', contain the implication that what language does is somehow external to language - but the idea that 'social reality' is essentially separable from language is defective, since, as we have seen above, there cannot be (be definition) a social or even a human without language. Language is more to society than just another cultural form. In many ways it denotes society, represents it at home and abroad. But in many ways it shapes and is shaped by society, and our perception of society too. Thus it may well be that within our own culture since the retreat from the word, we may be using false or inappropriate meta-languages in dictionaries, in linguistics, in literary criticism, in the social sciences - far more extensively than what one might have first imagined; in popular language this would be called 'paying lip-service' to conceptual or value systems that we are not linguistically sincere about. In other words, if some anthropological studies fail to convey adequately the mystery, the strangeness, the reality of another culture, it could be because of a defect in the meta-language involved, particularly since, as Malcolm Crick has explained, anthropology is an exercise in translation.<sup>36</sup> Cargo cults, to quote Steiner's remark, 'provide an uncannily exact, ramified image of the risks' involved here.<sup>37</sup>

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At this point perhaps some comments on the Palestine problem may throw a little light on the general points that have been raised so far. I started this paper by asking what sort of model of language could or should anthropologists be using if they are interested in the linguistic analogy, if they feel that society is in some way 'like language', and the discussion has led to the problem of meta-languages. So now I can put the question: what sort of language is used to describe the Palestine problem, and what suggestions can be made for an appropriate anthropological meta-language for Jerusalem?

It is remarkable, when one surveys the literature on the contemporary Middle East, quite how many adherents there are to the extreme view which has been discussed above, that a particular problem is nothing but a problem in language. Professor Walter Lacqueur, in a article in The Times (November 13, 1975) condemning the recent vote at the United National General Assembly which described Zionism as a form of 'racism', tried to explain the linguistic absurdity of such a position

on Zionism. Perhaps the United Nations vote was 'nothing but' an exercise in political warfare, but it is interesting to see how the attempt is to create a suitable mode of discourse in which to discuss what Zionism is. We are all familiar nowadays with meta-linguistic discussions concerning the definitional differences between a freedom fighter and a guerrilla and a terrorist, and the conflict in the Middle East is presented in the mass media in the terminology associated largely with political science: 'The 1973 war brought a great change in the Middle East situation'; 'Most of the standard Israeli perceptions of its situation have been demonstrated to be no longer valid - if ever they were'; 'The studies on political and economic development in the Middle East countries, published in an earlier volume in this research program, were all written in or before 1970, and were based on assumptions that today appear not only optimistic but entirely unrealistic'. This kind of language, which seems also to rely on metaphors derivable from a number of diverse styles and meta-languages, is what Robinson might call 'insincere'. Language which describes the Middle East as a 'powder keg' which can at any time be 'ignited', language which purports to grip reality through such notions as 'violations', 'lessons', 'rights', 'burning issue', and so on, language of this sort is cliché because it is a dead-stretch use of language - terms such as these are bandied about, ripped from their respective registers. But it is still one of the alternities, further evidence for human linguistic inventiveness, yet a clue thereby to our perception of the complexity of the Middle Eastern reality.

I do not wish to dwell further on this kind of language that is generally used in our society to conceptualise the Middle East conflict, but turn instead to a language model of Jerusalem itself. Different civilizations work differently with words, use language differently, as we have seen earlier; or, to put it the other way around, by isolating different ways in which language is used to grip reality we may have an a priori case for being able to recognise different cultures. After all, we cannot have a thing without having a way to see it or conceptualise it, and in that sense all real knowledge is subjective, rooted in the individual experience. Hence we need to know how societies use language - we cannot force our own notions or model of language or meta-language onto another society. Jack Goody's work on literacy in traditional societies attempts this, although he is essentially asking an a priori question about the social consequences of a predetermined category, namely literacy. The Sapir-Whorf Hypothesis, which, broadly speaking, attributed all thought to the structures of the language in question, neglected the question of literacy and writing, and in so doing used a model of language that was defective. It does seem necessary therefore to emphasise the importance of building a model of the native awareness of language and to study the mode in which this native awareness is conceptualised.

I referred at the head of this paper to the immense ethnic heterogeneity of Jerusalem and the linguistic

heterogeneity that accompanies it. It would of course be a relatively simple matter to give lists and set up a taxonomy, and in a sense one could set up a working definition of the political models of the Palestine problem in terms of their exclusive use of taxonomic criteria. In other words, a political approach to the demography of Palestine would perceive four religions, viz. Moslems, Jews, Christians and others, and three races, viz. Arabs, Jews and others. The number of languages used in Palestine seems to be something no demographer has felt comfortable to speculate on, since the model Hebrew, Arabic and others is clearly not conforming with the facts because of the huge foreign Jewish immigration into Palestine during the last ninety years.

