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Introduction

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THE IDEA for the Royal Society/British Academy discussion meeting (held in April 1995) at which the papers in this volume were presented goes back to a similar joint meeting held as long ago as 1966 on the theme of ritualization of behaviour. That meeting was organized by Julian Huxley, who in his introduction paid tribute to Darwin for having, as he put it, 'paved the way for a unified psycho-physiological approach in the study of the behaviour of not only animals but man' (Huxley 1966: 250). Since then, an avowedly Darwinian approach to the study of social behaviour has come to embrace a range of disciplines extending well beyond the frontier which conventionally separates the concerns of the British Academy from those of the Royal Society. At the same time, a growing understanding of the behavioural repertoire of primates has modified many earlier assumptions about the distinctiveness of human behaviour while re-emphasizing the mental capacities which do indeed mark off *Homo sapiens sapiens* from other species. And a growing body of research on the longstanding question of human origins has yielded an increasingly convincing reconstruction of where and when (if not exactly how) it occurred.

It is only to be expected that the closer to present-day human societies a Darwinian approach is brought to bear, the more hostility it will continue to generate (Stove 1994). In anthropology, moreover, the difference in approach between social and biological (or 'sociobiological') anthropologists has widened to the point that although, as a recent article in the journal *Science* points out, anthropologists are all trained to bridge the gap between cultures, 'today many American anthropologists find themselves divided by one of those very gaps—and are having a tough time spanning the chasm' (Holden 1993: 1641). But rival approaches to common topics are not always mutually exclusive to the extent that the protagonists may make them appear. Not only do different kinds of question call for different kinds of

answer (Foley 1995: 70, citing Tinbergen 1963), but the understandable propensity of researchers with new ideas to claim too much for them is, for all the controversy generated thereby, an encouragement to their correction by further research. Thus the signal inability of behaviourist psychology, despite its success in accounting for certain limited aspects of both animal and human behaviour in terms of operant conditioning, to deal with language-learning (Chomsky 1959) has helped to move the study of language in directions which have proved significantly more rewarding. Similarly the claim that language (if not, indeed, the whole complex of 'culture' as Tylor originally defined it) is 'socially constructed', although it derives from a presupposition which is palpably fallacious (Noble & Davidson 1991: 234–5), has nevertheless served to emphasize the need to reconcile the unquestionable diversity of human societies with the behavioural universals underlying it (Brown 1991).

It is, moreover, common ground that the operation of natural selection over the five or more million years since we diverged from our nearest genetic relatives continues to bear directly on contemporary human behaviour, whatever may or may not be the changes bound up with the transition from nature to culture. We can say with confidence that, among other things, young adult males are predisposed to homicide relative both to older males and to coeval females across the range of human societies (Daly & Wilson 1988; cf. Cronin 1991: 331); that certain facial expressions correspond to certain emotional states independently of variations in culture (Ekman 1973); and that all humans are genetically endowed with a common set of domain-specific, content-dependent, functionally specialized psychological modules and algorithms (Tooby & Cosmides 1992), including a 'language instinct' (Pinker 1994). It remains, inevitably, debatable how much of the variation in social behaviour patterns can be explained by hypotheses directly derived from models of natural selection. Even where the data show a consistent fit with what could be predicted in terms of maximization of inclusive reproductive fitness, there may be an equally good fit with what could be predicted by derivation from a model of autonomous cultural evolution. Moreover, the more complex the pattern of behaviour, the more difficult it is likely to be to disentangle the effects of genetic, ecological, demographic, and cultural variables. But in social, no less than in biological, anthropology, it is a matter of finding evidence which will enable competing hypotheses to be tested against one another.

It follows that reductionist hypotheses ought not to be dismissed a priori, as some social anthropologists and sociologists have been inclined to do. But the speed of change and breadth of range of social behaviour patterns which can be observed once the potential of our distinctive mental capacities begins to be realized is such that a reductionist approach becomes

increasingly difficult to sustain. For example, the claim that (even) monastic celibacy is explicable in terms of maximization of inclusive reproductive fitness (Alexander 1979: 80) can hardly fail to invite a comment to the effect that, as it is cautiously phrased by Boyd & Richardson (1985: 203), 'it seems more plausible to us that other mechanisms are at work'. Once units of information affecting phenotype are being transmitted by genuine imitation and learning from the mind (or brain) of one member of a conspecific population to that of another, the diversity of tastes, fashions, beliefs, rituals, styles, and techniques can be accommodated convincingly within 'gene-culture coevolutionary theory' only if the possibility of autonomous cultural evolution as well as selection for genetic fitness is allowed for and the 'adaptationist' issue left open (Laland *et al.* 1995). Whether the units of selection are taken to be discrete particles of information ('memes') or 'instructions' (Tschauner 1994: 79), or 'bundles' (Durham 1990: 203), or what many anthropologists still prefer to call 'traits' can likewise be left open, so long as it is recognized that 'the smallest units of phenotypic consequence need not be equal to the functional unit of transmission' (Durham 1991: 422). What matters, once again, is recognition of the possibility that episodes of innovation and diffusion of social behaviour *can* come about through 'rapid and multivariate patterns of purely internal cultural change' (Mellars 1992: 17).

