

Thesis

1375

THE DESCENT OF DARWINISM

(A Philosophical Critique of Sociobiology)



The last clear definite function of man - muscles aching to work, minds aching to create beyond the single need - this is man. To build a wall, to build a house, a dam, and in the wall and house and dam to put something of Manself and to Manself take back something of the wall, the house, the dam; to take hard muscles from the lifting, to take the clear lines and form from conceiving. For man, unlike anything organic or inorganic in the universe, grows beyond his work, walks up the stairs of his concepts, emerges ahead of his accomplishments.

(J Steinbeck - The Grapes of Wrath)

In man *creature* and *creator* are united: in man there is material, fragment, excess, clay, dirt, nonsense, chaos; but in man there is also creator, form-giver, hammer hardness, spectator divinity, and seventh day: do you understand this contrast?

(F Nietzsche - Beyond Good and Evil)

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This work is dedicated to my mother, and to the memory of my father.

Thesis Abstract

The following thesis offers a philosophical critique of sociobiology, which is identified as a recent attempt to produce a general theory of animal behaviour, encompassing an account of human nature. The first chapter examines the empirical and theoretical foundations of sociobiology, highlighting some of the philosophical topics regarding the relation of the natural and social sciences, and the attempt to offer an account of human nature within a largely mathematical and mechanistic theoretical framework.

Chapter two looks at the major specific areas of human behaviour featured in sociobiological accounts. A close examination of empirical evidence, underlying theoretical assumptions, behavioural categories and definitions, and finally deduced conclusions reveals several weaknesses and examples of fallacious reasoning. The third chapter continues to examine the account of human nature in relation to the broadest and most abstract features of social structures and interactions. The political dimension of sociobiology is examined - both in terms of its account of political behaviour, and in the theoretical opposition between sociobiology and left-wing ideologies. The sociobiological account of religious behaviour is rejected in favour of one couched in terms of social rather than genetically heritable dispositions.

Chapter four evaluates the attempt to respond to early criticisms of sociobiology. It is argued that the main theoretical stance regarding human behaviour remains little changed, and that the new theoretical models create even more conceptual problems, thus failing to provide a framework for an account of human nature. The final chapter applies some ideas from evolutionary theory to specific areas of philosophical controversy: the relation of mind to language; the ascription of mental life to other species; functionalist and epiphenomenalist accounts of consciousness. It is argued that empirical and theoretical considerations from the natural sciences may thus inform traditional areas of philosophical debate, creating useful interdisciplinary dialogues.

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Chapter One: A Critique of Sociobiological Methodology

1.1 The Scientific Origins and Aims of Sociobiology

The publication in 1975 of E O Wilson's massive text Sociobiology: the New Synthesis sparked off a heated dispute about the status of the so-called 'new science' of sociobiology. Whilst proponents of the theory such as Wilson defended sociobiology as capable of explaining within a strict scientific methodology the origins, scope and significance of social behaviour at the level of both individual and group displays, its critics accused it of glossing over the content and complexities of existing models of behavioural interpretation, and of reducing behavioural explanation to tautological statements about adaptational functions. (see for example: Baldwin & Baldwin, 1981; Kitcher et al, 1987, especially pages 61 - 74.) Not content with an attack on the scientific credibility of sociobiology, various groups have also pursued *ad hominem* style arguments, questioning the possible sociopolitical motivations of its supporters and the likely results of a practical application of the theory. The suggestion is that sociobiology may well find favour in the minds of those eager to believe a potentially divisive account of genetically-based differences in ability between various ethnic, sexual and socioeconomic groups within human society, regardless of its actual scientific credentials. (For examples of such criticisms, see: The New York Review of Books, Beckwith, J. and others, 1975 and Rose, S. et al, 1984.)

The range and intensity of reaction to Wilson's work, from layman to specialist, was not unlike that which occurred over a century earlier in reaction to Darwin's publication of The Origin of Species and the later work The Descent of Man. In both cases the dispute focussed not on the author's exhaustive and meticulous observations of many varied species, but on the inclusion in their theories of one particular species - *Homo Sapiens*. Just as Darwin's account of the origin of the human species by the process of natural evolution was reviled by some as a dehumanisation and debasing of man's previously isolated and privileged status in the natural world, so Wilson's account of the origins and guiding mechanisms of man's social nature is seen by some as an attack on the autonomy and meaning of our social structures and their products. The similarity of reaction to the works can be explained at least in part by certain features which they share in common. In both cases the authors generated speculative and radical theories for which conclusive empirical evidence was lacking. They both proposed a synthesis of data and method from previously distinct research disciplines, overturning existing theories in the wake of a new, all-embracing conceptual framework. In both cases the new theory was to extend so far as to include our own species, which had previously lain outside of the existing theoretical scope. Both theories posed a threat to a particular group, whose supporters quickly became the focal point for vehement opposition. In Darwin's case the strongest reaction came from (and still comes from) the supporters of the threatened Creationist account of the origin of all life on this planet. For Wilson the main opposition is from the practitioners of the social sciences and humanities - those areas Wilson has suggested should and will be 'biologized' by the new discipline (Cf Wilson,1975,p.4).

Wilson's vision of the transformation which he claims must occur within the existing scientific structures is certainly not a new one. As one critic of sociobiology has expressed it: "Wilson's monism is the latest version of positivism, the contention that the cognitive aims of natural and social science are basically the same." (Pickens,1985,p.67). In fact Wilson himself seems to see his work as part of the historical line of development to which Pickens refers:

A guiding principle has nevertheless reemerged from the combined efforts that once inspired Comte, Spencer and other nineteenth century visionaries before dying from premature birth and Social Darwinism: that all of the natural sciences and social sciences form a seamless whole, so that chemistry can be unified with physics, biology with chemistry, psychology with biology, and sociology with psychology - all the way across the domain of enquiry by means of an unbroken web of theory and verification. (Lumsden & Wilson,1983,p.171)

This vision of a unified science incorporating all aspects of man's knowledge, which began during the enlightenment with the successful development and application of the natural scientific method, received an initial boost with the publication of Darwinian theory which finally brought our own species' origins within the bounds of natural science. However, as Wilson comments, the early attempts to merge the two domains of natural and social scientific theory were soon to be tainted by their incorporation into the right-wing ideologies of Social Darwinism in America, and the rise of Naziism in Germany (see: Gasman,1971 and Stein,1987). The more recent and rapid development of theory and experimental success within the biologically-based sciences over the past fifty years or so has once again sufficiently enthused the supporters of sociobiological theory to predict that a century from now the rigid disciplinary frontiers at present mostly respected by both natural and

social science practitioners will be broken down. This process is envisioned as a mainly one-way colonisation of the social science territories by the biologically-based disciplines:

To maintain the species indefinitely we are compelled to drive toward total knowledge, right down to the levels of the neuron and gene. When we have progressed enough to explain ourselves *in these mechanistic terms*, and the social sciences have come to full flower, the result might be hard to accept...But we still have another hundred years. (Wilson,1975,p.571 - emphasis added)

It is clear from the phrase I have emphasised that the changes envisioned are not merely in the degree of cooperation and informational exchange between the natural and social science disciplines: rather, they constitute some form of reduction of description and explanation from the present largely non-mechanistic terms of the social sciences to the mechanistic terms and models of the natural sciences. It will be one of my main contentions that the enthusiasm with which such changes are envisioned tends to produce a smoke-screen, behind which lie many unresolved problems of both a theoretical and empirical nature. Though specifically aimed at the theory and practice of sociobiology, many of my criticisms in this area would apply equally to any attempted merging of the natural and social sciences, and as such they constitute some of the problems debated within the philosophy of science generally.

Given the above predictions by Wilson, coupled with sociobiology's sociopolitical ancestry, it is hardly surprising that the reactions from many quarters have been aggressive and tended to polarise interested parties into two strongly opposed camps. However, it is important to place sociobiology within a historical context, since the theories of Wilson and others have grown out of a body of established scientific research and

theory. Sociobiology was defined by Wilson himself as "the systematic study of the biological basis of all social behaviour" (Wilson,1975,p.4). Its scientific roots are to be found in the disciplines of ethology, ecology, population genetics, and the general application to animal studies of the Synthetic Theory of Evolution, which combines Darwinian evolutionary theory with the genetic mechanisms of heredity first discovered by Gregor Mendel. The systematic study of animal behaviour (ethology) was established in this century through the observational work and theory of such pioneers as Konrad Lorenz and Niko Tinbergen. (See for example: Lorenz,1937, 1965 & 1970 and Tinbergen,1951.) They began to observe and record animal behaviour in the wild, with a view to explaining the forms and origin of such behaviour in the same way that the morphological traits of a species are explained. Just as an animal's organs are regarded as the products of evolution by natural selection, and therefore to some extent a functional adaptation to the environment, so the behavioural traits of animals were examined from the same theoretical stance. (The idea that morphology and behaviour could be equally approached and explained from an adaptational functionalist perspective was already suggested in Darwin's own work. See: Darwin,1859, especially Chapter 8.)

An example of such a theoretical approach was Tinbergen's observation of Herring Gull colonies (Tinbergen,1953), which revealed in an apparently chaotic and disfunctional assemblage of individuals competing for limited resources a high degree of mutually-responsive behaviour. Such 'social' behaviour was explained as serving functionally to regulate and perpetuate the living conditions of individuals and of the colony as a whole. Territoriality, aggression, mating and rearing of chicks, role divisions,

reactions to predators both individually and collectively, and all essential aspects of the gulls' life were found upon close observation to be subject to very precise control by largely instinctive behavioural patterns. Such behaviour was coordinated throughout the group by the mutual exchange of instinctively recognised signals (as opposed to a system of learned signals). Through this system of signals individuals could affect and be affected by the behaviour of those around them. Possibly the most famous and impressive of these experiments in behavioural response are those performed by Tinbergen on the food-request behaviour of baby Herring Gulls. From the moment of hatching, the chicks peck at a red spot which is prominent on the tip of the parent's beak, in order to elicit regurgitated food. Through a series of simple but elegant controlled experiments, Tinbergen was able to show that this behaviour is species-specific and requires no learning for its manifestation, the behavioural response to the stimuli being wholly explicable in terms of genetic heritability. (For a full account of this work see Tinbergen & Perdeck, 1950.)

The importance of this approach to animal behaviour was its capacity to observe and explain specific features of behaviour in terms of their being a functional adaptation to some pertinent feature(s) of the natural environment. The instinctive nature of many responses to stimuli indicated that the behaviour was under a degree of genetic control, adding support to the premise that some behaviour at least is the product of evolution by natural selection. This theoretical stance brought together the study of all aspects of living organisms - both morphological and behavioural - and provided the foundations for the science of ethology, from which sociobiology has emerged as a more recent development. There were,

however, certain forms of behaviour which would not fit into the theoretical framework which viewed all behaviour as to some extent the adaptive product of evolution. To appreciate why this was so, it is first necessary to give a brief summary of the theory of evolution by natural selection, since it is this theory which underpins the arguments which I will go on to examine in detail later.

Evolutionary theory states that any population will tend to increase its numbers by reproduction (*ceteris paribus*) until such expansion is checked by the limiting factors of the habitat. At this point, further increase in numbers will result in a rise in the mortality rate. This is due to the effects of overcrowding, such as starvation; increased spread of disease; build-up of toxic wastes; increased competition for necessary resources; changes in relations to predatory species due to increased numbers; etc. However, as no two individuals are likely to be equal in their respective ability to respond to the demands and pressures created by a natural environment, those individuals less suited to the particular demands will tend to suffer and die. This process of 'natural selection' will therefore tend over a number of generations to filter out the less 'fit' or 'able', leaving a higher proportion in the population of those who, by virtue of their abilities, are in relation to the specific environment more fit or able than others. The continued appearance in each generation of unique or novel individuals is assured in sexually reproducing species by the novel recombination of genetic material during formation of the zygote, and in all species by the appearance of random genetic mutations. (It should be noted that 'random' in this context means with respect to pertinent features of the environment, and not 'random' in the sense of being totally arbitrary or incapable of being ascribed to any cause.

Mutations are not causally indetermined - they are merely 'blind' to environmental features and the adaptive needs of the species.) The result of these processes of recombination and mutation is the production of novel genetic types, on which are based the development of individuals with varying abilities which are to some greater or lesser degree gene-dependent and therefore genetically heritable. Over a number of generations the continuous elimination of those individuals less suited to the environment will tend to create a population of individuals whose abilities will show a degree of correspondence or 'adaptation' to pertinent features of the environment. (Just what counts as a 'pertinent feature' will vary from species to species, as well as over time for any particular species.) It is important to note that 'natural selection' as such is an inevitable outcome of an ecological relation between a reproducing population and its habitat. 'Evolution' in the sense of a continuing process of functional adaptation can only occur if the following criteria are fulfilled:

- 1) The existence of a breeding population consisting of individuals with varying abilities relevant to their prospects of survival and reproduction.
- 2) Genetic heritability of at least some of those abilities - i.e. they are non-random with respect to the individual's genotype.
- 3) The existence of competition for resources within populations with a tendency to increase their numbers to the carrying capacity of the environment, resulting in selective pressure and the differential success of individuals within the population due to individual abilities.
- 4) A reasonably stable environment, relative to the life-span and reproductive cycle of the average member of the species.
- 5) A sufficient number of generations for the process of natural selection to have a differential effect on the survival and propagation of individual

types within the population, and hence to change the distribution of traits within the population over time.

If the above criteria are met, then not only will there be selection of individuals within the population, but it will be a consistent selection relative to the species and features of the environment, resulting ultimately in the evolution of a population composed of individuals who show a degree of adaptation relative to the environment. Of course, this is only if the genetic variability in 2) above is such that it includes forms which are 'successfully adaptive' in relation to the given environmental pressures and demands: where such variability is lacking, the result is often extinction, or in some cases migration to a new and less hostile environment where this is possible.

The behaviours mentioned above which did not at first sight appear explicable within the theoretical framework of evolutionary theory include those normally designated by the term 'altruistic'. Altruism, by definition, is behaviour which decreases the altruist's fitness potential whilst increasing that of some other(s). The possibility of such behaviour being anything more than a freak occurrence runs counter to the logic of evolutionary theory. Since altruists would tend in the long run to be less fit than those they helped, or other non-altruists within the population, they would tend to be progressively eliminated from the population over a number of generations. Moreover, any 'genes for' altruism (i.e. a strong genetic basis for the behaviour) would also be eliminated in competition with more 'selfish' genes (i.e. genes which predispose their carriers to more selfish behaviour). It would thus seem impossible for altruism to evolve into anything like a common trait in any species.