Some mention of the problem of ethnicity has been made above: it is true that the taxonomic approach produces, as Professor Fredrik Barth calls it, a 'world of separate peoples'<sup>38</sup> and takes the question of boundary maintenance for granted. But, as suggested above, replacing the word 'people' with the word 'language' reveals an interesting and related problem. Listing languages is arguably an approach to language which carries all the defects of a taxonomic approach to ethnicity. Just as ethnicity or ethnic identification is, as Barth suggests, a matter of ascription, and also a matter of shifting contexts and roles, so too a functional or etymological analysis of precisely which language is being spoken by a particular person at a particular time begs the question of the native model of language through all its alternities. The notion 'Semitic language', for example, which links into one language - family Arabic, Hebrew, Amharic and others, is an 18th century construct of a scholar called Eichhorn; it is not necessarily part of the native model at all. Moreover there is no reason to suppose whatsoever that people who are polyglot 'know' which language they are using at any particular time: George Steiner says he cannot remember which language he cursed in when he had a traffic accident.<sup>39</sup> Still, it is possible to trace the deliberate use of language choice: consider for example this excerpt from Jacob Landau's study of the Arabs in Israel - Arab members of the Israeli Parliament when making speeches frequently employ Arabic even when they know Hebrew. Examples are Diyab

'Ubayd.. who learnt Hebrew in a Berlitz language school; and Yusuf Khamis... who often speaks in... Hebrew, but at other times in Arabic - to remind his potential electorate of his identification with them.'<sup>40</sup>

In other words there is evidence for a model of conscious language choice where it would be used to assert ethnic identity. Indeed, scholars have documented the importance of Hebrew for the Zionist movement; modern Hebrew is an interesting case in fact for its tendency in the modern Hebrew novel or poem toward a kind of language which identifies with past Hebrew and Jewish tradition but also reaches out for a new Israeli kind of Hebrew language where words of Biblical or specifically religious origin

are distanced quite deliberately. 'Language riots' in Palestine have been known: there was a famous case in 1847 in Bethlehem: in the church of the Nativity there, the church built over the spot where Christ is believed to have been born, there was a marble slab with the Latin inscription 'Here Jesus Christ was born of the Virgin Mary' - the Greek Church, backed by the Russian government, stole this slab; the Latin Church, backed by the French government, reacted badly - and the ensuing conflict is supposed by many historians to be an important cause of the outbreak of the Crimean War six years later.<sup>41</sup>

If, then, language is not the only way in which ethnicity is conceptualised, it is nevertheless an important and critical way. It is by no means 'the issue' in the Palestine problem, which semantically could perhaps be best described in terms of competing native concepts of the distinction between 'native' and 'foreign' with regard to the territory of Palestine itself. Israelis and Palestinian Arabs both claim that they are respectively 'native' to Palestine, and that the other group is respectively 'foreign' to it. Consider the phrase 'Jesus was one of us'.

Yet, Jerusalem the Holy City is not consistently religious: there is a huge heterogeneity of ethnic approaches to God. In that sense it is a city of alternities, and in that sense it is 'like language'. The annual cycle of pilgrimages, pilgrimages both religious and secular, can be described in terms of language: 'Easter is the time when Greek is spoken on the streets'. But what is it that threads the alternities together? How do the people who live there thread their own theory of shifting contexts of ethnicity together? How is the entropy avoided? Can language really be used as a model to describe all this? What sort of meta-language can be suggested here? If Palestine, like Northern Ireland, is described as 'a problem', what methods do we have to find 'a solution'?

Balancing cultural and/or linguistic energies itself requires energy, as we saw before in the case of Maxwell's Demon. There is in Jerusalem a long tradition of a lingua franca which is neutral with respect to ethnicity: it was fascinating to watch after the Israelis occupied the Jordanian part of the city in 1967 (I was living there for a year after the war), how Jews and Arabs communicated with each other in the English language in the shops, markets, neighbourhood and youth clubs, discotheques, and university. The use of English as a lingua franca to mediate tensions between ethnicities can be dated precisely: in the closing years of the 1840's, when James Finn, who was to be the British Consul in Jerusalem in the 1850's, set up his Jerusalem Cultural Society with an expressly ecumenical purpose. Later, however, English was 'replaced' during various periods by other languages, such as French and German, but it 're-emerged' in 1967 and it is to a great extent still in active use in Jerusalem. In a way somewhat similar to the position that writing has on literate societies, one can speak of a lingua franca that provides the

basis for the potential, latent, unity between man, between ethnicities, within a single ethnicity, that gives a thread to the alternities of language, that negates entropy.

