

## REVIEW ARTICLE

Robin Fox (Ed.). Biosocial Anthropology 1975. ASA Studies. London : Malaby Press. xii, 169 pp. £6.50.

When the late Professor Freedman composed his long essay on social and cultural anthropology for the UNESCO Survey of Current Trends in the Social and Human Sciences, he gave honourable mention to 'approaches from ethology' as a 'trend' - even a 'growing point' worth watching. Now with the publication of Biosocial Anthropology the growing point has become, in its own eyes at least, 'an' anthropology. And here is the first source of doubt. Is it 'an' anthropology in the sense that, say, economic anthropology might be: namely a set of techniques and debates to do with the analysis of a bounded segment of social reality, enriched though it might be with controversy about the location of the bounds? Or is it a comprehensive mode of thinking about the social, on a level with the major '-isms' of our day and capable of competing with, or superseding, them? Is a biosocial anthropologist a sub-specialist, such that there are some kinds of social fact he feels called on to know about and others that he does not? Or is he a revolutionary? Some of us, who were involved in early attempts to explore the possibilities of biosocial thinking, may not have finished pondering the implications of this choice, and so may be disinclined, as yet, to make it.

Biosocial Anthropology is the record of papers presented to one session of the special Decennial Conference of the ASA at Oxford in July 1973. The authors and editor have had two jobs to do. On the one hand, they have had to assemble research material illustrative of what can be achieved within a biosocial framework. On the other, they have had to confront the problem of how this framework is itself to be characterised. In this review I shall try, through comment on the individual contributors' material, to suggest how much progress the symposium achieves towards the second objective.

Robin Fox, in his introduction to the volume, adopts a less revolutionary stance than in many of his writings. His opening statement that biosocial anthropology 'views social behaviour ... as the outcome of an evolutionary process' leaves room for manoeuvre on the possibility of alternative ways of construing the social. A wise move, despite the confusing hint, simultaneously given, that 'culture itself' is 'only understandable in [evolutionary] terms'(2). Fox picks out four 'disciplinary areas' as contributing most to the theoretical basis of biosocial anthropology: comparative sociology, comparative zoology, physical anthropology and primate biology. In addition he distinguishes certain 'points of departure' as characteristic of the biosocial approach. Among these are a 'concern with the life-cycle', 'ease of learning and critical periods', the notion of 'pathology', and that of 'characteristic bonds' often synchronised with the life-cycle. This re-grouping of the concerns of traditional disciplines within clusters of core issues is an achievement for which credit is due to biosocial anthropology in its programmatic phase. Whether the opportunities thus presented are to be fully exploited, either in the rest of the programme or in concrete research under the biosocial banner, only time will tell.

W.D. Hamilton's paper 'Innate Social Aptitudes of Man : An Approach from Evolutionary Genetics' tackles an old problem in a new way. The problem, which Darwin acknowledged, is the paradoxical evolution of altruistic behaviour. It has as corollary the general question of the

order of entity on which selection acts, an issue of central importance in any discussion of social systems as evolutionary products, and one which has recently acquired a new and intriguing twist (cf. Dawkins 1976).

Hamilton is concerned with the possibility that certain phenomena conventionally assigned to a moral universe, such as cheating, xenophobia and guilt, may have a biological basis in the sense that selection may have created in human populations a genetic predisposition for these to be manifest under certain conditions. Avoiding, as we would expect, any vulgarly reductionist formulation, he indicates his general position with the aid of a seductive analogy: 'The problem facing a humane civilization may be how to complete a sketch suggesting some massive and brutal edifice - say the outlines of an Aztec pyramid - so that it reappears as a Parthenon or a Taj Mahal' (134). There remain however unexplored linguistic difficulties, which I can best identify with the aid of another quotation:

Consider also the selective value of having a conscience. The more consciences are lacking in a group as a whole, the more energy the group will need to divert to enforcing otherwise tacit rules or else face dissolution. Thus considering one step (individual vs. group) in a hierarchical population structure, having a conscience is an 'altruistic' character. But for the next step - group vs. supergroup - it might be selfish, in the sense that groups with high levels of conscience and orderly behaviour may grow too fast and threaten to overexploit the resources on which the whole supergroup depends (135-6).

