SOME PROBLEMS FOR MEDITERRANEAN ANTHROPOLOGY

I.

This paper has a less than admirable provisionality, for in it I want to air some of my misgivings about the nature and direction of the new and burgeoning field of Mediterranean anthropology, and while my misgivings arise from two sources — a reading of published ethnographic material, and my own limited field-work in an Ionian island village — certainly I could not claim to have read the entire corpus of Mediterranean studies, and thus far I have conducted only six months field-work in the village (during which time my grasp on the language was tenuous to say the least). Any general criticism I make of Greek, Mediterranean, or even European anthropology must, therefore, be seen as precocious and, in the strict sense of the word, ill-founded. In fact I would be only too happy to have my assertions rejected and my misconstructions pointed out. That is the hope in which this paper has been written. But secondly, though my specific concern is with certain problems which I think to be inherent in the anthropology of Mediterranean (or European) societies, unfortunately they are not, in my opinion, new problems peculiar to this ethnographic sub-division of the discipline; rather, I think it the case that Mediterranean or European anthropology merely throws into new and peculiar relief certain very old problems which lie at the base of the anthropological enterprise as a whole. This being so, I find myself taking on the world — at which point I become a little confused. I hope, therefore, that I may be given leave to proceed in a somewhat erratic fashion.

II.

Let me start with a digression. Anthropology, rather like philosophy, is not amenable to neat and easy definition. It is often quite embarrassing to be asked point-blank by an earnest layman, 'What _is_ social anthropology?' For anthropology has failed to accumulate over the years any tried and trusted body of explanatory theories, strategies and methods which could be applied confidently to whatever field of study the anthropologist has at hand.) Rather, we now find ourselves the heirs to a debris of competing -isms, occasional insights, ad hoc speculations, and dangerous generalizations from which we select as necessity rules. In short, anthropology cannot be definied in terms of a distinctive intellectual practice (though personally I find that no bad thing). More to the point, anthropology can no longer be defined even in terms of a distinctive subject matter — at least not in any way which would clearly establish for it its own and exclusive domain free from the incursions and prior claims of the 'cognate disciplines'. If, in the light of its present diversity, we are reduced to saying that anthropology is somehow concerned with 'the social', veracity is gained at the expense of integrity.

In the past this was not so. Anthropologists could state with a fair degree of confidence that however they did it, they did know what it was they studied: primitive societies. Of course Evans-Pritchard (and a good number of others) was very wary of the 'primitive' even by 1950, for it did not mean 'that the societies it qualifies are either earlier in time or inferior to

other kinds of societies' (1951:7). Nevertheless, though the word was 'perhaps
an unfortunate choice . . . it has now become too widely accepted as a
technical term to be avoided' (ibid). And as a 'technical term' it referred
to 'those societies which are small in scale with regard to numbers, territory,
and range of social contacts, . and which have by comparison with more advanced
societies a simple technology and economy and little specialization of social
function' (ibid:8-- to which might be added the further criteria of 'the
absence of literature, and hence of any systematic art, science, or theology'
(ibid). It should be stressed that Evans-Pritchard was accurately reporting
the state of play, and that he did forsee changes to come. Studies of
'non-primitive' societies had begun already,[2] and he emphasizes that,

> theoretically at any rate, social anthropology is the study of all human
> societies and not merely of primitive societies, even if in practice,
> and for convenience, at the present time its attention is mostly given to
> the institutions of the simpler peoples, for it is evident that there can
> be no separate discipline which restricts itself to these societies
> (ibid: 10, my emphasis).

But 'primitive societies' were nevertheless anthropology's 'convenient
practice', and their study was de facto its definition.

The state of play has, however, changed. Now one finds it neither very
practical nor very convenient to study 'primitive societies' -- and let me
momentarily forestall problems of definition simply by saying that it is
increasingly difficult for anthropologists to work in Africa, South America,
Melanesia, and other areas of traditional interest. But whereas Evans-
Pritchard could state that there was no theoretical necessity for anthropology
to be limited to the study of 'primitive societies', as anthropological
practice has in fact changed, it is now held as an article of some definitional
importance by those in the fore-front of the field that anthropology is certainly
not the study of 'primitive societies'. Thus Jan Ovesen in a recent review
article (1978:1) can refer to the 'traditional but erroneous opinion that
anthropology is the study of primitive societies', and Beidleman can severely
take to task I.M. Lewis' publication last year of an anthropological primer
on the grounds that, 'The emphasis is overwhelmingly upon exotic, preliterate
societies (far too many from East Africa) distant from most readers'
experience', whereas, 'Some of the most provocative and far-reaching work in
anthropology during recent decades has been as much concerned with research
in Western societies and cultures as with others' (1977: 741).

Now the present diversity of anthropology and the present flight from
the 'primitive' as the substantive marker of the discipline have to do with
much more than mere shifts in ethnographic location. A variety of
theoretical stances and concerns has also led us away from the (seemingly)
straight-forward description and documentation of the 'simpler peoples';
indeed one could argue that a definition of anthropology as the study of
'institutions' is as passé as a definition of it as the study of 'primitive
societies'. Nevertheless, to take an example from current rhetoric, the
from 'function' to 'meaning', from 'institution' to 'communication' is, I
think, intimately connected with a shift in the empirical base of anthro-
pology from the study of 'primitive peoples' to the study of, let us say,
'the forms of human experience'. Suffice to note that the passing away of
the conception of anthropology as a strictly empirical discipline removes
the necessity of locating for it a strictly defined empirical field of

study, i.e. 'primitive societies'. But there are other reasons for the demise of the 'primitive' which, with the advantage of hindsight, appear within the history of anthropology to have an almost logical inevitability.