A lingua franca is generally understood as being a linguistic form that brings people together, unites them in what would be otherwise an impossible situation. So it does, but, like the very working of language itself, it keeps people apart. The Israelis would like to think of the Arabs in Jerusalem as a minority; the Arabs in Jerusalem would like to think of themselves as a self-contained society under military occupation. If all Arabs learnt to speak Hebrew, the Israeli perception would be greatly strengthened, and indeed, for economic reasons, this is becoming increasingly the case. But the existence of and the use of and the capacity to draw upon a lingua franca marks the boundaries between Hebrew-speaking Jew and Arabic-speaking Arab. I am not arguing at all that language itself creates this situation: I am arguing that language here epitomises and itself expresses the situation. I am arguing that language here is a good model for the situation. I am saying that in this sense Jerusalem, with all its alternities, can be said to be 'like language'.

Fredrik Barth was puzzled not why ethnicities persist, but how ethnicities persist, in a situation of inter-ethnic contact. I would like to suggest that lingua franca is one answer for a model, in language, of how ethnicities persist. The crucial point here is that a lingua franca does not carry with it an ethnicity. There is a considerable amount of confusion on this point.<sup>42</sup> Of course there is an English ethnicity, but it is not qua English ethnicity that I am speaking about the use of the English language. And therefore to search for distinguishing linguistic features of Palestinian English or Jerusalem English would be to misrepresent the issue, for it would imply that such a form of English is a variety of 'real English' in the areas where it differs from the 'real English'.

What I am trying to do here is to put forward the suggestion that it might be through the notion of lingua franca that a meta-language for the Palestine problem itself be presented, that through the use of English (it is perhaps a hollow category, for any other language could fill its place) one could suggest a model of the problem, one could suggest a specific example of following out the thought that a society or a social form is 'like language'. The phrase 'like language' is misleading, for language does not reflect extra-linguistic features, it expresses them. But notice George Steiner's derogatory comments about the use of 'international English' spreading across the globe: he says it lacks a 'natural semantics of remembrance' which in term 'disqualifies [it]... from any but trivial or ad hoc usage'.<sup>43</sup> This is perfectly true: the English of Jerusalem, for example, is not to be seen in context of the history of the English Language in its native usage. But the reason for this is that the English of Jerusalem has its context with its own lingua franca past;



it does have nothing to do with Milton or Dryden. Steiner's point is an excellent example of how to describe a language in terms of one level of analysis alone; but this is totally inadequate for a model of language as such.

One final point. George Steiner criticised Chomsky for his emphasis on linguistic universals, an emphasis which, as we saw above, reminded Steiner of modern versions of the myth of Babel. Before Babel, as the Bible says, 'The whole earth was of one language and of one speech' (the New English Bible has it 'All the world spoke a single language and used the same words'). The late Arnold Toynbee, in a fascinating monograph of *linguae francae* describes the Babel myth as the myth put out by a disintegrating civilisation as a lament for a past when people were, as it were, all of one mind. The suggestion to use a *lingua franca* as a model for conceptualising Jerusalem could also therefore be regarded as appropriate for being symbolic of Jerusalem's ultimate apocalypse in the end of days,<sup>44</sup> when, as the prophet says, nation shall not lift up sword against nation and the wolf shall lie down with the lamb. Indeed, Toynbee's view is that *Lingua Franca* is the goal of history, rather than its past. Our own difficulty with meta-languages, our own retreat from the word, our own disintegrating civilisation, our own job as anthropologists, all of it is bound up with an obsession with language after Babel. Perhaps it is this reason why language is such a fashionable subject these days, perhaps it is why it is so commonly felt that society is in some way 'like language', and perhaps it would in some measure justify the notion of *lingua franca* as a model of language for Jerusalem and the problems of Palestine.

\* \* \*

And what of the unconscious model, the hidden Maxwell's Demon? The *lingua franca* that has no ethnicity? Is this the way to search for a 'solution'? For this we turn to Conan Doyle:

'Is there any point', the Inspector asked, 'to which you would wish to draw my attention?'

'To the curious incident of the dog in the night-time.'

'The dog did nothing in the night-time.'

'That,' Sherlock Holmes replied, 'was the curious incident.'

