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EDITORIAL NOTE

The idea for this Journal has come from the graduate students at the Subfaculty of Anthropology at Oxford: in particular from those at the Institute of Social Anthropology. Papers given at graduate seminars and ideas arising from work for diplomas and higher degrees very often merit wider circulation and discussion without necessarily being ready for formal publication in professional journals. There obviously exists a need in social anthropology for serious critical and theoretical discussion; JASO sees this at its main purpose. The Oxford University Anthropological Society established a Journal Sub-committee to organise the venture. We gratefully acknowledge a grant from the Wenner-Gren Foundation.

Our congratulations go to E-P, Emeritus Professor of Social Anthropology at Oxford on receiving a knighthood. We are all conscious how much we are in his debt: he is the first since Frazer to be so honoured. Certainly there is no other anthropologist in this country who so richly deserves this public recognition for his contribution to scholarship. He has encouraged the Journal from the outset, and in a sense, it is our tribute to him.

We are sadly grieved to hear of the death of Dr. Jean Buxton. She was a gifted anthropologist and a most charming person. She was originally trained at the Institute in Oxford and has maintained close ties ever since.

FORMAT

We shall produce one issue per term (three per year). Articles are welcome from students in all branches of anthropology and from people in other disciplines interested in social anthropology. Reviews and comments will also be welcome. For the present, it is preferred that the main emphasis should be on analytical discussion rather than on description or ethnography. Papers should be as short as is necessary to get the point over. As a general rule, they should not exceed 5,000 words. For future issues, papers should be submitted following the conventions for citations, notes and references used in the A.S.A. monographs. Communications should be addressed to the Editors, Institute of Social Anthropology, 51, Banbury Road, Oxford.

BACK ISSUES

We have a small stock of back issues still unsold. Individual copies are available at 30p. in the U.K. and \$1 abroad. Volume I complete (1970) is available at the following rates: U.K. - 75p. to individuals, £1 to institutions; abroad - \$2.50 to individuals, \$3 to institutions. The subscription for Vol. II (1971) is the same. (All prices cover postage). Cheques should be made out to the Editors.

ECONOMICS AND THE THEORY OF GAMES IN SOCIAL ANTHROPOLOGY*

This essay owes its existence to a belief that an injection of awareness of the nature of the mathematical models used by academic economists may help anthropologists to make contributions not only to 'the study of society' in the traditional sense but to economic theory itself. We should remember that economic theory is not simply something that is written on blackboards for the entertainment of students; that same model of 'perfect competition on atomistic assumptions' that seemed so thoroughly bankrupt on the pages of Lionel Robbins' famous text-book (1932) has been transformed into the techniques of linear and concave programming which have been, are being, and, perhaps, will be applied to societies of living human beings. In 1965 Robert Solow remarked that the short-run macro-economic model (used to determine policy in developed economies) was now "pretty well in hand", with no more than fifty years more being needed to "fill in the empty boxes". We shall see later just how wrong this claim turns out to be, and examine also the field of "Development Economics", an enterprise about which anthropologists might be expected to have fewer illusions (see Griffin: 1969). But many anthropologists are no doubt sufficiently sensitive to the taunt of "unprogressive" to follow the lead of Edel (1969) and accept the kind of 'underlabourer' role for anthropology that the much avowed success of economic science seems to allot to other disciplines in its system of patronage. Edel argued that the role of the anthropologist is to put flesh on to the bones of the linear programme by specifying preference functions, in particular using his knowledge of the culture's values to help ensure the consistency of the plan's targets, and by making sure that the engineer's production function is compatible with variables whose structural determinants usually lie outside the economist's orbit of empirical research.

All this raises the much debated questions of what 'Economic Anthropology' might study, where 'the Economy' might be located in a social system, what precisely is the meaning of 'Development', and so on. I hope some answers to these questions emerge in the course of my argument. We may begin with Karl Polanyi's characterisation of modern economic theory as 'the theory of a system of interrelated markets in a monetary economy' (Polanyi 1966: my emphasis). This is precisely what orthodox economic theory is not. The kind of economic model we shall be examining here is that of General Economic Equilibrium. Such models utilise the framework of micro-economic analysis to build up a model of the economy which explicitly takes into account its diversity in terms of goods, tastes, wealth and income endowments to individual economic agents, technological possibilities and so on. Macro-economics can be regarded as a special case of general equilibrium theory where the economy consists of one producer, one consumer and 'the government'. Such models constitute the theory of optimal resource allocation, the theme that clearly constitutes the economic background to the work of Raymond

Firth (see Firth: 1939).

The theory sets out to prove that if all producers in the economy maximise profits as individuals, then the whole economy reaches an optimal position, subject to the preferences of consumers being connected and transitive. (This means that each individual must be able to rank any allocation of goods in order of preference, so that he can express a preference between any two goods and that his preferences are consistent.) An optimal position can be specified at one, or a series of resource allocations such that:

- (a) producers obtain maximum revenue;
- (b) the outlay necessary for a consumer to secure any allocation preferred to the selected allocation is not less than that needed to secure the selected allocation.

Given further mathematical assumptions (see Koopmans: 1957), a set of relative prices can be computed at which the agents in the economy will act in such a way that they reach the optimum allocation. This is the essence of programming the economy.

Two things need to be noted here. Firstly, whilst the theory can specify an efficient point, it cannot specify the best of all possible worlds. Secondly, in an economy with many consumers and producers, if even one of those producers or consumers fails to behave in the 'rational' manner demanded by the theory, there is no 'second-best' position to which the economy can be moved (see McFadden: 1969). The whole edifice collapses immediately. Since the constraints on the model are very severe, and could not possibly be satisfied in a real economy, one might conclude that planning was futile and the theory ridiculous. The practising programmer, whilst forced to accept the logic of this argument - which he himself helped to construct - can only defend himself by asserting that some kind of control of what's going on is better than none. There we can leave the theory of resource allocation.

Models of this type clearly make no direct reference to money. Efforts were made to introduce it explicitly, notably by Patinkin (1956). The result of these efforts was to produce yet another theory of a barter economy! To understand this situation we need to look at the classical equilibrium of the economy as expressed in the theories of Léon Walras (1954). Looking at the problem macro-economically, the economy cannot be in equilibrium unless aggregate demand is equal to aggregate supply. In general equilibrium terms this means that all the markets in the economy must be cleared simultaneously; the sellers must sell their goods, the buyers buy as much as they want. This is clearly a case of successful barter. In the optimal resource allocation model a benign planning authority kindly computes a set of prices which enable buyers and sellers to transmit messages to each other about their respective desires. In the Walrasian system a little mechanism called 'tâtonnement' - which literally means "groping" - was introduced to make this possible. Buyers and Sellers come along to the market, but instead of trading with each other directly, they submit tickets to an 'auctioneer' on which they write 'offer prices'. Unless these prices are the same, the auctioneer sends the transactors away to

reconsider their positions. The process continues until an equilibrium price vector is reached throughout the market, and then and only then are the participants allowed to trade. This process of tâtonnement can be seen as a variation on the theme of 'perfect information', and helps us to see that non- tâtonnement processes, where trading is allowed at "false" (i.e. non-equilibrium) prices are those that characterise a monetary economy.

First, it may be helpful to look at the very foundations of the orthodox approach to general equilibrium models with money.¹ A transactor in the traditional economic model is faced with a problem of constrained maximisation. In Patinkin's original formulation this was represented as the maximisation of the utility of a desired quantity of goods and a desired money holding expressed in real terms as purchasing power. The consumer's choices are constrained by the fact that the consumer could not end up, after trading, with a higher value of goods and money than that of his initial endowment, which it was assumed had "fallen like manna from heaven". The problem was that the way these equations were written it was possible to carry out two types of transactions, goods for goods, and money for goods. The result of this is that if some transactors do not wish to hold any money at all, let us say only one transactor wishes to hold money, then money ceases to be used in exchanges at all, and accrues to this single, money-hoarding transactor. The consequences of this possibility are radical, and explain why it has been so difficult to incorporate money into the traditional value theory of 'classical' economics. For, far from satisfying Polanyi's definition of it, modern economic theory has failed to take into account the most basic structural feature of the economies it purported to describe. For as Marx expressed it, every transaction in a pure money economy must be of the form:

Commodity → Money → Commodity (where '→' stands for
"is exchanged for")

The existence of the cash nexus in every sphere of economic life, means that a monetary economy must be portrayed by a model which has at least three goods, only one of which, money, is directly exchangeable for both the others. The orthodoxy has rested throughout on the assumption that one should generalise from two-good models (see Clower: 1967), and has thus been unable to produce a monetary model that was distinguishable from the barter world of Crusoe and Friday.

The belated grasping of what should have been a first principle, has led two economists, Clower (1965) and Leijonhufvud (1968), into a critical re-evaluation of the work of Keynes; the orthodoxy, it must be remembered, had since Hicks' 1937 paper been steadily subsuming Keynes as a special case of the neoclassical model, "useful in practice but contributing nothing in theory". It was felt that Keynes' theory rested on very special assumptions about human behaviour, particularly 'sticky wages' and 'the liquidity trap', which were portrayed as frictions within the machine of perfect competition that resulted in periodic malfunction. His book 'A Treatise on Money' was largely ignored. Clower and Leijonhufvud used a general equilibrium reading of Keynes to reinterpret his work as an attempt to construct an economic model based on true monetary foundations, a basis which had been disguised by neoclassical

macro-economics and those who called themselves "Keynesians" alike. In this reformulation, the existence of the Labour market, and the fact, again unrecognised in Patinkin's equations, that workers were paid in money, not goods, became the crucial determining factor in the existence of unemployment disequilibrium states. Keynes' attack on 'Say's Law' came to be seen as in reality an attack on 'Walras' Law', the idea that the price system was capable of clearing all markets in the economy simultaneously. The familiar Keynesian idea of 'lack of effective demand' was reinterpreted as the information problem that results from dropping the assumption of tâtonnement, the fact that in a monetary economy information must be transmitted at actual ("false") trading prices. The demand for goods of the unemployed worker is "ineffective" because he demands a money wage; he cannot manifest his demand in terms of goods on the market, without the services of the Walrasian auctioneer as intermediary between worker and consumer goods industry. It is precisely in the "price-taking" atomistic market that these services cannot be available. In the Keynesian (reinterpreted) unemployment state the 'potential' purchasing power of the unemployed worker is non-communicable through the monetary medium. A situation results in which all markets are cleared except the labour market, where the excess supply of labour (the unemployed) is equal to the excess demand for money (wages). It is important to realise that Keynes' attack on the principle of 'perfect information' (the dual decision hypothesis) can only be coherently formulated in a theory of a monetary economy, whose basic principles, though recognised by historians and sociologists, escaped the attention of the mainstream of economic theory altogether.² Secondly, we should understand that the Keynesian model is just as much a model of 'rational' and 'maximising' behaviour as the orthodox approach. Where it differs from the latter is in shewing the limitations on behaviour resulting from the information situation of the monetary economic system - with decentralised decision making.

It might seem that economics was now in the process of undergoing a revolution which would at least make it useful for dealing with modern economies. But once these apparently curious assumptions like tâtonnement, or the idea that workers might receive their wages in milled steel, are dropped, the difficulties of constructing a mathematical theory of the economic system multiply considerably.³ A major programmatic statement of the limitations on system-building in economics was provided by Von Neuman and Morgenstern (1953). Their classic work not only supplied the foundation for the theory of games but sketched a perspective for the past and future of economics which should be of great interest to anthropologists. Indeed in the work of Fredrik Barth (1966) we have explicit recognition of this. Since like most formal theories, game theory has been used for flag waving rather than for serious analysis, it is not surprising that the result of this interaction should be a total distortion of the original arguments, and a set of conclusions which seem derisory.

* * * * *

What exactly is game theory? Von Neuman and Morgenstern argued that economic theory had been too ambitious in trying to set up general systems of universal application and should face up to the difficulties of handling mathematically even the limited problems of which we had adequate empirical knowledge. In particular they argued that mathematical economics in the marginalist tradition had largely been concerned with a 'pseudo-maximisation' problem, that of maximising two functions at once. Their theory of games was offered as 'a modest contribution' to economic science. In the light of their own programme, it is perhaps unfortunate that many of the popularisers of game theoretic notions have insisted on trying to make the edifice seem so vast and portentous that only disillusion and retreat have resulted from its application.

Game models can be classified as strictly competitive and non-strictly competitive. Into the former box go zero-sum and constant sum games. In games of this type one player gains at the expense of the other. Zero-sum games are the limit case where "winner takes all". Where the number of 'players' is restricted to two, zero-sum games have solutions, and provide the players with normative rules of how to play. In the case of non-zero-sum games, or zero-sum games with more than two players, solutions tend to be neither general nor in many cases plausible. This is a pity, for it is just here that the theory gets interesting since it deals with phenomena like collusion, side payments and open communication between players. As work in the field proceeded, the limitations of mathematical analysis became only too clear, as it was discovered that even apparently simple n-person games sometimes neither had solutions nor shewed in advance that they lacked solutions.⁴

I should make it clear that game theory is limited by the information situation. We do not need perfect information but we do need complete information. The player of poker who discards some cards has made a move. Another player knows that he has made a move, but does not know which cards have been discarded. Bluffing in a game of poker characterises the game as one of imperfect information, unlike chess where all the moves made up to a certain stage in the game can be observed by the other player. But in both types of game players must have full knowledge about all the payoff values of the game that can result from any given strategy available to them. In other words they must be able to assign probabilities to the outcomes. Next we need to formalise the concept of a strategy. Games can be written down in two ways, extensional form and normal form. The latter is more economical. We represent the game as if the players moved simultaneously rather than in sequence and can write the result down in matrix form. The structure of the matrix usually tells us a player's optimal strategy. Of course, the matrix must be able to take account of the fact that playing the game will quite probably alter the value of the payoffs and enlarge the number of strategies as the players proceed; it must therefore be comprehensive, which means, in the case of 'real-world' games, that we have to go quite deeply into the environment of the game and the way the environment may be affected by play. (In the real world, for example, a game may start with the players behaving in a strictly competitive way, but after a number of moves they may be in a position to collude, which further modifies the environment, and so on.)

The central theorem of game theory is called "min-max" (see Von Neuman and Morgenstern 1953: sections 13-17). The 'payoff' a

player gets depends on which strategy the other player adopts, and in a game with two possible strategies and two players, one of the players may gain £10 by playing his first strategy if the other player plays his first strategy, and may lose £20 by playing his first strategy if the other player plays his second strategy. "Min-Max" tells the players how to minimise their possible losses, and they select that strategy which ensures minimum loss whatever strategy the other player adopts. The values that represent minimum loss (maximum 'security level') for each player are termed the 'max-min' and 'min-max' values of the game. Clearly in the case of some games the max-min value may be the same as the min-max value and these games are called 'strictly determined'; they possess a 'saddle-point'. In games where max-min does not equal min-max, a saddle-point exists if players are allowed to play what are termed 'mixed strategies'. If a game does not have a saddle-point neither player can guarantee minimising his losses and there ceases to be an optimal strategy. The mixed strategy solves this problem, although it is almost impossible to make it sound plausible heuristically by means of a qualitative argument. The reader should imagine that the players select their strategies by means of a random device. The key point to grasp is that mixed strategies follow with perfect logic from the initial axioms of the Von Neuman and Morgenstern theory (op. cit.: sections 9-10). In effect, the player does not choose a strategy, but plays all possible strategies and chooses only the probabilities with which he is going to play them, thus introducing, in a sense, an infinity of available strategies.¹² What one has to decide here is whether, on Von Neuman and Morgenstern's premises, any quantitative result might arise from such a theory. Certainly one side effect of the theory of games has been several suggestive theorems in learning theory, and quite a number of ideas about information processes. But game theory in the formal sense, whatever its metaphoric contributions to other disciplines, has now been fairly fully incorporated into the framework of orthodox theory; nor is this surprising if one remembers that the min-max theorem is the formal equivalent of a linear programming problem, which was indeed expressed in min-max form in Von Neuman's paper: "A model of General Equilibrium" (1945).