Contrary to the theoretical argument from evolutionary principles, observation of many species had revealed time and again widespread displays of altruism within populations, requiring some explanation of its possible evolution and stability. After various proposals, an acceptable solution to the problem was formulated by W D Hamilton (Hamilton,1964). This involved an expansion of the existing concept of 'fitness' and a virtual redefinition in biological terms of the concept of 'altruism'. Hamilton's theory was especially successful in explaining the self-sacrificial extremes of altruistic behaviour displayed by castes within the haplodiploid insects. The incorporation of this aspect of behaviour into a conventional evolutionary framework became central to the development of theories concerning the genetically-based evolution of social behaviour. (For example, the work on social relations between members of a family group carried out by R L Trivers is an extension and detailed application of Hamilton's initial theoretical insight. See for example Trivers,1974 and my discussion of such theories,pp.126-134 below.)

Bringing together and building upon the explanatory perspective of the above-mentioned theories, Wilson carried out an extensive study of invertebrate species behaviour, the results of which were published in 1971 under the title The Insect Societies. In the last chapter of this work (entitled "The Prospect for a Unified Sociobiology") he expressed the hope that the principles applied to the explanation of social behaviour in the insect species could be expanded and applied to behaviour in the vertebrate species. He thus envisioned a general evolutionary science of social behaviour, based on the dogma that behavioural as well as morphological traits are to some extent genetically heritable. Being the product of evolution by natural selection, they could be systematically

observed and explained in functionalist terms of adaptations to the species' natural environment.

The pervasive role of natural selection in shaping all classes of traits in organisms can be fairly called the central dogma of evolutionary biology. (Wilson,1975,pp.21/22).

Thus social as well as other forms of behaviour came to be seen as the expression of specific genotypes, selected for their adaptive value to the species, and explicable in functionalist terms of the relation between the organism's needs and abilities, and the available resources and features of a competitive environment. The way was clear for Wilson and others to apply such theories to every kind of behavioural phenomenon: from the herding instincts of deer on the African plains, to urban violence in the cities of Western Europe.

It is, I think, clear even from such a brief and necessarily selective survey of the historical roots of sociobiology, that as a theory it is not the wayward or politically suspect brainstorm of any one individual. Rather, it is the culmination of progressive theoretical development in many distinct areas of research by leading professional academics. As a theory of behavioural explanation applied to all species but our own, it already commands a great deal of respect, and has resulted in a flourishing growth of speculative research and publication. (For a brief list of such publications, see: Wilson,1983,p189.) The application of sociobiological theory to the study and description of human social behaviour cannot therefore be dismissed as nothing more than a revival of the Spencerian ideology of Social Darwinism (as suggested in Midgley,1983). It is only through a detailed and clear discussion and evaluation of the statements of its supporters that sociobiology may be

correctly and fairly assessed as a potential theory of human social behaviour, and it is just such a task which will be undertaken in the rest of this thesis.

1.2 Qualitative / Quantitative Transformations

One of the first and most obvious problems raised by sociobiology, or for that matter any attempt to incorporate behavioural data within a natural scientific framework, is how to transform the primarily qualitative data of observation into a form which can be subsequently analysed and described using the mathematics-based quantitative models of natural science. This is not a new problem, since it has already been confronted and to some extent solved by those sciences such as psychology and economics, which must perform similar transformations upon informational data. However, even the most avid supporters of such disciplines would recognise the inherent problems, and the divisions of opinion as to the success which such procedures can claim. Wilson himself recognises the problem posed by his own avowed task of trying to "close the famous gap between the two cultures" (Wilson,1978,p.xii), and admits that the reduction of the social sciences to the natural sciences may in practice prove an unattainable goal:

I might easily be wrong - in any particular conclusion, in the grander hopes for the role of the natural sciences, and in the trust gambled on scientific materialism. (ibid,p.xii).

If the 'new synthesis' is to be anything more than an advance in interdisciplinary academic cooperation - an idea already dismissed (see above,p.4) and hardly a matter of great theoretical controversy - it would

seem to depend for its success on the feasibility of reducing behavioural descriptions for methodological reasons to some form analogous to the law-like statements of the natural sciences. Roughly, this means transforming the data of particular behavioural observation statements into general statements of relations between variable terms, all of whose properties must be rendered mathematically quantifiable on some recognised scale of comparison. Furthermore, such formulations must be sufficiently complex and amenable to fine distinctions, so as to ensure that they do not over-simplify, idealise, or in any other way significantly distort the phenomenal data which they are attempting to describe. Such a task may prove relatively easy when the phenomena in question are the behavioural characteristics of inanimate matter such as molecular structures or physical forces. However, precisely-rendered mathematical descriptions may prove less easily achievable when the phenomena in question are a rich and varied range of individual behavioural responses to an equally rich and varied range of environmental stimuli.

An example of just such an attempt, and one that is central to much sociobiological debate, is the description and explanation of altruistic behaviour modelled on Hamilton's theory of Kin Altruism. The behaviour in question is reduced to an expression of two theoretical subjects X and Y, who are denuded of all characteristics save that of their coefficient of genetic relation (e.g. $R=1/8$ for 1st cousins; $R=1/2$ for siblings or parent/offspring, etc.). Plus or minus values are then assigned to the likely pay-off resulting from a given behaviour - a quantification of the resultant 'fitness value' for the protagonist. A calculation and prediction is then made, concerning the relative likelihood of certain forms of

behaviour, and this prediction tested against observational data to see if the theory conforms to and is able to account for field observations.

The reason that Kin Altruism and similar theories form such a major part of sociobiological theory is precisely because they appear to succeed in explaining within relatively simple mathematical formulations important aspects of behaviour common to many different observed species. They thus have the appearance and law-like power of natural science formulae. However, in reality such models are unable to handle anything more complex than an idealised calculation. They propose an idealised protagonist, isolated in a single bipolar behavioural 'choice', all this within an environment whose actual multifarious features are reduced to a single plus or minus value on an imaginary scale of behavioural benefits drawn up by the teller of the story. Given such limitations, it is difficult to see how one could avoid almost any argument about likely behaviour being concocted and proven, merely by assigning appropriate values to the relevant variables. *Vice versa*, it would seem to be a problematic task to represent any actual state of affairs with sufficient accuracy, given such restrictive and limited modelling capacities.

An example of such reasoning occurs in Genes, Mind, and Culture (Lumsden & Wilson, 1981). Lumsden and Wilson propose to offer a sociobiological account and mathematical analysis of the social phenomenon of village fissioning in a South American tribe called the Yanonamo. The phenomenon had already received an anthropological analysis by Napoleon Chagnon (Chagnon, 1976). Lumsden and Wilson's proposal is to incorporate the observations made by Chagnon within a sociobiological framework, and show how the observed practices of village division and new settlement are

explicable by positing the existence of genetically heritable psychological dispositions called 'epigenetic rules', which supposedly guide behavioural decisions in response to certain environmental parameters such as population density (for a fuller explanation of 'epigenetic rules', see below, pp.222-225). Lumsden and Wilson proceed to draw up graphs and derive mathematical formulae concerning "threshold decision logic" (Lumsden & Wilson, 1981, p.160). This logic operates upon the two 'culturgens' (roughly definable as available options of cultural behaviour) of 'remain' and 'depart'. As I pointed out above, such an analysis offers a greatly simplified model of the actual circumstances of the phenomenon under investigation. Individuals are treated as if their decisions were made in isolation from one another, rather than taking into account the possible and likely effect upon an individual of the decisions made by those around them. The choice is simplified to the two possible and contrary outcomes of 'remain' or 'depart', rather than allowing for any intermediate stages of indecision, or further possible factors which might come into and affect the decision-making process. The only environmental factor considered relevant to the calculation of likely choice is that of overall population density, rather than allowing for more personal areas of motivation, such as family or broader social relations and stresses; the possibility of 'a new start'; the potential for individual betterment through higher prestige attainment within a new and smaller group; the differences made to individual choice by virtue of age, sex, marital and power status within the existing group. These social factors are left out of the mathematical calculation made by the authors, who apparently excuse this aspect of their work, and deny its significance to the accuracy of their models:

Although vastly oversimplified, the gene-culture translation models in the two-cultorgen Markov decision approximation account for significant

features of the village fissioning process.
(ibid,p.167).

In a severe attack on this aspect of sociobiological reasoning, Kitcher (Kitcher,1985) comments on the same example from Lumsden and Wilson's work, but comes to a conclusion very different from the authors' own:

In sum, we have an implausible solution for a problem about the social expression of human preferences, in a case where there are no detailed results that could be used to distinguish the solution from the most elementary qualitative analysis of the situation.
(ibid,p.371).

After a further analysis of the methods and results of Lumsden and Wilson's work, Kitcher once again offers scathing comments upon the adequacy of this aspect of sociobiological explanation, particularly when it is regarded as a potential substitute and improvement over existing techniques of 'qualitative' analysis already in employment within the social sciences:

In the first and last examples, the unnecessary mathematical apparatus actually stands in the way of offering better analyses of the situation. In the second example, Lumsden and Wilson manage only to provide a conclusion that is indistinguishable from results that we can reach by qualitative argument, given the data available. (ibid,p.376).

In conclusion, Kitcher offers a final blow to what he caricatures in his chapter title as "The Emperor's New Equations", and to the attempt of sociobiologists to offer an account of social phenomena within the language and models of quantitative mathematical formulae:

Genes, Mind, and Culture is an extreme example of a certain type of work. Complex mathematics is employed to cover up very simple - often simplistic - ideas... What is irritating, and occasionally amusing, about these uses of mathematics is that they serve to disguise the poverty of thought. (ibid,p.393).

The same problem with simplification of actual detail, in order to reduce the facts to a state in which they can be rendered within mathematical models, occurs in the work of Richard Dawkins (Dawkins,1978). In a discussion of Kin Selection theories, and their application to explaining human and other species' behaviour, Dawkins admits to the need for simplification of models. However, he does not appear to think that such a procedure is in danger of rendering any calculable results invalid, by virtue of their disregard for what may prove to be crucial factors affecting behaviour. On the contrary, Dawkins thinks that much of the decision-making process is both governed by much simpler rules than we normally envisage, and that such calculations are undertaken in a way which has very little to do with many consciously perceived factors of the environment.

So far, I have over-simplified somewhat, and it is now time to introduce some qualifications....Obviously, in real life, animals cannot be expected to count exactly how many relatives they are saving, nor to perform Hamilton's calculations in their heads even if they had some way of knowing exactly who their brothers and cousins were....Just as we may use a slide rule without appreciating that we are, in effect, using logarithms, so an animal may be pre-programmed in such a way that it behaves as if it had made a complicated calculation. (ibid,pp.102/103).

The question of explaining human behaviour according to the Kin Selection theories of Hamilton and Trivers will be dealt with in detail later (see below,pp.111-134). However, a brief excursion into the mathematical formulations of sociobiology reveals that there are many problems which, in the view of myself and at least some other commentators, are far from solved within existing sociobiological theory. (See also Alper & Lange,1981 on this topic.) The behaviour of Maynard Smith's 'Hawks and Doves' is open

to exact mathematical formulation and prediction precisely because they have been ideally created for such a purpose, possessing only mathematically formulable attributes. In this respect, they bear only a superficial similarity to the natural species which share their name. Nor are they to be confused with any other species for whom they supposedly stand as behavioural models, but which in fact behave in ways far more complex and within an ever-changing natural environment.

The success of much of sociobiology's proposed transformation of the existing social scientific description and explanation of behaviour would therefore seem to depend on the as-yet unresolved problems of devising methods for translating qualitative properties into quantitative variables. These must then be related within formulae which do not simplify and abstract out all the relevant information present in the original observations, since such a process achieves success at the expense of realistic description (see, for example, my discussion of the sociobiological treatment of human homosexuality and incest, pp.134-150 below). This problem, which is a general one for any kind of behavioural description, is made even more acute in the specific case of human behavioural description, by the above said desire to "explain ourselves in these mechanistic terms" - a methodological stance which runs counter to the view of human behaviour and its necessary explanation held by many practising social scientists and philosophers. It is to this topic that the discussion will therefore turn in the following section.

1.3 Behavioural Explanation

Sociobiology adopts a theoretical stance towards behavioural explanation whose origin I have attributed to researchers such as Lorenz. That is, behaviour is regarded - like morphological traits - as the functionally adaptive product of a process of evolution by natural selection. The 'central dogma' of sociobiology thus justifies the observer in regarding behaviour not merely as related to proximal effects of the immediate environment affecting the organism's responses, but also as related to distal or ultimate effects which have shaped the organism's phylogenetic (i.e. species-ancestral) development. From such a theoretical standpoint, phylogeny and ontogeny become inseparably related elements of any complete behavioural explanation. It is the traditional lack of concern for the phylogenetic aspect of behavioural development and subsequent explanation within the existing social science models which sociobiology attacks and seeks to redress.

One of the prime areas of controversy in the sociobiology debate concerns the relative emphasis given to proximate and distal factors when offering an explanation of any particular behavioural phenomenon. In the opening pages of his book, Wilson emphasises how these two elements operate on very different time scales, and yet remain inseparably linked in the overall process of individual development.

How is ultimate causation linked to proximate causation? Ultimate causation consists of the necessities created by the environment....The species responds to environmental exigencies by genetic evolution through natural selection, inadvertently shaping the anatomy, physiology, and behaviour of the individual organisms....These prime movers of evolution are the ultimate biological causes, but they operate only over long spans of time. The anatomical,

physiological, and behavioural machinery they create constitutes the proximate causation of the functional biologist. Operating within the lifetimes of organisms, and sometimes even within milliseconds, this machinery carries out the commands of the genes on a time scale so remote from that of ultimate causation that the two processes sometimes seem to be wholly decoupled. (Wilson,1975,p.16)

Although Wilson is ostensibly pointing to the difference in time-scale between the effects on an organism of distal and proximate factors respectively, I believe that the language he uses is very revealing of his general theoretical stance in this matter. He talks of the distal causes as the 'prime movers' of behaviour. Proximate causation is seen as an 'activation' of the 'machinery' embodied in the organism's physiological and behavioural structures. Through this, the 'commands of the genes' are carried out. I do not think it unfair to suggest that Wilson's language betrays a definite bias in his view as to the relative operational and consequent explanatory role of the two elements of behavioural development. It would appear that the reactions of the organism to proximate factors of the environment are almost of a passive nature; in the sense that they are a mere playing out of dispositions and structural capacities laid down during the phylogenetic ancestry of the species. Though I might agree with Wilson that previous behavioural explanatory models have tended to ignore the phylogenetic aspect of behaviour, I fear that his determination to redress this balance carries him too far in the opposite direction of emphasis. The result is a concentration on aspects of ultimate causation, which suggests to the reader that this is by far the more significant factor in a proper assessment of behavioural causes.