The difficulty in this case lies in the apparent congruence between the terms 'altruism' and 'conscience' which leads Hamilton to juxtapose them in a single frame. 'Altruism' entered the vocabulary of evolutionary biology (as did its converse, 'egotism') as an idiomatic, almost colloquial way of referring to a class of behaviour clearly marked out by independent defining criteria, namely behaviour which demonstrably diminishes the actor's chances of survival but serves the interests of his group. The human paradigm was of individual self-sacrifice; and in this case it was an exact and appropriate one. The moral connotations of the term do not, however, constrain debate about what is 'really' happening; at the level of the 'selfish gene' there seems indeed to be a doubt whether altruism can be said to occur at all where an individual sacrifices itself for close relatives. 'Conscience' by contrast stands in no such relation to an operationally defined class of events. The human paradigm 'acting rightly, or feeling that one should, without coercion' exhibits, were it necessary, the double relativity of human choice and of prescriptive systems.

I am not here making the oft-repeated point that humans live in a moral universe while animals do not. I am saying that 'altruism' like 'cheating' and 'xenophobia' but unlike 'conscience' belongs to a class of terms whose slippage from human to non-human contexts takes place in circumstances which have received less theoretical attention than they deserve. Enough has been said in the past about the gross application of human socio-political concepts to non-human spheres as if the former were straightforwardly descriptive. It now appears as an oddity in the language of behavioural science that the self-conscious objectivity of the fieldworkers' official stance regularly coincides with turns of phrase which tacitly invite the reading-in of invisible quotation marks on the part of the biologically well-educated reader. Certainly there is a 'so to speak' implicit in, say,

Wickler's remark (1969:198) that 'sometimes the (finches) quarrel about the best seat...' which renders any charge of simple anthropomorphism misplaced. Yet we may wonder whether there is more in it, whether there are reasons why language itself forces the observer into patterns determined by its own structures. I labour this point here not in criticism of Hamilton (to whose main thrust it is peripheral) but because it connects with suggestions I shall make later about the need to scrutinize the epistemology of our descriptions of the natural world.

I shall not attempt to assess Hamilton's mathematical argument, which seeks to refine a theoretical model under which there could be positive selection for altruism. The necessary modification seems to be that the model include a device for ensuring that the benefits of altruistic behaviour fall on individuals more likely to be altruists than are random members of the population' (140: his emphasis). I believe however that the advances contained in his work are accessible, at least intuitively, to the less numerate among us. Among the most interesting of his themes is that of strategy, that is of strategic options available within the life process at a number of orders of integration. At the intermediate level of groups, the situation of pack-hunting carnivores provides an illustration with quite plausible analogies for man, as anyone will recognize who remembers Thesiger's account of his despair when he and his Bedou companions, barely surviving in the wastes of the Empty Quarter, had no sooner managed the rare feat of killing a wild animal than beaming strangers appeared from nowhere to share the meal. At the individual level, a renewed interest falls on the deception and coalition games which are currently emerging as characteristic of higher primate groups. Hamilton draws from this material a number of bold hypotheses about the development of warfare, reciprocation, cheating and the mercantile virtues which seem destined to inspire a healthy controversy. Be that as it may, the strategic element in the life of complex social organisms places much social action squarely within the purview of some version of a theory of games. Hamilton, perhaps wisely, does not explicitly apply this formulation to non-human forms of social life. Yet the notion of strategy, if accepted as valid for non-humans, might justify a move in this direction. This in turn might prepare the ground for introducing or at least acknowledging a degree of controlled subjectivity in our accounts of non-human social life.

Where Hamilton explores links between macrosocial phenomena and events at the level of the replicating gene, Tiger points the finger in a different direction and seeks to connect the macrosocial with the somatic patterns of the organism. He provides an expert and much-needed review of this area, with a focus on studies of the somatic basis of non-specific sexual differences. Money and Ehrhardt are commended for their advocacy of a shift away from the old nature/nurture sterilities towards an interactionist view incorporating the concept of a 'program'. There follows a comment worth quoting:

Of particular theoretical interest to social anthropologists must be the contents of the phyletically written 'program' and what are the 'phyletically prescribed environmental boundaries'. This is in a real sense another version of the traditional quest for 'universals' in human societies, or functional prerequisites. However, to the extent the enterprise can depend on verifiable and cross-culturally applicable statements about human propensities, an augmented precision becomes possible that is unavailable to those focusing solely on sociogenic processes (122).