Game theory's most serious limitations are revealed precisely in those fields where it might become most interesting. One example is what, at first sight, looks like a simple two-person co-operative game. Co-operation enlarges the set of possible payoffs for both players; they can both be better off, which is the reason for the initial co-operation. The problem is, 'how do they split the spoils'? There is quite a literature on the solution to this game, simple though it is as a sociological phenomenon. There are two basic approaches. One is to examine the question of the strength of the two participants. Obviously, if the game takes place more than once, the threat of a refusal to co-operate next time round is a powerful one, even if one player has the power to enforce his decision. Alternatively, even if one is able to force his decision, and the other announces that 'he won't play any more', a compensation which is just big enough to make it worth his while, that is which enlarges his payoff beyond the limit of the non-co-operative game, may encourage him to co-operate once again. Obviously there are limits to what can happen that seem, in abstract, quite plausible, but we cannot determine the solution with certainty from a mathematical description of the game. The other approach is to specify a 'fair division' of the

spoils, so that with the introduction of an arbitrator a unique solution can be defined. Many of these approaches cannot be reconciled with the Von Neuman and Morgenstern axioms, and those that can seem open to the objection of implausibility or are based on excessively restrictive assumptions. In particular, some solutions of this type suggest that the best course for the players might be to deceive the arbitrator by disguising their true preferences. These kinds of suggestion often came out of game theoretic discussion, and are sometimes themselves susceptible to game theoretic analysis - one might term it "the theory of the optimal lie". They sometimes have a certain amount of real explanatory power; one example is provided by the question of the behaviour of decentralised plant managers in the Soviet economy, where the theory of bilateral monopoly was found to account quite comprehensively for certain biases in the input/output figures the enterprises were sending back to Gosplan.

But in the last analysis, game theory has proved of limited utility in economic, sociological or political analysis. True, it serves as a good metaphor for making work of theoretical triviality seem more portentous than it is. I am thinking particularly of "Stratagems and Spoils" here, but I will deal with that in the last section of the essay. It is also true to say that if one searches hard enough one will find phenomena that could be handled by formal game theory. But game theory scarcely ever provides any qualitatively new results, and on that record it must be judged, although it has done much to clarify and sophisticate some older results (see Luce and Raiffa: 1957).

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I hope that in the light of what I have said the debate on maximisation theories now seems a most curious sort of undertaking. Those who embraced the economics of Lionel Robbins, far from using economic theory as an explanatory device, seemed to be groping around for some of its basic assumptions, which are now summarised as 'convexity properties'. Without convexity properties the mathematical model collapses for mathematical reasons, but as a theory it would surely have been more barren than it is if that is all it had to say. As many critics have pointed out, the study of 'economising behaviour' disperses the economy into every aspect of social life, with results that are plainly ludicrous. But a 'formalist' position does not have to rest on so tenuous a basis. It is still, in principle, possible to go beyond 'economic theory' as we now have it, and construct formal models of 'primitive economics'. But such a programme would encounter the same difficulties that make modern economics what it is. Certainly we can write down useful little pieces of symbolism for heuristic purposes as Steiner did in his "Notes on Comparative Economics" (1954), but I think it most unlikely that one could at the moment get better results than orthodox-style economic theory in terms of global models, and we have seen just how unsuccessful such models have been. Formalism, in the mathematical sense, must proceed piecemeal, if its results are not to be totally trivial, and in saying this I am only following in the tradition of Von Neuman and Morgenstern. Yet at the same time, I think that mathematical models

do need to be introduced into this field more than any other. In the last stages of functionalism, quantification and models that seek to make it rigorously possible, have become a dominant concern. The anthropologist is in a unique position to examine critically the central concepts of development economics, particularly the notions of 'the subsistence economy' and 'economic surplus' that form the basis of the theoretical work in that field. But he will achieve nothing by applying his critique in an 'ad hoc' and unsystematic way. The Development economist has no qualms about locating 'the economy' in another culture, and he will ask the anthropologists precisely the kinds of questions to which Edell's paper seeks to provide answers. If the anthropologist does answer those questions, it is my opinion that he will be denying the validity of what is most useful in the tradition of theory that sprang out of "Argonauts of the Western Pacific" through Mauss rather than through Firth. In fact he will even be denying the validity of the fundamental proposition established by Polanyi (1957), Dalton (1961), and Godelier (1968), among others, that "the anthropological perspective forbids us to describe the economy without showing at the same time its relation with the other elements in the social system." (Godelier op. cit.). So simple a proposition, almost a paradigm statement of the functionalist perspective, implies that we need to do more, much more, than answer the economists' questions. We must ask ourselves exactly what we have discovered about the nature of 'primitive economies' and examine its implications. The result of such a review should be a denial of the economists' questions, and their substitution by more useful ones.

First of all, we know that primitive economic transactions do not correspond with the notion of 'barter' as envisaged in economic theory. In particular, we have been able to establish, following the classic statement of Mauss (1950), that money in its modern form is a means of annihilating social relationships. Mary Douglas' paper "Primitive Rationing" (1967) is a very useful general statement on the question of primitive currencies and pseudo-monies. Polanyi's work on the 'archaic' economy of Dahomey (1966) serves as the most dramatic example. Here we have a highly centralised society, utilising a system of 'economic planning', in the sense that economic decisions are made explicitly in an institutionalised manner. Yet in Dahomey the use of cowrie shell as a standard of value institutionally similar to a modern currency, but expressing a system of social positions, necessitated a radical financial policy. The stabilisation of the transformation rates ('price ratio') between commodities became a necessity in Dahomey because once all social positions had been given a quantitative expression in terms of cowrie - formally equivalent to the introduction of money - the status (and therefore administrative) system could only avoid disruption by a kind of financial management that would seem to militate totally against the western theory of resource allocation. Dahomey is simply an extreme case of the phenomenon observed in those societies featuring 'spheres of exchange', for example the Tiv (Bohannon and Bohannon:1970) and the Fur (Barth:1966), where the native economy fights a rear-guard action against threats to its status system from the introduction of European currency. Douglas describes the presence of three rates of exchange for raffia cloth in the Lele economy in these terms. From this kind of data, a very interesting feature of the 'primitive economy' begins to emerge. Prices are administered by the institutional framework, through the creation of scarcity. Control on

the rates of transformation between commodities is thus exercised in a way that corresponds to a kind of 'planning', but a planning whose object is society and whose organising structures are plainly homologueous with the organising structures of society in general. What economic theory and economic anthropology both lack is a completely adequate theory of price determination. It is well known, as Marx pointed out (1938: chapter XIX), that whilst modern economic theory of the kind I have outlined calls itself 'Theory of Value', (see Debreu: 1959), it tends to be very good at explaining price adjustment and fluctuation in the short run, but relies on the determination of price by cost in the tradition common to Marx and Marshall for its long-run model (see Godelier: 1968: II:S). In the case of the economic formations of primitive societies, we clearly need a more sophisticated approach than this. Steiner pointed out that one could not begin to understand a primitive economy without appreciating that there were transformations of commodities that created value in excess of use-value or production cost. The Potlatch is perhaps the supreme example. Here is a transformation in which maximum value was created by the annihilation of the use-value of an object, by its physical destruction. Steiner's formulation also incorporates a phenomenon of conspicuous consumption, the increase of value by arranging objects in a ritual way, so that the sum of the use-values of the commodities taken individually was exceeded by the prestige-value of the ordered aggregate. Now whilst one could deal with this situation in an orthodox formal model by including the commodity aggregate as a new commodity, to do that would be to destroy rather than enhance our understanding of the phenomenon. One could not, in any case, include the destroyed copper as a new commodity since it has left the system of circulating prestations completely. It is possible, no doubt, to construct a formal model of the Potlatch in terms of a strictly competitive game (similar to oligopoly), but here again the result would be misleading. To capture the full structure within which the contestants make their moves, one would have to take into account not only a complex pattern of threat, bluff and risk-taking but also the overall framework of credit, access to the system, and the effects of particular moves on the flow of resources within the system as a whole. Here we are likely to be near, or perhaps beyond, the limits of our present mathematical competence.⁵ Secondly, behind the potlatch lies the more general question of the basic structure of primitive economic formations, in particular the question of distribution.

One of the most striking features of 'primitive economic organisation' is the way in which competition for status is often kept sharply separate from the question of the organisation of society at the 'subsistence' level. Restrictions on the convertibility of goods between spheres, restrictions on the alienation of property, most notably land and one's own person, the principle of redistribution and the specification of rights of access to the means of production, all these conditions control distribution within the "substantive" economic infrastructure, whilst scarcity and competition - one might borrow Lévi-Strauss' use of the term 'entropy' here - are restricted to a secondary level of activity and circulation. The impact of money on this kind of 'dual economy'⁶ must ultimately bring about the deconstruction of the entire social framework.

This observation brings in not only the question of the impact of the colonial economy, which has been intensively studied, but also the evolutionary and historical aspects of the theory of comparative economics. Polanyi suggested that the market system plus money owed its origins to the effects of the technological innovations of the 18th century, with the attendant increase in risk in capital accumulation, and the pressing need to ensure the maintenance of adequate supplies of raw materials. This theory seems to be defective both historically and as an explanation. The transformation of land and labour into pseudo-commodities had taken place over two hundred years earlier in England. What we really need to examine is the break-down of feudal relations themselves; we have to account for that radical transformation somehow, and I offer a tentative hypothesis. The essence of the prestige economy is monopoly of the means of obtaining status. If, in any 'dual economy' type of society a group does not have access to the coupons essential to obtain prestige goods, they may be able to break into the infrastructural economy by exploiting the scarcity of imported goods; if they can establish a new set of transformations outside the prestige sphere, and secondly utilise that framework of transformations to create their own standards of prestige, thus introducing marketability into a social relation that had previously been subject to social control, number is clearly introduced into an economy which had previously been dependent on quality, and capital accumulation becomes possible. The importing merchant, the archetypal entrepreneur, cannot base his trade on the principle of reciprocity, since his own social position is undefined. The 'monetary revolution' may thus be seen to be an event of the same quality as the neolithic revolution, and it was against such dangers that the archaic economy of Dahomey stood firm. So simple an hypothesis is clearly historically inadequate, but the 'evolutionary' perspective may serve to illustrate the apparent resilience of the primitive economy to the exploitation of 'potential surpluses'. It also tends to suggest that 'money' needs to be rather carefully defined, since its 'unit of account' function seems to precede its 'exchange function' in time - contrary to the economists' emphasis - and it can fill that function without becoming the universal standard and liquid unit that constitutes a modern currency.

The lesson for the development economist is clear. Rather than complain of 'inelastic prices' of the kind Mary Douglas discusses, he would do well to ponder on his assumptions and the effects of his actions. We are faced with the basic category problem that Marx discussed in his brilliant "Introduction to the Critique of Political Economy" (1968), of rebuilding our conceptual apparatus from the ground up. What is needed seems to be something approaching a theory of 'Social Development' rather than a 'Development Economics', and the recognition of this fact should lead to a reconsideration of the notions of "subsistence" and "surplus" that lie at the bottom of modern development economics. Social optimality as defined and possibly in a sense achieved in a primitive society is clearly not necessarily related to efficiency of production in the substantive economy.⁷ This leads us into the thorny thicket of the relationship between 'development' and 'modernisation'; fortunately at least some of the unfortunate recipients of 'development' are able to work out their own solutions to this question without the intervention of what Thomas Balogh once termed the "goodie-goodies".

Finally, I want to examine the work of Fredrik Barth and his disciple, Professor Bailey. Barth (1966) attempts to set up an epistemology and a new type of model for social anthropology. Attacking structural-functionalism for accepting form as a datum, he argues for the construction of 'generative models', from which he hopes to derive the form of social institutions from patterns of social interaction. He also makes the extraordinary claim that the operations of his models are 'logical', in the same sense that game theoretic models are logical, adopting explicitly what he takes as the Von Neuman and Morgenstern paradigm that: "The logical operations whereby forms are generated should mirror actual empirical processes which can be identified in the reality which is being analysed". (Barth 1966).⁸ Quite how his work does correspond with this paradigm rather escapes me. Firstly, the operations in his 'models' are certainly not 'logical' (in the sense relevant to axiomatic systems), and indeed sometimes not even plausible. Secondly, despite pretensions to "methodological rigour" what Barth actually does with his 'models' - especially that of unilineal descent systems - seems more reminiscent of Gluckman and Fortes than Von Neuman and Morgenstern. Even if we separate Barth's programme from his performance, the difficulties of even approaching the analysis of a single social institution are immense, especially in the present state of our mathematical techniques. This is not to argue against trying, quite the contrary, but anthropologists should realise that the limitations of mathematical economics represent as much the limitations of mathematics as the ideological limitations of economists.

But Barth, of course, does not even try. The result of this is that he struggles vainly at the intuitive level and makes exactly those mistakes which the mathematical theories were designed to correct. His 1967 paper, "Economic Spheres in Darfur" fails on its own terms, since although he manages to formulate a linear programming problem - without seeming to be aware of the fact - he lacks the tools to carry his argument to a useful (and logical) conclusion. Far worse than this, since it leads to incorrect results, is his 'idée-fixe' that consistency in social values might be explained with reference to the collective "groping" of individuals in individual "transactions". The argument here (Barth: 1966) is further confused by his failure to distinguish sufficiently clearly 'value' in the sense of 'preference' and 'value' in the sense of 'exchange rate'. But the major mistake was his rejection of the "particular formalism" of the theory of games; either the theory of games or the theory of non-tâtonnement processes would have shewn him (rigorously) that his collective "groping" was more likely to lead to unstable 'values' in the sense of exchange rates and inconsistent patterns of revealed preference. In particular, when talking of social values, he argues that the process of transactions would eventually establish transitivity of social preference. This extreme assertion is clearly contrary to the 'possibility theorem' derived by Arrow (1966) as indeed are all attempts to derive a unique and consistent social ordering from individual preferences in a situation where the choice involves more than two alternatives (and this is a matter of formal logic). Barth's only escape from this dilemma would be to argue that there was complete unanimity, as he seems to be suggesting when he speaks of 'imitation'. But the generative role of transactions then disappears into the Kantian categorical.⁹ In fact Barth's 'model', far from explaining the generation of consistency

and similarity, might logically work in precisely the opposite direction. We can see here quite clearly that Barth's "methodological rigour" is a sham, and it is the very absence of that quality from his work that makes it so desperately inadequate to its own avowed objective.

Professor Bailey is at least more honest. He confesses in "Stratagems and Spoils" (1970) that he is unable to understand formal game theory. Unfortunately, he then continues to spice his work with allusions to zero-sum games, a concept which ought to be irrelevant to his argument. In a sense it is highly relevant; for, like Barth, had he been able to understand formal game theory, he would have been better able to appreciate the limitations of that form of theoretical construct, the model based on individualistic social interaction, as an explanation of historical and sociological phenomena. Despite its pretentious sub-title, Bailey's book turns out to be a series of very poor metaphors, designed to dignify an otherwise trivial form of intellectual parasitism. For it is by now becoming clear that it is not merely the mathematics that limits us in this case, as Bailey seems to imagine, but the whole conceptual apparatus of individualistic models that is inadequate to the task in hand.¹⁰ When we read that "since social change is worked out through the actions of men and their failure to act", it can thereby be reduced to a series of games which will but rarely be capable of solution (and therefore seemingly low on explanatory power), the hollow ring of 'trendyness' becomes unbearable.

We must conclude from the sad experience of these two writers that Gluckmanesque "naivety" can only lead to abysmal failures and the ridicule of other disciplines. In particular, making another discipline's mistakes all over again seems a sorry achievement for a life's work. Only a full and informed grasp of the successes and failures of other disciplines will make it possible for anthropologists to pursue their own data to the level of theoretical adequacy. Economic anthropology has long been in the grip of a mythological view of economic theory, a view from which it must be emancipated if it is to make the fresh and distinctive contributions to science that this essay has suggested lies ahead of it. But awareness must be strongly tempered with criticism; for if the anthropologists' results end up looking like those of orthodox economic theory, "we may be sure that they are wrong."

John Gledhill

Notes

- * This essay is a revised version of a paper read at Mr. Ardener's Tuesday seminar in Queen Elizabeth House, Oxford, during the Hilary term 1971.
- 1. This approach rests on Clower (1967), although, as I remark, he is only 'rediscovering' an observation of Marx. See also, Von Neuman and Morgenstern (1953) 2.2.1.
- 2. Economists are particularly blind to the results of other disciplines. In attempting to 'explain' why money enters the general equilibrium model at all, they usually resort to pseudo-evolutionary speculation. Here is an example:

"To lend intuitive color to our story, suppose that all individuals in our barter world live on a wooded island (perhaps in company with the odd snake and tiger) and must seek out other individuals as and when they wish to engage in economic transactions. We need not conceive the society to be primitive in an anthropological sense; on the contrary, we may suppose that institutions for the protection of individual limbs, lives, property and the sanctity of exchange contracts are as highly developed as might be desired by the most ardent believer in laissez-faire..." (Clower: introduction to Penguin readings in Monetary Theory, 1969).