This differential emphasis on the factors involved in behavioural causation and explanation is even more prominent in the chosen vocabulary

of Richard Dawkins. In Dawkins' account of the evolution of life on this planet, he begins with the now-familiar vision of the simplest proto-organisms replicating mitotically in the primaeval soup. As their environment changed, they adapted by becoming more complex. We are told that they "built survival machines for themselves to live in." (Dawkins,1978,p.21). Millions of years of adaptive evolution by natural selection has placed the descendants of these earliest replicators "safe inside gigantic lumbering robots, sealed off from the outside world, communicating with it by tortuous indirect routes, manipulating it by remote control....Now they go by the name of genes, and we are their survival machines." (ibid,p.21).

Dawkins' choice of language here - albeit colourful - is hardly neutral. Once again, the images are of machinery and robots under remote control. More than that, the machines appear rather lifeless or stupid, in comparison with the genes which inhabit and manipulate their movements to their own ends. The overall picture is one of behaviour which is to be understood in terms of the ultimate ends of the genes and therefore of the phylogenetic ancestry, rather than in terms of more immediate relations between the organism and the surrounding environment. Such an approach to behavioural explanation tends to play down the fact that a balanced behavioural explanation must include both elements, but that the relative weighting assigned to each element will vary from one case to another, according to the behaviour in question and the complexity of the organism's responsive capacities. With respect to a particular genotype, therefore, and depending on the particular behavioural trait in question, the organism may display a greater or lesser degree of 'flexibility' in its development and response to environmental features. All that can be

properly said of any given genotype is that *it delimits a possible range of phenotypic development*. This point was made by the eminent biologist Theodosius Dobzhansky in a review of another work on evolution.

Genes determine not "constant characters" but the norm of reaction of the developing organism in different environments. Grasse ignores the interactions of different genes and environments and also interactions of different genes.
(Dobzhansky, 1975)

The tendency of sociobiological explanations of behaviour to ignore or underplay the role of proximal factors affecting ontogenetic development and the complexity of the genotype/phenotype relation, is nowhere more controversial than in the application of sociobiology to explanation of behaviour in our own species. The ability of the human organism to respond to a wide range of immediate environmental factors, affecting morphological as well as behavioural development, means that in *Homo Sapiens* more than in any other observed species, the relation of genotype to phenotype is 'open'. Any account of behaviour which is couched solely in terms of proximate factors of the immediate environment may well satisfy many kinds of questions, but it is not a complete account in the eyes of the behavioural scientist. Likewise, however, an account which emphasises the ultimate factors and effects on the phylogenetic developmental history of an individual, to the detriment or even total exclusion of proximate factors, is equally inaccurate, since it suggests that the relation of genetically heritable structures and the ongoing relation of the organism to factors of the immediate environment is closed rather than open. This much-criticised aspect of sociobiological theorising is played down as being an inconsequential question of degree or emphasis in a book by Tennant and von Schilcher:

The differences between open and closed programmes are basically only quantitative. They stem from varying contributions from the genetic and environmental sources respectively to the total information required for the development of a trait. (Tennant & von Schilcher,1984,p.36).

What Tennant and von Schilcher regard as *only* a quantitative difference within the process of individual development, however, can make *all* the difference when it comes to constructing and implementing a framework for explanation of the various forms and stages of that organism's development. Such 'basically only quantitative' differences can, as the same authors apparently go on to recognise, result in vastly different potential for behavioural development, and the assimilation and subsequent need for explanatory inclusion of immediate environmental factors in any proper account of behaviour. In short, the more open to developmental change due to immediate environmental factors the organism is, the more a complete account of behaviour will have to take such factors into consideration, rather than concentrating on known or inferred elements in the organism's phylogenetic history. This relative dependence upon a continued interaction with the environment for the subsequent development of individual behavioural traits is, despite the previous quote, in fact recognised by the same authors quoted above:

The decisive difference, as Lorenz saw clearly, is that *Drosophilas*, cicadas and crickets brought up in isolation will sing their ancestral roles perfectly, whereas a Kaspar Hauser prima donna would be somewhat disappointing in her debut as Madame Butterfly. (ibid,p.55).

Though the general aim of sociobiology may have some substance in its criticism of the shortcomings of many traditional behavioural explanatory models, I would argue that in the writings of sociobiologists, there is

often a tendency to commit an equal though opposite crime of over-emphasis. This may well lead to the reader gaining the image that much if not the vast majority of all animal behaviours can be explained in terms of genetically heritable traits, which have been laid down over the course of the species' phylogenetic history, and which override any more immediate factors of proximate environmental influence. Such a criticism of the general theoretical and subsequent methodological stance of sociobiology is, in my opinion, very damaging to the project, and will appear in many of the later sections of this thesis.

The above criticism is of an area of theorising which could perhaps be improved upon, if the writers concerned would present a more 'balanced' view of the issues involved. However, there are elements of sociobiological reasoning which do not appear to be so open to possible resolution and the satisfaction of critics. One of these is the already-mentioned concern of sociobiology to couch all behavioural explanations within some form of 'mechanistic' framework (see above, p.18). Such a theoretical and methodological stance would seem to make sociobiology a natural ally of the Behaviourist school of psychology. The methodology of Behaviourism, advocated most notably by B F Skinner, attempts to formulate all behavioural explanations within a neutral language of third-person observation of overt action in response to some identified stimulus. The logic behind such a procedure is to ensure as much as possible a maintenance of scientific objectivity on the part of the observer. Any reference to 'internal' psychological states of the subject under examination are held to be invalid or meaningless, since they are taken to be a matter of conjecture or inference, not accessible to direct verification by observation, which is the basis of methodology in other

areas of the natural sciences. In opposition to this, there are those who insist that in the case of at least some instances of behaviour (normally within our own species) explanations can only be given if observed behaviour is related to inferred psychological states of the subject, these states providing the necessary explanatory link between the given stimuli of the environment, and the production of the behavioural response in question. Such 'mental-dependent' explanations of behaviour are the norm rather than the exception within such areas as Philosophy of Action, Ethics and the social sciences. The strictures of a behaviourist methodology are seen to place impossible and radical demands upon the traditional explanations of human behaviour, couched in terms of motives, intentions, feelings, reasons and other such 'internal' states of the subject. As one defender of the non-mechanistic, non-Behaviourist school of human behavioural explanation has put it: "to see something as an action is to see it as something done with a purpose." (Wilkes,1978,p.21. For further philosophical literature on the same point, see for example: Peters,1969; Winch,1958; Melden,1961). The difference of opinion concerning the underlying theory and resulting explanatory terminology of human behaviour is one of the major issues within the Philosophy of Science and the Philosophy of Mind. It is not clear, however, from the writings of the sociobiologists that they always appreciate the complexity which their position involves, and the nature of the conceptual issues inevitably raised. For example, Wilson often uses the language of intentional behavioural descriptions with reference to internal mental states of the subject when explaining human behaviour. Such psychological terminology would appear to occupy only a temporary place within sociobiological theory, however, since one of the further predicted transformations of the social sciences consists in the systematic translation of existing 'folk

psychological' terminology into the presumably more accurate and objective terminology of neurological science, and the description of brain states and events occurring within a fully-mapped brain structure. Such a move is seen by Wilson as the essential step toward understanding and ultimately coming to control the present and possible future state of our own social existence:

The transition from purely phenomenological to fundamental theory in sociology must await a full neuronal explanation of the human brain. Only when the machinery can be torn down on paper at the level of the cell and put together again will the properties of emotion and ethical judgement come clear....With our present inadequate understanding of the human brain, we do not know how many of the most valued qualities are linked genetically to more obsolete, destructive ones. (Wilson,1975,p.571).

The projected move which Wilson envisages and regards as a necessary element of his project - from the phenomenological observations and terminology of 'folk psychology' to the objectively verifiable observations of the new 'neuropsychology' - is very much in its infancy and a topic of heated dispute amongst practitioners and theorists within the relevant disciplines. Within philosophical circles, the level of debate on this very topic has recently been raised by the production of a series of articles and a book by Patricia Churchland, which argue against the views of writers such as Wilkes or Melden, and support the same transformational programme which Wilson envisages (See: Churchland,P. S.,1986 & 1980 for example). Attempting to bring these two opposing parties together are writers such as Donald Davidson, who argues that explanations in terms of reasons or other psychological states are compatible with talk of behavioural 'causes' (Davidson,1963). Without wishing to enter into a full discussion of the issues involved on both sides of this debate, I think it

still relevant to point to some general aspects of the present situation which raise doubts as to the feasibility of the sociobiological position.

Firstly, the equation or reduction of mental phenomena to neurophysiological states does not yet exist in practice, and it is far from clear (contrary to Wilson's apparent view of the matter) that such a theoretical move could be made at all. Secondly, even if such a 'new psychology' were to emerge from the wedding of existing psychology and neural science, it is not clear that it would necessarily replace altogether existing explanations of behaviour, couched in terms of the subject's intentions, reasons for or feelings towards doing something. These are arguably just as well-qualified candidates for inclusion in behavioural explanations as anything which may be subsequently identified as occurring in the subject's brain. Thirdly (as I will argue more expansively in the final chapter), it is not clear that one can relate behaviour to the physical states and events occurring within a subject's brain and CNS, without explaining at least some of these states and events by further reference to consciously perceived psychological phenomena, and the intentional aspect such phenomena have for the subject. In other words, psycho-physical reduction may not necessarily eradicate all references to psychological states, because of the relations of meaning such states have for the subject by virtue of their phenomenal properties.

The above arguments are in obvious need of further elaboration, but they do begin to point out some of the ways in which the sociobiological project might overlook some of the theoretical problems it raises, concerning the relation of the natural and the social sciences. It is in just such areas of discussion that the sociobiological commitment to a

thoroughgoing mechanistic scheme of behavioural explanation - leaving no 'nomological danglers' as it were - leads to an over-ambitious and over-simplified vision of the problems entailed and the likely path to their resolution. Having cast doubt upon the practicality (or even desirability) of a post-Skinnerian psychological methodology, and lacking as yet anything like the promised neuroscience which will replace existing psychology, it would appear that for the present at least, sociobiology's proposed objectification of existing social science methodology and terminology is without an alternative framework in which to express its observations. Until such time as a framework is found and shown to be an improvement over existing methods, Wilson's promised transformations would appear to be held back for practical and possibly more fundamental theoretical reasons.

1.4 Metaphor

A criticism levelled at much of the sociobiological writing which is more stylistic than methodological is the abundant use of metaphorical language. This is in itself no cause for criticism, but it is an aspect of writing which is open to abuse, and can lead to confusion and the reader being misled - intentionally or otherwise. The point was highlighted by the philosopher Mary Midgley in a paper aimed at the work of Richard Dawkins. Midgley comments:

Foremost among the snags of this sociobiological language is the equivocal use of words like 'selfish', 'altruistic', 'spite', and 'manipulate', a use which not only suggests psychological egoism to the surrounding peasants, but clearly at times misleads the writers themselves. (Midgley,1983,p.368).

Midgley's criticism is concerned with Dawkins' practice of using certain words or expressions in a 'biological' sense, in order to explain more easily or graphically some aspect of the non-intentional, unconscious mechanisms of evolutionary genetics. This would be permissible, if he did not then go on to use the same words or expressions in their normal context, without indicating that there has occurred a significant change of meaning between the two uses. The following quotation from Dawkins exemplifies the kind of invalid reasoning which may result from a careless or misleading use of certain words.

Like successful Chicago gangsters, our genes have survived, in some cases for millions of years, in a highly competitive world. This entitles us to expect certain qualities in our genes. I shall argue that a predominant quality to be expected in a successful gene is ruthless selfishness. *This gene selfishness will usually give rise to selfishness in individual behaviour...* My own feeling is that a human society based simply on the gene's law of universal ruthless selfishness would be a very nasty society in which to live. Let us try to teach generosity and altruism, *because we are born selfish.* (Dawkins, 1978, pp. 2/3 - emphasis added).

No sooner are the words 'selfish' and 'selfishness' stripped of their normal connotations of intentional behaviour on the part of a conscious agent (if this can be done), than they reappear within the same quote in the context of their normal meaning. Dawkins seems to suggest by this that one can draw a direct implication from the unconsciously 'selfish' mechanisms of the genes, to either conscious or unconscious selfish behaviour by individuals. The implication made explicitly here and implicitly throughout the rest of Dawkins' work is that 'selfish genes = selfish individuals'. It is no wonder that Midgley feels impelled to criticise such linguistic conjuring so strongly. The obvious fallaciousness of the above argument is apparently realised in later passages, where

Dawkins argues that on occasions the best strategy for a selfish gene may well be to live inside an altruistic, cooperative individual. Even if his reasoning concerning the evolutionary strategies of genes is correct, one cannot go on to draw direct inferences about human behaviour in the way his language above suggests. Dawkins' use of such key words and phrases throughout his work builds up the impression that there exists a simple continuity of behavioural properties from the genes to the individual organism, and in the human case to society as a whole. Any apparent examples of altruism are therefore denied as being *a priori* impossible, and in the light of this initial judgement reinterpreted in some way that permits them to be incorporated into the 'selfish behaviour' explanatory framework. (Just how this is done will become clearer in the sections dealing with Kin and Reciprocal Altruism. See below, pp.111-126 & 151-167.) The metaphorical use of certain words and expressions is therefore a dangerous and misleading tool, since it can give the reader the impression of continuity of some form or other, where there is in fact no continuity, or at least not of the simple form suggested by the continuity of language used in the description.