This revealing passage exhibits, to my mind, much of the strength and weakness of the brand of theoretical underpinning which Tiger and Fox, in particular, have been seeking to establish for a science of the biosocial. On the positive side, the evidence cited by Tiger should be enough to convince anyone that somatic and social factors can co-act, and it ought to be someone's business to be interested in their co-action. If officer cadets in the U.S. Navy consistently show low levels of testosterone secretion during the low-status phase of their training when 'degradation ceremonies' and the like are rife, levels which rise as and when the structure allows status and self-esteem to go up, then it is pertinent to ask what relevance this finding may have to the explanation of mechanisms perpetuating systems of sharp inequality - slavery, say. Similarly, Tiger raises questions about the Pill which can be answered only within a frame capable of embracing both the endocrinal and the social. Yet the passage I have quoted shows that we are still in deep trouble over human universals, propensities and programmes. It is embarrassing to have to re-assert what I have said before, but if we are engaged in the 'traditional quest for universals in human societies', how can this quest depend on 'verifiable and cross-culturally applicable statements about human propensities'? The term 'propensity' is itself merely confusing here, with its ambiguous coverage of 'tendency' and 'capacity'. Programmes, in some sense, there may well be - it would be astonishing if there were not - but we have not yet been told how to identify them.

N. Blurton Jones's paper 'Ethology, Anthropology and Childhood' commands respect as a demonstration of the scientific virtues of ethology in the classical tradition. Ethologists, he says, 'study the behaviour you can see people doing' (71). Beautifully put; and this very clear-mindedness forces reflection on the nature of the mental operations involved in 'seeing' a subject 'do' anything. I hope Blurton Jones will forgive me if I suggest that much of the strength of his work lies in his refusal to theorise prematurely or over-grandly. I mean this as praise. The unflustered, 'lateral' empiricism of Blurton Jones and 'people whose work I like' - how does the creature conduct its affairs in the world in which it lives? - may seem to divide their work from that of anthropologists, particularly those of a non-positivist turn of mind. Yet this is an empiricism which has a capacity to transcend itself in response to what I can only call the demands of appropriate explanation. There remain in the back of the mind doubts about the initial attraction of children as objects of ethological study, Are they (like mental patients, also very popular) unconsciously seen as more like primitives or animals than are fully functional grown-up Westerners? In Blurton Jones's case (though not, perhaps, in all recent ethological studies of children) the interest is amply justified by his concern with development. His way of approaching his material is a real contribution to the quest for common understandings between biological and social scientists.

It would not be appropriate for me to attempt a detailed appraisal of Michael Chance's paper on 'Social Cohesion and the Structure of Attention' since I have worked with him on the topic and have a view somewhat different from his on the 'advertence' concept. (Very briefly, Chance sees advertence as 'defining the manipulation of group attention where it is used mainly for the acceptance of an individual within an existing group' (111), and thus as belonging

within a theory of display; while I prefer to look on it as pointer to a new kind of treatment of the observer-observed relation, a treatment which allows for explicitly performative, rather than flatly behavioural, modes of description.) Chance's line of argument in this paper closely follows that of his earlier essay (1973). What is best in it is, I think, still the perception of the increased flexibility in the organization of social relations allowed by the 'hedonic mode' of interaction. Chance's insight here and elsewhere lies in drawing a connecting line between the possibilities of creativity in social relations, the capacity for self-monitoring, and the selection pressures favouring functional elaboration of the primate and hominid brain. The implications of this linkage have been extensively discussed and further elaborated elsewhere, notably by Fox (1972).