The ignorance among anthropologists of the nature of economic theory, and in particular, its extreme limitations, is, of course, equally serious.

3. Even the existence of the orthodox competitive equilibrium requires qualitative (i.e. topological) mathematical argument of some complexity. See Von Neuman (1945), and Koopmans (1957). Leijonhufvud's quite detailed book is non-mathematical.
4. See Shapley and Shubik (1969). The significant result of this paper was that the games that were needed for the theory of perfect competition did have determinate solutions. These results are all concerned with what are termed 'n-person inessential games', i.e. those games in which it does not pay a player to join a coalition. The basic theorem - 'the core of the economy' - states that when the number of economic agents reaches denumerable infinity, no one of them can affect the price at which a transaction is made. In the 'old-style' theory this vital number was expressed merely as 'many'. Shapley and Shubik's "balanced games" comprise the core. One might conclude that mathematical analysis in this field was therefore confined to the trivial, though the rigorous delimitation of triviality is clearly important.
5. Nevertheless, it is important to try to make some progress in this area. The Potlatch is just such an example of the possibilities of making a successful attack on limited and definable problems, with a view ultimately to achieving a more general understanding of the nature of primitive social formations in the global sense. When examining the Potlatch we should be conscious of the larger phenomenon of which it is an exemplification, in order to guide our questioning of the data in the most fruitful direction.
6. My use of this term has, of course, nothing to do with its use in Development economics.
7. In a real sense "planning" is much easier in the 'substantive' primitive economy, precisely because of the simplification of the information problem which I have tried to show characterises the economy in which transactions must be carried out through a true monetary medium. Primitive economies are not characterised at the infrastructural level by uncertainty as to the actions of economic agents, though when making decisions with respect to the ecological environment they (like us) are faced with the uncertainty of nature. The economic behaviour of native populations is puzzling to the planner precisely because it is more structured than he imagines, and structured in a way to which his preconceptions leave him blind.

8. Compare this statement closely with Von Neuman and Morgenstern (1953) Section 4.1.3.
9. Suppose that transactions constituted a learning process by which every participant discovered the values of others. 'Imitation' cannot explain why a certain value is selected as the norm. If the 'majority view' triumphs, then logically, there must be a minority whose values differ. Furthermore the isolated transactor could not know which was the 'right' value, without the intervention of a mechanism like 'tâtonnement'. See Arrow (1966) for an examination of the Kantian alternatives. We should also note that Lévi-Strauss' ('le Cru et le Cuit':1964) accepts Ricoer's characterisation of his work as 'a Kantianism without a transcendental object.'
10. This implies that mathematical argument in this field would have to take a different form if it is to be possible at all. The real danger of a book like Bailey's is that its effect is actually to suppress the results of the formal work, and restore a measure of credence to results that are rigourously untenable.

Bibliography

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| Arrow | 1966 | Social Choice and Individual values. |
| Bailey | 1970 | Stratagems and Spoils. |
| Barth | 1966 | Models of Social Organisation, RAI. |
| | 1967 | Economic Spheres in Darfur, in ASA 6, ed. Firth. |
| Bohannan and Bohannan | 1970 | Tiv Economy. |
| Clower | 1965 | The Keynesian Counter-revolution: A Theoretical Appraisal; in Hahn and Brechling, editors, <u>The Theory of Interest Rates</u> . |
| | 1967 | A Reconsideration of the Micro-Foundations of Monetary Theory, <u>Western Economic Journal</u> . |
| Dalton | 1961 | Economic Theory and Primitive Society, <u>American Anthropologist</u> . |
| Debreu | 1959 | Theory of Value. |
| Douglas | 1967 | Primitive Rationing, in ASA 6. |
| Edel | 1969 | Economic Analysis in An Anthropological Setting, <u>American Anthropologist</u> . |
| Firth | 1939 | Primitive Polynesian Economy. |
| Godelier | 1968 | Rationalité et Irrationalité en Economie. |
| Griffin | 1969 | Underdevelopment in Spanish America. |
| Hicks | 1937 | Mr. Keynes and the Classics; a suggested interpretation, <u>Econometrica</u> . |
| Koopmans | 1957 | Three Essays on the State of Economic Science. |
| Leijonhufvud | 1968 | Keynesian Economics and the Economics of Keynes. |

- Luce and Raiffa 1957 Games and Decisions.
- Marx 1938 Capital, vol. 1.
- 1968 Introduction to the Critique of Political Economy, (from the Grundrisse), in Horowitz, editor, Marx and Modern Economics.
- Mauss 1950 Essai sur le Don.
- McFadden 1969 A Note on Second-Best Pareto Optimality, Journal of Economic Theory.
- Patinkin 1956 Money, Interest and Prices.
- Polanyi 1957 The Economy as Instituted Process: in Trade and Markets in the Early Empires.
- 1966 Da homey and the Slave Trade.
- Robbins 1932 The Subject Matter of Economics.
- Sharpley and Shubik 1969 Market Games, Journal of Economic Theory.
- Steiner 1954 Notes on Comparative Economics, BJS.
- Von Neuman 1945 A Model of General Equilibrium: Review of Economic Studies.
- Von Neuman and Morgenstern 1953 Theory of Games and Economic Behaviour (Revised Edition).
- Walras 1954 Elements of Pure Economics.

"ANTHROPOLOGY WITHIN AND WITHOUT THE IVORY TOWERS"

I want in this paper¹ to make a temporary bridge between the thinking of theoretical anthropologists conducted as it is within the cosy confines of this most prestigious university, and that of the many lecturers and teachers in colleges and schools outside, as well as the mass of intelligent lay public with little formal education who nevertheless aspire to know what you, in your ivory towers, are doing, and what you have to say to them about Man, the social animal. There are analogies in the position held by university courses in anthropology in the past with the idealistic struggles of lesser institutions of learning today, to spread sweetness and light among the masses, which I think bear consideration. I intend, therefore, to exploit what I believe to be my marginal position in social anthropology to talk about the educational implications of the subject.

It was in this University, not far off a century ago, that anthropology was first thoroughly established as a subject by that rationalist Quaker, Edward Tylor. The Oxford diploma is not only the oldest one-year course, but it is the original postgraduate diploma course which was ever initiated; and one which, as Marett remarked when he held the Oxford chair for one year in 1934, many other university courses later used as a model. Today, a year's postgraduate course alone can hardly cover the tremendous field of knowledge into which social anthropology has blossomed since Radcliffe-Brown taught here first about 35 years ago. It can only be an introduction to the research degrees for which this University is famous. What a world of difference, one might think, will separate the student attracted to such a course from those for whom the raw, new upstart courses of the colleges and institutes of education, the colleges of further and of adult education, and the polytechnics, cater. Yet in a curious way, these parvenue institutions have inherited some of the cast-off purposes of the late 19th century, and have been seized with the same moralising fervour as earlier inspired this University. They have tried to introduce not only adults and adolescents, but in some cases even children, to "the study of man and civilization, not only as a matter of scientific interest, but because we have in it the means of understanding our own lives and our place in the world ... and to guide us in our duty of leaving the world better than we found it." If you do not recognise that quotation, let me continue it: "In times when subjects of education have multiplied, it may seem at first a hardship to lay on the already heavily-pressed student a new science. But it will be found that the real effect of anthropology is rather to lighten than increase the strain of learning. So it is with the science of man and civilisation, which connects into a more manageable whole the scattered subjects of an ordinary education."

Those words with which Edward Tylor began his little introductory book on Anthropology in 1881, have been used as a coda with which to end one of the most modern introductions to Social Anthropology, that which Godfrey Lienhardt published in 1964, and they are still relevant.

In the interim, some twenty years ago, your Emeritus Professor,

Evans-Pritchard, in introducing some published talks given by the B.B.C. in 1952, somewhat sourly observed that students of other subjects and people interested in different kinds of scholarship tend usually to think of anthropology in terms of theories put forward about 50 years ago. "New knowledge," he then said, "is very slowly absorbed outside the small circle of specialists who create it ... laymen cannot be expected to read all the large monographs and all the innumerable papers in learned journals; so it is the duty of anthropologists to present to the public from time to time in more popular form, conclusions they have reached and the problems they are seeking to solve."

Perhaps this remark, in its implications of academic "noblesse oblige", dates somewhat. Perhaps it is the conclusions rather than the methods which need public interpretation. It might appear that the lay public today falls upon those large volumes, and devours, quite undigested, both the cooked and the uncooked, both the wild and the cultivated forms of anthropological thought, almost as soon as the specialist has published them. Nevertheless, he makes the point, which I wish to stress, that there are traditional moral obligations of some force and standing in this subject which demand a fairly constant stream of communication, which I believe should also be in more than one direction, between the universities and the intelligent lay public, between both teachers and students, and from places of learning well outside the spires of Oxford, or Cambridge, the towers of London, or even the great blocks of Sussex, let alone the ordinary buildings of Manchester, or Edinburgh or Durham. And even more so is this interpretation necessary today than 50 years ago, when no shop could have sold a book on, say, Frazer or Malinowski by the thousands, as they recently have done for one on Lévi-Strauss by Edmund Leach, or on ritual by Mary Douglas.

Now in some quarters, while it is admitted that there ought to be this communication, to the outside world from the universities, it is often not considered that there should be any necessary counter-communication. The relationship between what goes on within the universities and without has been differently conceived at different times, and discussion of it is nothing new. Nevertheless, it is a discussion which needs to be continually kept alive, as conditions both within and without the universities change, so necessarily affecting the relationship. Sir Eric Ashby recently pointed out that it was the wealth of Oxford and of Cambridge which enabled them to preserve a great deal of freedom both from the state and (in their more vigorous phases) from the church. This power was used to allow each master: "freedom to do his own thing" - Sir Eric's use of the modern jargon of the left. But he goes on: "If academic freedom was not often questioned in nineteenth century England, it was because no one much cared what professors taught or wrote; it was a freedom which did not matter."

Today it does matter. It is of concern at every level. The pressures of public opinion range from the most recondite at the apex of the system, where professional councils award research money, and direct students to where they may pursue their particular form of research, through those of intermediate prestige, business and other foundations whose funds endow new chairs or pay for library buildings, till we reach the third estate of longhaired, unshaven and untaught (I did not say unteachable) students who loudly demand that their course have some social relevance. By their physical actions of sitting down, shouting down, or breaking down, this new group may succeed in disrupting the conventional structures of university teaching, at least temporarily, in some places. Although a new risk in the university, what I wish to stress is that these kinds of things have been happening before, but at a different level in the educational hierarchy. School teachers, appalled at the aggression and intellectual

indifference of schoolchildren, have sought teaching posts in colleges and institutes of education; some already there, harassed by the demands for formal teaching and lack of time for their own research and writing, take wing further up to full university posts. But the dilemma which drives them all is the difficulty of reconciling the desire to learn more oneself with the obligations to teach more to others. This is a direct outcome, isn't it, of the explosion of student numbers, and of educational opportunity at all ages, and for both sexes and all social levels, something very few of us could seriously deplore or seek to alter.

There has been a kind of inflationary demand for knowledge in all fields, but particularly in the fields of the behavioural sciences, which, like all inflationary demands, can be seen as devaluating the whole category of goods demanded, by eliciting a stream of substitutes of less and less value from the original scarce good. Can one defend such a dilution? Is it possible to popularise without debasing a subject or unduly distorting its methodological principles?

The R.A.I. called a special series of meetings in 1964 to discuss the teaching of social anthropology outside university departments, and even then opinion was divided between what Paul Stirling called the Mandarins - who wanted anthropology for Mature Minds only, and the Missionaries - who felt it had a Message for Everyone. But no doubt as an indirect result of their deliberations, a friend recently reported to me that her school age daughter has been taking part in a Project on Witchcraft, and moreover that, based to some extent on Lucy Mair's popular study, it was well conceived and reasonably carried out.

Now although such a course would have been impossible without the help of professional popularization, whatever of value was learnt certainly was not presented an "anthropology".

So the first thing I want to say about anthropological teaching in the market place, is that it has mostly to be done indirectly. Most people think of the subject as having concern only and mainly with primitive peoples, who are to be studied in order to show how much wiser and better we in the civilised world now do things. It is accepted as a subject of study for overseas students, mainly for giving an outline of the facts of social structure it is expected that they will meet, but not necessarily as a systematic way of looking at that structure.

Perhaps one of the main reasons for this viewpoint lies in the unfortunate dominion which Margaret Mead's work has had over that of all other anthropologists in the field of popular education. In fact, her name seems to be the only one known to the "educationists"; and students, with no prior knowledge of the structure of simple societies or of the methods or general aims of social anthropology in general, have been introduced to these books by the thousand in training colleges. It is not surprising that they have swallowed the story of Samoan girlhood or New Guinea childhood whole, much as they might some novel, and have acquired absolutely no general principles from them at all.

It is, therefore, true to say, and I think one can be glad about this, that pure theory of social anthropology as such is not, in general, taught badly or wrongly by unqualified people, as it very often is in the case of sociology. Anthropology, if it is taught, is taught "by stealth" as to the schoolchild who does not say she is doing an "anthropology course", but a study of witches, So it is also in the new degree courses for teachers and

general studies courses. Many aspects of the new syllabuses in education clearly call for handling by someone with an anthropological point of view, but they do not, I think, get that kind of approach very often. So I will now try to show you (i) what I personally believe a social anthropologist should try to get across to non-specialist students, (ii) how one can try to get it across, and (iii) what the student reaction is.

(i) The Main Aim

The most fundamental insight to be gained should be that the behaviour of man in society is patterned, and that the social patterns have some meaning. Also that there is always a sense in which the patterns hang together and relate to each other. The social constraints on behaviour are not only essential to our development as "persons", but they also explain, or excuse if you like, the limitations on what each person can do with his own personality. It is the extent to which individual freedom to behave and to interact with other individuals is limited, and our power to change the imperfect conditions of our own lives, which I believe it is important that students understand. The American-inspired 'culture and personality' school of thought has laid undue stress, to my mind, on how socialisation is supposed to make us feel differently, rather than just behave differently, in different cultures. The stress on psychological conditioning which this viewpoint emphasises is naturally onerous, especially to the young; it degrades their sense of personal integrity and individual power and personal responsibility. Almost exactly the obverse conception is stressed by a purely sociological analysis which may seem to point to the possibility of a complete emancipation of the individual by altering the structure of his society in such a way as to free him of the so-called artificial restraints of class or caste, the bonds of sexual role, kinship obligations, and so on. This point of view is naturally more attractive to the young, suggesting to them that social re-organisation, political or ideological revolution can free a man to do or to become just what he pleases. And it is undoubtedly one of the attractions of current sociology courses.

But neither viewpoint is, in my opinion, quite valid, although each stresses an aspect of the eternal dilemma of the human condition. Cultural conditioning stresses the impotence of persons. Sociological analysis stresses man's omnipotence to free himself by changing the social system. Neither represents accurately the reality of our social world. But some conception of the compromises which men everywhere have had to make can most effectively be understood by the study of social anthropology, because it examines behaviour in many different types of society, and recognises the difference between what is done and what is supposed to be done. It can lift our vision beyond immediate problems, and suggest a valid philosophical acceptance of the inconsistencies and vagaries of social life.

Some answer, even faltering or tentative, to the question of what life means is one of the most urgent demands of the intelligent and idealistic young today; whether they be children in school, subject to cramming with all sorts of technical expertise to fit them into a society so huge and impersonal they often feel they are being treated as things and not people; or privileged students with time in universities to gain some detachment from, and insight into, the system before they also are overpowered by it.

The relevance of what the young had to learn in our own past as in other cultures could be justified by the immediate exigencies of the situation. In social systems which changed more slowly, or in which sheer poverty dominated life, disease, disaster and lack of technological mastery of the environment gave the young little time or opportunity to question the "relevance" of what they had to learn .. A Malay peasant in Kelantan who

did not learn to fish or grow rice starved. A Tikopia who did not learn the traditional respect for the gods relinquished his rights to the protection and collaboration of his kin and his neighbours. A Trobriander who did not see the relevance of kula exchange must have opted out of the main stream of social, economic and ritual relationships. Whether the Bemba girls understood the "relevance" of their chisungu initiation rites or not, to refuse to go through with them would be to refuse marriage and the only viable life for women at the time. To question the values and fail to gain the skills of the industrial economy of nineteenth century England was to court starvation if you were poor, social ridicule if you were rich.