A further example of confusion caused by linguistic misuse is Dawkins' use of the word 'deceit' in his description of some adaptations which occur in predator/prey relationships. Dawkins introduces a special use of the word by way of an anecdote which would appear to be aimed at just the philosophical concerns expressed by Midgley. It is necessary to quote Dawkins at some length:

The notion of an animal telling a lie is open to misunderstanding, so I must try to forestall this. I remember attending a lecture given by Beatrice and Allen Gardner about their famous 'talking' chimpanzee Washoe....There were some philosophers in the audience, and in the discussion after the lecture they

were much exercised by the question of whether Washoe could tell a lie. I suspected that the Gardners thought there were more interesting things to talk about, and I agreed with them. In this book I am using words like 'deceive' and 'lie' *in a much more straightforward sense than those philosophers*. They were interested in conscious intention to deceive. (ibid,p.68 - emphasis added)

It is interesting to note that Dawkins suggests his own use of the terms to be more 'straightforward' - thereby imputing any perversion of the normal meaning of the terms to 'those philosophers' busy with their annoying and irrelevant hecklings! I would argue, however, that it is Dawkins who is using terms in a non-normal sense, and that 'deceive' is normally understood to contain a sense of conscious intention. To confuse the situation further, Dawkins goes on to use the same terms to explain the evolution of 'deceptive' mimickry patterns in butterflies, the false bait of the Angler fish, and the seductive sexual displays of Bee Orchids. Then, with no pause or indication of changed meaning he speaks of the clearly conscious and intentional behaviour of human beings:

As we shall see, we must even expect that children will deceive their parents, that husbands will cheat on wives, and that brother will lie to brother. (ibid,p70)

It could be argued that Dawkins regards such cases of deceit as non-intentional, since the behaviours are thought to be under the influence of genes controlling unconscious mechanisms, but such an argument would be irrelevant since this view has been neither fully explained nor supported at this point of his work. What is masked by an ambiguous use of a single term to cover two different kinds of phenomena is precisely the difference between them which would normally demand two sets of descriptive vocabulary to capture their features. The butterfly 'deceives' its predators by the evolution of certain markings on its wings. This is a non-

intentional product of adaptive forces over which the butterfly has no control whatsoever, resulting in a capacity for misinformation by a process of chance mutational changes and their natural selection. In the case of human individuals lying and cheating, it is the intentional practice of misinformation brought about not by chance, but by conscious decisions to behave in certain ways with a view to the future consequences of such behaviour. The brother's decision to lie to his brother is a conscious choice; the butterfly's 'deceit' of its predator does not involve any kind of choice at all. The fact that Dawkins himself says he finds such distinctions 'uninteresting' may explain his apparent reluctance or inability to observe and reflect such distinctions in his choice of language. Unfortunately for Dawkins, a lack of interest in something does not constitute a refutation of that thing's existence, or grounds for ignoring the linguistic conventions upon which others rely for drawing distinctions.

Apart from such specific misuses of language, there is a more general objection to the prevalence of metaphorical and analogical language to be found in sociobiological writings. If the proponents of sociobiology are indeed claiming to bring existing methods of behavioural explanation closer to the practices of the natural sciences, then they must support their arguments with the combination of empirical evidence and theory found in other scientific disciplines. This is not to say that the use of metaphor is outlawed, since in many cases it may help to convey complex notions in a more visualisable fashion. But it should not be expected to form a large part of the argument in support of the overall thesis. In reply to the charge of excessive use of metaphor, Dawkins defended his position:

the lines he quotes were not intended as metaphor at all. With certain qualifications, I think of these statements as literal truth....Similarly "survival machines" and "robot vehicles" economically compress several important ideas....I say again, these are not metaphors. An animal is a robot vehicle, and DNA does work in mysterious ways. I of course use true metaphors as well - the very title of my book is one - but I hope this is in all cases either obvious or carefully explained in the text. (Dawkins, Fox & Greene,1976,pp.709/710)

In the light of such explanations, I find it very difficult to apply the categories of 'metaphorical' and 'literal' use at all to Dawkins' language. One even begins to wonder if 'true metaphors' are to be distinguished in some way from metaphors simpliciter. I would suggest that such confusing use of linguistic conventions is liable to mislead the reader; for example by playing down the distinction normally made between intentional and non-intentional kinds of behaviour. This reinforces Dawkins' general thesis that behaviour in the human case as well owes more to the blind and unconscious machinations of insensible particles of DNA than to the conscious deliberations of individuals in society.

A final example of altered linguistic use may be found in the sociobiological explanation of communication. It is defined by Wilson as:

action on the part of one organism (or cell) that alters the probability pattern of behaviour in another organism (or cell) in an adaptive fashion. (Wilson,1975,p.9)

Dawkins' definition is similar:

A survival machine may be said to have communicated with another when it influences its behaviour or the state of its nervous system....A great number of survival machines promote their genes' welfare indirectly by influencing the behaviour of other survival machines. (Dawkins,1978,p.67)

The picture once again is of an aspect of social behaviour which is seen wholly in terms of the selfishly advantageous manipulation of others via some aspect of interaction. Wilson goes on to say that his definition "conforms both well to our intuitive understanding of communication and to the procedure by which the process is mathematically analyzed." (Wilson,1975,p.9). Though I may agree with the latter half of his statement, I must disagree with the former. Both authors define and explain communication solely in terms of the advantage it confers on individual genes, and since this is the special dogma of sociobiology rather than an everyday appreciation of social intercourse, I consider it wrong to suggest that it forms a part of the lay person's 'intuitive understanding of communication'.

Communication, according to the sociobiologists, is a form of gene-directed behaviour geared to increasing the individual's capacity to obtain an optimum share of available resources via the advantageous manipulation of others. (This applies equally to communication amongst conspecifics as part of their range of social behaviour, as it does to members of different species engaged in competition for resources, or in predator/prey relationships such as the example of Angler fish above.) The same general definition and functional analysis of communication is supposed to hold for human society as well. For example, the relative prevalence of honesty and deceit displayed by individuals or society as a whole would not be regarded by sociobiologists as a reflection of social factors of morality and other forms of social conditioning upon individuals. Rather, such social factors would in turn be explained by reference to the need for individuals to compete within a predominantly selfish environment, thus relating all communication ultimately to the model of individuals as

necessarily involved in selfish competitive struggle with each other. This is clearly stated in Wilson's analysis of what he takes to be the general and necessary components of human social communication:

Deception and hypocrisy are neither absolute evils that virtuous men suppress to a minimum level nor residual animal traits waiting to be erased by further social evolution. They are very human devices for conducting the complex daily business of social life. The level in each particular society may represent a compromise that reflects the size and complexity of the society. If the level is too low, others will seize the advantage and win. If it is too high, ostracism will result. Complete honesty on all sides is not the answer. (ibid,p.552)

Once again, a key feature of human behaviour - in this case communication - is virtually redefined in its portrayal, by its link to the underlying dogma of sociobiology that all aspects of social behaviour are ultimately geared to the selfish advantage of the genes (and possibly that of the individual protagonist as well). From a consideration of the high premium paid to successful manipulation of others by skillful communication, aspects of the high-level structures regulating human social behaviour are explained in terms of the adaptive advantages supposedly operating at the level of selfish genes, with the result that the image of behaviour as 'selfish' permeates all levels and aspects of behaviour, from the gene to the individual and the social group as a whole.

To summarise, there is a tendency in sociobiological writing to emphasise certain supposed features of behaviour by the use of metaphorical and other persuasive linguistic devices. Though this may in some cases be excused by the need to introduce the reader to novel concepts and images, it is a practice open to abuse, with the result that important distinctions normally made become overlooked, and aspects of

behaviour imputed to one level of description become invalidly generalised, colouring one's appreciation of some other level of description. Such misleading factors have done much to arouse anger on the part of sociobiology's critics, and to detract from whatever truth there might be in the sociobiological project.

1.5 Cross-Species and Cross-Cultures

Much of the empirical evidence offered in support of the sociobiological thesis comes from the relative comparison of the behaviour of different species. In the case of humans, this cross-species data is augmented by cross-cultural comparisons, to assess which aspects of human social behaviour show evidence of being universal to the species. For example, species might be compared in respect of their communication capacities, to see which sensory channels (chemical, visual, auditory, tactile) are being utilised and in what ways. The point of such comparisons is both to shed light on the particular behaviour under examination, and to see if the observed behaviours can be correlated with other aspects of the organisms in order to construct a phylogenetic tree of the given behaviour's ancestry. The logic of such 'phylogenetic analysis' in the case of our own species is given by Wilson:

By comparing man with other primate species, it might be possible to identify basic primate traits that lie beneath the surface and help to determine the configuration of man's higher social behaviour.
(Wilson, 1975, p. 551)

The behaviours which Wilson believes to be homologous between our own and other species of primate are:

aggressive dominance systems, with males generally dominant over females; scaling in the intensity of responses, especially during aggressive interactions; intensive and prolonged maternal care, with a pronounced degree of socialization in the young; and matrilineal social organization. (ibid,p.551)

Despite the frequent occurrence in sociobiological literature of such behavioural comparisons, Wilson goes on to admit that the method of cross-species comparison is far from foolproof:

The possibility remains that some labile traits are homologous between man and, say, chimpanzee. And, conversely, some traits conservative throughout the rest of the primates might nevertheless have changed during the origin of man...Finally, it is worth special note that the comparative ethological approach does not in any way predict man's unique traits. (ibid,p.551)

In the light of such an admission, one might think that the method of cross-species comparisons has little in its favour - at least as far as gaining insight as to our own species' behavioural origins is concerned. As mentioned above, however, it is one of the key elements of sociobiological methodology. For example, in his work On Aggression Konrad Lorenz offers an analysis of human aggressive behaviour which predates but serves as a theoretical precursor to sociobiology's own pronouncements on the same subject.

human social behaviour, far from being determined by reason and cultural tradition alone, is still subject to all the laws prevailing in all phylogenetically adapted instinctive behaviour. Of these laws we possess a fair amount of knowledge from studying the instincts of animals. (Lorenz,1966,p.204)

The method does raise some problems, however. Whereas a limb or an organ such as an eye recognisably perform the same general function in many species, and can be traced back across species through a history of

phylogenetic development, it is not *prima facie* obvious that the same can be done in the case of behavioural traits. Though it may make sense to identify and analyse the function of some morphological feature of an organism in abstraction from other features, it does not seem to be so simple in the case of behaviour. Is the 'aggressive territoriality' displayed in Lorenz's fighting fish experiments really comparable with aggressive behaviours in other species, such as birds, apes and even humans, in the same way that all of these species can be said to possess some form of morphologically and functionally comparable form of visual sensory organ? Lorenz and others clearly think that such comparisons, at least in the case of some species, are both fair and revealing of the historical origins and the functional purpose of behavioural traits. This is underlined when he states that an extraterrestrial observer would conclude that:

man's social organization is very similar to that of rats which, like humans, are social and peaceful beings within their clans, but veritable devils towards all fellow-members of their species not belonging to their own community. (ibid,p.204)

Wilson is a little more cautious, but equally supportive of the method of cross-species comparisons, stating that "The correct approach using comparative ethology is to base a rigorous phylogeny of closely related species on many biological traits." (Wilson,1975,p.551). Despite this note of caution, sociobiological literature in general continues to be replete with cross-species comparisons of humans with every aspect of primate and non-primate species' behaviour. The overall aim is to convince the reader that much of what we take for distinctively 'human' behaviour, ascribable to elements of our rationality and unique social nature, is in fact a phylogenetic inheritance of evolutionarily tried and tested behavioural

strategies, held in common with many other species. Such a view must be open to question, however, since it can also be argued (as the quote from Wilson himself above suggests) that any particular behaviour is the function of both the unique physiological attributes of the species in question, and the environmental features obtaining at the time. If behaviour is the product of a complex nexus of forces operating on individuals who in some cases - because of their capacity for learning - are unique and distinct from one another in their responsive capacities, then the question of meaningful ethological comparison becomes far more debatable than that of comparative morphology. In the case of human behaviour one has to add to the level of 'instinctive drives' which we supposedly share with many other species the effects of self-conscious thought, linguistic and other symbolic forms of conceptual reasoning, and the influence through imitation and conditioning of specific social and general environmental factors impinging on the individual. Of course, insistence on such factors and their irreducibility to 'drives' or 'biological functions' is precisely the point of difference between the social scientists and the sociobiologists. However, I feel that the onus is on the sociobiologists to prove that such factors are either irrelevant to assessment of human behaviour, or else are reducible in the way suggested to 'biological' factors, and thus explicable within a framework which makes no reference to the categories and models of traditional social science explanation.

The method of cross-species analysis of behaviour is only one of the techniques proposed by sociobiologists as a means to assessing the likely heritable component of some behavioural trait. The other main avenue is that of comparative anthropology: the comparison of individual and

collective behavioural traits across different geographical and historical human cultural groups. Of particular interest to sociobiologists is evidence of the lifestyle of our distant ancestors. The argument is that any similarities which can be identified between our own cultures and those of our Pleistocene forefathers - since they persist in spite of environmental, technological and every kind of cultural difference - are strong candidates for behaviours which are under a high degree of genetic control (i.e. 'closed' as opposed to 'open' behavioural patterns). That is, they are passed on through genetic inheritance mechanisms, and remain relatively unaffected by factors of environmental conditions and the effects on individual development of learning. Underlying this method, however, is a further assumption from evolutionary theory, which is that human culture is a relative newcomer on the evolutionary scene, and that we owe most of our behavioural patterns to the time preceding culture, when behaviours were being selected for through natural selection of genes.

We can be fairly certain that most of the genetic evolution of human social behaviour occurred over the five million years prior to civilization, when the species consisted of sparse, relatively immobile populations of hunter-gatherers. On the other hand, by far the greater part of cultural evolution has occurred since the origin of agriculture and cities approximately 10,000 years ago. Although genetic evolution of some kind continued during this latter, historical sprint, *it cannot have fashioned more than a tiny fraction of the traits of human nature.* (Wilson,1978,p.35 - emphasis added)

From the above quote I believe that the underlying assumption already pointed out becomes clear. What the sociobiologist does is to begin the whole project by assuming that most of 'human nature' is in fact fixed by genetic inheritance, and then go on to deduce that this process of fixing by natural selection must necessarily have occurred during the era of our

species' history prior to the rise of civilisation, which is a relatively recent event. It may well be true that 'most of the genetic evolution' of the species occurred prior to the rise of civilisation. What does not necessarily follow from this is that the *social* nature of humans was fixed during this period, or is fixed in such a way at any period. What the sociobiological viewpoint leaves out of the picture is the alternative and, in the absence of further argument or evidence, equally viable viewpoint that sees human social behaviour as something far more flexible, and dependent not on genetic factors of inheritance, but rather on social learning and conditioning processes inexplicable except by reference to prevalent social factors.