Jonathan Webber

Notes

1. Edwin Ardener, Language, Ethnicity and Population, Journal of the Anthropological Society of Oxford, vol. III, no. III (1972), p. 131.
2. See A. Goodrich-Freer, Things Seen in Palestine, (London: Seeley, Service 1914), p. 47.
3. Emphasis in original. See Peter Doughty and Geoffrey Thornton in their General Introduction to the series 'Exploration in Language Study' and reprinted in each volume, for example in M.A.K. Halliday, Explorations in the Functions of Language, (London: Edward Arnold 1973), p.3.
4. Ibid.
5. Malcolm Crick, Ethology, Language and the Study of Human Action, Journal of the Anthropological Society of Oxford, vol. VI, no. 2 (1975), p. 109.
6. George Steiner, Extra-Territorial: Papers on Literature and the Language Revolution, (London: Faber and Faber 1972), p.62.
7. Steiner, op. cit., p.64.
8. Major C.R. Conder, Palestine, (London: George Philip 1889), pp. 23-4.
9. Martin Thom, The Unconscious Structured Like A Language, Journal of the Anthropological Society of Oxford, vol. VI, no. 2 (1975), pp. 79-105.
10. Thom, op. cit., p.83.
11. Thom, op. cit., p.92.
12. It would no doubt be possible to provide a historical explanation to account for the appearance of such notions of 'language' in works of psycho-analysis. 'Language' is surely a fertile idea there, and it would be pointless to criticise Lacan for not practising linguistics. Indeed, any sweeping criticism of work in another discipline may be little more than a tilting at windmills. Yet it remains noteworthy that many linguists themselves today try to measure the depth of linguistics by its closeness to physics or formal logic, whereas ultimately the only final test of linguistics is the depth of understanding of language that it can offer. Hence in this context it is the currency of the linguistic analogy in other disciplines that attracts attention, rather than its implications for those disciplines.
13. George Steiner, After Babel: Aspects of Language and Translation, (London: Oxford University Press 1975), p.113.

14. Dell Hymes, quoted in Steiner, After Babel, p. 472.
15. See Steiner, After Babel, pp. 220 ff. He introduces the notion 'alternity' at p.222: 'We need a word which will designate the power, the compulsion of language to posit 'otherness'... Perhaps 'alternity' will do: to define the 'other than the case', the counter-factual propositions, images, shapes of will and evasion with which we charge our mental being and by means of which we build the changing, rather fictive milieu of our somatic and our social existence. 'We invent for ourselves the major part of experience,' says Nietzsche...'
16. The two men exchanged correspondence, excerpts of which are reproduced with an interesting commentary by Steiner in his article Tongues of Men, reprinted in Extra-Territorial, pp. 102-125.
17. From a preface to one of the editions of the New English Bible, quoted in Ian Robinson, The Survival of English, (Cambridge: Cambridge University Press 1973), p.41.
18. Ibid.
19. Halliday, op. cit. (see above, note 3), pp. 9-10.
20. Halliday, op. cit., p.16.
21. See the introduction by Barth to Fredrik Barth (ed), Ethnic Groups and Boundaries: The Social Organization of Culture Differences (Bergen etc: Universitetsforlaget 1969), p. 9-38.
22. Crick, op. cit. (see above, note 5), p. 109, p.114.
23. See Steiner, After Babel, pp. 110-111.
24. Ian Robinson, The New Grammarians' Funeral: A Critique of Noam Chomsky's Linguistics (Cambridge: Cambridge University Press 1975). This new book received scathing reviews in the New Statesman and the Times Literary Supplement on November 21, 1975: the TLS reviewer saw fit to comment 'In what must set some kind of record for inaccuracy in publishers' blurbs, the dust-jacket proclaims "This is a rigorous argument, without polemic".' John Sturrock in the New Statesman writes 'Robinson's belief is that Chomskyan linguistics... are... so riddled with elementary fallacies it is a wonder Chomsky has ever recruited a single disciple. This overkill first spoils Robinson's case, then invalidates it.'
25. Robinson, The New Grammarians, p. 138.
26. Robinson, The New Grammarians, pp. 63-4.
27. Robinson, The New Grammarians, p. 66.

28. Robinson, The New Grammarians, p. 45.
29. Robinson, The New Grammarians, p. 63.
30. Robinson, The Survival of English, chapter 2.
31. Robinson, The Survival of English, pp. 176-7.
32. Robinson, The Survival of English, p. 40.
33. Quoted in Robinson, The Survival of English, p.58.
34. David Holbrook, in a letter to The Spectator in 1961, quoted in Robinson, The Survival of English, p.59 fn.
35. From Wittgenstein's Remarks on Frazer's 'Golden Bough', translated by A.C. Miles and Rush Rhees in The Human World, no. 3 (1971), quoted in Robinson, The Survival of English, pp.116-7 fn. Robinson is one of the three editors of this quarterly review; he states that he was there publishing the first English translation of that text.
36. Malcolm Crick, Towards a Semantic Anthropology: Exploration in Language and Meaning (in press).
37. Steiner, After Babel, p.300.
38. Barth, op. cit., p.11.
39. Steiner, After Babel, pp. 115-6. The idea is reminiscent of the lines in Lewis Carroll in The Hunting of the Snark:

I said it in Hebrew - I said it in Dutch -  
I said it in German and Greek -  
But I wholly forgot (and it vexes me much)  
That English is what you speak!