In the nineteenth century the idea of the primitive may not have been well-defined, but it was well understood; that is to say its referents were unambiguously communicated. Primitive societies were both chronologically prior and inferior, and one knew where to find them, which peoples they were. And given that anthropology operated under the cover of a generally accepted evolutionary theory, it was equipped with both a clear object of study, and a clear reason for studying it. There were ourselves and others, and the distinction was qualitative: civilization and savagery. The interest lay in hypothesizing the transition. But with the advent of field-work the great change commences. Savages (and their sex lives) continue to exist; so do primitives (with their law and economics); but historicist speculation is banished, and with this a certain integrity is granted the 'simpler peoples'. At least they are no longer malingerers on the road to civilization. They are fully-fledged societies in their own right. But, once the teleological relationship between savage and civilization is thereby broken, and once, furthermore, a considerable effort is expended in showing the 'rationality' of bizarre beliefs and customs, the 'internal cohesion' of odd ideas and practices, then obviously it becomes increasingly difficult to maintain any clear-cut qualitative set of differences upon which to base a radical division of the world into 'them' and 'us', into 'primitives' and 'non-primitives'. Hence the inverted commas; hence the 'technical definition' — except of course, that it was anything but a technical definition. It was extraordinarily pragmatic and ad hoc definition straining merely to encompass by the enumeration of certain rather arbitrarily selected features, and none too successfully at that, those societies which anthropology had established already on other grounds to be the objects of its study. In fact, if anthropology was not studying primitive societies (without the inverted commas), and if there was no special hierarchical relationship between primitive societies and non-primitive societies, then anthropology was merely studying 'other societies', and the choice of which other societies was an historical contingency. Anthropology becomes a sociology of the exotic, and what is exotic becomes strictly relative to the culture of the anthropologist.

This being so, without any need to consider the practical difficulties now attendant on pursuing studies in the traditional stamping grounds of anthropology, it becomes almost the result of the progression of anthropology's own internal logic that the question should be put, 'Why not study our own culture?' or 'Why not study those European cultures whose traditions overlap our own?' After all, if one cannot define the primitive, and if the primitive has no special place in the grand scheme of things, and if, consequently, our own society has no priveleged status, and if we are all relativists now, then to continue to limit our studies to a handful of societies whose only criteria of selection lie in a discarded theory of the past seems simply perverse. One sees Mary Douglas arguing almost exactly this. The most Durkheimian of present anthropologists is compelled to take Durkheim to task because he did not 'push his thoughts on the social determination of knowledge to their full and radical conclusion' (1975:xi), blocked as he was by two unquestioned assumptions: that primitives were utterly different from us, and that part of our own knowledge was grounded in objective scientific truth. And to make amends, Mary Douglas attempts an across-the-board analysis of cultural symbolism and categorization in

which the Lele pangolin and the European meal are deciphered between the
covers of one book. It is a noble enterprise with which I would not quarrel,
and the logic of its undertaking is compelling; but though I would not wish
to reassert the notion of the 'primitive', certain problems are generated by
such an approach and certain other considerations obscured.

Let us accept the relativity of knowledge; let us dispense with any
notion of the primitive, any privileged status for our own society; let
us assume that in theory European societies are as susceptible to
anthropological scrutiny as any others. One salient feature of traditional
anthropology, perhaps its characteristic feature, remains unaffected by any
reconsideration of the status of the primitive vis à vis the civilized:
namely, that those societies with which anthropologists traditionally have
concerned themselves were at least different from our own. Neither more
nor less complex than our own, perhaps; nor different as a class of societies
from our own, perhaps; but nevertheless each different in its own way from
our own. The point is not insignificant, for it substantially modifies any
view (a view which I have been trying to trace) which sees the dissolution
of any categorical barrier between 'them' and 'us' in terms of 'primitive'
and 'non-primitive' as rearranging the societies of the world in such a way
as to make the decision to work in Spain or Greece or England or Ireland
quite as viable a choice as to work in the Sudan, or the Amazon or New Guinea.
In retrospect and with all the advantages of hindsight I would assert that
it was never the question of the societies which anthropologists studied
being simpler or more primitive than our own which was important; it was the
fact -- the very simple fact -- that they were just different from our own
which gave anthropology its particular perspective and allowed its particular
insights.