It is this underlying assumption of the sociobiological project which explains the amount of effort expended on examining and discussing the social life of extant hunter-gatherer societies. Unable to infer anything more than a very rudimentary and fragmentary picture of the lives of our ancient ancestors from archaeological evidence, the prime focus for debate has become the few remaining hunter-gatherer groups, whose lifestyle has remained as yet untouched by contemporary influences of social and technological life. As Wilson says:

The best procedure to follow...is to extrapolate
backward from living hunter-gatherer societies.
(Wilson,1975,p.565)

Such societies stand at a pivotal point for ethological researchers: they share the same place as Western technological society on the scale of biological evolution, and yet they represent a form of cultural organisation which marks the starting point of the cultural history of modern technological society - a point from which such societies departed

some 10,000 years ago. Any cultural traits which the two forms of society are found to share in common are therefore taken to serve some useful and essential function in human society, regardless of specific environmental factors. They are taken to be 'human cultural universals'. Once again, however, this fact is taken as proof of something further, when assessed from the sociobiological standpoint. It is seen as providing evidence for the existence of genetically heritable biopsychological drives towards certain kinds of social behaviour. This is in my view another example of a false deduction from the existing evidence.

It may be true that there are discernable factors of social organisation which are universal or nearly so in our species, but the reason for this does not necessarily have to be a 'biological' one in the sense intended by sociobiologists. It may be that the culturally universal traits constitute the best, easiest, most economical, manageable or obvious solution to a given organisational problem, and are therefore arrived at over a period of time by all societies who face such a problem. Some aspects of life such as gathering of resources; protection from hostile forces and environmental dangers; distribution of goods and tasks; care of the young and helpless; discipline and allegiance to the group as a whole, etc. are factors intrinsic to every social group, and demand resolution. It is hardly surprising, in my opinion, that similar groups find similar forms of coping with such problems, and that dissimilar groups still have solutions which are recognisably based on the same principles. Such basic problems, in the absence of a very generous environment and a wholly heterogeneous range of individual skills and capacities (by which I mean to the extent of being based upon a heterogeneous range of morphological features) are bound to exert forces and create demands which only admit of

a fairly restricted range of solutions in the form of group behavioural organisation.

What does not follow - as sociobiology insists - is that similar social organisation necessarily constitutes evidence for the operation in individuals of 'biological' mechanisms (i.e. genetically heritable) affecting rational decisions and ultimate social behavioural organisation. To give an absurd example, you don't have to be 'genetically wired for bridge-building behaviour' in order to adopt bridge-building as a solution to the problem posed by impassable ravines in one's path. What such behaviour does imply is the perhaps less interesting fact that humans are 'genetically wired for' the *capacity* to conceptualise and construct bridges as one possible solution to the problem posed by otherwise impassable ravines. The existence of cultural universals is a pointer to the range of human capacities, ultimately in some way limited by our biological heritage and species-specific abilities: but the mere universality of a trait is only evidence of a capacity - *not* a biological cause in the sociobiological sense. (The invalid nature of this aspect of sociobiological reasoning will be shown in more detail in the sections of this thesis dealing with Dawkins' and Wilsons' later attempts to combine early sociobiological theory with factors of existing social theory, to produce a more balanced 'coevolutionary theory'. See below, pp.194-217 & 218-248.)

It is interesting to note that Wilson seems unsure as to his view on how much post-cultural society has affected or can affect the so-called 'heritage' of genetically heritable tendencies evolved during the Pleistocene era of human development. Having stressed the importance of the five million years of our species' biological development as being the

dominant factor in present-day behavioural capacities, he states in another of his works:

There is no reason to believe that during this final sprint [of 10,000 years of culture] there has been a cessation in the evolution of either mental capacity or the predilection toward special social behaviours. (Wilson,1975,p.567)

His proposal following this quote that "substantial changes can occur in the span of less than 100 generations" and that it would therefore "be false to assume that modern civilizations have been built entirely on capital accumulated during the long haul of the Pleistocene" (ibid,p.567) would appear to be in opposition to the argument put forward in the initial quote from 1978. Equivocation on such a central issue in the sociobiological thesis is evidence of doubts as to the precise relation of biological to cultural change. It is this problem (already alluded to above) which resulted in the later attempts to adopt a biocultural coevolutionary model of behavioural adaptation, to be examined later. The underlying assumption still appears to be that all cultural change or stasis must be explained in terms of underlying factors of genetic change or stasis. In contrast to this, I would argue that evidence of convergent social organisation between distinct cultural groups may point to the operation upon human behavioural capacities of similar processes of cultural evolution. This may operate in such a way as to produce results suggestive of convergence due to underlying genetic similarity and resultant effects on behaviour. However, in the absence of independent evidence to support the genetic explanation, the opposing explanations remain equal contenders. To claim - as sociobiology does - that cultural similarity is in fact evidence for genetically heritable behavioural predispositions is to be guilty of an invalid form of inference, and once

again points to a flaw within the basic methodological approach to human behavioural studies.

1.6 Units of Selection

As mentioned in my brief survey of the historical roots of sociobiology, one of the main areas of discussion within evolutionary biology has been the details of the process of natural selection (see above, pp.9/10). At the centre of this debate has been the question of what exactly is being selected - i.e. what is 'the unit of selection' which is the object or target of the process of evolution by natural selection.

The traditional Darwinian view was that the process of evolution by natural selection operated upon individuals within breeding populations. Those individuals that displayed the most 'suitable' characteristics as regards the struggle for survival within their natural habitat would (*ceteris paribus*) survive and reproduce in greater numbers. Those less suitably endowed by nature would tend over time to decrease as a percentage of the total population. (It was to capture this 'sorting' or 'selective' aspect of the process of evolution that Darwin borrowed the now notorious phrase "the survival of the fittest" from his contemporary Spencer, who had coined it to describe a law of economic development.) Provided the traits which made certain individuals more suited or 'fit' in respect of their environment were genetically heritable, the process of natural selection coupled with reproduction over many generations within a relatively stable environment would result in evolutionary adaptation of the species to its habitat.

As I pointed out, this account appeared to be inadequate in respect of certain forms of behaviour commonly labelled 'altruistic' observed in many species. A slightly altered theory of the selective process was therefore suggested to explain the evolution of altruistic behaviour. This was the theory of 'group selection', based upon an insight by Darwin himself concerning the group behaviour of humans, and further debated and developed at the beginning of this century by evolutionary theorists. The position found its most forceful and complete expression in a book entitled Animal Dispersion in Relation to Social Behaviour written by V C Wynne-Edwards (Wynne-Edwards, 1962). In this book it was argued that some behaviours are selected not because they enhance the fitness of any individual, but rather because they enhance the fitness of the group of organisms as a whole, though detrimental to particular individuals. Wynne-Edwards argued that such behaviours as flocking in birds was a means (albeit unconscious) of carrying out a form of population census, as a result of which future population size would be controlled via hormonal control of clutch size. Thus it was argued that individuals would forego higher rates of reproduction so as to relieve the overall strain which the group placed on available resources, thus benefitting the group as a whole. Other examples of 'altruism' were the warning cries emitted by some species of birds in response to predators which gave the rest of the flock a greater chance of fleeing, even though it increased the chances of the crying individual being caught by revealing its location to the predator. The explanation was that a flock of birds with some 'altruists' or a shared degree of 'altruistic' behaviour in all members would as a group have a greater fitness potential than a flock consisting entirely of 'selfish' individuals. Thus selection could operate at the level of the group, where certain forms of behaviour - though unfit for certain

individuals - were advantageous for the group as a whole, relative to non-altruistic groups.

These assumptions of the group selectionist view were attacked by critics who saw it as theoretically unorthodox in its detraction from the individual as the focus of the evolutionary process. One of the main critics of group selection theory was G C Williams (Williams,1966), who questioned both the observations and reasoning of Wynne-Edwards' work. Williams argued, for example, that rather than increasing the individual risk of predation, the alarm cries emitted by individuals might in fact confuse the predator, or cause panic in the flock drawing attention away from the individual and thus actually enhancing their fitness potential. In a little over a hundred pages Williams argues that the apparent cases of group-directed altruistic behaviour are easily explained as "occasional malfunctions, examples of kin and therefore genic selection, misplaced parental behaviour, incidental statistical consequences of individual activities, spurious or inconclusive" (Cf. Williams,1966 chapt. 5-8, from which the above quotes are chapter headings). The most important feature of Williams' counter to the theory of group selection was that the true unit of selection was neither the individual nor the group, but the gene. Drawing on the arguments of theorists such as J B Haldane and especially the recently published work of W D Hamilton (Cf. Hamilton,1964), Williams argued that social aspects of group behaviour could be explained by reference to 'kin altruism' brought about by the process of 'kin selection'. The forms of behaviour described by group selectionists as being for the good of the group as a whole could be redescribed as enhancing the fitness potential of the individual, once it is seen that it is in the individual's interest not just to promote their own welfare but also that of their

genetic relatives. The idea of 'individual fitness' as the potential of the individual to survive and reproduce, thus passing on to future generations some percentage of their genetic endowment, is replaced by that of 'inclusive fitness'. Under this new theory, an individual may increase their own fitness potential by behaving in such a way as to benefit a relative - even at some personal cost - since by helping a relative they help someone who has at least some genes identical to their own. The shift of focus from individuals to their genes therefore alters the evaluation of what is or isn't a potentially advantageous trait, just as the group selectionist theory did before.

The new theory of kin selection was thus seen to be an extension of the original Darwinian perspective, since it still saw the individual's behaviour as being geared towards some kind of 'self-propagation' - even if this was actually achieved via the promotion of relatives' welfare. Appeals to group selection theory were regarded as positing a new form of evolutionary selective mechanism altogether, and dismissed on grounds of parsimony as unnecessarily complicated explanations of the observed phenomena. The mathematical games-theory models of theorists such as Maynard-Smith (Cf. Maynard-Smith, 1974 & 1983) consolidated the theoretical advantage of the genic selectionist perspective, and appeared to show that group selection would require such extreme and unlikely environmental conditions for its occurrence that it probably never occurred at all in nature. (For a further discussion of this debate within biological science, see J Cassidy, 1978.)

Despite the acceptance of the genic selectionist perspective, there continued to appear attempts to resolve the question of whether group

selection ever occurs at all, or could have played a significant part in the history of biological evolution. More recently the debate has reemerged due to an increase in speculation on both sides: a debate in which the emergence of sociobiological theorising has played a substantial part. The initial rejection of the group selectionist perspective has been reassessed by Michael Wade (Wade,1978), who argues that many of the theoretical models used against the ~~group selection~~ theory were inherently biased, and that the unlikely environmental parameters which they suggested were necessary for group selection to occur were inaccurately calculated.

these theoretical conclusions and the assumptions from which they have been derived can be reevaluated in the light of recent empirical studies of group selection with laboratory populations of the flour beetle, *Tribolium*....It will be shown that, in addition to the many assumptions which are unique to any specific model, the models in general have a number of assumptions in common which are inherently unfavourable to the operation of group selection. Alternative assumptions derived from the empirical studies are suggested. (Wade,1978,p.102)

In addition to Wade's empirical evidence from the flour beetle, there are two more examples of supposed group selection commonly cited in the relevant literature. These are the selection of the segregator distorter t-allele in the house mouse (*Mus musculus*) and the changing characteristics of the Myxoma virus which infects the Australian rabbit (*Oryctolagus cuniculus*). Both of these are cited in an important paper on the unit of selection debate by R C Lewontin (Lewontin,1970).

The arguments are complex, but may be summarised as follows. The t-allele has a distorting effect on the process of meiotic division, such that it is represented in a higher percentage of the male's sperm than normally expected (at around 80-95% as opposed to the normal probability

x

of around 50%). However, males that are homozygous for the t-allele are sterile - a factor which obviously places limits on the spread of the allele throughout the population. The mice's sexual behaviour is such that they live in small, isolated breeding groups of several females dominated by one or two males. There is therefore a high probability that some groups will consist of females and only sterile males, and the group's overall fitness potential will therefore be equal to zero, since fitness is a measure of reproductive potential. The fitness value of the females in such groups is therefore also zero, being a factor *not* of individual fitness related to respective genotypes, but a factor of belonging to a certain group and its overall properties.

The second example is that of the Myxoma virus. This virus infects the body of the host rabbit, finally causing death. It is spread from one rabbit to another by mosquitoes. However, the mosquitoes will only bite live rabbits: once the virus has killed its host it has therefore effectively destroyed the means by which it may be further propagated. It was observed that the virus had over time evolved to a less virulent form. Since virulence equals the rate of viral reproduction, and this measurement is also that by which the organism's 'fitness' is evaluated, evolution towards avirulence appeared to run contrary to the logic of competitive evolution. Less virulent strains of the virus should by definition be less 'fit' than their more virulent counterparts and therefore be progressively eliminated over time - a prediction opposite to the observed phenomena. The explanation offered was once again in terms of group selection, summarized in a review of Lewontin and others' arguments by Elliott Sober:

According to this story, viral strains of lower virulence are *altruists* in the crucial sense described above. They are less reproductively successful within each group. But since a group has a better chance of

colonizing and of avoiding extinction if its average virulence is low, relatively avirulent strains may be overall more fit than strains of higher virulence. (Sober,1984a,p.332)

Once again, it would appear that properties of the group overrule properties of individual organisms, resulting in selection for the group which runs counter to the normal process of maximisation of individual fitness potential. The examples are not without problems, since there is discussion by various commentators as to the correct definitions of what constitutes a 'group' or a 'deme' and even - as Sober points out - how one defines 'altruist' (for example, see Sober,1984,p.330). The same examples are also discussed in a recent work by the eminent biologist Ernst Mayr (Mayr,1988). Mayr sides with an interpretation of the Myxoma example, in which it was argued that it was in fact a case of individual rather than group selection (Cf. Futuyma,1979). His argument is that selection is not acting upon the overall properties of the collective virus strains present within any single infected rabbit, but only on the *most virulent* strain. This is the one which is the deciding factor as to the length of time until the host's death, and therefore also of the probability of the virus being spread via the mosquito to another host. Selection is therefore taking place *between* infected rabbits, according to the relative virulence of one strain in each case. It is not taking place *within* rabbits, between the various strains present. The evolution of avirulence is therefore disadvantageous only if considered at the 'local' level of competing strains within a host, but clearly advantageous if considered in the context of competition between particular viral strains at the level of the whole viral population, coupled to the particular details of the means of further infection of hosts and propagation. Mayr thus criticises Lewontin and Sober for characterising avirulence as conferring " 'a complete lack of

selective advantage' " on the organism, since it is in this case clearly the opposite (Mayr,1988,p.121 - quoting Lewontin,1970). Mayr concludes his discussion of the units of selection debate by citing what appears to me to be a far more plausible example of group selection - namely that of cultural group selection within human populations. It seems to me that human populations often undergo quantitative and resultant qualitative change as a result of competition between culturally defined groups, whose membership and group characteristics have little or nothing to do with individual genetic constitution and resultant behavioural properties. An obvious example would be the outbreak of warring on any level between cultural groups, which often results in an overall change in population structures, though there would appear to be no direct or necessary connection between individual genetic constitution and relative fitness potential. Mayr thus concludes that "Such cultural groups are found only in man and supply, in my view, the only well-established cases of group selection." (ibid,p.122).