To say this, however, is not to assume that culture is unique to the human species. This issue has sometimes been bedevilled by definitional disputes over what is to count as 'culture'. But culture is unique to humans only under a definition so restrictive that the distinction between genetic and exosomatic transfer of information is itself lost. Rare as genuine imitation and learning as opposed to (mere) stimulus enhancement appears to be in other species (Boyd & Richerson, this volume), there is no doubt that it exists, and not only in our closest genetic relatives. Nor can it any longer be disputed that chimpanzees, in particular, can not only make, use and reuse tools but transmit different styles of tool use independently of population density or environmental parameters (Boesch *et al.* 1994; Boesch, this volume). Moreover, the capacities of individual animals such as the bonobo Kanzi (Savage-Rumbaugh & Rumbaugh 1993), who are studied in non-natural laboratory or domestic environments where they learn or imitate the behaviour of humans, have to some degree diminished what was previously taken to be the divide between human and primate intelligence. Nevertheless, the recent conclusion of a primatologist whose own work has done much to bridge the hominid/pongid gap is a firm restatement that 'chimpanzees do not have human culture, material or otherwise' (McGrew 1992: 230); and the most significant difference between 'them' and 'us' from the viewpoint of biology and the social sciences alike is the innate capacity

of three-year-old human infants to construct novel sentences whose lexical and syntactic complexity is far beyond even the most carefully trained adult chimpanzee.

When, how, and why this capacity evolved is perhaps the most intriguing puzzle confronting researchers concerned with the question 'what is being selected for what in the transition from nature to culture?' (Aiello, this volume). The advantages to a species endowed with language are evident. Speech-users can maintain social cohesion in extended groups, explore unfamiliar and inhospitable territory, distinguish friends from enemies, and plan the acquisition and storage of material resources far more effectively in consequence. But to list the advantages is not to reconstruct the evolution, or explain its uniqueness to the human species. Nor does it yield the answers to such questions as how linguistic competence relates to other mental abilities, or what inferences (if any) can be drawn about language from archaeological evidence for increasing sophistication in technology or art, or when the necessary neuroanatomical changes (whatever they were) took place, or why humans resemble birds more than they do primates in their vocal development, or whether gestural rather than verbal communication may hold the key to the origin of language. But it seems safe to suppose not only that the evolution of the capacity for language was both gradual and complex but that as it is further elucidated the implications for the relation between natural and cultural selection will be increasingly revealing.

Meanwhile, it is as well to remember that although language cannot be said to be necessary for the exosomatic transmission even of complex information (Bloch 1991), once it is fully developed it brings about a further change of equal importance for the evolution of social behaviour patterns. As it is put by Whallon (1989: 438), other people come to be identified not merely as individuals but 'in terms of social categories, among which mutual rights and obligations are defined by the system'. At this point, the term 'role' takes on a meaning beyond its meaning in the theory of natural selection, where it denotes differences such as male and female, or owner and intruder, which are perceived by contestants in asymmetric contests for territory or resources and thereby cause them to adopt one strategy rather than another (Maynard Smith 1982: 204). Roles are now positions in social space whose incumbents share expectations and beliefs about their capacity to influence each other's behaviour because of the roles that they occupy. Moreover, whereas in cultural evolution the units of selection are replicated by transmission from one person's mind (or brain) to another's, in social evolution they are units of *reciprocal* action—with, of course, the possibility of mutation without which evolution of any kind comes to a standstill. At this level, the equivalents of the genes of natural selection (and the 'traits', 'bundles', or 'memes' of cultural selection) are the practices by which roles

are defined as such (Runciman 1986). In consequence, just as it is bound to be difficult to disentangle the effects of natural from those of cultural selection, so is it bound to be difficult to disentangle the effects of cultural from those of social selection.