The odd thing is that this is well recognized, and that its recognition
stems from almost the same train of relativist reasoning which we have been
attempting to trace. It is recognized in Edwin Ardener's language when he
talks of the 'critical lack of fit of (at least) two entire world-views,
one to another' -- a critical lack of fit, moreover, from whose apprehension
'the anthropological "experience" derives' (1971:xvii). Indeed, one of the
now more oft quoted descriptions of anthropology is in terms of 'cultural
translation' -- a phrase coined, I gather, by Evans-Pritchard, but the title
of a recent volume of essays edited by Beidelman; Beidelman who latterly
has objected because the ethnographic content of Lewis' book was 'distant
from most readers' experience'. There is a contradiction lurking here some-
where, even perhaps a paradox; for a while it is a relativist view which in
denying the existence of two radically different classes of society holds
instead that there are merely 'other societies' all of them equally amenable
to anthropological analysis, including our own, it is this same relativist
view which in denying any privileged 'scientific' status for our own cultural
assumptions reduces anthropology to 'the translation of culture' -- an enter-
prise which must at least assume that there is something which requires
translation into something else, and hence demands (I would have thought) that
what is studied be significantly 'distant from most readers' experience.'
Indeed one could (unkindly) liken Mary Douglas to someone who, having
methodically moved along the surface of a Moebius strip, did not realize
that suddenly she was facing the other way round; for whilst in the absence
of any categorical distinction between 'primitives' and ourselves the move
from studying exotic cultures to studying our own seems a logical and
innocuous progression, precisely because we are dealing with mere relative

points of view the enterprise in fact has changed radically: in the first
case the task was to encounter the seemingly strange and bizarre and, by a
process of 'cultural translation', to render it acceptable to 'common-sense'
so that we might understand it; in the second case the task is to take the
accepted and common-sensical and, by some other process of 'translation',
to render it momentarily strange and bizarre so that we might seek to
understand it.

Now I am not arguing that either of these tasks is invalid, or that
as anthropologists we should not pursue them both. The point is that they
are not tasks of the same order, and that for us the study of our own society
and those basically similar to it does not form a continuum with the study of
exotic, 'different' societies. To assume it does is misleading. True, one
can argue that the study of exotic societies has always led to a reconsider-
ation of the notions, categories, concepts and even institutions of our own
society; nevertheless, a reflection on the validity, or the arbitrariness,
of one's own cultural assumptions brought about by their confrontation with
those of another society is not at all the same thing as an attempt to see
beneath the surface of one's own culture by means purely of a rigorous
scepticism or a sort of self-induced alienation. It was, after all, always
the practical problem of cultural translation, the 'critical lack of fit'
between one's own culture and another, that supplied the empirical basis for
investigation.

The first thing to be said is that the investigation of one's own
cultural apparatus is a very much more difficult affair. As Needham has
stated,

> . . . we too must be thinking about social facts in comparably
> invalidating ways, and {that} in our case also the influences responsible
> can . in principle (if not now) be identified . . . Obviously, this
> radical kind of critique is the hardest of intellectual undertakings,
> for we cannot by deliberation alone detach ourselves sufficiently from
> those tacit premisses which themselves frame or constitute thoughts --
> but we have to try (1976:84).

Yet those 'tacit premisses are inextricable, presumably, from what Mary Douglas
refers to as 'implicit meanings' and which she would have us uncover and
explore in the context of our own society as much as in the societies of
others.

But there is more than a question of relative difficulty involved --
at least if we are concerned with the writing of ethnography. It is hard,
of course, to say what the writing of ethnography should aim now to be,
but one would imagine that a still indispensible requirement was that something
of the experiential nature of the society investigated should be conveyed.
The old question was whether this ever really could be achieved. Could we
come to think of witchcraft, or think with witchcraft, in the manner of a
Zande? Perhaps not. And translation remains translation -- necessarily a
distortion. But given alien institutions and alien concepts, a form of
translation was the best that could be hoped for; it constituted our only
means of grasping the unfamiliar. But what happens when we strike the already
familiar and proceed to render it into something else, to treat it in the
same way as we have been accustomed to treat the exotic? The net effect is
that something which was in the first place quite 'comprehensible', which in

no way offended common-sense, is made to seem mightily peculiar. This is all right if we know what we are up to. If Mary Douglas attempts a structural analysis of a meal, we are not given to suppose that the form of her analysis corresponds to what that meal means for us, or to the way we see it as the almost daily participants in that little piece of ritual. She is uncovering implicit, not ostensible, meanings. But if, as anthropologists returned from the field, we report on the habits of the Greeks, or the Italians, or the Spaniards, habits which, for the most part, I would assert not to be truly bewildering and not to create any crisis of 'understanding' in the generally accepted sense of the word, and if we report on them in the manner, say, of Mary Douglas, then we run the risk of making the peoples who inhabit our ethnographies seem very much more strange and alien than in fact they are. We run the risk of 'anthropologising' them.

Now what I have attempted to do so far is to sketch in the background to what I consider to be a fundamental problem for Mediterranean anthropology. That it should constitute a fundamental problem rests, of course, on my assertion that in the Mediterranean we are dealing with societies basically similar to our own, or at least with societies which, on the surface of it, are comprehensible in terms of our own 'common-sense' assumptions about the nature of the world. To justify this assertion empirically would be impossible. Intuition has to remain the ultimate arbiter of what does and what does not require 'cultural translation', just as in the study of language itself an appeal to linguistic intuition has to be made to determine what does and what does not fall within a particular language's bounds. But if we turn to some of the anthropological writings on Mediterranean societies, I think we can see that the problem does exist, and that there are a range of difficulties which it creates.

III.