But today, the impersonal and impartial structure of the welfare state, even in marginally welfare-orientated societies in the West, gives the young economic support of a kind even if they do not conform; education has enabled them to question and challenge both the structure and the purposes of society and the relevance of these purposes to their own education. When wealth has provided leisure to ask questions, and science seems to offer the power to provide solutions, questioning is natural; and not only intellectual questioning, but organised political and physical testing of the system is now possible in a way it has rarely been before.

If one can learn how other people, in other societies, have dealt with the problem of law and of law-breaking, of conformity and deviance, of respect for the gods and for desecration of the temples, of the rites and duties - as Maurice Freedman has called them - or the constraints and advantages, or sheer impossibilities or marriage, of the uses of art, and the meaning of ritual and of religion, one can perhaps see new meaning in what were otherwise regarded as useless patterns of our own social life.

In talking of the anthropologist's vision, Lévi-Strauss says that such observations only become possible by virtue of the distance from which they are glimpsed. How do we get students who have not been in the field, students who have no time to read "The Gift", or "The Argonauts", or to learn the complicated methodology of kinship studies, to see these structures of social control and their purposes, and to comprehend something of this world view?

(ii) Method

The first priority is that, whatever the subject be called, it must be made attractive. The necessity to make the students like what they are doing initially is not only that one learns better if so motivated, but also because learning social anthropology can be a very disturbing experience. We all know about culture shock. If we do not actually suffer from that, all field workers have suffered self-doubt, loneliness, anxiety, depression, or frustration partly because, alone of all of the research workers, he must eat, sleep and play, as well as work, in his laboratory. There is an analogy in the feelings experienced over a first field trip with the experiences incidental to a course of psycho-analysis. There is a very good reason why it should be so. In each case, the individual has to go through some kind of regression. He has to re-orientate all his predilections, learn even to speak all over again, he has to learn how to behave, he has to ask for many of the things which he owned before; he has to acquire a new status, new friends, play new roles, suspend judgment on nearly every issue which he perceives. This is what enables him to record, understand and analyse what goes on before him with as few preconceptions as possible.

The young student who comes first to college expects to increase his knowledge by receiving "nubs" of it, as it were, directly from his tutors. He

opens his intellectual mouth and often expects the tutor to feed hunks of information into it, rather as keepers at the zoo feed penguins. The students believe they know what they want, although they may have differing expectations of the way they are to get it. They may see the tutor as exhorting hard and painful labour as the price for these rewards, or they may see him as a friendly, public-spirited person willing to give away his treasures to any that will politely ask for them.

This is not, however, what really happens in any learning situation, although it may sometimes appear to happen. If hunks or hunks of knowledge are really tendered to the student like this, he will not be able to use them or "digest" them in the terms of the analogy; either he will reject them - vomit them up so to speak - or they will pass painlessly away from him in the process of rendering them back again in an examination. Why? Because that system leads the student to juggle with words and with phrases which he has picked up on the course without truly understanding to what sort of reality they refer. I have seen it happen often in the teaching of sociology, that what is learnt is a string of words, a kind of jargon or jingle which does not illuminate the reality of social relations, but prevents them from being seen. Labels, which should enable one to distinguish conceptual categories, can easily be used as a shield to prevent one having to go through the painful process of looking at them oneself. So words are bandied about without any proper conception of the things to which they relate. Social relations cannot be seen like cells under the biologist's microscope - one has to learn to see them through their effects.

The teacher's task, then, is to help the process of seeing things in a new way, of undoing lifelong habits of judging in ways learned in childhood, and yet without destroying self confidence too much.

This is where the analogy lies with the traumas of the field experience; students must unlearn much of what they bring to the course in order to benefit from the relearning which is offered to them. Unlearning makes one vulnerable. The teacher has to balance the extent he must allow vulnerability to allow relearning, with the danger that, if the student is made too vulnerable, he will withdraw, and reject all that he might acquire, by refusing to go on thinking and observing in the new ways which are required of him. So that if the subject is initially not made very attractive, or if the goals don't seem worth while, the student will give up.

It is easy to seem to be rather metaphysical in trying to describe the hazards of teaching in this way, but although it may be true that all real new learning is at the cost of abandoning preconceived learning, in the sociological field it is giving up the early convictions and moral preconceptions of one's childhood which may cause shock and confusion, and considerable emotional strain may result. I tell my students that they may expect to be more confused and uncertain than when they arrived before they get to the end of the course, but that somewhere about two thirds of the way through light will dawn.

Lévi-Strauss recalled that Marcel Mauss referred to anthropology as an "original mode of knowing rather than a source of particular types of knowledge", and he describes the field research situation as the paradigm of that concept. He describes in his inaugural lecture, "the field research with which every anthropological career begins (as) the mother and wet nurse of doubt, the philosophical attitude par excellence. This anthropological doubt does not only consist of knowing that one knows nothing, but of resolutely exposing what one thought one knew, and indeed one's very own ignorance, to the buffetings and denials which are directed at one's most cherished ideas and habits by other ideas and habits which must needs contradict them to the highest degree."

I think you will see why I compared the difficulty of learning the perspectives of social anthropology with those experienced in psycho-analysis, which can also be regarded as a "mode of knowing". And I am not in the least confusing the two, any more than Lévi-Strauss confuses the two, when I say, that, in their effects, both may be very similar both in difficulties encountered and the rewards gained. These are, for instance, the emotional and intellectual assurance which can come from having subjected oneself to rigorous self-examination, either on the couch or in the loneliness of the field worker's tent, from having looked at oneself either through the spectacles of the psychiatrist or the oddly distorting spectacles of friends and informants in the other culture. Each acts as a mirror, at once illuminating and disturbing, in which one sees oneself through alien eyes and one's behaviour mirrored by the behaviour of others.

So the student must be persuaded not only to look into those revealing mirrors, but to maintain his regard there, analysing what he sees. I try to get the class into the position of a group with its own system of norms and sanctions, and I try to get the students to do in the tiny temporary isolation of the class situation what the anthropologist does in his really isolated field over a much greater length of time. I try to get one student to hold up a mirror to another and then to get them all examining what happens in the class as a micro-social system. I challenge and get them to query every generalisation about behaviour and every moral judgment which they make - quite ruthlessly at first. A very illuminating - if dangerous - method of getting students to think about what is meant by social control, and what is the meaning of a positive and negative sanction, is to ask them to consider seriously why they come to class or lecture at all, what would happen if they did not, why or whether they have any freedom in this matter, how they manage the system if they see themselves as not having freedom, and so on. Nothing which they regard as certain is allowed to go unquestioned, including the relationship of students and tutor to each other.

Now I don't wish you to get the idea that I practise what I believe is called psycho-dynamics, or group therapy; but there is something analogous with that perhaps, in that one makes the situation - which Malinowski was always exhorting his students to look into - of the classroom as the social laboratory in which the work of examining social relations can go on. Of course, this cannot be done without benefit also of reading, attending some formal lecturing, and writing in addition.

What I have found useful is to tie in closely what one is giving in lectures, in classwork, and tutorials and reading. For example, after a lecture to show how some concept, like "the family", "crime", "disease" is more complicated than seems at first blush, and is capable of different interpretations in different societies, I get students each to read one of the Spindlers' whole-society series of small semi-popular monographs. Then they are to try to write a report on this without using technical terms, to pinpoint something in it which particularly catches the fancy as bizarre, odd or inexplicable. Whatever this is, does not matter; the next exercise is for the student to try and read and think and find out for himself an explanation of the situation in which this bizarre custom occurred and any possible explanations he can come up with to account for it. This exercise must be carefully discussed with him as soon as possible so as to show where he is remotely on the right track, and where he can be clearly made to understand that he is on a track already shown to lead nowhere useful or in a dead end. In a sense I suppose one allows students to go through, very quickly and under supervision, those original explanations and "errors" of analysis which some of the earlier armchair theorists perpetrated, with the advantage that we can now show not only where they may have been mistaken, but why we know that they were mistaken.

The sugar on the pill, so to speak, at any rate in my field, is that the student nearly always has some personal hang-ups, about authority, or sexual relations, or religion, or social class or what-have-you, and that these will, without his knowing it, influence what he finds to be "bizarre" in the other culture; and his need to solve his own perplexities, whether directly intellectual or not, is the motive which keeps him at it, doing the further reading, thinking and writing around the subject, until he has got absorbed with the intellectual chase, and lost track maybe of his original question in the enthusiasm of asking more.

What happens then is that a great potential for attitude change is engendered, and, if the proper materials are put before the student, he is on the road to learning how to find out for himself the things he originally imagined the tutor would feed to him. He learns to look for his own intellectual nourishment, and also to be more tolerant of other people's tastes and habits.

(iii) Reaction

This is the third area I said I would describe, the students' reaction. At first there is confusion, perhaps rage and indignation. But one warns them about this, and holds up encouragement. In the end, students come to feel that they have a new perception of social relations, which is going to alter all their new learning, teaching and social behaviour quite considerably. One can perhaps not do more in a one year course than to send students out of it feeling differently about things than when they first arrived, thinking differently - even if not brilliantly, and behaving differently.

I have tried to describe what I think anthropological insight, gathered through exploitation of a synthetic or artificially created field work situation and followed by theoretical analysis, can do to bring detachment and objectivity about one's most personal and subjective points of view, even for the outsider to the subject. In a slight paraphrase of Edmund Leach's words: the anthropologist can provide "a new set of hypotheses about familiar materials" - in this case not just about myth, but about "the way we live now". The student can "look again at what he thought was understood and begin to gain entirely new insights Faced with the challenge of a new point of view he is able to see the familiar in quite a different way, and to understand something which was previously invisible." The student who has never been in the field, or before doubted the correctness of his ethnocentric morality, begins to grasp that "the order which we perceive in the world is something we impose upon it and that man has choice to order the world in different ways." At the least, it will be salutary for him to know that other people have ordered it in different ways, and that there is no one specific way of ordering a good world for us here and now.

You will notice that although the anthropologists have always seen themselves as working within their ivory towers to solve problems of their own conceiving, in fact the kinds of attitudes they held and the sorts of problems they attacked were much influenced by the intellectual atmosphere around them. Rationalism and relative moral arrogance dominated thought in the 19th and early 20th centuries. In the middle of this century there followed a preoccupation with absolute objectivity in the observation and collection of facts by early field workers, bent on establishing a clearly structured picture of societies "as they really were" and deliberately rejecting comparisons or value judgments. This coincided with the period of retreat from colonialism.

In the last decade or so there has been a return to generalising studies of man's ways of structuring his conceptions of reality; it accompanies a period

of philosophic doubt about our own way of living and anxiety about the implication of change - not now seen as always "forward and up". For these reasons the anthropologist has a great deal to offer the perplexed, doubting, agnostic and alienated young today. What is more relevant, in an age of conflict and fear, of disorder and anxiety about death, than to know how other people have handled these situations, what solutions they have offered, and, even more importantly, where, like us, they have been baffled by failure and tormented by the gap between the ideal and the actual?

To me, anthropology provides the detachment, protective armour, and modicum of hope which some others find in politics and yet others get from religion.

I will end as I began with the words of Tylor, the missionary teacher: "Anthropology can provide that carrying frame for mountaineers, whose extra weight more than compensates the convenience of its holding together and balancing the load of knowledge." But as for the original knowledge - that must come from such as are young and are still in touch with field research. Hopefully, they will never entirely forget the practical implications even of some of their most theoretically orientated researchers.

Rosemary Firth.

- (1) This paper is an abbreviated version of a talk given to the Friday Seminar at Oxford during the Hilary Term, 1971.

BIBLIOGRAPHY

- Ashby, (1969) The Academic Profession Oxford
- Evans-Pritchard, E.E. et al (1954) Institutions of Primitive Society. A series of broadcasted talks by various speakers.
- Freedman, M. (1967) Rites and Duties. L.S.E. G. Bell & Sons.
- Leach, E.R. (Editor) (1967) The Structural Study of Myth and Totemism. Tavistock. p.xviii.
- Lévi-Strauss, C. (1967) The Scope of Anthropology. Cape. pp. 42, 43.
- Lienhardt, R. (1964) Social Anthropology. Oxford p. 163.
- Mair, L.P. (1969) Witchcraft. World University Library.
- Marett, R.R. (1936) Tylor. Chapman and Hall.
- Mead, M. (1928) Coming of Age in Samoa. Penguin. 1943.
- Ruel, M.J. (1968) "R.R. Marett" in Encyclopedia of Social Sciences. vol. lx (pp.565ff.)
- Spindler, G. and L. (Editors) Case Studies in Cultural Anthropology. Holt Reinhart and Winston. New York.
- Stanner, W. (1968) "A.R. Radcliffe-Brown" in Encyclopedia of Social Sciences vol. xliii (pp.365ff.)
- Stocking, G. W. (1968) "Edward Burnet Tylor" in Encyclopedia of Social Sciences vol. xvi (pp. 170ff.)
- Tylor, E.B. (1881) Anthropology Ann Arbor p. 275.

HOW THE CONCEPTS OF FUNCTION AND STRUCTURE WORK

This paper has two main aims. (1) To show that the study of structures and the study of functions are two complementary aspects of a single process: the establishment of a taxonomy within which scientific investigations can get under way. (2) To prove that functional analyses have special characteristic features appropriate to a certain stage of investigation, and are therefore not identical with sociological analysis as a whole. Functional analyses are, furthermore, teleological, contrary to what most functionalists claim.

(1) To start with, I should like to dispense quickly with a red herring. It is often said that functionalism in anthropology relies on an analogy between a society and a biological organism. The notion of 'function', it is said, has its primary use in connection with parts of the body, such as the heart, the lungs, the kidneys, and so on. The function of these organs is to pump blood, extract oxygen from the air, and to excrete waste fluids, all of which conduce to the survival of the organism. However, I believe that talk of the function of an animal part is itself based on an analogy between animals and machines, and the primary use of the term 'function' occurs in connection with machine components which have been designed and brought into existence with some purpose in mind. It would be a long job to defend my double analogy thesis on historical grounds. To do so would be partly to trace the history of the argument from design. I do not propose to do so here, for in any case the question of whether functional analyses of social units are historically based on an analogy is irrelevant to us now. No one claims, surely, that a functionalist must actually think of a society as an animal in order to count as a functionalist, nor indeed that he should make any use of the alleged analogy in the process of coming to functionalist conclusions. So as far as the logic of functional analysis is concerned, if it has a logic, we may ignore the question of analogy. The subject-matter of a functional analysis is only relevant here in so far as different types of entity may impose different methodological constraints on us in our attempt to identify their function. Exactly the same considerations apply to the question whether structural analysis in anthropology is based on an analogy with organic structure, and whether organic structure is analogous to machine structure. It will be obvious from this preliminary remark that I propose to deal with the relation between structure and function in abstracto. I do not worry too much whether my examples are mechanical, biological or cultural, because from the cybernetical point of view the formal relationships are identical, whatever the status of the terms of the relations.

My first job is to show how the study of function and the study of structure are inextricably intertwined. I define these terms in the same way as the Concise Oxford Dictionary.

Function: activity proper to anything, mode of action by which it fulfills its purpose.

Structure: manner in which a building, or organism or other complete whole is constructed, supporting framework or whole of the essential parts of something.

In anthropological literature there is a convenient ambiguity between "X is a structure" and "X has a structure", which mirrors the confusion in English over notions like 'shape', 'colour', etc. Is a structure or a shape or a colour an abstract particular, or is it a property of a concrete particular? The C.O.D. says that a structure is a manner in which X is constructed, thus opting for the latter usage. I propose to ignore this distinction since anything I say in one way can be translated into the other.

It is a platitude that the things that may be said to have functions are entities or units of some sort. In other words, it is a truth of logic that X must be something before it can do something. Among the things which have functions, an important subclass consists of things which have internal structures, or if you prefer, things which are structures. Not all things which have functions need to have a structure, however, since from the point of view of the investigation in hand, a given unit may be taken to be the smallest or most basic, and so no attempt is made to break it up into smaller parts or to analyse its structure. Of course, a different, more molecular investigation may be interested in doing just that, but then this new investigation will in turn treat some other units as unanalysed. The dichotomy between part and whole is like a grid that can be shifted around on top of the data, relative to our interests. However, in using the terminology of 'part' and 'whole', we do presuppose that something has a structure, namely the whole. The clue to the structure of the whole is the relations between its parts. Thus, whenever we propose to analyse the function of something when that thing is taken to be a part of some bigger thing, we set out presupposing that the bigger thing has a structure. Given the relativism of parts and wholes, the whole logical space of possible investigations divides itself into a hierarchy. At the apex is a whole which is not itself a part; at the base are parts which are not taken to be composed of parts. The apex may or may not have a function, but any unit below the apex must have a function, in the sense that it plays a part in the wider system. The units at the base may or may not have structure, but any unit above the base must have a structure, since it is composed of parts. Thus, every unit, except for those at the bottom and the one at the top, must have both a structure and a function. That is, apart from the exceptions at the top and bottom, the same things that have structures have functions, and the same things that have functions have structures.