40. Jacob M. Landau, The Arabs in Israel: A Political Study (London 1969), pp.196-7.
41. See James Finn, Stirring Times, or Records from Jerusalem Consular Chronicles of 1853 to 1856 (2 vols) (London 1878), vol. 1, pp. 10-11. 'All the world knows that the Russian war of 1853 to 1856 sprang from a controversy about the rights of guardianship at the Christian Sanctuaries of Jerusalem and Bethlehem, as claimed by the convents respectively of Latin and Greek rite.  
'The near connection in which the Latin and Greek communities stand as either joint or part guardians of the Sanctuaries which belong to our Lord's history... soon degenerated into hostility and strife, not for a dogma or creed, as Christendom has in other places so often witnessed, but for possession or custody of locality, inch by inch; and this state of things was perpetuated through the lapse of several centuries. The animosity ripened into personal violence, to the scandal of other Christians who heard

of such doings from a distance, and the ridicule or contempt of unbelievers.' (Finn, op. cit., vol. 1, p.5).

42. Consider in this connection a letter to The Times on December 1, 1973 from one Professor Harry J. Lipkin, of the Weizmann Institute of Science at Rehovot, Israel: 'Sir, most people in Britain are probably unaware of the great British contribution to the Egyptian-Israeli talks at kilometer 101 on the Suez-Cairo road. The English language has enabled both sides to communicate freely without the tower of Babel of interpreters, translations and mistranslations. When both sides are ready to admit that free and efficient communication is more important than the prestige of speaking one's own national language, it is already a great step forward.

'As long as the Egyptians and Israelis are free to speak to one another in English, there may be some hope for peace. But as soon as the Tower of Babel of the United Nations enters the picture, there will be difficulties. The French translation of the United Nations Security Council resolution 242 differs crucially from the original English version, and each side can be expected to insist on the version most favourable to itself.

'One might ask why it was necessary to have a translation at all. At many international scientific conferences I have seen French physicists react with relief and enthusiasm to regulations requiring them to speak in English because French is not recognized as an official language.

'These scientists are more interested in communicating their new ideas to the world than in vainly attempting to revive the past grandeur of France. The scientists have already decided that there is only one language needed for international communications in the Western World and that this language is English. The politicians would do well to follow their example.

'Sincerely yours,

'Harry J. Lipkin.'

Notice his use of the phrase 'great British contribution' to describe the use of the English language, and 'vainly attempting to revive the past grandeur of France' to describe the use of the French language, despite the fact that he is speaking about 'free and efficient communication' as such. This sort of confusion seems not uncommon.

43. Steiner, After Babel, p.470.

44. A study of the distinctiveness of the approach of a linguistics obsessed with universals of language to contemporary political questions presumably remains to be written. Chomsky's own concern with the peace movement, with a universal and fundamental political philosophy that stresses the brotherhood of man and the dignity of human life, is not altogether separable from his well-known rejection of Skinner's stimulus-and-response view

of human language behaviour. 'Habits' and 'conditioning' are not, in Chomsky's view, to be used to describe human behaviour at all, whatever their appropriateness for studying rats in the laboratory. Human beings are different from animals or machines - this difference should be respected in science as it should be in government; it is this conviction which underlies and unifies his politics, his linguistics and his philosophy. (See John Lyons, Chomsky, (London: Fontana/Collins 1970), pp. 13-15). His notion of 'socialist internationalism' leads him, interestingly, to advocate a federal solution to the problems of Palestine which would be based on separate social and political institutions for Jews and Arabs, alongside national institutions embracing both. The concept of lingua franca as advanced in this paper similarly emphasizes a continued and continuing separation of ethnic identities, Jewish and Arab, though leaning specifically on a model of language alone to express that. (cf Noam Chomsky, Israel and the Palestinians, in Uri Davis, Andrew Mack, Nira Yuval-Davis (eds), Israel and the Palestinians, (London: Ithaca Press 1975), pp. 368-409; and notice his view that 'It is unrealistic (sic) to dismiss long-range proposals as 'utopian'. They may provide the only basis for the simpler and more immediate steps that will reduce tension, permit the growth of mutual trust and the expression of common interests that cross national lines...' (Chomsky, op. cit., p.397).)