John Davis' People of the Mediterranean (1977) is a convenient point of departure, since it offers a review of most work up to 1975. In as much as Davis' book is polemical, it has two contentions: that Mediterraneanists have failed to be comparativist, and that they have failed to be historical. The actual validity of these contentions need not concern us here; what is worth considering are the implications of Davis' proposed remedies; for at almost every stage what Davis desires is the collection of a body of 'hard', quantifiable, statistical data: income-distributions, land-holdings, migration patterns etc., and though in my own case I am appalled by the difficulties I will face in trying to obtain the sort of information Davis wants, nevertheless I have considerable sympathy with his aims. Anton Blok's The Mafia of a Sicilian Village, 1860-1960 (1974) receives high praise from Davis for its thorough investigation of the economic conditions which have obtained in 'Genuardo', backed up, as it is, by the material of local records, agricultural reports, and actual numerical data. I would consider Jose Cutileiro's A Portugese Rural Society (1971) to be a similarly praise-worthy book, and Loizos' writings fall into the same class.[3] But -- and here is the crunch -- admirable though these works are, if they are to be the models of future research, what they spell is that the anthropologist will have simply to join the ranks of the social and economic historians. Whatever it was that really distinguished the great ethnographies of the past, which distinguished anthropology as a subject from history and sociology, has gone; gone because what is lacking is precisely that 'critical lack of fit' between 'two world

views' which prompted the investigation of a range of alien concepts and categories and which, conversely, led us to reflect on the nature of our own habits of thought. What replaces this is the steady compilation of empirical data which, rather than challenging our own world view, merely documents the unknown course of those entities which already inhabit our conceptual universe.

For many (and I suppose I would include myself amongst them) this does not seem very satisfying. We are not attracted to anthropology by the prospect of a life spent counting sheep -- even supposing that there are very many sheep left to count. The need (and perhaps it is merely a romantic need) to assert the distinctive nature of anthropology and its contribution to the study of human society is still there. And here we note something of a split in Mediterranean anthropology. I stress, however, that I am not talking about two camps or schools within the subject; rather, every now and again it seems that anthropologists of Mediterranean societies do want to show that they are more than local historians or village sociologists.

Two books stand out as exemplars of 'traditional' anthropological concern within the Greek context: the justifiably renowned works of John Campbell and Juliet du Boulay, respectivrly Honour, Family and Patronage: a study of institutions and moral values in a Greek mountain Community (1964), and Portrait of a Greek Mountain Village (1974). These were, I think, the inspiration of many of us presently trying to work in Greece. But it should be noted that Campbell did his field-work amongst the Sarakatsani in 1954 and 1955, over twenty years ago, and that, moreover, the Sarakatsani were always rather an odd bunch -- transhumant shepherds living a precarious existence in the interstices of settled communities. Du Boulay, for her part, is commendably honest in stating that she was documenting a 'dying village' and a dying way of life. Both, perhaps, represent the last cases of anthropologists being able to find in Greece communities which were significantly alien and significantly at odds with our own world view. The trouble comes in trying to emulate them in other contexts.

In a recent paper (1978), Malcolm Chapman drew attention to two passages from du Boulay's book in which, talking of the imminent demise of her village and its absorption into the larger society of modern Greece, she refers to a

> ... change from traditional and symbolic thinking to modern and secular thinking (1974:6)

and then later comments that,

> ... whatever may have been its limitations and its defects, there is no doubt that when it was integrated to a living tradition it gave to life both dignity and meaning -- qualities which are conspicuously lacking in the type of society which threatens to succeed it.
>
> (ibid: 258)

Chapman interprets this as both the cri du coeur of someone who believes herself to have witnessed an historical tragedy, and as an example of a confusion of thought which seeks to oppose the symbolic to the literal and

the sacred to the profane. Yet while I accept Chapman's criticism, I think
I can also appreciate the seriousness of what du Boulay has registered; not in
terms of an historical tragedy or the triumph of the secular, but in terms
of the dissolution of the sort of community sufficiently closed and integrated,
and sufficiently different from our own, to allow the writing of traditional
holistic ethnography with all its concern for the inter-relatedness of things
and an alternative view of the world; for now that such communities are
passing away, regretfully I find myself on the side of the sheep-counters.
To do otherwise seems peculiarly dangerous.

In his Curl Prize essay (1969), Davis commences by stating that,

Honour has now the status of an ordering concept used by
anthropologists of the Mediterranean basin to bring together a
variety of phenomena which are not found in all Mediterranean
communities and which are not always related in quite the same way
in different societies. (1969:69)

As Davis makes clear, 'honour' is not without its problems when used cross-
culturally since it tends to group rather disparate phenomena. But even
when one sticks to a single community, the use of 'honour' as an 'ordering
concept' is not without its difficulties. Despite its prevalence in
writings concerned with modern Greece, the odd thing is that the indigenous
terms which it glosses -- timé (honour, worth, value), philotimo (honour,
pride, dignity, self-esteem), egoismos (pride, self-regard) and their reflexes,
and dropi (shame, modestry, prudence) which is generally included in the
same complex -- are not words which one finds regularly or even frequently
on everybody's lips. Whatever 'honour' is within the Greek context, it is
not the translation of a single high-frequency indigenous term. Rather
it seems to be used by anthropologists to embrace a variety of verbal state-
ments (sometimes as simple as the statement that someone or
something is good or bad), and to embrace a wide variety of non-verbal, or
not specifically verbal, behaviour which is seen by the anthropologist
(and hopefully by the Greeks themselves) to relate to the concept of honour.