At the beginning of his essay, 'Comparative Ethology of Incest Avoidance', Norbert Bischof pegs out his ground with an ambiguity which (whether intended or not) nicely illustrates the present uncertainties of the biosocial exercise: 'At the present time comparative ethologists are interested in making the study of nature available for the comprehension of cultural phenomena' (37). Bischof, like Hamilton, has set himself an old problem: the so-called incest prohibition and its natural or cultural roots. I say 'so-called' because the nub of Bischof's solution is that the rules about incest are best construed as labelling devices which cultures attach to choices and avoidances which would in any case 'naturally' tend to occur. The articulation of incest rules in man thus becomes 'an act of self-interpretation' (63); an attractive idea so far as it goes, and a great improvement on older and cruder demands that we choose between two equally vulgar forms of determinism, the natural and the cultural. Yet the incest problem in its traditional version is not quite disposed of. If the received view is true, that societies set up a category of incest (however defined) which they then ban with great determination and fuss, then the problem of incest rules lies in their rule-like character. We can modify Freud's objection (which Bischof himself cites) to a biological-cause explanation: why choose incest to have deeply-felt rules about?

Bischof's point is well taken: that Levi-Strauss was wrong to assume incest between biological kin to be 'a natural phenomenon found commonly among animals'. Any theory which equates animal-to-man with nature-to-culture by using the 'incest taboo' as pivot for both is clearly mistaken; such theories are in any case faulty on other grounds as well. Bischof presents a mass of evidence to show that biological incest is rarely found in animals under natural conditions, and that in species whose social organization includes individual bonding, devices exist which seem aimed at the systematic avoidance of incestuous mating. His survey of mammalian social structures, incidentally, exemplifies a mode of deductive analysis which I for one have long been hoping to see. His argument that the biological final cause is likely to be 'the increase of variety through the recombination of genetic material' (57; his emphasis) rather than avoidance of the supposed evil consequences of inbreeding, carries conviction. Yet the sceptical Durkheimian will still ask, so what? If incest avoidance exists in nature in the sense that animals show it for good selective reasons, are social scientists obliged to take note of this fact in their accounts of rules about incest avoidance? As in Tiger's case, a brave attempt is made at an inclusive framework of explanation but the result is still disappointingly tenuous.

Robin Fox shares with Bischof an interest in the possible evolutionary basis of general features of human kinship systems. His contribution, as he says himself, has to be read in continuity with his earlier paper (1972). We can in passing note the latter's point of departure:

Rules of marriage...have to do with the allocation of rights over women...The modern theory of kinship in fact sees all kinship systems as sets of rules regarding the allocation of women as mates, or the 'circulation' of women among the kinship units of the society...Kinship systems, then, are systems of rules about the exchange of women and the relationships set up by this exchange.

Without indulging in too much tit-for-tattery (what about the allocation of rights in men's sexual services? - and these are real rights, women quarrel about them) we can gently question whether such a 'modern theory of kinship' can possibly aspire to the scientific virtue of completeness. One of the most interesting features of Fox's writings generally is the way in which, seemingly dazzled by the analogies and homologies between 'dominance' in a non-human world and 'control', 'possession' and the like in a human one, he treats these (which are properties of relationships) as if they were adequately descriptive of systems. He is, of course, not alone. Yet a serious treatment of the notion of system, as it relates to the cross-specific analysis of social organization, is surely one of a number of preconditions for any form of theoretical advance.

Fox's present paper is entitled 'Primate Kin and Human Kinship' and at its core is a bold and original theory: that the characteristically human pattern of kinship organization arose from the putting together of elements of 'alliance' and 'descent' found separately, not together, in existing primate structures. It is a beautiful theory; but I doubt whether the data are complete enough to support Fox's claim that descent and alliance are never found together in non-human primate systems. For example, do we know all there is to be known about female-female relations in one-male systems? It is true as Fox says that in hamadryas the 'son' does not routinely succeed the 'father' as focal male of a breeding group; but can he be sure that all kin bonds are lost to the young male during the long process of peripheralization and re-entry to the breeding centre of the group? Is it impossible that because of his relation to 'mother', the young male may find it easier to kidnap a young 'sister' than an unrelated female infant as founder-member of his harem (thus going against Bischof's theory, however)? Might not a newly-recruited female assimilate most smoothly to a harem which already contains a 'mother' or 'sister'? Could not the quality of relations among the females itself influence the stability of a harem and the male's chances of holding it together and hence exert selective pressure? Even a slight tendency for any of these to happen - and they would reflect patterns known to occur in other primate groups - would amount to a coincidence of 'descent' and 'alliance' factors (as Fox defines them) in determining the composition of breeding groups. Kummer's picture (e.g. 1971) of the organization of hamadryas society, in space and time immediately fascinates the anthropologist, with its Levi-Straussian circulation of females between breeding groups, and its tantalizing hints that a male's female-based links with different groups may influence his 'political' career in the post-breeding phase of the life-cycle. Yet we should be cautious, and at least await the results of thorough long-term study before ruling out this or that pattern in the service of grand theories.