REVIEW ARTICLE

Marxist Analyses and Social Anthropology

Edited by Maurice Bloch. London. Malaby Press. 1975. 240 pp. £6.75.

This volume makes no claim to an encounter between social anthropology and Marxism. On the other hand, both the title and the content give the impression that social anthropology in some way already includes Marxism, and it should be said at once that this impression is misleading, resting, as it does, on the fact that none of the contributions says anything terribly "Marxist". In short, it's all too well-behaved to be interesting or innovative. Referring to Firth's The Sceptical Anthropologist (reprinted here), Bloch assents that

"In a way Godelier and he (Firth) represent two sides of a debate which both are anxious to maintain." (p. XII).

Firth's position was, of course, that of the "reasonable" man and, in effect, conciliatory. "Marx's theories offer to social anthropology a set of hypotheses ..." which should be treated like any other hypotheses since they're of the same type. By way of contrast, we may note what Ardener (1971) had to say about Marxism, and psychoanalysis.

"These systems are like scythed chariots which slice away positivist reality around them."

The great weakness of Marxist Analyses is that the scythes have been discretely removed. In the present case the cutting edge should result from the fact that Marxism was, and in some quarters still is, a radical political movement. The A.S.A. decennial conference (from which this collection of papers comes) was not, I suppose, the place for baldly political interventions but one does wonder what became of Marx's XIth thesis on Feuerbach.

"The philosophers have only interpreted the world in different ways; the point is to change it."

Apart from any further considerations, the lack of interest shown in "changing the world" means that Marxist Analyses consistently ignores the very real epistemological challenge which Marxism presents as a system.

Even under this handicap, Marxist Analyses displays a number of points of interest, some of which, at least, are of considerable importance for any debate "au dela de structuralisme". On the other hand, the present sluggishness of that debate may be due in part to that peculiar hold of Marxist rhetoric over middle-class intellectuals which is evident in a number of these papers. Surely stray allusions to Marx, grading into adherence to terminology long outgrown, are of little help either intellectually or politically. The "asiatic state", for example, should by now have had its day.

The first paper in Marxist Analyses, Godelier's Modes of Production, Kinship and Demographic Structures, is by far the

most wide-ranging of the contributions and it is difficult not to discuss the others in terms of it. It should be said, however, that many of the points Godelier makes are already published in Horizon, Trajets Marxistes en Anthropologie (Godelier 1973) and reappear here in a rather skeletal form.

"What is attempted is a contribution to the study of the problems of 'structural causality' of the economy: the effect of ... the mode of production on other levels of the social organisation".

(p. 3).

In the attempt, Godelier takes as his basic source Yengoyan's work on Australian Aboriginal demography. When one returns to Yengoyan's original papers (e.g. 1968 and 1970) one is left with the impression that his material has not so much been translated into a new analytic space as simply glossed with Marxist terminology. For instance, Godelier's discussion of "relations of order" (the objective properties of other levels which mediate determination by the material base) is unavoidably hollow since the only logically necessary constraints are those of Yengoyan's model and historical necessities are unestablished. What a society does with "... constraints internal to kinship..." is no more outside history than anything else but we are left with the assentation that the analysis

"... confirms Morgan's findings: relationships of consanguinity change less quickly than those of alliance and, since modifications in the system of alliance are immediately reflected in the family, new types of family appear at the same time as do new alliance rules."

(p. 6).

The system in question is classificatory and Yengoyan (1970) provides examples of its flexibility. Not only can we not glibly assume that a structure is invariant or a purely dependent variable but in this case the possibilities of confusion are all too obvious (vide Needham 1971).

Friedman's model of inter-systemic contradictions between sub-systems provides an expression of the necessarily mutual dependence of all the variables. Tribes, States and Transformations, a relatively lengthy exposition of his analysis of the Kachin of Upper Burma and their neighbours, actualises many of Godelier's earlier (1973) suggestions and in some respects moves beyond them. The self-containing quality of Friedman's model is admirable and the model generates the empirical discontinuity between gumsa and gumlas elegantly enough. More important, it effectively eludes the problem of "determination in the last instance by the economy"; a problem which persists in most of the other papers. (e.g. Godelier p. 13). Rather,

"We have tried to demonstrate how all these variations are parts of a single system of transformations in which particular variants are "determined in the last instance" by the transformation of the conditions of production which limit the possibilities of variation of the relations of production and of the entire social structure."

(p. 197)



In Friedman's model, where the conditions of production constrain the other sub-systems but are also themselves constrained by those sub-systems, not only does the last instance never arrive but it does not haunt and confuse the analysis. His paper demonstrates an appreciation of the fact that a social formation may be "expanded", for the purposes of analysis, in a number of different but equally valid ways. Before taking up this point we might note that, while Friedman's analysis deepens our understanding of Kachin "political systems" considerably, the outright disagreements with Leach (1954) are less frequent than one might expect. Most noticeably, "the state" looks surprisingly similar in the two accounts.