Well and good; I am not arguing that anthropologists should restrict
their analyses of the operations of a particular concept to the investigation
of the use of a particular word -- though it must be admitted that this
situation does not allow a rather free interpretative hand. But since
'honour' is not an entity, real or imagined, and must be understood rather
as a notion which both motivates an individual's actions and is used by an
individual to classify and judge other people's actions (or situation), one
does tend to find anthropologists leaping inside people's heads with
alarming alacrity. Again, I would not want to dispute the welter of
evidence (and indeed my own observations) that Greek moral judgements are
often rather different from those, say, of an Englishman, and that the
concept of honour does constitute an important part of that difference.
In the hands of so skilled an observer and writer as du Boulay, for example,
the concept of honour does emerge fully-fleshed from the mass of intricate detail
which it resides. But if 'honour' is a 'collective representation' of
singular importance within the Greek context, it must also be admitted that
it is a collective representation of a particular and rather difficult sort.
For it does not resemble even so intangible an entity as, say, 'soul' about
which people can express at least certain ideas as to its nature, location,

ontological status etc., and this regardless of whether or not they actually 'believe' in its existence. 'Honour', on the other hand, is expressed and formed in the context of mutual assessments and estimations, and in the absence of the regular usage of a particular term, it is surely too easy for the anthropologist also to enter into a series of classifications and judgements which are then placed under the rubric of 'honour' and attributed to the people being studied. After all, if I am offended by someone's actions, a slight or an insult (as occasionally I am), and I show my offence in some way or other, it would not be difficult to credit me with a notion of 'honour' and to say that it was my 'honour' which had been offended -- especially if my use or non-use of the term 'honour' was deemed unimportant. Without wishing, then, to discredit the work of the better Mediterraneanists, I do think it the case that we have been forced to enter a field of investigation which is surely amongst the most difficult and uncertain: namely, the notions and feelings that inform and motivate individuals' actions. And in the hands of the less subtle, the ascription of 'honour' to the Greeks as a primary value the possession of which makes them immediately from ourselves, starts, I think, to come very close to ethnographic distortion.

But I have just used the word 'forced', and I think this is the crux of the matter; for in the northern Mediterranean context the alternative to 'sheep-counting' so often appears to entail just such dangerous fields of investigation. It is, surely, much easier to write about those societies which show gross difference at the 'institutional' level, for example those which practise prescriptive alliance or potlach -- even if, as we know, no clear distinction can be made between 'institutions' and 'ideas'. It is easier even to recover a cosmology than to document an attitude. And it is, I think, the lack of gross institutional differences between our society and Mediterranean societies which forces us to concentrate on other issues generally too labile to fix in the language of sociological discourse, without, that is, committing an ethnographic injustice whereby people become as crude as the concepts we employ to describe them.

Family and kinship is an interesting case in point. Somewhat peevishly Davis remarks that it is about time anthropologists started saying something a little more than that in Mediterranean societies the family is 'important' (1977:167). The trouble is that it is quite difficult to say more than that; for the family in Mediterranean societies is rather like our own except somehow more so. On the structural or formal level -- the level which, after all has most exercised the talents of traditional anthropologists -- the Greek, Italian or Spanish family is virtually identical with our own.[4] Variations certainly exist from one community to another with respect to such matters as residence on marriage (patrilocal, matrilocal, neolocal) and the forms of inheritance and dowry, and these are worth investigation; but when one comes really to explain the importance of the family in Greek or Mediterranean societies one finds that it is not a system of rules or any formal qualities which attract notice; rather it is the family's dominance as a moral institution, its closeness, the degree of loyalty it demands from its members, the amount of emotional commitment invested in it, more simply the ubiquity of its mention in daily discourse, which is striking. But once again one is on dangerous ground when analysis is attempted: far away from structure and right into sentiment. Once again generalizations are suspect, because sentiments are malleable. If someone is rich, or powerful, or successful or simply liked, and he is a relative, then the idiom of kinship and the family tends to clothe all reference to him; if he has none of these qualities, or their antitheses,

then the question of his relatedness is seldom mentioned. Family and
kinship do not determine sentiments, at least not with any precision,
but simply afford the framework for their existence and for their expression,
and when one hears it baldly asserted as a matter of established fact that
the fundamental loyalties of Greek villagers are to their nuclear families,
and one also knows that certain brothers hate each other, that certain sons
do not speak to their fathers, and that some mothers bemoan their children
(as they do in our own society), then, despite the fact that one knows
that being the member of a Greek family is not quite the same thing as
being the member of an English family, once again one begins to wonder
about the validity of such statements which seek to account for the collective
persuasions of a community, and about their validity as generalizations which
say something precise about the Greeks which could not also be said about us.

It is, I think, the lack of truly alien institutions, rules, and systems
on the one hand, and the sentimental pragmatism which, in their absence,
appears to govern actions on the other hand, which has led many
Mediterraneanists to espouse something called 'transactionalism' as a label
for their activities. It would be out of place for me to argue here what is
wrong with transactionalism, but it is perhaps worth quoting a passage from a
recent article by Friedl, one of the leading Mediterraneanists, to indicate
its direction:

> In this situation it seemed best to use the individual as the unit of
> study. The research strategy assumes that individual actors allocate
> their time and energy with reference to a set of goals, that they
> have an awareness of the restrictions of their environmental setting
> which makes some behaviour intended to accomplish the end rewarding
> and some not, and that allocations will be continued or changed
> depending on the actual outcome of earlier behaviour. In other words
> individuals keep on making decisions, and one important part of the
> process is the conscious weighing of alternative choices (1976:364).