Now the question arises for any branch of science: What are the appropriate units of study for our science? This is the question Durkheim asked in the first section of his essay on Montesquieu (1965). Of course, no science is totally in the dark about its own range of subject-matter when it asks this question. It must have some idea of its external boundaries, otherwise nobody would suspect that on the tree of knowledge it constituted a branch. Yet it may have but a hazy idea of how to chunk up the area of reality that lies within its boundaries. Zoology, for example, did not exist as a systematic discipline until Darwin's theory of evolution by natural selection provided a

comprehensive method for defining and classifying the entities that are its subject-matter. It is a simple matter to show that for any set of data, there are indefinitely many ways of describing and classifying it, so Darwin's theory is just one of a number of possible theories compatible with the same data. What makes Darwin's theory of descent with modification the right theory is that it brings systematic unity to the whole of zoology by means of a hierarchical diachronic taxonomy into which all future paleontological, morphological and embryological findings will fit. Similarly, in any other branch of science, anthropology for instance, there will be an indefinite number of possible ways of chunking up the subject-matter into units. The right way, if as I assume we can talk in terms of there being a right way at all, is the way which brings as much systematic unity as possible to the whole field. If there were no right way, then the field could not be systematised, and so could not count as amenable to scientific treatment. So the task for a rational study of structures is always to taxonomise, to create or discover the appropriate units of study, with an eye to introducing system into the mass of data. Structuralism, in particular the work of Lévi-Strauss, seems to me to represent an attempt to create the right taxonomy for anthropology. Of course, the study of structures, and the analysis of structures into component structures, just is taxonomy. The point of good taxonomy is to group structures in theoretically interesting ways, so that once it is done, we can make generalisations and construct theories about the entities which our taxonomy has crystallised out. The permutation of elements which is widely believed to be the hallmark of structuralist taxonomy is in fact characteristic of other fields as well as linguistics and anthropology. For example, Darwin's concept of species as dynamic entities is now understood via the concept of the gene-pool, defined as the sum-total of genetic information in an interbreeding population. The number of possible gene-combinations in a gene-pool greatly exceeds the number actually realised by the members of the species. New generations are reshufflings of genes. But the new gene-combinations are always drawn from the original structural matrix of possible combinations, which defines the genetic potential of the species.

Defining one's units of study is not just a preliminary, however, especially in subjects where the data is complex, since one must not suppose that the units will, so to speak, fall out in advance of theory-building. Rather it is through theory-building and testing that we successively approximate to a rational taxonomy.

This is where functionalism comes in. Let us imagine that we find ourselves in the initial stages of carving out a science. We don't yet know, in a strict sense, what are the appropriate units of study. We may have certain terms at our disposal, such as the term 'biological family', but we have reason to believe that these terms are not going to be able to support an edifice of systematised knowledge of the sort that we hope to achieve, and so we are looking for new units. Useful units may not yet have words to describe them in our language. Our job is therefore to create taxonomic units and to invent words for them if necessary. The sorts of units that look as though they will be fruitful may be abstract, because the relationships between their parts may be more important than the identity of their parts. This does not bother us, as we have a handy substantive which enables us to talk of relationships themselves as

units or entities. This substantive is 'structure'. What, however, are the constraints which regulate our imaginative task of creating new structures? There must be some constraints, for we know a priori that the number of possible structures that can be abstracted out is limitless. We want to put forward only useful ones, ones that will be illuminating from the wide perspective of systematic anthropology as a whole. The main constraint, I suggest, is that the structure distilled from the amorphous network of data, let us say data concerning kinship phenomena, should actually do something interesting. Out of the whole range of things it does, the most interesting things will be those that pertain to its role in larger structures of which it is an element. Indeed its role (a functional notion) is the main guide to its location in the larger structures ('location' being a structural notion). This is, I submit, the constraint that Lévi-Strauss was working with when he suggested, in his early work The Elementary Structures of Kinship, that the basic unit or, as he says, 'atom' of kinship is the structure (brother, sister, father, son). Why did he choose this instead of choosing the relationship between, say, grandfather and sister, or that between mother, father, sister, brother, and patrilineal parallel cousin, or any other logically possible combination of terms? The reason is that Lévi-Strauss' unit of kinship is the minimum necessary to understand the avunculate, and thus the key to understanding how kinship systems work.

It is the minimum unit for logical reasons. As he says in his paper Structural Analysis (1958, chap.II), "In order for a kinship structure to exist, three types of family relations must always be present: a relation of consanguinity, a relation of affinity and a relation of descent -- in other words, a relation between siblings, a relation between spouses, and a relation between parent and child." (1958, p. 46). He accounts for the avunculate by showing that it is basic, because it is constitutive of the basic unit. "The primitive and irreducible character of the basic unit of kinship, as we have defined it, is actually the direct result of the universal presence of an incest taboo. This is really saying that in human society a man must obtain a woman from another man who gives him a daughter or a sister. Thus we do not need to explain how the maternal uncle emerged in the kinship structure. He does not emerge, he is present initially. Indeed the presence of the maternal uncle is a necessary precondition for the structure to exist." (1958, p. 46).

Finally, and most importantly, he explains how kinship systems can be shown to function when we view them as composed of the basic unit. "We must understand that the child is indispensable in validating the dynamic and teleological character of the initial step, which establishes kinship on the basis of and through marriage. Kinship is not a static phenomenon; it exists only in self-perpetuation. Here we are not thinking of the desire to perpetuate the race, but rather of the fact that in most kinship systems the initial disequilibrium produced in one generation between the group that gives the woman and the group that receives her can be stabilised only by counterprestations in the following generations." (1958, p. 47). Thus a functional constraint is built into Lévi-Strauss' choice of (brother, sister, father, son) as his basic unit. He recognises that we could conceive of an analogous symmetrical structure, equally simple, where the sexes would be reversed. This structure, involving a sister, her brother, brother's wife, and brother's daughter, would obviously satisfy the three logical constraints just as well. But

this theoretical possibility is eliminated on empirical grounds, since such a structure would be incapable of performing the function which he was all along bearing in mind. As he says in Les Structures Elémentaires: "If, then, in the final analysis, marriage with the father's sister's daughter is less frequent than that with the mother's brother's daughter, it is because the second not only permits but favours a better integration of the group, while the first never succeeds in creating anything but a precarious edifice." (Needham, 1962).

I think we see here a paradigm of taxonomic reasoning, involving, first, a substratum of empirical information about the prevalence of matrilineal cross-cousin marriage; second, an explicit statement of the logical requirements to be satisfied by any putative kinship unit if it is to be capable of yielding systematisation; third, a reciprocal adjustment of structural possibilities to functional requirements. My main intention is to draw attention to the third. The study of any branch of science in the process of establishing a decent taxonomy reveals that decisions to adopt such and such as the basic unit in terms of which agreed facts can be stated are regulated throughout by functional hypotheses. We choose what things to talk about with an eye always on their explanatory potential. What the units at the basic level do determines what the units at higher levels are, hence explains why they are as they are. The failure of purely functionalist attempts to explain social facts can be viewed as partly the result of incorrect taxonomy. Unless one sees the factors constraining one's choice of appropriate units from a wide perspective, one is sure to choose arbitrary, ephemeral and parochial units. These will break down under the rigid formal discipline of functional explanation à la Hempel, for example, since their identity conditions, and correspondingly the identity conditions of the systems of which they are parts, are incapable of being fulfilled over reasonable stretches of time. However, once one develops the overview of a taxonomist, one sees that structural analysis and functional analysis are not only complementary, but also that together they exhaust the logical space which all sociological theories occupy. It is not only absurd to think of structuralism and functionalism as opposed to each other, but it is also senseless to think of either of them as being opposed to theories on a lower logical level, i.e. theories defined in terms of their characteristic subject-matter, or their characteristic methods.

(2) My second part is a proof that functional analysis is teleological. To do this I need to define functional analysis. But a lot of definitions of it have been given by functionalists, all different. Impatient perhaps with the vagueness surrounding this topic, Kingsley Davis (1959) suggested that functional analysis was no different from sociological analysis as a whole, since it was concerned in a quite general way with the inter-relations between the elements that make up society. He proposed on these grounds that the notion should be scrapped. There is, I think, much to be said for this conclusion, but unfortunately Davis' grounds are not correct: functional analysis is a special kind of analysis, because a function is a special kind of activity, as I shall show in a minute. It is not, therefore, identical with anthropological analysis as a whole, but is merely an essential aspect of it, just as structural analysis is an essential aspect of it, but not identical with anthropology as a whole.

Functional analysis has been attacked on all sides. Hardline positivists say that functional explanations are invalid backward causal explanations, or that they are unverifiable, or that they are mere heuristic devices; while Verstehen theorists and Wittgensteinian philosophers argue that the sort of insight they provide into social facts is not the objective scientific sort that most functionalists take it to be. It has been criticised both for being merely causal and for not being genuinely causal; both for being linked with evolutionism and for being incapable of explaining changes through time. In a spirit of friendliness, Nagel and Hempel among philosophers, and Merton, Talcott Parsons, Homans and many others among sociologists, thought they would inject functional analysis with respectability by tightening it up, by defining its terms. The philosophers, especially, thought the main sources of trouble were untestable teleological assumptions implicit in functional ascriptions. If only, they felt, talk of functions could be empirically cashed in terms of the survival of something, as Darwin had done for species, then the teleology would be made manageable! The result of their efforts to formalise it has not been a resurgence of functional analysis, however, because what they call a logically proper piece of FA has to satisfy so many difficult methodological conditions that it is practically impossible to carry one out. In any case, the enterprise of formalisation was motivated by a muddled reductionism among the philosophers of science, who did not understand the positive role of teleological sentences in the activity of theory construction.

Hempel, in The Logic of Functional Analysis (1959) starts by correctly pointing out that not all the consequences of the heart's beating are functions of the heart. "A function of the heart is to circulate the blood" is true, whereas "A function of the heart is to produce heart-sounds" is not true, though it is true that the heart does produce heart-sounds. The difference lies, he says, in the fact that circulation of blood is a necessary condition of the survival of the organism, while the production of beating-sounds is not. He then formulates the general conditions for the truth of a functional ascription of the form "A function of X is to do F". These are (i) that X should in fact do F, (ii) that F should be a necessary condition of the survival and well-being of the whole of which X is a part. This general schema is then applied to functional analysis in sociology, where problems immediately arise over the term 'necessary condition' and over the definition of 'survival' and 'well-being' when predicated of society as a whole. If the problem of functional equivalence and the problem of defining the 'functional unity of the whole', to use Radcliffe-Brown's terminology, could be overcome, the teleological connotations of the functional statement would be tamed by treating a society as a homeostatic system, in which deviations from the normal values of given social variables would be compensated by corresponding adjustments in social variables elsewhere in the system. Nagel has set out such a formal model in his paper 'A Formalization of Functionalism' (1956 : pp. 247-83). To explain how a social practice or institution performed its hypothesised function would then amount to showing that it was interrelated in the reciprocal manner outlined. Since this interrelation is an empirical matter, functional ascriptions would be subject to experimental confirmation. Once they have accounted for functional statements in this way, Nagel and Hempel have, they think, analysed what it means to call a whole system teleological, and so there is no longer any sting left in the epithet.

This is what is called a reductionist approach to teleology, because it reduces teleological systems, by definition, to systems incorporating negative feedback mechanisms. From one point of view it brings teleological

systems into the realm of scientific investigation. From another point of view it completely misses what is essential about teleological language in science. The view you take depends on how you conceive of teleology. I believe that if we do define it in Hempel and Nagel's way, we needlessly rule out certain intuitively acceptable functional ascriptions, and we fail to capture the essential point of making functional claims in science.

Remember that Hempel is putting forward an analysis of what functional claims mean. He says that "X has a function" means "X conduces to the maintenance of a system of which it is part". This is quite a plausible hypothesis when X stands for an internal organ of an animal. But it is quite implausible when applied to everyday tools, and not very plausible when applied to social structures. If Hempel were right, we could not say that hammers and screwdrivers had functions, since they are plainly not essential for the survival of a system of which they are part. The claim that they are essential is in any case vacuous unless Hempel can specify what systems they are parts of. But it is not clear in advance that a hammer is a part of any system at all. Perhaps 'function' is being used in a different sense when applied to artefacts designed for a human purpose. But the trouble is, these same objections apply to social phenomena. If Hempel's linguistic recommendation caught on, we should be unable to put forward speculative functional hypotheses like, Veblen's theory of conspicuous consumption, where we do not wish to imply that impressing one's neighbours is a necessary condition of survival. In a word, the suggested definition is far too strict. Survival is not the only ultimate goal which validates a functional ascription, though it is a very important, indeed privileged, one.

This strict legalistic conception of functional analysis commits what Whitehead called 'the fallacy of misplaced concreteness'. It ignores what is essential about attributions of function. They are inherently free and easy, and need to be so in order to fulfil their characteristic scientific role of suggesting new experiments. They do this primarily by generating new data and directing observations. Consider two scientists looking through a microscope at some living scab tissue, one of whom knows that the function of scab formation is to facilitate the regeneration of normal skin, while the other does not. For both, the microscope reveals a number of cellular activities, but for the one who does not know the point of what is going on, the movements of the particles have no meaning. He cannot integrate the separate events into a single goal-directed process, and so he cannot sum them up with an overall description. Without a functional hypothesis to regulate his observations, he will not know which changes are significant, nor what objects in the picture to attend to. Sometimes, unless he can classify the entities in broadly functional terms, he will not even know what counts as an object and what is mere background. This illustrates that one of the roles of functional language in science is, roughly, to organise one's observations.

A functional hypothesis is, according to my definition, a hypothesis of the form "X does F in order to achieve G", where G stands for a goal. The presence of the phrase 'in order to', or 'for the sake of' marks the sentence as unmistakably teleological. G can stand for anything you like, as long as you view it as something that must get done. There is no need to say that this is how you are viewing it, however, since your commitment to the teleological sentence form already indicates that you regard G as a future state to which some value is attached. Thus if anyone put forward the functional hypothesis that the function of heart-attacks is to produce quick deaths, it would be obvious from the fact that he was using the functional sentence form that he was presupposing a pro-attitude towards quick deaths. Because the positive evaluative element is implicit in the description of some object or activity as 'functional', it is misleading to talk, as Merton does (1959, esp. Chpt. 1.)

of the 'dysfunctions' of social phenomena. All he means by 'dysfunction' is 'unfavourable consequence from the point of view of a wider system'. But as I have shown, once one has adopted one's standpoint whatever it may be, unless the activity of an item is being viewed as good or useful from that standpoint, it is not being viewed as a function of that item at all. That is why we do not say "The function of the heart is to produce heart-sounds"; though of course we might say it if we had a physiological theory which said that heart-sounds were useful.

Let me make this clearer. Any functional sentence, e.g. "A function of witchcraft persecutions among Navaho Indians is to lower intragroup hostility" (Kluckhohn: 1944) can, in my view, be transformed into a sentence with 'in order to' or 'for the sake of' in. Thus we get "Witchcraft persecutions among the Navaho lower intragroup hostility for the sake of G". In my analysis, a new term G occurs which lay below the surface in the original. What does it stand for? As far as logic is concerned, it can refer to any future state you like. The important thing is its relational property of being something that is being regarded as a goal towards which the phenomena are teleologically directed, that is, something which stands out as a destination. The fact that we do not need to specify what it is explains why it is left in the deep structure of the original functional sentence. In this example, its force is already negatively encapsulated in the term 'hostility'. Its role is to add emphasis to the statement that witchcraft persecutions do, de facto, decrease internal tensions, by suggesting that there is a pressure of events to make sure that this gets done. The idea that something more than mere contingency is involved manifests itself in the assertion that if some obstacle should prevent witchcraft persecutions from performing their postulated job, then Navaho society would overcome or bypass the obstacle, say by throwing up a different practice that did the same job.