Marx and Engels wondered why the history of the East appeared as a history of religion. The contributions to Marxist Analyses can now confidently explain how history can appear as "kinship", but they seem unwilling as yet to dissolve "economy" with the same vigour. A clear differentiation between "economy" and "material production" is long overdue since the term "economy" lies in the midst of a cluster of related matters of perhaps more fundamental importance. One of the more pressing of these, touched upon but unresolved in Marxist Analyses, is that of the superstructure/infrastructure metaphor. In his contribution to this volume (Economic Scale and the Cycle of Petty Commodity Production), Kahn notes that

"Godelier particularly emphasises that kinship relations, for example, can actually become the social relations of production, and not merely a reflection at the level of ideology of the economic. Economic relations, then, are not relations between people and things, but relations between people with a material element or implication. These relations might, at the same time, be superstructural relations, thus making the layer-cake approach to social structures an untenable one." (p. 147).

If we demystify "the economic" and concentrate on what can be meant by "material", the problem is fundamental. As Feuchtwang notes in Investigating Religion (the third of the papers here),

"Marx's materialism precisely is not a fundamental categorical separation of thought from material human being." (p. 67).

Godelier's notion of "symbolic labour" (1973) and indeed Althusser's earlier usage of the concept of production (1970) already have currency. The realisation that the most tangible examples of "production" are governed by (intangible) "relations" leaves most of the contributors to Marxist Analyses in the position where everything is infrastructural. The resolution of the problem, when it comes, may look Nietzschean from one point of view, it may look Maoist from another, but it seems as though at present the necessary rethinking is hampered by adherence to the old terminology. Indeed, a lingering economism is visible in a number of places throughout the volume. A partial clarification of the problem is to be found in Friedman's exposition of "fetishization". (vide Friedman 1974). Certainly the solution does not lie with Feuchtwang's interpretation of Marx.

"Every human practice - all production - is social, intentional and significant" (p. 67).

Such a view has been castigated often enough (e.g. Banaji 1970) and it's disappointing to see it reappear at this stage.

As a whole, Marxist Analyses presents a slightly dated and inadequate appearance. Kahn's paper and Bloch's Property and the End of Affinity might still contribute to the unfortunate belief that marxist analyses are no more than something to do with "economic anthropology". Worse still, Bloch's use of "capital" (= earthworks), almost à la Salisbury, reveals a fetish in Madagascar only with the aid of a fetish here at home; the very one that Marx himself revealed some time ago. Ternay's Classes and Class Consciousness in the Abron Kingdom of Gyaman is subject to exactly the criticisms which Godelier directed at his previous thinking on modes of production and it represents but a small advance in our thinking about "class" since Marx's manuscript broke off at the vital point. Again, Fenchtwang's investigation of religion appears embarrassingly inadequate, pursuing as it does the idea of a simple parallelism between the "religious" and the "economic". He begins with Althusser's analysis of ideology and ends up with an account which loses the religious experience and requires, in effect, a conspiracy theory of society.

Despite these various shortcomings, Marxist Analyses will, no doubt, be widely read by students of anthropology. The fact that this is so, itself demonstrates a noteworthy state of affairs; Marxism has, in its present form, been thoroughly domesticated. The contributors to this volume seem well aware of the fact and are comfortable with it; there are no worries expressed, for example, about the possibility of "Byzantinism" (Gramsci 1971). So far as I know, only one of the contributors (Fenchtwang) has suffered any discomfiture on account of his "Marxist" affiliations, and the reasons why most of these authors call themselves "Marxist" are to be found, one suspects, in academic fashion rather than political commitment. This is no bad thing per se but we would do well to be aware that the exercise under review has little to do with political activism. What is important here is not the straightforward question of espousal of the cause of "their people" by individual anthropologists. The point at issue is that which I raised at the outset, and lies rather closer to home. Marxism is one of the two major "quasi-positivist" systems and its advantage over positivism lies in its "guess at the programme" to use Ardener's (1971) phrase. This "guess" is by no means uneducated and depends for its usefulness on its situation within a political practice. "Validation through praxis" is something more than a cry from the epistemologically lost: It's an admittedly unclear and poorly articulated perception of the need to include ourselves and to situate ourselves in the analysis. If we exclude this aspect of Marxism, as this volume does, we may be left with pieces of excellent anthropology (e.g. Friedman's paper) but, at the very least, anthropology throws away a chance to go beyond itself.

Paul Dresch.