A general verdict on the state of play in biosocial anthropology as represented in the book must be that the 'theory' has not caught up with the 'work'. It is a tantalizing state of affairs. The material presented, and questions raised, by the contributors testify loudly to the need for a coherent theoretical frame; and this, precisely, we lack. This is a serious condemnation only if we fail to see the book for what it is: good documentation of an incomplete phase in what may yet turn out to be a valuable synthesis of different research areas. On this view, Fox does a disservice to the biosocial movement by his impatient efforts to specify a firm theoretical structure. I for one do not share his cheerful confidence in the 'neo-Darwinian synthesis' (2) if this is to be incorporated wholesale into the new discipline as its sole explanatory principle. This is not the place to attempt a substantial attack on the problem; but I should like to conclude by mentioning two major difficulties (there are others as well) which must be overcome by any comprehensive biosocial theory.

The first difficulty is about method. What are we trying to explain? I have already commented on the suspect procedure of citing variation as an index of variability; and Blurton Jones in this volume cites Bowlby's observation that the selection pressures influencing the plasticity of a character may be quite different from those determining its development and phenotypic emergence. We have a legitimate interest in 'biological givens' and their relation to 'cultural responses'. The problem, as Fox sees it, is how to get at the givens; and it is made worse of course if his preferred method ('the comparative study of society' to illuminate 'the range of variation open to human social arrangements') is shown to be questionable. As he rightly says, the relationship between givens and responses must be problematical. But a crucial aspect of this is that the givens and responses are likely to include one another many times over and at more levels than the purely material or causal. So we must be on our guard, and prepared to meet at the outset questions of the greatest philosophical and semantic complexity.

The second difficulty is epistemological. Model-making in this as in other fields is heavily dependent on data, 'facts' and the like. Among the types of observation we are dealing with are those of ethologists on animal behaviour and social organization. Because ethology styles itself as an empiricist, non-subjective mode of inquiry, it is typically assumed that the 'facts of animal behaviour' are unassailably 'there', whatever dispute there may be about their relevance to human life. But it is quite easy to show that this confidence is not always justified: as in Young's eloquent demonstration of the intrusion of socio-political prejudice into biological theory (1973) or in the failure of primatologists until very recently to notice females' participation in primate societies in any capacity other than as mothers or as an admiring audience to male dramas. While many would lament these as chinks in the armour of the old paradigm, it is at least arguable that they may hint that the uncompromising empiricism of ethology's official stance may be due for re-examination. The notion of 'observing animals' groups together a number of mental operations which may differ in the nature of the demands they make on the observer. Comparison of Tinbergen's painstaking studies of digger wasps and sticklebacks with van Lawick-Goodall's equally painstaking study of chimpanzees might lead us to suppose these differences to be a linear and uninteresting outcome of the taxonomic distance of the species in question from ourselves. That this is not the whole

story, is attested by the odd case where an animal species, though apparently very different from man, seems to make a more than usually powerful claim on the sensitive observer's human powers of understanding: witness Lorenz and his geese, or Michael Fox (1971) and his wolves. The roots of such affinities might be traced in a number of ways and it would be wrong to be dogmatic; what I am suggesting is that benefit might result if such features of the observer's relation to the observed were brought to the fore, rather than kept at the unofficial periphery, of behavioural analysis. (Hence another intriguing twist: the operation wherein the human observer sets up a relation to the animal groups he investigates is itself an anthropological issue.)

I make no attempt at theorise systematically here. My point is that far-reaching changes are possible in the spistemological self-conception of at least one of the component disciplinary areas of the biosocial synthesis; and any such changes will influence the intellectual balance of the whole in ways that are at present largely unpredictable. Therefore, despite the provocative originality of many of the contributions to Biosocial Anthropology, this is not the right moment for the movement to settle into a respectable discipline or sub-discipline. Leave it all to brew a little longer.

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