The fact that G has some imperativeness attached to it explains why most people who have written on this topic identify G with some biological, psychological or social need ultimately cashable in terms of individual or group survival. But it is wrong to do this, as a hypothetical case invented by Sorabji (1964) illustrates. Suppose there were an organ which only came into operation when a person had incurable cancer, and which cut off all pain from the cancerous area. We should not hesitate to say that doing this was its function, even though it had no survival value. My theory can explain why it is so tempting to link function with survival by definition. The ultimate validation of any functional ascription must be a future state that is regarded as valuable, or part of the essence of the thing manifesting the state. Vital needs are privileged candidates for this position because if they were unfulfilled the system would soon cease to exist. From the system's point of view it is better to exist than not to exist. Survival, maintenance of equilibrium, adjustment to the environment etc. are privileged G's, in the sense that without them there would no longer be a system to talk about. But equally, there may be another point of view from which it appears better that a certain system should not exist. It seems to me that an item which secures the self-destruction of the system of which it is a part may without contradiction be said to have this job as its main function, and not merely as an unwanted side-effect of some other function. The only requirement for so viewing it is that there should be a perspective or a theory in which this case of auto-destruction is right and proper.

But not all functional hypotheses are equally useful. What are the constraints on theoretical perspectives within which a given activity may be viewed as functional, apart from the rock-bottom empirical constraint that the item should in fact perform the activity which is being presented as one of its functions? The main constraint is the same general taxonomic consideration

which regulates structural hypotheses, namely that the functional ascription should lead to systematisation of a wide field of data. Ideally, the function we assign to an item in a larger whole should fit in to a hierarchical organisation of functions. Once again we see why survival-value has a privileged position among possible functions: it unifies a mass of disparate functions by organising them into a hierarchy of which survival is the apex. But there may be more than one hierarchy and more than one apex. Anthropological taxonomists have a wide-open field where they can construct alternatives. In these circumstances, the more functional hypotheses we can concoct the better, so long as we bear in mind that ingenuity must eventually meet the harsh demands of systematics, i.e. simplicity, consistency, coherence. To quote from Lévi-Strauss's essay 'Social Structure' (1958 : 280): "Though many models may be used as convenient devices to describe and explain the phenomena, it is obvious that the best model will always be that which is true, that is, the simplest possible model which, while being derived exclusively from the facts under consideration, also makes it possible to account for all of them. Therefore the first task is to ascertain what those facts are."

To sum up. I have tried to prove two points. First, structural hypotheses are regulated by hunches about possible functions, and functional hypotheses are tailored to our choice of structural units. Rational taxonomy proceeds by the mutual adjustment of function and structure, and provides the framework within which particular anthropological theories can be stated and tested. Second, functional hypotheses are teleological ways of looking at things. They have an empirical aspect, because "A function of X is to do F" cannot be true unless X does do F. But they also have a non-empirical aspect, since the goal-directedness we impute to X is projected on to it rather than discovered in it by examination. The main point of describing the facts by means of a teleological sentence, which asserts more than is strictly warranted by the facts, is that each functional ascription represents a mini-theory that can generate new observations and suggest new avenues of research. Most will be knocked down, but some will stand provided they are capable of fitting into a systematised body of knowledge. The ones that pass through the filter will be ones that ascribe functions to genuine structural units rather than arbitrary units. As the system grows and the right structures get crystallised out, the functional ascriptions become increasingly entrenched until there ceases to be any point in saying that they are not objective. Like the sentence "The function of the eye is to see", they turn into tautologies, as performance of the function is seen to be constitutive of the identity of the structure.

Andrew Woodfield.

BIBLIOGRAPHY

- Davis, K. 1959. 'The Myth of Functional Analysis'. American Sociol. Review.
- Durkheim. 1965. 'Montesquieu & Rousseau - Forerunners of Sociology'. Ann Arbor, paperback, 1965.
- Hempel, 1959. in Gross (Ed.) Symposium on Sociological Theory. New York.
- Kluckhohn, 1944. Navaho Witchcraft.
- Lévi-Strauss, 1958. Structural Anthropology.
- Merton, 1959. Social Theory and Social Structure.
- Nagel, 1956. Logic Without Metaphysics. Free Press.
- Needham, 1962. Structure and Sentiment.
- Sorabji, 1964. 'Function'. Philosophical Quarterly, Oct. 1964.

The Rubbish of Racism

In a recent issue of JASO, T.C. Weiskel has given us a cautionary tale about racism and boundary maintenance in a style of discourse that can trace a respectable history from Herodotus to Rousseau and his latterday sympathisers. While the conclusion of Weiskel's exercise might be better received in a Theosophical publication, its appearance in a Journal concerned with social anthropology as she is spoken at Oxford requires that his argument be examined for more than its obviously laudable proscriptions.

Now, my reference to authors, ancient and modern, was to show a tendency present in the "travellers' tales" of Europe, but which is not unknown in reports of foreign peoples by those outside Europe that of using a foreign culture as material for a parable for twitting aspects of one's own culture which one deems as undesirable. (see den Hollander, Thrupp). Whether or not one's representation of the foreign culture is accurate is unimportant so long as the critical homily is conveyed.

Let us look at Weiskel's argument in brief it consists of four major parts. First, all cultures construct artificial boundary systems (40 1). Second, "The ecological niche which is implied by swidden agriculture can be seen, then, to give rise to a system of conceptual bounding which differentiates.." it from that of hunting and gathering cultures (44). Third, racism is a function of this new nature/culture dichotomy (45). Lastly, this gives rise to a feeling that, "as with the physical environment, one's only proper relationship towards those who are outside is one of conquest and subjugation...." (45).

His "heroes" are Turnbull's Naturvolk, the MaMbuti, and he sets them off against their aggressive neighbours, the Bantus. Perhaps unknowingly drawing his inspiration from this 19th century German romantic concept of 'nature's children', he proceeds to show us how they live in harmony with their econiche, whereas those who exploit the land (the Bantu swidden agriculturalists) are in constant conflict with it.

Then, his tale takes a sudden lurch, as the Naturvolk join up with another Romantic idea (appropriately French, though not unknown elsewhere) - the contented rustic and the pastoral. Rather than being happy Mexican peasants (a la Redfield), we are given Indian villagers whose conflictless (apparently!) lives are ruled by Dharma (46-7). We are told that lacking an exploitation mould, "In the realm of social relations sedentary agriculturalists mediate the inside/outside dilemma through systems of ritualised hierarchy" (47). On the "other side", we still have "systematic predatory expansion", but this time performed by Euro-americans.

His argument "takes a lurch" because rather than the smooth evolutionary scheme upon which the components of his argument appear to be based (going back to Tylor and Maine), he constructs a somewhat Leachean alternative opposition model -

Mambuti :: Bantu : Peasants : Euro-americans

He concludes that we must learn to realise re-cycling and holds out hope that, "Indeed our technological achievements may be leading us to the type of cyclical comprehensions characteristic of feudal society or the Indian peasant" (50).

Now, in his examination, he encounters two major problems - both of them related to the consequences of his theoretical orientation. The first is that the consequences of his structuralism (of which Crick and Heelas speak

so highly in other parts of the same issue) force him into a certain kind of tidiness and consistency. He cannot, for example, postulate as he does "... a society in which nature and culture are not opposed..." (47) since this, if nothing else, is the very basis of the technique, as handed down by the Master in La Pensée Sauvage and in the Mythologiques. As this consistency of the dialectic⁴ requires that rigidity be maintained in paradigms, we are inevitably led to his second problem. If sedentary agriculturalists with an orientation to the whole require a hierarchy to mediate their categories of inside/outside, does this mean that if Euro-americans adopt an ecological point of view, they must perforce also accept its attendant hierarchy? This is the logical outcome of his reasoning, though, I doubt, if he would really accept this as part of his "... fundamental overhauling of Western categories of self-understanding" (50).⁵

However, the first part of his discussion, largely based upon Mary Douglas (1966) is very interesting and one would like to see Weiskel take his development of a we/they opposition out of the "inside/outside dilemma" further and, instead, make some comments about what I feel to be the universal existence of the human sentiment of inclusion/exclusion.⁶ Does there exist a people who do not have a group against whom they exhibit prejudice? The author's Indian peasants formulate their suspicions of inferiority - Srinivas' Coorgs (see 1952) - against tribal groups, as well as speakers of the other of India's many major languages. His Mambuti enjoy playing clever tricks upon their Bantu "masters", and generally hold them in low esteem. We have data showing that when a human group "lacks" such an "outside group", they may even "invent" one. DeVos and Wagatsuma indicate how, with the decline in numbers of Ainu and this group's relative geographical isolation, an "invisible race" of Eta bears the brunt of much of Japanese prejudice (1967). The Basques, not content with discriminating in their traditional legal code (foros) against the usual Iberian outsiders, gypsies and Jews, have their own invisible "race" of agotes who, in their sinister manner, exist alongside Eskualduna and are thought to be behind any number of misfortunes encountered by the Basques in their land.

As I have indicated above, these sentiments run very deep and are by no means restricted to what historians have dubbed, "The Age of Discovery". After the conquest of Mexico, Bernardo de Sahagun had to argue to his ecclesiastic superiors that the Indians of Mexico were members of the human race so that they would not have them slaughtered simply as an inconvenient breed of indigenous pest. And, just over a hundred years ago, members of what is today the Royal Anthropological Institute were debating whether or not African Blacks were human or not. Partly through the efforts of anthropologists, most people (though not all!) now accept that human beings from national states other than their own are, in fact, members of the same animal species.

But, anthropologists themselves have also been guilty of this common ingroup/outgroup prejudice by elevating their own Euro-american folk categories to the level of scientific theory when they have spoken about "primitives"⁷. Only recently has our subject been able to shake itself free of this long cherished belief of dividing the world into "civilized" and "primitive" peoples. Weiskel's analytical "template", if you will, makes conscious reference to this spurious division and this is unfortunate. It obfuscates the issue with which he so ably begins to grapple.

In contradistinction to Weiskel, I would like to suggest in this brief note that only when we are able to understand the ubiquity of prejudice (often couched in terms of a concept of "race", fictive or otherwise) can we then make constructive suggestions as anthropologists and as citizens for obliterating this sentiment from Euro-american as well as other societies (for example,

Bangla Desh, Nigeria, South America, etc.)

The only difficulty, of course, with the slow method I advocate is that the peoples against whom prejudice is directed may not be willing to wait for our efforts to bear fruit and decide through revolution to follow their own courses of action. Weiskel's initial joining of our science with ecology is aposite here too, for we must hope that nature itself does not "decide" to rebel against technological man's prejudice, discrimination and exploitation!

Grant McCall.

NOTES

1. Hardy (1958) records the reactions of an early Moslem traveller to his voyage from India to England.
2. Haaland (1969) notes how the sedentary Fur farmers cast an envious eye on the seeming freedom of the Baggara nomads who share a contiguous econiche. See also Storry 1965.
3. In some ways, this is analogous to the Victorian historians' description of the Middle Ages in Europe as a 'dream of order' in the social chaos of the 19th century (Chandler: 1970). In a future issue of this journal I hope to be able to publish an alternative, non-hierarchy model for village India based upon the use of Dharma as Douglas's concept of order, with purity and pollution being rendered as social order and social disorder. My model, however, will be founded upon the notion of conflict as central to my transactional analysis.
4. The dialectic is based, of course, upon de Saussure who never intended the categories to be interpreted with such rigidity (see Barthes: 1967).
5. The idea of hierarchy and stratification (whether social or "ideological", as in Louis Dumont's case) is most characteristically Euro-american and it is only since the overwhelming British influence on India in the 19th century (reformulation of Hindu law, the periodic census reports, etc.) that the notion has come to have even partial meaning at the village level in India. The "inconsistencies" in the hierarchy model have been noted frequently by researchers from Srinivas to Mayer (See Yalman: 1969).
6. I suppose that the consequence of Heelas's highly derivative review ("Tensions and Onomastics", in JASO, Vol. II, No. I) in this regard would be for this sort of task to be taken over by either a geneticist or by one of his "irridescient", metamorphosized anthropologists. This extension of Heelas's argument takes on an absurd look largely because of his failure (along with that of his mentors) to realise that anthropology has always been characterised by eclecticism and serendipity: that is, the subject, insofar as it may be said to be so beyond the degree stage for its practitioners, has always been best characterised by a normative view - i.e. "Anthropology is what anthropologists do" (see my note four, McCall: 1970). Proscriptions, such as those offered by Heelas, are best left to the individual to sort out, with respect to the particular problems with which he is concerned. Each anthropologist should probably have (and often does have) his own views as to what the proper methods and goals of the discipline ought to be. Equally, each should feel free to verbalize these ideas for comparison with those of his colleagues. However, to predict that anthropology will perish should one's individual orientation not be followed is unrealistic in the light of the history of the field. Hopefully, the anthropologist's

"point of view", as Kroeber characterized the essence of anthropology, will never find itself limited to the strictures of either a formalistic or a functionalist (as well as future alternatives) nature. Barnes (1969) has some relevant comments in this regard. The different approaches each have their uses - e.g. Orenstein - and to deny choice, if indeed it may be done in any meaningful way, is tantamount to proposing something like a Lord Longford committee for anthropology!

7. This subject has received attention in JASO on previous occasions in articles by James and Lyons (Vol. 1, No. 2) and comprehensive reviews on the problem exist in Hsu and Montagu. From the standpoint of Euro-americans, some authors have traced the "civilized/primitive" and "western/eastern" dichotomies back to the Greek distinction between themselves and the Persians (Iyer: 1965 : 12-19).

BIBLIOGRAPHY

- Barnes, S.B. 1969. "Paradigms - scientific and social." Man (N.S.), 4, 94-102.
- Barthes, Roland. 1967. Elements of Semiology. London, Cape.
- Chandler, Alice. 1970. A dream of order. London, Routledge.
- den Hollander, A.N.J. 1966-1967. "Countries far away - Cognition at a distance", Comparative Studies in Society and History, IX, 362-376.
- DeVos, George and Hiroshi Wagatsuma, eds. 1967. Japan's invisible race: Caste in culture and personality. Berkeley and Los Angeles, University of California Press.
- Douglas, Mary. 1966. Purity and danger. London, Routledge.
- Dumont, Louis. 1970. Homo hierarchicus. (Translated by Mark Sainsbury). London, Weidenfeld and Nicolson.
- Haaland, Gunnar. 1969. "Economic determinants in ethnic processes", in Fredrik Barth, ed. Ethnic group and boundaries. Bergen/Oslo, Universitetsforlaget. 58-73.
- Hardy, Peter. 1958. Part four: Islam in medieval India, in Wm. Theodore de Bary, et. al. eds., Sources of Indian Tradition. Number LVI of Records of Civilization. Sources and studies. London, Oxford. 567-528.
- Hsu, Francis L.K. 1964. "Rethinking the concept of 'primitive'", Current Anthropology, 5, 169-178.
- Iyer, Raghavan. 1965. The glass curtain between Asia and Europe. London, Oxford.
- McCall, Grant. 1970. "American anthropological interest and prospects in Basque studies", Current Anthropology 11, 161-164.
- Montagu, Ashley, ed. The concept of the primitive. London, Collier-Macmillan Ltd. 1968.
- Orenstein, Henry. 1970. "Death and kinship in Hinduism: Structural and functional interpretations", American Anthropologist, 72, 1357-1397.
- Redfield, Robert. 1968. The primitive world and its transformations. Harmondsworth, Penguin.
- Srinivas, M.N. 1952. Religion and society among the Coorgs of South India. Oxford, At the Clarendon Press.

- Storry, Richard, 1965. "Japanese attitudes to the West", in Iyer, 1965 (above). 120-134.
- Thrupp, Sylvia L. 1959-1960. "Hierarchy, illusion and social mobility", Comparative Studies in Society and History, 11, 126-128.
- Weiskel, T.C. 1971. "Rubbish and racism - the problem of boundaries in an ecosystem." Jour. Anthr. Soc., Oxford. Vol. II, No. I.
- Yalman, Nur. 1969. "DeToqueville in India", Man (N.S.) 4, 123-131.