It would seem that much of the debate still depends on further empirical observation of actual species in nature, but that there are also problems of conceptual and definitional origin to be solved. I cannot see any purely conceptual objection to the possibility of group selection occurring, so the question would appear to be rather that of how one defines it, and under what kinds of circumstances it would be more or less likely to occur. Having said this, there are in my opinion conceptual reasons for thinking that evolution via selection of entities larger than the individual organism has not played a very substantial role in the natural history of life on this planet - contrary to the claim made by Wynne-Edwards in his original characterisation of the group selection

theory. The reason for this is given by Lewontin in his summary of the unit of selection debate, quoted at length below:

consequences from the point of view of comparison between levels of selection. First, the rapidity of response to selection depends upon the heritability of differences in fitness between units. The heritability is highest in units where no internal adjustment or reassortment is possible since such units will pass on to their descendent units an unchanged set of information. Thus, cell organelles, haploid organisms, and gametes are levels of selection with a higher heritability than diploid sexual genotypes, *since the latter do not perfectly reproduce themselves*, but undergo segregation and recombination in the course of their reproduction. In the same way, individuals have a greater heritability than populations and assemblages of species. (Lewontin,1970,p.8, emphasis added)

In general one could say that the most controversial aspect of the whole unit of selection debate has been the theoretical changes of perspective suggested by the genic selectionist views of Hamilton and Williams. This has formed the basis for the more recent work of Richard Dawkins, and in particular has led to the swarm of controversy surrounding Dawkins' 'selfish gene' interpretation of human and other examples of social behaviour. Dawkins' theoretical justification for claiming that it is the gene rather than the individual which is the unit of selection is threefold. Dawkins argues that for something to be the object of the selection process it must have a certain kind of 'stability'. This quality of stability is further defined in terms of three essential properties: longevity, fecundity and fidelity. (Cf. Dawkins,1978,pp.18/19.)

The first property is essential since a replicating entity that doesn't last long enough in some fixed form will become outnumbered by others that do, and will not have as long to reproduce itself. The second property is essential, since if it does not reproduce itself at least as

fast as its competitors, it will become outnumbered and represent a decreasing percentage of the overall population in the competition for survival. The third property is essential since if it reproduces inaccurate copies of itself, it will very quickly become something else in future generations. (This is essentially the same point made by Lewontin in his summary concerning the relative 'heritability' of different entities in evolution. See the emphasised passage of the last quote.) Dawkins thus sees these three properties as essential for anything that is going to be successfully selected for in the process of evolution. They are deduced from the logic and dynamics of the process of competition and selection, rather than being based on any particular empirical observations or reasoning. They are then used as defining characteristics, and applied to the realm of natural objects to see which kinds of things possess them, and are therefore likely candidates to be units of selection.

Dawkins' conclusion is that the only things which do in fact qualify as units of selection are determinable sections of DNA - the genes. (Strictly speaking it should be said that the above view leads to the conclusion that competing *alleles* are the units of selection, but I will follow Dawkins in referring to 'genes' rather than 'alleles' in this case.) Any entity larger and more complex than the gene fails to possess the requisite triad of properties. Using the same line of argument as Lewontin above, Dawkins argues that the effects of segregation and recombination in sexual reproduction disqualifies the individual as a potential unit of selection, since it does not last long enough or reproduce sufficiently accurate copies of itself. Needless to say, any entities larger than the individual are disqualified for the same reasons.

In sexually reproducing species, the individual is too large and too temporary a genetic unit to qualify as

a significant unit of natural selection. The group of individuals is an even larger unit....They are not stable enough through evolutionary time....Each individual is unique. You cannot get evolution by selecting between entities when there is only one copy of each entity! Sexual reproduction is not replication....Individuals are not stable things, they are fleeting....When we have served our purpose we are cast aside. But genes are denizens of geological time: genes are forever. (Dawkins,1978,pp.36/37)

Without the existence of such long-lasting, self-replicating entities anything more than localised, temporary evolutionary change would not be possible, since the process of selection would have no focus on which to bear consistently over time. Since it is the genes which are the apparent focus of selection, all living organisms become in Dawkins' view mere 'vehicles' through which the genes develop further methods for competing with one another. It is this theoretical point which unites Dawkins, Wilson and all those writers who insist that social behaviour can be described and explained through a thorough grasp and application of biological principles.

But the individual organism is only their [the genes'] vehicle, part of an elaborate device to preserve and spread them with the least possible biochemical perturbation. (Wilson,1975,p.3)

Replicators began not merely to exist, but to construct for themselves containers, vehicles for their continued existence. The replicators which survived were the ones which built *survival machines* for themselves to live in. (Dawkins,1978,p.21)

It can thus be seen that in essence, the whole sociobiological perspective on animal behaviour is reached by means of two major theoretical stages. The first - already mentioned as 'the central dogma' - is that all behaviour is the product of evolution by natural selection. The second is in the definition of the process of evolution by natural selection. Though leaving it an open question as to whether selection ever in fact occurs

at levels taxonomically higher than that of the gene, the sociobiologists all maintain an explanatory perspective which views the gene as the fundamental focus of the selection process. Everything else becomes a 'vehicle' or 'extension' of the intergenetic struggle for resources and competitive reproduction, and must be described in suitable terms. This applies equally to all forms and complexities of behaviour - from the apparent 'altruism' of drone bees to familial strife and global political strategies in our own species. At this point I would therefore like to look at this other side of the unit of selection debate, and finally to relate the consequences of such a view to the overall project of sociobiological theorising.

The debate between the genic and the individual selectionists has been examined recently in a paper by Sandra Mitchell, in which she attempts to resolve the dispute between the two parties (Mitchell, 1987). Mitchell argues that the whole question of identifying the unit of selection is ill-formed. The causal process of evolution by natural selection consists of two stages. Firstly, there is the process of 'interaction' between entities which are competing with one another in a given environmental context. As a result of differing individual properties or traits possessed by these entities, they will be more or less successful at surviving this competition and reaching the stage of reproduction. This is the second stage of the process, since for evolution to occur there must be differential reproductive success resulting in the replication of entities which will in turn determine to some extent the characteristics of the future generation. When viewed in this way, one can distinguish two kinds of entity within the overall process. The first is the entity involved in the struggle for survival to reproduction - the

'interactor' or 'vehicle' in sociobiological terminology. The second is the entity which is copied and thus survives in replicated form into future generations - the 'replicator'. Mitchell summarises her own account of the process of evolution, showing its relevance to the controversy:

Given the necessity of both transmission and interaction for a process to qualify as evolution by natural selection, an ambiguity arises concerning the *unit* of selection. Is the unit of selection the entity involved in transmission or the entity involved in interaction? (Mitchell,1987,p.358)

Mitchell, D L Hull and other commentators on this topic consider that the above characterisation of the process as involving two stages shows how the whole question of the unit of selection is therefore ill-formed in the first place, since it fails to make sufficiently fine distinctions in its search for a single entity.

the phrase 'unit of selection' is inherently ambiguous. Sometimes it means those entities which differentially replicate themselves, sometimes those which interact with their environments in ways which are responsible for this replication being differential. (Hull,1981,p.26: cited in Mitchell)

The surprising thing is that both Dawkins and many of the defenders of the individual selectionist thesis recognise the two stages involved in the evolutionary process, and yet they continue to dispute which is the unit of selection after having admitted the ambiguity of the very expression.

Mitchell goes on to examine Dawkins' insistence that the gene is the only viable candidate for the unit of selection. Longevity is discounted by Mitchell as a distinctive criterion, since genes themselves do not actually survive. In fact, they normally fare a lot worse in this respect than individual organisms. What does survive is 'copies' of the genes. Mitchell

therefore invokes the type/token distinction against Dawkins, concluding that just as individual organisms are subject to mortality, so are individual genes. Longevity cannot therefore be used as a defining criterion to distinguish replicators and vehicles in the way Dawkins requires. ✕

Though I agree entirely with Mitchell's point here, I feel bound to defend Dawkins to some extent. In the passage immediately following my own quotation from Dawkins (see above, pp.54/55) he goes on to admit precisely the point about replication resulting in 'copies' which Mitchell accuses him of missing. All that is in fact missing in Dawkins' own account is the use of the philosophical vocabulary of type/token distinctions.

Genes, like diamonds, are forever, but not quite in the same way as diamonds. It is an individual diamond crystal which lasts, as an unaltered pattern of atoms. DNA molecules don't have that kind of permanence....But a DNA molecule could theoretically live in the form of *copies* of itself for a hundred million years....What I am doing is emphasizing the potential near-immortality of a gene, in the form of copies, as its defining property. (Dawkins, 1978, p.37)

It would appear from this that Dawkins is not in fact resting his case for the gene on the literal survival of individual bits of DNA. Since Dawkins recognises that longevity simpliciter cannot be invoked to distinguish genes from individuals, I would suggest that it is longevity in the sense of successfully producing future generations of copies which is important. The question therefore becomes that of what constitutes reproduction of a 'copy', which would seem to bring the focus of the argument to another of Dawkins' triad of criteria for the unit of selection - that of fidelity. Dawkins argues that in sexually-reproducing species at least, the degree

of fidelity in reproduction of entities larger than the gene is too low for it to be the focus of consistent forces of selection:

Your children are only half you, your grandchildren only a quarter you. In a few generations the most you can hope for is a large number of descendants, each of whom bears only a tiny portion of you - a few genes - even if a few do bear your surname as well. (Dawkins,1978,p.37)

This geometrically progressive 'watering-down' of one's total hereditary traits through future generations again leads Dawkins to insist that the gene is the only viable candidate for the unit of selection. Only the gene is reproduced with sufficient fidelity in future generations for it to be the focus of selection and make evolution and adaptation to the environment possible. However, Mitchell again claims that Dawkins is only telling a part of the story:

Whereas genes operate as coherent entities in transmission, organisms, or groups or some other entity may operate as integral wholes in interaction with the environment...Whatever fidelity in transmission a replicator has, to fulfill the destiny of survival, appropriate events have to occur to its corresponding interactor. Coherence in interaction with the environment is just as clearly characteristic of interactors. (Mitchell,1987,p.361)

In this case I must agree with Mitchell's criticism. Dawkins rests his argument on the importance of fidelity, which he only considers as applying to the transmission of hereditary traits via the copying of DNA structures. However, these structures are only differentially selected in the first place, and can only undergo quantitative changes of representation in the population over time if there is a corresponding 'fidelity' of interaction between entities and their environment. This side of the evolutionary process thus focusses on causal processes not involving genetic replication but behavioural interaction of individual

organisms (and possibly larger entities such as groups of organisms, populations, species, etc.). Clearly, if there were no fidelity or coherence at the level of interaction, then selection would cease to be consistent. The differential success of individual genes in replicating themselves would cease to be due to 'selection' as that process is normally understood in the context of evolution. Without the processes involved in the interaction of entities, there would be no consistency in the resultant differential 'fecundity' of competing genes. The property of 'fidelity' is thus seen to be inextricably linked to the process of evolutionary change as a whole - involving both interactors *and* replicators.

It would thus seem that, despite Dawkins' own acceptance of the characterisation of evolution by natural selection as a two-stage process, and despite his own avowal of at least one of the critical points of which he is accused by Mitchell, he still fails to see that his own criteria for identifying a single class of entities as 'the units of selection' fails to serve that purpose. The defender of the individual organism as the unit of selection fares no better in the debate. Mitchell's example in this case is R Brandon, though the arguments apply equally to anyone defending such a position against the genic selectionist view (Cf. Brandon, 1985).

The individual selectionist's argument is based on the fact that selection can only occur amongst interacting entities displaying variable traits - i.e. it is on the strengths and weaknesses of their phenotypic properties that entities (individuals or groups) are selected. Now it is possible for two entities to be *genotypically* identical, and yet differ *phenotypically* (due to differential ontogenetic development, or perhaps polymorphism for a particular trait). Conversely, two organisms could be

genotypically different, and yet *phenotypically* identical (again, phenotypic convergence could be due to ontogenetic factors of development). In both cases selective forces will act upon the phenotypic traits *irrespective* of the particular genotypic differences or similarities. This leads the individual selectionist to state that it is the characteristics of individuals (or groups) which are the focal point of evolutionary forces of selection, since they affect differential reproductive success regardless of specific genetic properties. The latter are 'screened off' by the properties displayed phenotypically. This form of argument is used by the biologist Stephen Gould in his rejection of the genic selection view:

Selection simply cannot see genes and pick among them directly. It must use bodies as an intermediary. A gene is a bit of DNA hidden within a cell. Selection views bodies....Parts are not translated genes, and selection doesn't even work directly on parts. It accepts or rejects entire organisms because suites of parts, interacting in complex ways, confer advantages. The image of individual genes, plotting the course of their own survival, bears little relationship to developmental genetics as we understand it. (Gould,1984,p.123)

However, as Mitchell points out, the gene selectionist could use the same form of argument in support of their own perspective on the issue. In the case of gene-linkage (the 'hitch-hiker' effect) some replicators increase their probability of representation in future generations by attaching themselves to another part of the chromosome. (The above-mentioned t-allele in the house mouse is an example of just such an effect.) Genes can thereby increase their fitness potential with a degree of independence of their actual phenotypic expression, since their reproduction is linked to the fitness potential of the part of the chromosome to which they are

attached. In such cases it can be argued that the replicators are 'screening off' the interactors during the selection process.