I dare say, though I don't think this gets us very far,[5] But in an
increasingly complex, increasingly mobile, increasingly open society, which
more and more resembles our own and less and less expresses an easily
discernible coherence, a fixed pattern of institutions and beliefs which
can tidily be summarized, the temptation is to throw in the towel and, if
one wants to talk about more than emigration statistics (and the article by
Friedl is about emigration), to start talking about individual actions on the
basis of some simplistic maximising 'psychological' model.

Not, of course, that the Mediterranean world lacks its institutions,
institutions of some magnitude: the Catholic and Orthodox churches, for
example. And they have a stake in such areas of traditional concern as
kinship and marriage. But their study presents fresh problems for the
anthropologist. Compadrazgo, 'god-parenthood', or in Greek, koumbaria,
has attracted considerable interest. In what I consider to be a good essay,
Stephen Gudeman (1971) has attempted a general analysis of the institution,
and returns to it in a later essay (1975). Sensibly enough, Gudeman is
anxious to get away from a purely 'functional' account, from the reduction
of compadrazgo to its contribution towards 'solidarity', the socialization
of children, the provision of emotional satisfaction, inter- and intra-
class cohesion, and the provision of aid, labour, ceremonial assistance,
housing, and so forth, for god-parenthood is not simply an idiom for these

functions; it has its own irreducible content. To understand this, the village perspective must be abandoned, and Gudeman turns to Church doctrine and history, tracing godparenthood from its first mentions in church writings, through St. Augustine and the various Councils and Synods up to its present fixing in the Catholic church in 1918. Despite a development which was not 'unilinear', he concludes that 'the complex has always been based upon the ideas of spiritual rebirth, spiritual paternity, and consequent spiritual relationships (1971:54). In short, there is a fundamental distinction to be made between the 'spiritual' and the 'natural' person.

It is in the third section of Gudeman's essay where he seeks to apply this distinction between the 'natural' and the 'spiritual' to his field-work data that I become a little uneasy.[6] That the baptismal set reduplicates the biological family is virtually self-evident; but that such a difficult theological notion as the distinction between the 'spiritual' and the 'natural' informs the thought of peasant villagers seems to me rather more dubious. (And lest I sound condescending, let me state that though in one sense I can follow church doctrine on the matter, I certainly find the distinction far too difficult to comprehend. I might adhere to it as an act of faith, but I cannot understand it). It should be noted that, 'the peasants themselves do not explicity recognize this model' (1971:57). Rather, it is seen 'in the way they have "thought out" their social relations. . .' (ibid). But I very much fear that what enables Gudeman to see them 'thinking out' their social relations in terms of a 'spiritual/material' opposition is precisely his knowledge of church doctrine, a knowledge which the peasants do not have. Once again, if the 'spiritual/material' opposition is a collective representation, it is a collective representation of a rather particular sort. Of course it can be argued that the nature of the liaisons which may exist between collective representations and the apprehensions of individuals has always been a difficult question and that, further, on the analogy of language, there is no need to argue for explicit knowledge in order to maintain the existence of such collective representations as ordering concepts. But at least in the case of relatively closed societies, one can assume that such ordering concepts as one discovers are integral to the society whose social relations one is observing. Though I think we would all agree that the nature of god-parenthood cannot be fully understood or 'explained' by studies at the village level, and that a recourse to history and to the great traditions of the church as a whole is thereby necessary, the problem which seems to me to have been avoided is just how, and on what authority, one reintegrates knowledge derived from these sources back at the village level. An ideology recovered in one area does not necessarily inform the practice of another area, even though the existence of that practice cannot be explained without it.

Here we face the second major problem of working with 'non-primitive' societies; for not only do their institutions lack the gross differences from our own which allow easy comment; they also lack integrity. We are not dealing with discrete, bounded societies whose collective representations can be understood by reference to the institutions of that society, or by their inter-relatedness to each other. This precludes not only the simpler forms of 'functionalist' analysis, as Gudeman well understood, but also many of the varieties of contemporary 'structuralism'. In fact I do not think that the 'openess' of most European societies is a separate problem from their not being sufficiently 'alien'. Both problems can be seen to relate to the notion of 'cultural translation', which implies not only that we should be able to encounter something requiring translation, but also that

there exist (at least) two discrete 'cultural languages' or 'codes'. It
has always seemed to me a rather simple point of elementary logic that
if one is going to employ the linguistic analogy for cultures as a whole
and claim that what should be analysed are not entities but relationships,
and that 'meaning' resides in the 'system', then it is a necessary pre-
requisite for such analyses to be able to specify the bounds of the system
within which such relationships are formed. This we cannot do. As a matter
of fact, the problem obtains in linguistics itself. Chomsky, for example,
has to maintain the necesaary fiction of an ideal speaker-listener whose
'competence' bounds the language. And if one wants to keep with the fashion
of linguistic metaphor, then I would say that the problem of working with
European societies is rather like the problem of dealing with 'creole'
languages. Of course there are 'systems', of course there are 'orders',
and 'codes', but their encompassing structure is not stable, and systems
and codes slide past each other and refuse to be frozen into a monolithic
block; into a culture.