Robert Ardrey's THE SOCIAL CONTRACT

Robert Ardrey's new book, The Social Contract (Collins, London 1970) is a collection of animal stories loosely bound together by a common ideology. This ideology is a curious one and it is also likely to be the most influential aspect of this book. Ardrey is read widely. In recent years a certain kind of popular anthropology has been virtually the only material of its sort to reach the general public; it is based on the proposition that man is less far removed from his animal relatives than has been commonly supposed. Ardrey is one major exponent of this point of view; Konrad Lorenz and Desmond Morris are others. But Ardrey's approach is more polemical than that of Lorenz and Morris; he is explicitly supporting a number of ideas based on the fundamental principle that man possibly faces evolutionary disaster if he cannot find ways to live in accord with his innate biological heritage. Ardrey finds that the increasing complexity of industrial society is warping human behaviour in such a manner that fundamental human drives are contradicted or given little chance for expression. It is this attitude toward society which led Ardrey to dedicate his book to Jean-Jacques Rousseau.

Ardrey is in fundamental sympathy with Rousseau's arcadian utopianism although he cannot accept Rousseau's belief that there was a time before the social contract brought natural man into the restrictive orbit of unnatural society. Ardrey maintains that society is the natural condition of man and that the basic conditions for society are written into human genes. He therefore seeks to define the natural condition of man by reference to the natural conditions of society, and he does so mainly through an examination of the nature of non-human societies especially those of the other primates. What he finds leads him to condemn characteristic features of modern society found in both 'capitalistic' or 'socialist' countries.

This book is written by an American and mainly for Americans, but its basic message is likely to have a far wider appeal; if only for this reason I think his book worth a detailed review. It is ultimately based on an antique stratum in western political thought, one which may be finding an unusual new form in modern North America. This is difficult to characterize since it is generally not a system of ideas consistently expressed or consistently adhered to. Its closest well-known relation appears to be the political thought of Thomas Jefferson. But at first it seems that this or its consequences is what Ardrey is most dead set against.

The first sentence of The Social Contract is deliberately provocative: "A society is a group of unequal beings organized to meet common needs."⁽³⁾ By 'unequal' Ardrey means genetically unequal. He is thus not against Jefferson's thought as such. Jefferson's propositions were largely ethical in intent: that all men should be regarded as equal and given opportunities as though this were in fact

the case. What Ardrey is against is the supposed modern liberal view that all men are equal in potentiality and that usually this potentiality is blocked only by adverse external circumstances. He maintains that this idea is wrong, pernicious, and an affront to biological knowledge. Ardrey is in search of the innate background to human behaviour and therefore attacks those who support theories of environmental or social determinism; so, in his early pages, he attacks cultural anthropology, sociology, and behaviourist psychology in the forms of Sol Tax and Melville Herskovits, Durkheim, and B.F. Skinner respectively; on his own side he counts Noam Chomsky (!), several psychoanalysts, and, led by Lorenz, a large selection of ethologists. Curiously the only support he musters among anthropologists is from Claude Lévi-Strauss who somewhere suggests that a desire for prestige is somehow innately determined. In his pursuit of the innate Ardrey examines and decides in favour for the existence of: racial psychological differences (intelligence included; he does however find that blacks are fine athletes); the natural subservience of women (given the chance, women will vote for men); a tendency to follow the leader once a true leader has emerged; a tendency to strive against obstacles; a real or symbolically transformed territoriality, i.e., self-definition through exterior symbols such as money and, of course, territory itself; and, in connection with this last, a natural xenophobia - fear and hatred of the stranger. Virtually all of these conclusions are based on observation of the societies of the higher animals and on analogous commonsense observations on man. I will devote little attention here to Ardrey's animal evidence though it takes up the majority of his book and though it is essential in giving his argument its surface plausibility. It is human society which is Ardrey's main concern, and it shall be mine as well.

Ardrey's argument is analogical throughout, and though Ardrey says on occasion that one cannot reasonably argue from animals to man, he systematically ignores his own advice; a similarity noted between animal and human behaviour is taken as proof of the innate background for this behaviour in man. Ardrey only infrequently has detailed references to the nature of man in society and this reference is usually anecdotal in nature.

He refers to a number of innate needs which he believes exist in man and which account for the nature of human society; but most of his examples are taken from animal behaviour studies. There is a "...triad of innate needs, common.....to men and all higher animals. There is identity as opposed to anonymity; there is stimulation as opposed to boredom; there is security as opposed to anxiety." (168) Men naturally find identify through groups and their symbolic representations, though Ardrey is not specific about the kinds of groups that will serve. For him the family has no final validity; he finds that functionless groups are not cohesive and that in our time the family is largely being replaced by the peer group.

The drive for stimulation is the most clearly established of Ardrey's triad. Observations of infant behaviour and studies of the results of sensory deprivation clearly show what appears to be an innate demand for exterior stimulation. Ardrey relates this drive to the factor of 'aggression', a term which he takes over in its technical sense from psychoanalysis. This is its manifestation in action:

"We seek self-fulfillment. Within the limits and the directions of our individual genetic endowment we seek such a state of satisfaction as will inform us as to why we were born. We have no true choice. The force that presses us is as large as all vital processes, and were it not so, life would return to the swamp. If there is hope for men, it is because we are animals. This is the aggressiveness that many would deny."(257)

Though "self-fulfillment" is somewhat vague what he seems to mean by it is 'meaningful identify with purposeful group activity'. Ardrey does not here take 'aggression' to signify a tendency toward physical violence. But in his first book, African Genesis, he traces man's descent from a carnivorous, weapon-using half-man and clearly indicates his feelings about our ancestry by his referring evocatively to this being (an australopithecine) as "Cain". Ardrey's preference is generally weighted toward a belief in an inherent violent streak in man and elsewhere in The Social Contract states that: "What we have in our genetic endowment is the rejection of strangers and probably the propensity for violence. These have not been abolished."(277)

The last of his triad of drives, security, is also considered by him to be the least powerful. Men will seek identity and stimulation before security; security is however rather more important to women. It is the case with most social vertebrate species and 'therefore' it is the case with man.

One of Ardrey's other main concerns, territoriality, is closely related to the factor of identity. Territoriality is seen to produce in man phenomena such as 'personal space', a small domain which moves around with one and within which one dislikes to admit others. Territoriality also produces identifications with symbolic outside objects, as Ardrey puts it 'conventional objects conventionally competed for,' e.g. property: "...a cultural institution, such as private property, which accords with natural law rarely fails."(210)

The above drives are the main constituting forces of human social life. But the external environment also has its long-term demands and these demands are what Ardrey takes to have been responsible for the evolutionary appearance of the basic drives in the first place. A changing world demands changing capacities in the individuals which must deal with it. It was for this purpose that sex came into being and eventually, in social species, a range of instinctive behaviours to deal with the problems of sex. Sexual reproduction is a means for the rapid spread of mutations, mutations which may be of value to species or local group survival. The following statement indicates Ardrey's evaluation of the importance of the individual in this process and also, it seems, his general ethical evaluation of the value of the individual per se:

"Variation: the variant individual who makes little sense in today's climate, but who may save us in tomorrow's; diverse isolates, spreading the risks of total population commitment, the recessive gene, hidden here, hidden there, waiting for new environments to perform the selective alchemy of transmuting dross into shining metals."(54)

But he argues that it is the case in social populations that selection must be of value to the group generally. For example, individuals may come into possession of behaviour patterns detrimental to their personal survival but of advantage to the group as a whole. Thus male baboons will attack a leopard while the reproductively more valuable females make their escape.

In social species there will also be rank ordering because an ordered group is a more viable entity than an anarchic group. This, says Ardrey, generally will be arrived at by competition, but it must be assured at the same time that competition does not harm the society at large; many species get by this through competing 'for conventional objects by conventional means;' hence the war of all against all is avoided and bloody intra-group struggle uncommon. And these conventional objects and conventional means are genetically encoded. Various devices may ensure that subordinate males accept their subordination; Ardrey notes that the subordinate males of some species may be subject to 'psychological castration' simply as a result of their subordinate status.

Ardrey sees society as a balance between necessary order and necessary disorder; disorder is necessary so that individuals with the necessary traits under the circumstances may rise to the top, and order so that they do not destroy society in doing so. A proper balance between these two forces helps ensure the long-range genetic health of the population. But man does not always allow his societies to adopt the best form from a genetic standpoint:

"Animal justice [i.e., full equality of opportunity] was perhaps the first natural law that civilized man began systematically to violate. Advantages of birth offer no guarantee of genetic superiority. Restrictions of caste, of class, of occupation, of poverty distort, or suppress the phenotypic flowering of genetic endowment in the maturing individual. But the accident of the night [sex], in all its rich, random resource, became in man socially aborted. There have been revolutions, it is true. But human history has far more frequently witnessed the decline of empires, the vanishment of kingdoms, the disappearance of peoples genetically exhausted through order's injustice." (45)

Unfortunately Ardrey gives no evidence whatever for this last proposition. Apparently he is saying that the character of a population may alter for the worse (toward unadaptability) by an interference with gene flow throughout the population and the consequent less rapid distribution of valuable genetic traits. Since Ardrey does not indicate what groups he has in mind it is difficult to see exactly what he believes genetic stagnation to consist of. Only in small, highly interbred populations do any deleterious genes become common enough to be an observable menace to general well-being. However there are several means by which society could wittingly or unwittingly influence the direction of its evolution. Sexual selection is one example; an ideal of male or female beauty may influence who has how many offspring. This is a classical Darwinian mechanism used to explain the apparently unviable absurdities of creatures such as the male peacock. If specific psychological characteristics are genetically influenced or determined then the same

mechanism could alter a gene pool in a certain direction thus affecting 'racial psychology'. Ardrey definitely believes that this can happen in human populations and that in fact it has happened and happened often. But he is never explicit about how it comes about in practice and never points to an actual population in which it is observably at work. The following statement, combined with his unverified assertions about genetic stagnation in unspecified populations, is his way of 'proving' his point:

"The overwhelming environmental change which independence /cultural isolation and consequent partial breeding isolation/ has introduced provides overwhelming disproof for the acceptance of cultural relativism. Some populations, such as the Kikuyu in Kenya and the Ibo in Nigeria, have contained superb potentiality for change. There were fit for tomorrow.....But some populations have so far demonstrated little or no such potentiality."(58)

Now, it is possible though not particularly parsimonious to account for findings such as these by reference to genetic traits; but note well that Ardrey has not established their existence. Note also that his 'overwhelming' disproof of cultural relativism is neither overwhelming nor a disproof; it is assertion pure and simple. Something which could, just possibly, be at least partially true is presented as though it were incontrovertibly true. His disproof of 'relativism' cannot be a disproof since here, as elsewhere, he does not give an example of a cultural explanation with which he could juxtapose his own explanations. To argue with something it is necessary to state clearly what one is arguing with.

It is well known that Kikuyu and Ibo are exceptionally active in trade and politics; an explanation of these patterns of behaviour, is available from sociology, cultural anthropology, and social psychology. Since Ardrey does not point to any trait save success which could have something to do with genes, and since even this is debatable on genetic grounds alone, it would appear that his case is almost entirely trivial. Unless he can show that the traits in question are somehow genetically determined; unless he can demonstrate from the actual pattern of preferential marriage and natural or social selection that it is at least possible that the quality of Kikuyu and Ibo life can be due to genetic factors, then Ardrey's case is irrelevant. It is possible, at least in principle, to demonstrate that a given population may be biased toward selection of a certain trait; if sexual selection were at work, which in African societies it generally is not, then a certain trait could conceivably be selected for. If it is the case that men with a greater degree of some genetically determined psychological quality somehow leave more children or at least make it possible for others who carry this trait to leave more offspring, then again it is possible that a given trait may be selected for. Ardrey does not do any of this, and I have to say that his assertions are empty and even dangerous.

Ardrey believes that the Kikuyu and Ibo represent processes which may be going on in society everywhere; the contrary case, that there can be unfavourable selection and genetic stagnation, is meant in a universal sense as well. Is it also meant to apply to modern

societies? Apparently so; Ardrey's main fear seems to be conformity, and it is his belief that traits such as 'conformity' may be selected for or against genetically. We have seen what he says about the evil effects of social institutions which get in the way of gene flow. Elsewhere he says:

"A population must achieve a fair degree of adaptation to its environment if it is to survive in the present. And if fitness for today were the sole criterion, then cultural relativism would be theoretically sound. But adaptation can be too perfect. When selection for conformity has persisted through a sufficient number of generations, all may seem well; yet reduction of variants will have affected the population's gene pool and reduced its prospects of survival tomorrow. Either variation so wild as to render future survival dubious, or conformity so narrow as to endanger the future, becomes the character of a genetically inferior population."(55)

Conformity is a bad thing; wild nonconformity is a bad thing. It is possible for a population to select for one aspect of life or another; any genetically isolated population has the capacity to do so. Therefore Ardrey's implication is that this may happen within the various functional and cultural sub-groups of western society, at least in so far as they are isolated from the others. How conformity as such may be selected for genetically is beyond me; I have already illustrated the difficulties of applying such reasoning to a specific society (Kikuyu and Ibo). Still Ardrey seems to believe that it is possible that such selective pressures, particularly those for 'conformity', may operate in western society because of the necessity for men to conform to institutions which demand uniformity. How it could happen I do not know; any effective argument along these lines would involve digging up Lamarckian genetics again, and this Ardrey cannot do. But if conformity is somehow established in our genes, the results may be these:

"...we pray.....in our industrial organizations, on our collective farms, in our churchly councils, in our processes of government, in our relations between states, in our righteous demands for world government, in our most seemly prayers that someday we shall all be the same.(...) As life is larger than man, so is life wiser than we are. As evolution has made us possible, so will evolution sit in final judgment. As natural selection declared us in, so natural selection, should our hubris overcome us, will declare us out."(367)

I think he is possibly more concerned that an egalitarian totalitarianism will somehow take over the selective process itself than that selection will take place in a more random fashion; this however is quite unclear.

The Ibo and Kikuyu example is not the only one that Ardrey uses to justify his claims about psychological differences between populations. His prime example is derived from statistical surveys of I.Q. test results made in the United States; here Ardrey is on slightly firmer ground if only for the reason that much work has in fact been done in this area. I am scarcely a specialist in I.Q.

testing, but it is the case that this is an intensely debatable area both for political and for methodological reasons. I do not intend to review the entire nature-nurture controversy; I will simply point out what Ardrey has done with the data as it stands. He has declared that intelligence is related to racial heredity; blacks are statistically inferior to whites in their ability to manipulate the kinds of facts and processes tested by I.Q. tests and this difference is genetically based. Of course it is true that the statistical evidence does exist; it is its meaning which is in question. Most sociological and psychological statistics suffer from a fundamental problem which makes them very difficult to interpret; whatever uniformities appear in them tend to be overdetermined, caused by several factors rather than just one. The controversy over I.Q. testing is greatly complicated by this kind of problem; class differences must be evened out, cultural differences taken account of, motivational aspects of the testing process itself analyzed, etc., etc. I cannot settle this argument; it still goes on in the journals and elsewhere. But I will say that Ardrey has pretty well ignored these complexities; his conclusion lacks force for this reason. I.Q. tests rate culture-bound abilities with which not all persons even in the same general culture are equally familiar. Until it is certain that statistical differences in I.Q. results are not due to differences (for example) in child rearing and to differences in general cultural background then genetic arguments seem a waste of time. I might say that child-rearing in particular has a profound effect on the 'intelligence' of children; this effect can be dramatic, as witness the by now numerous studies made on children deprived of maternal care in early childhood. I am not trying to make a radical claim for social determinism; I am simply stating that in proven fact culture does influence the potentiality and the content of learning to a very great degree and, most likely, usually to a greater absolute degree than most genetic differences between individuals.

The fact that Ardrey in general does not cite any clear evidence for his case will not be noted by the general public; and I suspect that some parts of his book will be seized upon with glee in some quarters for reasons which are not exactly motivated by scientific objectivity. Much of all three of Ardrey's books give a kind of covert support to certain ideological biases; Ardrey knows full well that he is open to the charge of racism but disclaims responsibility by invoking pure scientific curiosity and by accusing the liberal spokesmen of prejudice in the other direction. I do not think that a charge of racism can be personally levelled against Ardrey; his general ideology, which I will discuss in a moment, does not logically permit it. But I have shown that his attitude toward evidence is scarcely responsible, and it is this evidence which leads him to make the dogmatic assertions which I sum up below:

1) Groups which have been genetically isolated are likely to differ genetically from the groups from which they have been isolated. An intra-breeding class or caste may differ from the surrounding society, and the results of this inbreeding may turn up in psychological traits, conceivably of a rather subtle nature. Ardrey's own examples permit me to say that this is what he believes. Thus cultural features and skills in enclave groups such as the Chinese away from China, the British working-class, the blacks, the Jews, university professors and royalty may be genetically determined and

and will, as such, stand in contrast to the genetic makeup of the culture in which they find their place. Ardrey's assertions can and most likely will be used to justify a number of beliefs expressible in the form: "The X are all the same; they'll never change." Ardrey's assertions can also give the naive fuel for the belief that the populations should be kept separate lest the one contaminate the other. Ardrey does not support such beliefs himself.