It would therefore appear that neither of the two sides in the debate have sufficient grounds to claim that they can identify exclusively 'the unit of selection' and apply that definition to every case of evolution by natural selection. I therefore agree with Mitchell when she says:

Brandon asks what effect adaptation has on the differential reproduction of organisms, while Dawkins asks what effect adaptation has on the differential transmission of genetic replicators. The point is that when we identify a trait as being an adaptation, both effects are necessary....Debates on *the* unit of selection shift attention away from the dual nature of the process of evolution by natural selection. By instead committing oneself wholeheartedly to accepting the necessity and importance of both processes of transmission and interaction, not only can the distinctions elicited by each of the steps be drawn, but the symbiotic relationship between the steps can be discerned. (Mitchell,1987,pp.364/365)

Though the above analysis of the unit of selection debate must go some way at least to resolving the controversy, I believe that it can be still further clarified by linking it directly to the context of sociobiological reasoning, and appreciating the part which the genic selection view has to play there. This may reveal why, in spite of their apparent agreement on the important points of the issue, there is still a fundamental opposition between the respective supporters of each of the two contending views.

The whole question of how the selection process operates came to prominence because of the puzzle created by the explanation of so-called 'altruistic' behaviour. In the face of cut-throat competition between individuals struggling against one another in the state of nature it would

appear impossible for altruism to evolve. Certainly, a group with at least some degree of altruistic behaviour occurring within it would have an advantage over a competing group composed entirely of non-altruists. But all change must occur initially at the level of point mutations or genetic changes due to recombination affecting individual behaviour patterns. Any altruist daring to rear its head within a group would run the almost certain risk of seriously diminishing its individual fitness. The most controversial aspect of sociobiology is in its application of this argument about the process of evolution to human social behaviour. It is here that the genic selectionist view has its most important and far-reaching consequences for behavioural explanation.

As already pointed out in my discussion of Dawkins' use and abuse of metaphor and other aspects of language, sociobiology suggests that since the genes are necessarily 'selfish', then we must also be necessarily selfish in all our social interactions. What other possible kinds of behaviour could we achieve after so many millions of years being moulded by the evolutionary process? I would argue, however, that there are crucial differences between the behavioural context of genes or non-intentional organisms, and that of intentional organisms such as human beings. I must therefore clarify this by explaining just how such kinds of behaviour differ and why. The behaviour of a non-intentional organism occurs firstly through a random process of mutation, and is 'valued' by a process of selection in an environmental context, the full informational description of which is unavailable to the organism's information-bearing structures prior to its genesis. In short, the 'adaptation' of the organism to the environment is achieved (if at all) via a process of blind trial and error, in which - due to the absence of a direct informational link from the

environment to the genes - there can be no possibility of pre-adaptation. (Except in very rare cases where a species just happens by chance to possess traits which are suitable to some emerging feature of the environment.) This model governs the description and explanatory framework of all behaviour due to evolution by natural selection.

If one then turns to human social behaviour, it becomes clear that there are important differences. Firstly, behaviour is not 'isolated' as in the case of organisms dependent upon point mutations/recombination for behavioural novelty. Humans can normally communicate their intentions, desires, hopes, etc. and thereby achieve a degree of coordination of behaviour at a group level. Secondly, this communication can occur *prior* to actual action, thereby reducing the risk of any individual behaving 'out on a limb' in a way so different from the rest of the group that they seriously diminish their own fitness potential. Thirdly, information of the actual or likely state of the environment can be assimilated also in advance of action, thus giving human behaviour at least its 'intentional' aspect of being pre-adapted toward some future state of affairs and intended goal. The net result of these abilities is that group behaviour of a genuinely cooperative and altruistic kind can be agreed upon and carried out by all concerned with some certainty of success. The isolated organisms of the sociobiological models of genetic evolution do not have the means to achieve such a state of behavioural coordination. Dawkins and others would no doubt counter the above claim by stating that our patterns of reasoning and behavioural inclinations are also the products of evolution, and must therefore be necessarily tied to the 'selfish' dynamics which has governed the millions of years of our ancestry.

In reply I would say that this argument only follows if one accepts the further hidden premise that our conscious thought is very strongly and directly tied to the behavioural dynamics which govern the historically earlier epochs of evolution, and which still governs much of the living world. Sociobiology precludes genuine altruism to man because it is precluded to other species; but the strength of the behavioural continuity implied by such reasoning is in need of further support, since it does not agree with the *prima facie* characteristics of human behaviour and with the capacities unique to man which I have pointed out above.

Given the above analysis, I believe one can see why the unit of selection and the 'genes-eye view' arguments play such an important role in sociobiological reasoning, and why - despite arguments to the contrary - they are so heavily defended. By establishing the gene as the 'focus' or 'target' of the whole evolutionary dynamics, and attributing certain definitive characteristics to this process and the entities involved, it is a simple step to explain all further forms of biological phenomena from within the same explanatory perspective. As the title of one of Dawkins' books suggests (The Extended Phenotype) all further instances of behavioural interaction then become mere extensions of the dynamics and properties functioning at the level of the gene. In so doing, however, sociobiologists in effect deny the possibility of evolution ever producing genuine novelty or different forms of behavioural interaction or development. The theoretical stage is thus set for a one-act play which begins and ends with the genes taking all the principle roles.

1.7 Complexity and Behavioural Laws

The fundamental methodology of the physical sciences is that of atomistic analysis. Phenomena are investigated, and their behavioural properties hopefully explained by their actual or theoretical decomposition into their constituent elemental parts. These parts are then theoretically reconstructed into the original whole with the aid of mathematically-based models and formulae, in which elements, their relations and other properties are abstractly represented by descriptive laws. The fundamental premise behind such a procedure is that the whole to begin with is no more nor less than the sum of its constituent parts, plus the relations existing between those parts and any emergent properties which arise from levels of structural complexity. Such theoretical assumptions and resultant methodological procedures have already established themselves within those sciences which deal with structures and phenomena in the inanimate world. In the animate world of biological organisms and structures, however, the level of complexity in terms of combinations of elemental parts, their resultant relations and properties is many orders of magnitude higher than in the world of inorganic phenomena. Since the biological world is primarily one of growth and change, the properties of entities and their forms are continually shifting, and the changes they undergo are often revisable at future stages in response to a range of factors affecting their development. Generally, one can capture the differences between the two areas of phenomena by imagining a scale of entities rising in complexity of internal structure and resultant properties and relations to the external world, beginning with the most elemental of posited entities - the sub-atomic particles of matter - and rising through atoms, molecules, organic molecules, cells, organs, organisms, populations,

ecosystems, and possibly further. As one moves along the scale, the individual entities become more differentiated from one another, in the sense that two atoms of hydrogen resemble each other more than two cells from an animal's skin tissue. Likewise, the latter resemble each other more than two individual organisms even belonging to the same species, such as two orchids. Moving along such an imagined scale, there is not only an increase in complexity of the entity (though the precise definition of 'complexity' is in itself open to question), but a corresponding increase in 'individuality' or 'uniqueness'. This feature of the organic world has, in my opinion, serious consequences for the avowed aim of sociobiology to render descriptive explanations of behaviour within law-like statements and models.

Firstly, there is the problem posed by complexity itself. Given the nature of the phenomena under investigation, it may turn out that - though there are deterministic factors involved in producing the events concerned - they are so complex and subject to such a degree of sensitivity to any number of factors, that in principle the precise outcome of any event is unpredictable. This is *not* to say that its occurrence is random: merely that its outcome is unpredictable within existing or foreseeable methods of observation, since these are incapable of ascertaining or bringing within a simple formulation the totality of factors involved. The events may well be determined, but their explanation involves factors which to the observer are indeterminable. Wilson does admit the relevance of this kind of objection to the proposed project of sociobiology:

The mind is too complicated a structure, and human social relations affect its decisions in too intricate and variable a manner, for the detailed history of human beings to be predicted in advance by the individuals affected or by other human beings. You

and I are consequently free and responsible persons in this fundamental sense. (Wilson,1978,p.80)

Though I would credit Wilson for his honesty in recognising a considerable objection to his proposed unification of the natural and social sciences, I would take him to task over the quasi-philosophical conclusion of his statement. As I have made clear above, the question of whether an observer has knowledge of determining factors is separate from the question of whether certain events are or are not in fact determined. The former is an epistemological question whilst the latter is metaphysical. Such a distinction does not appear in Wilson's discussion of the matter, who I suspect is eager to allay the fears and accusations of critics who accuse sociobiology of implying some form of 'genetic determinism'. It is perhaps for this reason that he mistakenly equates indeterminability with indeterminism.

Wilson goes on from this admission to emphasise that all is not lost: despite our inability to predict specific cases of individual behaviour, more general or broader categories of behaviour can be successfully predicted. The predictive power of such law-like formulations - unlike those of chemistry or physics - is in inverse proportion to their degree of specificity. Thus, behavioural 'laws' if possible at all will be necessarily stochastic in form.

Moreover, the statistical properties of populations of individuals can be specified...cultural change is the statistical product of the separate behavioural responses of large numbers of human beings who cope as best they can with social existence. (ibid,p.81)

Such a statement raises at least two important points of interest. Firstly, it clearly implies an approach to behavioural explanation which is

modelled on the methodological assumptions of the 'hard' sciences (i.e. physics and chemistry). It implies that descriptions and even predictions of 'cultural change' can be given through knowledge of individual propensities to behaviour, and thus that social behaviour is merely a summing or aggregate of individual tendencies. Such an individualist stance as regards the explanation of social phenomena is not an uncontested approach to this area of knowledge, and once again takes for granted the idea that heritable tendencies of individuals are explanatorily prior to the effect of being part of an existing social group. Secondly, it leaves open the question of how much informational detail must be lost, and how much retained, in order to reach the descriptive level at which the supposed statistical laws of human behaviour can be reliably formulated. If the new science is to offer any more than those areas of knowledge it claims to replace, then it must display a greater degree of accuracy, detail, explanatory power, or some other recognisable quality of distinction. It is not clear from talk of 'statistical properties' that it has anything more to offer than folk-sociological predictions such as that the combination of fine weather and a bank holiday weekend normally leads to high volumes of traffic heading for holiday resorts!

Despite such open-ended doubts, Wilson and others envision a time when behavioural scientists, with the aid of comparative techniques and a rigorous scientific methodology, will be able to 'map the human biogram'. By this he means that they will be able to give a full account of the human behavioural range and the most typical responses within that range. Once again, however, this vision conceals a large unspoken premise. The premise is that human nature is in some way 'fixed' and 'static' and thereby incapable of significant future change. Only if this were so could

Wilson claim the possibility of such certain and complete knowledge of the bounds and predictability of human behaviour. In contrast to such a view, I would suggest that there is no reason to think that human behaviour is in any respect 'fixed' and amenable to such prediction. There is no reason to think that from a biologically evolutionary point of view our species has ceased to undergo further genetic change, or will cease to do so in the future. Moreover, such 'predictions' as present-day observation of human behavioural reaction makes possible carry no necessary implications for the future, since behaviour is always manifested in response to a given social context, and Wilson cannot guarantee knowledge of future social developments, or of the effects these may have even on existing behavioural propensities. The most therefore that sociobiology's supporters can claim for it is that it might be able to explain and to an extent predict human behaviour as it has been and perhaps now is: but it cannot develop the kind of *a priori* forms of knowledge which would give it the predictive power and corresponding role in future social planning which its supporters claim on its behalf. An awareness of such problems and limitations to knowledge and predictive power is, however, far from evident in the sociobiological texts:

Pure knowledge is the ultimate emancipator....But I do not believe it can change the ground rules of human behavior or alter the main course of history's predictable trajectory. (ibid,p.100)

There are some channels into which the tide of human history cannot be swept; and there are evolutionary cul-de-sacs we must navigate past. (Tennant & von Schilcher,1984,p.136)

The message appears to be that human behaviour is fairly well fixed in its main characteristics, but that - rather paradoxically - a knowledge of

such controlling factors will enable future social planners to turn the relentless tide of history to their own favour:

With pacifism as a goal, scholars and political leaders will find it useful to deepen studies in anthropology and social psychology, and to express this technical knowledge openly as part of political science and daily diplomatic procedure. (Wilson,1978, p.123)

Apart from the obvious inherent tension in such a combination of views, I believe that the doubts I have raised above show that the whole question of a scientific approach to the analysis and possible prediction of social behaviour is fraught with conceptual and practical problems largely ignored by sociobiology's supporters.

As mentioned above, the phenomena of animate life appear to have a different structure from those of the inanimate world, since behavioural properties and the precise nature of forms appear to be more open-ended, and dependent upon a much wider range of factors to which developmental stages are in turn sensitive. The subject matter of the so-called 'hard sciences' tends to be elemental and repetitive in most cases, whereas that of biology - let alone the social sciences - is by nature more complex, developmental and dynamic in form. This is nowhere more so than in the study of human behaviour. It is in this sense that the study of social change and development is seen by many to be the study of 'historical' processes, since the changes and future developments are seen to be the product of highly complex and thereby largely unique events and forces, rather than the repetition of relatively widespread and stable structures and events. The production of genuine novelty which imbues an event with a sense of history is something which does not occur at the level of physics or chemistry, and it is this which differentiates biology from the rest of

the scientific world, and also provides the rationale for denying a natural scientific historicist account of human nature. One can argue, of course, that the historically unique events appearing at one level in nature are dependent upon the law-like predictability and repetition of events occurring at other levels, but even such an admission does not justify a programme of reductive explanations from the macro- to the micro-level, with the expectation that such a programme will in fact be explanatorily more efficacious. In some cases there may indeed be reason to believe that a behavioural manifestation at one level is explicable in terms of some other level. Freudian psychology is based upon such interchanging of levels of phenomena and their explanation - e.g. apparently conscious behaviour in terms of unconscious motivations or autonomic reflexes, and vice versa. Such schemes are not necessarily suitable for every kind of phenomenon, however, and at the present stage of knowledge it is not even clear what the proper description and explanation of phenomena is even within different scientific disciplines, let alone to suggest 'transformational' or 'reductive' laws of some kind to bring all phenomena within some vast general synthetic scheme as sociobiology seems to envision.

The above criticisms aimed at some of the more general methodological issues raised by sociobiological theorising are, in my view, largely ignored or brushed aside by its supporters. They reveal a certain lack of depth on the part of the theorists, who fail to consider the further implications of their thesis, or else the necessary foundational assumptions to which their views commit them. However, there are also problems of a more specific nature even within the sociobiological framework of analysis. It is to these specific questions that the rest of

this thesis will now turn, concentrating more on the supposed application of biological principles to the explanation of various aspects of human social behaviour. During the course of this critique, it is hoped that the points already made will serve both as a general foundation, and will reappear in more specific examples of what I have been at pains to point out as serious weaknesses within the theoretical structure of the sociobiological project.