Let me give a trivial example from my own field-work. Following the
church wedding, there is an interesting piece of ritual in the village where
I stayed. The bride and groom repair to the bride's house where they eat,
drink, and dance for a short time with a small number of guests. The bride
then takes final leave of her parents, and with the groom goes to her in-laws'
house, where she is greeted by them at the door. She is embraced and given an
axe with which she crosses the thresh-hold three times; she is then given a
pomegranate with which she does likewise before throwing it through the open
door, she is then given a loaf of bread with which she again crosses the
door-way three times; finally she is given a glass of water from which she
sips, and which she then throws over her shoulder onto the crowd of onlookers
behind.

Naturally, asking people what this means doesn't get one far. One old
woman told me that 'Iron makes a strong marriage', but everyone else shrugged
their shoulders, and though they were keen to point out the ritual to me as
an example of a practice unique in their village (I very much doubt that it is),
all they would say about it was, 'Well, that's the way we do things here', or,
'That's the way it's always been'. I was not so naive an anthropologist as
to have hoped for anything other; that form of response appears to be one of
the few universals in anthropology. But how successful can one hope other
interpretative tactics to be? When the on-lookers to that wedding consisted
of university professors and schoolteachers as well as illiterate shepherds,
and when the bulk of the men of the village had sailed, as seamen, quite literally
from China to Peru, can one make a 'structural analysis' of the ritual which
relates it to other 'codes' in their 'social system'? I think not. The
repitition of the triple-crossing, thrice times three, is connected of course
with a variety of church ritual which reiterates the motif of the holy trinity.
But pomegranates? Water? Inside? Outside? Pomegranates, seeds and fertility;
bread and water, the basic necessities of life; a gesture of entering the
house; a gesture of renouncing ties outside. I can think these up; so could
the villagers if pressed. (And we can say similar things about throwing rice
at our own weddings; they throw rice in the village too.) It seemed to me
that what was important about this ritual to the villagers was precisely
their realization that it was a piece of ritual, and their pride in it as an
assertion of regional identity. The ritual itself was, if you like, bounded,
and perceived as important as a unit; the society within which it took place,
however, was not bounded, and I see no a priori reason to expect, and could

find no empirical evidence to suggest, that the elements of that ritual were to be found manipulated elsewhere in the thought of practice of the village -- except, of course, for the motif of the triple-crossing, and that is as pan-Hellenic as Orthodoxy itself.

Not that structural analyses and 'semiotic' approaches have not been tried in the Mediterranean world. David Gilmore has recently published an article on 'The Social Organization of Space: Class, cognition, and residence in a Spanish town'. Social class, he says, has long been considered as a 'structural principle in Mediterranean and Latin American communities', however, 'A Class model may be more than a principle of divison by which people are placed in categories: it may also be internalized as a mental image or paradigm by which the universe of cultural and natural phenomena are ordered' (1977:437). Gilmore then seeks to show the 'generative role' of the class principle and its power as a 'perceptual framework' with respect, in this case, to 'social space'.

Briefly, the town of Fuenmayor has three relatively distinct socio-economic classes: the senoritos, the land-owners; the mayates, small-scale farmers; and the jornaleros, day-labourers. On the whole, the senoritos live in the fashionable centre of the town; the mayates live in a surrounding area, the periferia; and the jornaleros live in the barrio, an area on the outskirts with unpaved streets and inadequate sewerage, electricity, etc. With the exception that a good number of the wealthier middle-class, the 'strong' mayates, also live in the centro, and that the centro is not dead centre, this is all a matter of fact. But suddenly we find that in the interest of symbolic anthropology, this is all a 'perceptual framework', a way of 'ordering cultural and natural phenomena.' Thus we find that 'The barrios are (also) said to reflect a style of life associated with poverty and working class deprivation' -- scarcely suprising since everyone there is working-class, deprived and impoverished -- whereas,..'People tend to think of the centro as consisting of the seven well-paved and immaculate streets and the tree-lined plazas that radiate outwards from this oldest part of the town settlement.' And, 'Since the centre is thought to be the environment of the rich and powerful as well as the fount of political power, the labourers avoid going there' (my emphases throughout). And if one wants a 'symbolic confirmation' of all this, just turn to the cemetery where one finds that the senoritos have beautiful mausoleums and burial niches on a top terrace, the middle-class have well-kept burial niches on a second terrace (costing 500 pesetas), and the poor day-labourers are placed on a third terrace, untended and covered with weeds (and where the burial niches cost 250 pesetas, or nothing at all if one is buried at public expense). One can only be amazed at the lengths to which people will go to organize their conceptual universe.

IV.