Let me give one brief example taken from Sperber (1985: 86) where he says, in talking about pre-literate cultures, that 'in an oral tradition, cultural representations which are hard to remember are forgotten'. This is not the circular proposition which it may look like at first glance, since it is a question to be settled empirically whether formulae which can be shown to be easier to remember are in fact transmitted either vertically or horizontally within the population under study more frequently than others which can be shown to be more difficult to remember. But suppose we apply it to the much-studied transmission of oral epic poetry in Archaic Greece. It is well known that the replication and diffusion of the *Iliad* and *Odyssey* was facilitated not only by their hexametric form but by their use of stock epithets, recurring themes, conventional sequences, and repeated lines. But the mnemonic function of devices like these is far from furnishing an adequate explanation of their remarkable survival and spread throughout the Greek-speaking world. For whatever the powers of memory, enhanced by these devices, of non-literate populations, the transmission of long oral epics required training and application on the part of semi-professional singers who had to earn at least part of their living from patrons in whose courts or households they performed when called on to do so (Kirk 1965). Thus, for all that oral traditions in pre-literate societies are a textbook example of exosomatic transmission from one person's mind (or brain) to another's, they at the same time take us into the 'civilized' world of roles, ranks, and institutional relationships of power and patronage which is as familiar to archaeologists reconstructing the social structure of non-literate peoples from their grave-goods and the spatial distribution of their material remains as it is to social anthropologists studying them directly in the field, or sociologists and historians studying them through documentary records.

This particular example leads into topics outside the concerns of the contributors to this volume. But on any topic where the possibility of cultural selection is present, we have to address the difference between behaviour which is and is not, in the words of one social anthropologist, 'activity devoid of control by a knowing subject' (Ingold 1983: 5) or, in the words of another, the indisputable fact that humans are 'intrinsically active and choice-making beings in pursuit of particular goals and objectives' (Robarchek 1989: 909). Well—yes. But who ever thought otherwise? The question which needs to be asked is 'so what?'—or, more circumspectly, 'what follows for the study of social behaviour patterns in animals and man?' One thing which immediately follows is that the question 'what is it

like to be the way they are?' arises in a new and different form. That question is pointless when addressed to termite workers which can be said to 'know' that they have reached the boundary of a ground plan only in a sense similar to that in which cells at the boundary of a liver 'know' that they are not in the middle of it (Dawkins 1982: 204), fascinating but ineluctably opaque when addressed to vervet monkeys at the foot of Mt Kilimanjaro alerting one another to the presence of predators without, however, attributing to each other minds like their own (Cheney & Seyfarth 1990, and this volume), and deceptively straightforward when applied to present-day humans interviewed by survey researchers about their tastes, life-styles, and political opinions. It is, however, a question to be categorically distinguished from explanatory questions, whether motivational, functional, or genetic. It calls for different methods; it appeals to different criteria; it leaves the researcher with discretion in answering it of a kind which does not arise in the formulation and test of explanatory hypotheses; and it may even be as well answered by a work of fiction as by an ethnographic monograph in the manner of Malinowski or Evans-Pritchard or a slice of 'thick description' in the manner of Clifford Geertz (1973: Chapter 1).

What, then, is the difference which it makes when explanatory hypotheses *are* being formulated and tested? In one sense, the answer is: none. The question 'what is it about the antecedent history and present environment of this population which makes its patterns of social behaviour what they are?' is the same where the history and environment cause its members to have purposes and goals in consequence of which they behave as they do as it is where the history and environment cause their behaviour to be an immediate and instinctive response to the stimuli which evoke it. The members of !Kung San foraging bands are aware of the function of meat-sharing in generating a sense of mutual obligation, just as the members of university departments of anthropology are aware of the latent social functions of the seminar and the coffee-break. But the functions would be the same even if they weren't. It is not the fact of self-awareness which explains the pattern of social behaviour of which the behaving selves are aware. Conversely, it is a commonplace in non-Freudian as much as in Freudian psychology that even people who are intensely self-aware may fail to understand (in the explanatory, as opposed to the descriptive, what-it-feels-like sense) the motives which impel them to behave as they do. The point is, rather, that since something different *is* going on 'inside "their" heads' from the vervets' heads, and the vervets' from the termites' heads, we can hardly fail to be better placed to explain both human and animal social behaviour patterns if we can succeed in finding out how it works and what it does. Thus, the potential value of models of rational human action (however 'rational' is defined) lies in their capacity to account for the behaviour

observed by relating it to the modules and algorithms inside 'their' heads whether or not 'they' are aware of it and whether or not 'we' have an empathetic sense of what it is like to be one of 'them' (cf. e.g. Cohen & Machalek 1988, 1994). And as further progress in this direction comes to be made, the 'chasm' separating biological from social anthropology will more and more effectively be spanned.

This will surely involve further interdisciplinary co-operation of the kind which has been growing steadily over the last thirty years and is, indeed, exemplified by the papers in this volume and the discussions which followed their presentation at the joint meeting. In saying this, I am well aware that for both Popperian and other reasons it is impossible to forecast the future course of scientific enquiry. But I do not think it foolishly optimistic to suggest that we are witnessing an integration of approaches which will turn out to be as important as the old 'new synthesis' which integrated evolutionary theory with population genetics. Indeed, I am prepared to venture the prediction that this new 'new synthesis', which has already enlisted primatologists, anthropologists, archaeologists, psychologists, demographers, and linguists, will increasingly involve even general sociologists hitherto as remote from biology as myself.

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