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Implicit Meanings: Essays in Anthropology, Mary Douglas.  
London: Routledge & Kegan Paul, 1975, 325pp., £7.50

The feeling one gets from a first reading of this remarkable book is one of optimism. In a discipline which has repeatedly been threatened with extinction both from without - as the 'primitives' disappear or are absorbed into industrial society - and from within - as "diversification" occurs, leading to "social anthropology, which has in any case only a nebulous and unconvincing definition...falling apart" (Needham 1970;39), this work is a reflection of Professor Douglas's disregard for the wailings of the Jeremiahs. The golden age of anthropology, one feels, is just over the brow of the next hill, and our journey is already mapped for us:

When anthropology can recognise that thought is the central organising activity, that all social activity is symbolic, and that all behaviour contributes to the constituting of reality, it will be ready for a big theoretical revolution (122)

The aims and skills of Professor Douglas: her desire to "turn the telescope (of ethnographic study) the other way" (213) so as to study 'us' in the same way as we have studied 'them'; and her willingness to borrow inspiration from other disciplines without threatening the integrity of social anthropology, should, one feels, be an example to us all.

And yet this collection of articles, written originally for a variety of audiences, and to which "the author has made slight alterations in the texts" (vii), leaves one with a nagging doubt about whom she is addressing. That which was written for New Society ("Heathen Darkness"), and which will have enlightened its readers, is too bland to be accurately called an essay in anthropology; it is an essay about anthropology for lay readers, who are presumably those who need to be warned to "Take care" when tempted to "touch each other more", because "Uncontrolled, such a practice would rip up any system of communication"(216). Fortunately, this tone is not characteristic of the work as a whole.

Despite the diversity of themes in this collection, it is possible to talk of Implicit Meanings as a whole, the first of which is the already mentioned attempt to use the techniques of anthropology on elements of our own 'culture' so as to dissolve the distinction between the studier and the studied. This requires close attention to details of our daily lives which normally go unquestioned, so that "The humble and trivial case will open the discussion of more exalted examples".(249) But can we generalise that reaching for the Sunday papers is a signal? And the assertion that "Meals properly require the use of at least one mouth-entering utensil per head" (255) evokes, among other things, a certain rhyme concerning the eating of peas with honey: funny, but, it would appear, structurally proper. The discomfort one feels on looking through Professor Douglas's reversed telescope stems from the ambiguity: are 'we' really different from 'them', despite the claims presented here; or are our accounts of 'them' as naive as these accounts of us? A final example to make my point: "To domesticate an animal means to teach it to bring organic processes under control. To socialise a child means the same thing" (213 - my emphasis).

Ambiguity, of course, is Professor Douglas's stock-in-trade, and this brings us to the most positive, if deceptively simple, aspect of the contribution to anthropology provided here. The struggle for neatness and order prevalent in this writer's works gains a good deal from attention to symmetry. In the writings on the Lele, the Hebrews, the Karam and others, this element is fairly obvious (which is not to say unimpressive, especially when one compares her work on pigs in Hebrew culture, in "Deciphering a Meal", with

the work of Marvin Harris on the 'same' subject - Harris 1974:35-46). But symmetry of a different order is detectable in the way in which 'established' theories and methods are assessed: the title of the work balances the search for the explicit found in much contemporary anthropology; 'foregrounding' is contrasted with 'backgrounding'; we learn on page 71 that "...there may be some validity in arguing the other way"; the article "Do Dogs Laugh?" is "offered as a preface to Professor Jenner's discussion of endogenous factors. I will suggest a parallel set of social factors exogenous to the biological organism..."(83); and so on throughout the collection, leading us to learn that "a new, more general trend enables this generation to make a fresh approach"(91). For Professor Douglas, it seems, this trend consists largely of the turning over of anthropological stones in order to see if the negation of established thinking makes as much sense as the original statements themselves. And where the stone has already been turned, as in the debate outlined in her introduction to the selection of "Critical Essays", between those who treat the symbolic as the crux of anthropological investigation, and those who see it as "mere show, an illusion", the answer lies in the mediation of the two, in a plea for "the symbolic system always to be presented with a scrutiny of the social system in which it is generated" (128)..

The work as a whole is an attempt to achieve a balanced harmony in anthropology, to remove the anomalous and 'polluting', and is to that extent successful. As cleanliness is to Godliness, structural order is to good anthropology. It is to be regretted, then, that this collection as a whole does not fulfil the promise and hope of some of its parts. But the "big theoretical revolution", one presumes, is yet to come. It may, perhaps, arrive when the two Professor Douglasses, sensitive ethnographer, and imaginative theorist, finally settle down together.

Keith Patching

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