2) Women are genetically destined to a lesser charge of aggression than are men. Women are naturally inclined to take the subordinate position. This is a hot matter also (to say the least) and has been ever since Margaret Mead stated the alternative relativist viewpoint. Ardrey's conclusions would have been quite acceptable to the Kinder-Küche-Kirche beliefs of National Socialism.

3) Mankind is naturally 'aggressive' and probably violent. Hence one could reason that repressive policing may always be in order. Ardrey does not comment on the need for police, save to say that increased social violence may lead to a surfeit of them. A natural violent streak is a dubious idea; the existence of 'aggression' in its technical sense may not be in question though the choice of the word 'aggression' for the innate factor driving much of human behaviour in its general activity relative to the world is distinctly inappropriate and misleading.

4) Man naturally cleaves to certain external objects in accord with the "natural law" of territoriality. 'Property' is one form that this takes. This idea may have something in it though very possibly not for the reason and with the implications that Ardrey thinks; I will discuss this briefly at the conclusion. In any case the manner in which Ardrey states this proposition is very unlikely to cause any discomfort on Wall Street and in the Monday Club.

Enough said. It is my conclusion that Ardrey's evidence seldom gives any definitive support to any but a pre-judged and intuitive acceptance of these propositions. I cannot think, given the state of things generally, that these conclusions are harmless. They are in fact grossly irresponsible in a book destined for the large public to which Ardrey appeals. However, I cannot fault him for suggesting what he suggests; the problem is that his suggestions are presented as affirmations and as scientifically 'proved' affirmations at that. It is a paradox in Ardrey that he puts forward what actually turns out to be some kind of absolute egalitarianism but yet a good part of his theory has profoundly nonegalitarian implications. It may at least be said that Ardrey does not support these implications himself, nor does he suggest any action which should be taken on the basis of his findings. At most he would say that some things, e.g. the subordination of women and man's desire for property are so deeply rooted that they can probably never be completely expunged. And, on the whole, his more positive statements actually support a certain kind of conservative ideology so extreme as to be revolutionary, and ideology not realized in this or perhaps any other century. To this I now turn.

All of what follows is based on the idea that man's genetic heritage comes into conflict with forms of social organization that do not permit an expression of basic drives. And we have also seen that Ardrey is sceptical of any social organization which gets in the

way of gene flow; it is this attitude which I believe saves Ardrey from the charge of racism. The genetic effects of society may be long-term; Ardrey is more directly interested, in the latter part of his book, with the interaction of culture and biological drives as they now stand and in general he sees great and increasing potential for social violence in what is going on. Such violence, formerly expressed in war, is a redirection of energies now denied that outlet by nuclear detente. The young are chiefly implicated in this. Thus Ardrey manages to include within his scheme virtually all disturbing phenomena of our time; this is no doubt a considerable selling point. But it is this aspect of Ardrey's book which is the most interesting and suggestive for anyone interested in practical concerns.

As I have said the basic point is that modern society is providing increasingly little opportunity for the exercise of man's biological drives. This is Ardrey's ultimate explanation for youthful revolt. If every being requires 'self-fulfillment' and an outlet for its charge of aggressive energy, and if it comes to be commonly realized that in fact modern society provides little chance for this, then there is trouble. Ardrey points to certain sociological findings to account for this malaise. He examines studies on industrial psychology and discovers that men work best and most purposefully when they are implicated directly in the planning of whatever the project happens to be. Men under such conditions are not, according to Ardrey, working in accord with a stimulus-response-reinforcement model in which money is the positive reinforcement and its lack the negative:

"...capitalist and Marxist share the same idée fixe of the almighty dollar; that man works exclusively for reasons of economic determinism. The Hawthorne workers /the workers of the electric components factory where the pioneering industrial study was done/ had been motivated by identity, not money - by being people different."(159)

Stated somewhat differently, Ardrey seems to believe that men work best and most happily when they are implicated in the results of their labour.

Ardrey also examines studies conducted by urban sociologists on city neighbourhoods. He discovers that, given a chance, neighbourhoods are self-establishing, self-regulating, and exclusive relative to other neighbourhoods. Again men are directly implicated in rewarding human activity. The antithesis to this is the anomic tower-block housing estate. In general these aspects of Ardrey's thought bear a startling resemblance to classical Marxism (the above quotation notwithstanding).

Of course it is true that Ardrey relates all the phenomena above to his three innate needs; but they are so vague as to be almost meaningless in this context. Nonetheless he has hit on things which are socially interesting. Given the fact that Ardrey is pointing to the above studies as illustrative of the nature of man, and given that he is against restrictive social organizations Ardrey's ideas come close to both Marxism and also to a certain kind of conservatism; he fits uncomfortably within the two positions but finally appears to opt for the latter.

Ardrey would look with approval at a society maintaining maximum flexibility within the confines of biological imperatives. The Russians prevent some forms of hereditary privilege by making hereditary wealth impossible; this presumably keeps everyone up to the competitive mark as well. I wonder if Ardrey approves of this sort of measure. It is certain that he would not approve of bureaucracy in almost any form; in this he is close to agreement with the radical critiques coming both from within the western and from within the presumably Communist world. He appears to see the bureaucratic state as a prime cause of the social malaise:

"Human youth recognizes that a few achieve identity. But it is a shrinking few, as organizations devour each other, while youth grows in numbers. And so there are those among the young - today some, tomorrow more - who suggest that if something does not give, then they will tear the place down as a house not worth living in. There is nothing unusual, in the quest for identity, to find those who will contemptuously reject security's last offer."(173)

He finds this quite correct biologically. But elsewhere he advocates restraint. The division of labour, he says, makes modern society very delicate, and youth should consider this before making irrational attacks. Again a paradox appears; Ardrey is unwilling to go along with his own argument, and so stops short of advocating anything really in accord with what he often states are the conditions for human satisfaction. His belief that society must strike a balance between order and disorder leads to the following deeply felt, but rather shallow proposition:

"What is at stake in our times is not the survival of man, but the survival of man's most rewarding of all inventions, democracy."(287)

Ardrey cannot or does not deal with the fact that a democratic form is something of a farce in a society which he himself characterizes as made up of ever more embracing bureaucratic organizations. But its defence is all that he can positively suggest, and with and as a part of its defence a return to nothing other than 'individual responsibility'; otherwise there will be no alternative to the police state:

"As a people normally gets the government it deserves, so a society normally receives the punishments it asks for. And so long as we support the Age of the Alibi, just so long must we inhabit the Age of Anxiety. There must come a limit, of course, when the social order to endure accepts violent means to suppress violent disorder. And we shall then see an endless procession of concentration camps, death penalties, public whippings, and police ascendancy. It is the likelier outcome, no doubt."(340)

The American mind can be very tortuous indeed when it comes to political reasoning. Ardrey has blocked every possible solution to the problems which he poses. He suggests the desirability of self-determination, of worker's control, community organizations, decentralization, and at the end of it all can only return to what can only be built upon these bases and which cannot really precede their establishment - democracy and responsibility.

A curious route indeed. Ardrey's formulations are something in which many would like to believe. Ardrey's dream comes from a time at least as far back as Jefferson; it is a dream of a pristine society built upon a base of autonomous, self-determining, free small farmers and merchants. This is still very much a live ideal in North America among so-called 'conservatives' and so-called 'radicals' alike. Every now and then it takes a political or quasi-political form. It has been noted in the rise of the Populist movement at the end of the last century and it can even still be detected at work behind such phenomena as George Wallace. There are also still many who actually believe that what is needed is a return to a pure capitalism in which enterprise is neither fettered by monopoly nor by government interference, a system in which each man can rise as far as he is able. The followers of the American novelist cum philosopher Ayn Rand believe just this in spite of its apparent absurdity. The curious birth and success of the Conservative Party of New York may indicate a new and perhaps more effective leaning in this direction. The fact that the beliefs which persons of this persuasion actually express are often inconsistent and even brutal gives no very good reason to discount it all as either unimportant or stupid.

But how very odd to find this stance supported again by an argument based upon biological imperatives and natural law. Though it is not very convincing in general, the biological argument may have some use in application to certain cases. For example, little enough is known about the makeup of the human mind; Ardrey mentions Chomsky at one point in his discussion of innate factors in human mentality. Chomsky points to the existence of innate factors which make it possible for the infant to assimilate the complex grammatical structures of language. Along this same line it could also be said that human cognitive organization may have its own demands, and that these demands could lead to what Ardrey characterizes as innate territoriality, xenophobia, identity through symbols, etc. What these demands could be I am in no position to say; there is work going on in psychology which may point towards at least the asking of the proper questions. But in general Ardrey is so devoted to biological arguments and analogical arguments from the observation of human and animal behaviour that he pays no attention at all to much material which could bear on his case for good or ill. The result is that he became so thoroughly muddled that there was no possibility that he might have given some kind of sense to the analysis of the biosocial nature of society.

I can only conclude this review by stating that I believe that Ardrey has written a harmful book. It will probably have most appeal to those who would support an essentially absurd bureaucratic 'democracy' or worse. Again, I can only say, how very odd. Surely this cannot have been what Ardrey set out to do.

Michael G. Kenny

BOOK REVIEW

Ecstatic Religion : An Anthropological Study of Spirit
Possession and Shamanism

I.M. Lewis. Pelican. 1971.

Prof. Lewis's recent book is an ambitious functionalist-comparative study of an aspect of religion which he claims has been neglected by social anthropologists. As the editor of *Man* and head of anthropology at the London School of Economics we can expect his work to be eagerly read by his colleagues. And as the book appears in paperback in a series designed to show the subject off to a wider public we must expect it to be fairly influential. Lewis is aware that his enterprise requires special pleading, and he is careful to avoid some of those ethnocentric errors which marred an earlier comparative tradition; for instance, he does not rank religious systems as the Victorians were wont to do, nor does he engage in speculation over the genesis of religion as such. Nevertheless, his endeavour seems to be marred by several rather profound methodological errors which ought to be exposed.

Firstly, he says that 'cultural distinctions' are 'often of much less consequence than functional similarities' (pp. 13-14). This, he suggests, is 'generally taken for granted in most of the fields in which social anthropologists work'. This stance enables him to ignore conceptual levels, categorical and linguistic problems and so to violate the cultural logic which one had assumed it was the task of the anthropologist to grasp. Hocart (1935) asks: 'How can we make any progress in the understanding of culture if we persist in dividing what the people join and in joining what they keep apart?' Lewis seems to have learned nothing from the ghastly failures of others who have attempted comparative work. Hocart's point, of course, does not make comparison impossible, but it does require the venture to be conducted with certain special types of conceptual tools; of this formal requirement Lewis seems completely unaware. The point is to generate generalities from grasping cultural significance not to confirm general theories through riding roughshod over cultural meanings by wielding some sociological hypothesis like possession is a means by which women protest about their jural inferiority, etc. This is where the difference between Lévi-Strauss's and Lewis's comparative work lies. It isn't a matter of Gallic splendour but a simple methodological superiority in Lévi-Strauss's work. One only wished that Radcliffe-Brown, whose idea of anthropology as comparative sociology still sets the task for Lewis in 1971, had actually engaged in some extensive project himself, then it would have emerged rather sooner just how unproductive the enterprise would prove.

All comparative work involves a problem of sources. We cannot evaluate Lewis's performance in this respect. What is worrying, because it does not seem to trouble his (down-to-earth commonsense) sociological approach, is the categories with which he performs his analysis. Let us remember the one time commonsense certainty that the earth was flat; sociological commonsense is no more privileged simply by virtue of its being a part of an established academic discipline. We are aware of the difficulties involved in using such terms as 'pathological' and 'hysterical' in our own culture; the problems concerning their application to other cultures are even more considerable. For instance, the concept of deviant would qualify as an 'odd-job' word (Wittgenstein) in our own category system. We cannot simply plonk it into another system of discourse without serious thought. But the objection does not stop with these psychological terms which are easily recognisable as being awkwardly culture bound. What qualification can we assume 'mystical', 'witchcraft', 'ancestor cult' or even 'religion' to possess that fit them for comparative purposes? Or

may we assume that these categories are used as 'automatically' (p.21) as the questions that Lewis asks?

If Lewis really wants to indulge in this type of comparative work he should at least bear in mind Evans-Pritchard's remark on the Frazerian style of analysis. That is, he should compare in their completeness the situations of possession among the Eskimos, hysteria in a London mental hospital and the experiences of a Christian saint in order to determine whether such a category as ecstasy genuinely subsumes these disparate phenomena. After all, it is only in the fullness of context that the terminology has any meaning at all, and without this contextualisation it may not be realized that ecstasy, in reality, is of as little explanatory use as the term matrilineal. It is only too obvious that we as yet simply do not understand enough about different modes of consciousness to embark on Lewis's type of venture. Why, for instance, is there no mention of James's The Varieties of Religious Experience which one had assumed would have some relevance? At least it would make the difficulties rather plainer. All Lewis does by seeing possession or witchcraft as protests against society or symbolic strategies of attack is to create a category of anomalous behaviour which requires a special type of explanation. In fact it is exactly the same procedure that the Victorians employed in their treatment of primitive belief and which Evans-Pritchard had already cogently criticized. Frazer assumes context and purpose are obvious and then imputes certain mental processes to savages. Lewis, in his way, repeats all these errors - and then charges Lévi-Strauss with being a neo-Frazerian! This criticism holds even though Lewis (p.36) claims his treatment is not to be regarded as a complete explanation. Lewis, and here he is in good keeping with most sociologists, (and the complete opposite of Evans-Pritchard) simply seems to have no feeling for culture. We cannot feel that resort to such concepts as 'deprivation' or 'ecstasy', really enables him to grasp the 'meaning' of any of the examples he discusses. It only confirms his sociological qualities that he should not really be concerned with meaning at all. As such his book strikes us neophyte Oxford anthropologists as vulgar in the same way as Gluckman's Custom and Conflict sociology. Lewis simply doesn't seem to sense how systems of meaning should be understood. The 'validity of my comparisons should be judged by their inherent plausibility and by the extent to which they contribute to the understanding of religious experience'. We remain unconvinced, and the use of example after example would do nothing to enhance the plausibility of the analysis.

We ought also to enter a comment concerning his statement that the importance of functional similarities as against cultural distinction is accepted by most anthropologists. True this might be of those in the backwaters of British social anthropology, but, as with his questions that the social anthropologist 'automatically asks' (p.21) we can only say it is not true of all. Many anthropologists have radically different interests to these displayed by Lewis and it is interesting that the newest anthropological trends receive no bibliographical mention in his book. Not even that sensitive study by Lienhardt of Dinka self-knowledge in Divinity and Experience receives a mention. When Lévi-Strauss gets a treatment that is nothing short of juvenile (p. 14-15) it is clear that we cannot accept Lewis's claim to speak for anthropologists. In fact, it is quite clear (p.30) that his approach and pre-occupations are consciously sociological. Perhaps a sociologist's evaluation of Ecstatic Religion would be different, but we can only feel embarrassment that in 1971 the title should contain the word anthropological. It is also laughable that he should regard it as bravery (p.178) to consider psychology and thus to extend the provenance of anthropology. One is reminded of that other London pronouncement in Jarvie's 1964 book 'over to Lévi-Strauss' when other departments had been there years before.

If Lewis's book represents anthropology to the general public as a rapidly changing discipline, one can only feel that the direction implied is the wrong one. Anthropology can advance by redefining its problems. Lewis seems unaware of this and is content to produce answers to problems set by his forbears. Here he is in good company. There are still departments where anthropologists devise good measures of divorce rates or where students are encouraged to produce excellent definitions of age-sets. All this refinement is of no value if the problem itself was originally ill-conceived. It is like expending a great amount of energy to establish the exact weight of phlogiston. Lewis's Ecstatic Religion strikes us as similarly outdated and misdirected. There seems to be a vast difference of interests between ourselves and the Professor at L.S.E., and to use a joke he himself uses, we can only hope that enthusiasm for his type of work is not catching.

Two Diploma Students.