Somewhat pretentiously I stated at the beginning of this paper that I thought Mediterranean anthropology raised some fundamental issues for the discipline as a whole, and perhaps I should attempt briefly to justify that remark (though I hope that some of my reasons have been at least adumbrated). I have argued that the fundamental reason why Mediterranean anthropology is floundering is that it is not dealing with societies basically different from our own. As a result, with some notable exceptions, too often it is teetering between the tedious and the silly. But at this point we should ask again why

this should be so. Some ideas have been already suggested. The first is
this: that although we must admit in principle that societies similar to
our own (or even our own society) are as susceptible to anthropological
analysis as any others, as susceptible as the 'primitive societies' with
which anthropology has traditionally dealt, in practice a radical difference
appears, and we find ourselves 'translating' phenomena which are quite
comprehensible already in terms of our own 'common-sense' assumptions about
the nature of the world into some sort of 'anthropological language,' which,
if taken as an ethnographic account of the peoples we are studying, makes
them appear from our own strictly relative point of view to be very much
odder than they are. Inextricably connected with this is the fact that in
the absence of what I have called 'gross institutional differences' there
comes a genuine crisis of 'what to say' about Mediterranean societies. There
seems little point in noting the brands of soap-powders used by villagers.
It is this situation, I think, which forces Mediterraneanists to explore those
areas which are among the most cloudy and unsure -- collective persuasions,
attitudes, sentiments -- or at times to clothe the common-place in a prolix
language of pseudo-analysis.

Perhaps all this is only to say that Mediterraneanists are faced with a
rather more difficult task than their predecessors who worked in other areas
of the world, and that, when the concern is not with the (very ncessary)
compilation of 'hard' sociological data, a much greater emphasis needs to be
placed on the ethnographer's almost novelistic skill to recreate and convey
those subtle but cumulative differences which certainly do exist and which
certainly do give to Mediterranean life a particular quality. But if this
is so, another question, or at least a suspicion, is raised. Could it be
the case that, although almost anything said about an unknown and alien
culture was bound to hold some intrinsic interest because it was alien and
unknown, the analytic manipulations performed on it also seemed more
acceptable just because one's common-sense view of the world was deemed to
be inoperative? Would, for example, something analogous to Gilmore's
argument about 'class cognition' and 'social space' be more acceptable if it
was located in the depths of the Amazon jungle or in 'darkest' Africa?

These are questions which I certainly cannot answer; but one of the things
which has consistently worried me in working with Mediterranean material is
the notion which, in one form or another, I think we all carry round as part
of our professional baggage, and which I think is implicit in almost all
theoretical stances, and which is certainly integral to the predominantly
relativistic view-point we possess: namely, that cultures are bounded entities,
that we encounter total 'social systems', that we can speak of the 'collective
representations' of the so-and-so, and that there are discrete universes of
'socially constructed reality' and nicely intact 'world-views'; in short, that
as anthropologists we can throw a net around the thoughts and practices of a
collectivity. This is, I know, a rather old question, but it still seems to
me to be a rather important one, and the fact that so many attempts to
circumscribe or lay-bare the lives of Mediterranean communities in terms either
of some form of structural analysis or some investigation of basic categories
of thought appear (to me, at least) as rather unsuccessful, whereas, in other
more exotic contexts, similar enterprises did not, may mean that wittingly or
unwittingly Mediterranean anthropology's contribution to the discipline as a
whole will be in terms of promoting some careful reconsideration of the very
notion of 'a culture' or 'a society' which we so readily, almost instinctively,
employ. For it was the dissolution of a radical distinction between 'them' and

'us' in terms of 'primitive' and 'non-primitive' which allowed the advent of a European or Mediterranean anthropology in the first place, it is nevertheless the maintenance of a radical distinction between 'them' and 'us' in terms of cultural boundaries implied by the very search for a society's set of 'collective representations', its 'social system' or its 'cultural code' which impedes us now.

Let me end with an anecdote. My host, 'Bill', owned a cafe-bar in the village, newly constructed out of concrete, bricks and mortar. One day he decided it needed a new exterior window in a side wall, and disappeared by caique to the town, whence he returned with a complete window, already primed and glazed. The policeman strolled past, and Bill beckoned him in to hold the window up against the wall. Bill then quickly drew a pencil from his pocket and traced the outline of the window on the wall. I was still trying to think of the words in Greek for 'tape-measure', 'set-square' and 'plumb-line' when a sledge-hammer was placed in my hands and I was told to knock out the hole. We jammed the window in. Credit where credit is due - it was only about an inch down on the left-hand side - but I could not help feeling that I had just participated in about the shoddiest piece of workmanship I had ever encountered. Then it suddenly occurred to me that had I just witnessed someone erect by eye a grass hut or a wind-break somewhere in Africa or New Guinea, rather than feeling put out by the absence of tape-measures, set-squares or plumb-lines, I should probably have admired the native skill and even written about the process at some length. It was the bricks and mortar which were misleading me. Things were different here, but I was judging them as if they were the same. But then it also occurred to me that by saying that I too was erecting a sort of window; a glass-wall which guaranteed that what lay on the other side had its own rules which were not mine.

Roger Just

NOTES

1. Cf. Needham, R. 1973.

2. Various 'non-primitive' societies studied by American and British anthropologists prior to 1950 are listed by Evans-Pritchard (loc.cit).

3. Loizos 1975 and 1975a.

4. It is worth noting that by far the most interesting book on Mediterranean kinship is that by Hammel (1968), which was largely concerned with Yugoslavian material; and in Yugoslavia one does (or did) find an institution of considerable 'difference' — the zadrugas, kinship groupings which were also corporate bodies with respect to land-holding etc. and, importantly, with respect to god-parenthood ties.

5. The approach is credited in a foot-note to Barth and Boissevain, the best known 'transactionalists'.

6. In fact Gudeman's field-work was conducted in Panama, not the Mediterranean — but the problems are the same.

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