Chapter Two: The Individual and the Family Group

2.1 Sexual Differentiation

The first question which a sociobiological account of sexual differentiation must address is that of the evolution of sex itself. Why should there be two sexes in some species? Asexually-reproducing species have an advantage in terms of not having to undergo the process of somehow encountering and then successfully mating with another individual. In addition, asexuality would seem to confer a further benefit, since any genetic mutations occurring which result in phenotypically advantageous traits will be passed on to the next generation 'intact', whereas in sexually-reproducing species any favourable allelic mutation will have to face the 'diluting' effects of meiotic segregation and recombination. It would seem that by far the 'better' of the two processes is that of asexual reproduction (in the sense of being advantageous for the genetic replicators involved). However, sexual reproduction does confer one potentially advantageous property absent in asexual reproduction: it ensures a higher rate of genetic diversity and novelty in successive generations. It is precisely the shuffling and recombination of genes, plus the possibilities for further changes due to mutation and other effects such as sequence reversal which make sexual reproduction an evolutionarily advantageous process. Since it is less 'accurate' in terms of copying whole genetic sequences from generation to generation, it leads to greater individual diversity, and the possibility of a faster rate of genetic change across generations, thus favouring more rapid forms of species and

individual evolution. If the ecosphere were totally stable, then there might be an evolutionary advantage to be had from an equally stable form of reproduction, but in the face of changing habitats, and the need to evolve in response, a form of reproduction which combines the two elements of reproductive fidelity with a degree of continual novelty and potential diversity is more likely to be selected for. Thus the evolution of sex in its most fundamental sense can be shown to be explicable within the evolutionary perspective.

However, the evolutionary perspective can go much further than merely offering an account of the causal origins of sex. It can also draw more detailed conclusions as to the likely further development of sexual differences, and their effects on the morphological and behavioural characteristics of different species. The first point to be noted is that in all sexually-reproducing species the male is identified as the producer of the smaller gamete - the sperm - whilst the female is the producer of a relatively larger gamete - the ovum. (For a possible evolutionary explanation of sexually-relative gamete dimorphism, see Dawkins, 1978, pp.153/154.) Being many times larger (in the case of our own species, many thousand times larger), the female gamete requires a greater initial energy investment by the female for its production. Since the ova are not produced in anything like the quantity that sperm are, their maturation represents a far greater proportion of the female's life-span than in the case of the production of sperm. In mammalian and many other species, this initial expenditure by the female prior to fertilisation is followed by an even longer period of energy investment, due to the need to incubate the developing zygote - either internally or externally of the female body. Again, in many species, hatching or birth of the young initiates a period

of further energy expenditure, during which the female may be involved in greater energy expenditure than the male in order to feed, train and care for the offspring, though this is by no means an exclusively female burden. These further differences in behaviour may, according to sociobiology, be traced back to the original dimorphism of the sexes in terms of the production of the gametes.

The consequences of this gametic dimorphism ramify throughout the biology and psychology of human sex. (Wilson,1978,pp.128/129)

As we shall see, it is possible to interpret all the other differences between the sexes as stemming from this one basic difference. (Dawkins,1978,p152)

The reasoning behind these statements is based on the genic selectionist view of evolution. Since it is taken to be in the interests of every individual (or rather of every individual's genes) to increase their fitness potential by producing as many offspring as possible, the male of a sexually-reproducing species begins the whole process of reproduction at an advantage over the female, since his relative energy investment in producing a gamete is much lower. He need do no more than the minimum necessary to give his genes access to the female gamete during fertilisation. From this, one can deduce that males are potentially capable of parenting many more offspring than females, and that there will be a corresponding evolutionary advantage to be had by the male that gains mating access to as many females as possible. The genes of such males would gain a higher than average representation in the gene-pool of future generations. There will thus be an evolutionary drive or tendency for males to evolve morphological and behavioural characteristics which will aid them in their competition with rival males for access to females, and

- as will be seen below - to compete in various ways with the females as well.

Females, on the other hand, may be expected to evolve capacities enabling them to 'select' or 'assess' to some extent the competing males, with a view to only engaging in sex with males who have desirable genetic attributes, and who are perhaps likely to share some of the subsequent burden of rearing offspring. Since the males can fertilise any number of females, whilst a female can only be fertilised once, and must then (in some species at least) undergo a term of pregnancy, the females are the limiting resource in the 'Battle of the Sexes' as it is dubbed by Richard Dawkins. However, the males do not have it all their own way in this competitive arena. One advantage that the female has over the male is that she can normally be sure of identifying her own offspring. (This is not necessarily the case in species such as fish, where fertilisation takes place outside the female body.) The male is therefore often in the position of being a potential cuckold, and this may be expected to affect the behaviour of the male prior to committing himself to any form of pre- or post-coital investment of energy, such as the gathering of nesting materials or the provision of food for the developing offspring. The combination of the above factors results in typical sexual differentiation as regards physical attributes and behaviour. The female will tend to be passive, waiting for a display from the competing males to indicate which of them has the best genes to pair with her own in producing offspring. The female is therefore not in need of morphological attributes evolved to attract the opposite sex - hence the characteristic 'dowdiness' of many females of various species. The male, on the other hand, has to convince the females that he is superior to the other males around him, and perhaps

that he is willing and ready to commit himself to some form of pre- or post-coital investment in the offspring. He will therefore tend to be aggressively competitive and eager to mate with any available female. He must, however, also make sure that he is not about to mate pointlessly with an already-carrying female, and be cuckolded into investing energy into the rearing of someone else's offspring. (In species where the male energy expenditure prior to and after mating is negligible, one would expect this to be reflected by indiscriminate attempts to mate with any available female, since the male in this case has nothing or very little to lose in terms of investment, and a great deal to gain in terms of future representation in the population.)

The result of what Dawkins terms a "relationship of mutual mistrust and mutual exploitation" (ibid,p.151) is the complex behavioural strategies involved in pre-mating displays and eventual pairing, and the subsequent division of labour according to sex during the subsequent rearing of offspring. The best strategy for the female is coyness, thus forcing the male to invest time and energy in order to woo her, and in some cases to provide resources which will be essential to the survival of the eventual offspring. The male response must ensure the defeat of potential rivals, successful conquest of the female, and the above-mentioned caution as to being cuckolded:

he can wait and see whether she is harbouring any little step-children in her womb, and desert her if so....Providing he can isolate her from all contact with other males, it helps to avoid being the unwitting benefactor of another male's children.
(ibid,p.159)

(One way to test the truth of this evolutionary deduction about male behaviour would be a cross-species comparison of the respective courtship

durations and male behaviour in relation to the incubation period of fertilised eggs in a range of species. I know of no such data in support of Dawkins' behavioural prediction.) Further sociobiological explanations of behaviour cover such phenomena as the 'Bruce effect' in mice. In this and other species, males have been observed killing the young offspring of a deserted female, thus freeing her from parental responsibilities and speeding her return to sexual receptivity for his own advantage. In most species, once successful mating has occurred, the male is under some onus to remain with the female, both because of the decreased likelihood of successfully mating with another available female, and because desertion could mean the death of the offspring. After a certain period of time, desertion by either partner might be an optimal strategy, though such a period would tend to coincide (again for reasons which one can deduce from evolutionary logic) with the growing independence of the offspring and the corresponding lack of demand on either parent.

The possible permutations of cost/benefit to each of the sexes in every species are clearly more complex than the rudimentary analysis offered in the simplified sociobiological models. Accurate evolutionary explanations would have to take into account the specific details of each individual species, in order to show the 'logic' behind the particular forms of behaviour. It would appear though even from such a simplified analysis that evolutionary considerations both confirm existing observations, and place them within a context where morphology and behaviour once again appear to be deducible from a knowledge of the competing forces within the evolutionary process. Dawkins admits the somewhat idealised nature of his own portrayal of sexual evolution:

For simplicity, I have talked as though a male were either purely honest or thoroughly deceitful. In

reality it is more probable that all males, indeed all individuals, are a little bit deceitful, in that they are programmed to take advantage of opportunities to exploit their mates. (ibid,p.167)

According to Dawkins' view then, the evolution of sexual reproduction and the initial difference in size of gametes leads to the further evolution of a whole range of morphological traits and behavioural 'strategies', all geared towards the maximisation of fitness potential in competition with rivals and even the sexual partner. The same view is expressed by Wilson and other sociobiologists, who then apply the same form of reasoning to assess and predict the effects of such biological mechanisms on the arena of human behaviour. Wilson believes that the deductions concerning the behaviour of other sexually-reproducing species hold true for the human species as well:

It pays males to be aggressive, hasty, fickle and indiscriminating. In theory it is more profitable for females to be coy, to hold back until they can identify males with the best genes. In species that rear their young, it is also important for the females to select males who are more likely to stay with them after insemination.

Human beings obey this biological principle faithfully. (Wilson,1978,p.129 - emphasis added)

Wilson goes on to explain how he thinks these initial differences become ramified and transformed into further differences at the more abstract levels of human society. This leads to the supposed universal phenomenon of male dominance in all important spheres of social life and organisation:

The physical and temperamental differences between men and women have been amplified by culture into universal male dominance. History records not a single society in which women have controlled the political and economic lives of men...Here is what I believe the evidence shows: modest genetic differences between the sexes; the behavioral genes interact with

virtually all existing environments to create a noticeable divergence in early psychological development; and the divergence is almost always widened in later psychological development by cultural sanctions and training. (ibid, p.133)

The sociobiological analysis is clear: initial morphological differences put a premium on certain forms of behaviour, which result in further differentiation - the end product of which is the male/female stereotype. The stereotyped qualities are assertiveness versus passivity; competitiveness versus coyness; aggressiveness versus docility; etc. Such differences, we are told, are the product of millions of years of evolution which has selected genes affecting both sexes, but is ultimately explicable in terms of reproductive dimorphism.

Much of the evidence cited in support of the sociobiological thesis concerning sexual differentiation in human society has come from studies of the Kibbutz society in the modern state of Israel. Sociological researchers have pointed out that this location offers unique opportunities for observations into sexual roles:

The kibbutz, with its deep ideological commitment to the equality of all human beings, and, of course, equality of the sexes, also offers women the independence prerequisite to equality. All in all, the kibbutz is perhaps the most likely place for the development of equality of the sexes. (Tiger & Shepher, 1975, p.260)

According to the sociological accounts, the programme of equality of participation in all areas of life - regardless of sex - appeared to meet with a degree of success initially. However, by the second and third generations a change was noted towards the kind of sex-based role

divisions prevalent in most other societies. Tiger and Shepher recorded the change at the end of their study of the kibbutz:

the sexual division of labour has reached about 80% of maximum....women are less active in the General Assembly than men....At the highest level of the kibbutz, women make up only 14% of the personnel....Women are overrepresented in higher nonacademic education leading to such jobs as elementary school teaching, kindergarten teaching and medical nursing. Men are overrepresented in higher academic education leading to such jobs as agriculture, engineering, economics and management. (ibid,p.262)

The same phenomena are cited by Wilson in his discussion of sexual differentiation within human society (Cf. Wilson,1978,p.139). Tiger and Shepher go on to consider and reject various possible explanations of the apparent return to sex-based labour and social division within the kibbutzim, all of which attempt to explain the phenomena as a result of sociopolitical factors. They are: the argument from insufficient revolution; the socialisation argument; the male conspiracy argument; the retreat argument, and the external influence argument (Cf Tiger & Shepher,1975,pp.263-269 for a full account of these arguments). They conclude their summary of the sociological explanations negatively:

The five arguments we have presented all have some point to them, yet none can sufficiently explain why, despite structural advantages and ideological fervor, the kibbutz has not lived up to its goal of abolishing sexual division of labor. (ibid,p.269)

This is followed by a final statement which reveals what is emerging from their analysis of the changes in the kibbutzim: "It should be clear that we regard any formal sociological explanation for what we have described as partial and, by itself, inadequate." (ibid,p.269). Tiger and Shepher anticipate the publication of sociobiological views of such phenomena by

concluding that in the human species, as in all others, behavioural range and propensity is confined by genetically heritable tendencies and capacities. Taken together, these form the 'biogram' of the human species, defining the outer limits and the norms of human social behaviour. This view is contrasted with the 'tabula rasa' theory of individual development, which they reject as an inadequate model for either human linguistic or general behavioural development. Just as Wilson was to maintain in his work published in the same year, Tiger and Shepher propose that human social development is under the limiting control of genetically hereditary factors which are pre-social in their origins. That is, they are to be traced back to the genetic heritage of the individual as a member of the species, and not to any experiential factors within the individual's lifetime - or at least not exclusively so. This is the theory which, in their view, provides the final solution to the puzzling phenomena of sex-based role division which their work has uncovered.

Our biogrammatical assertion is that the behavior of these mothers is ethologically probable: they are seeking an association with their own offspring, which reflects a species-wide attraction between mothers and their young. (ibid,p.272)

The sociobiological basis of their conclusion is made even more explicit a few pages further on:

If the predisposition of mothers to be with their offspring is a positive attraction, not a negative retreat, it is because of our mammalian and primate origins and the long, formative hunter-gatherer period of our evolutionary past. (ibid,p.276)

The statement is identical in its theoretical underpinnings to the conclusion of Wilson regarding sex-based differentiation of roles in human society. It supposedly provides the missing element to the explanation of

the persistence in the kibbutzim - despite attempts to alter it - of the male/female division of labour and the nuclear family as the basic unit of social organisation and reference.

It is to the advantage of each woman of the hunter-gatherer band to secure the allegiance of men who will contribute meat and hides while sharing the labor of child-rearing. It is to the reciprocal advantage of each man to obtain exclusive sexual rights to women and to monopolise their economic productivity. (Wilson,1978,p.145)

The hunter-gatherer reference is made because this was supposedly the state of social organisation prevalent for the two million years or more of our pre-culturally developed past. It was during this period that we evolved the basic behavioural traits which, in the view of Wilson and others like Tiger and Shepher, still dominate and explain human social organisation and individual behavioural tendencies. The final conclusion is that, as the evidence of the kibbutzim experiment in social reorganisation supposedly shows, changes in social organisation at the level of conscious planning cannot be effected if they go against the genetically heritable and therefore 'innate' behavioural dictates of individual and group biopsychology.

There are three main points which I would like to raise in opposition to the conclusion of Wilson, Tiger and Shepher regarding human sexual role differences, and the supposed deductions to be drawn from the example of the Israeli kibbutzim. The first is a reiteration of a criticism I made within the analysis of general sociobiological methodology (see above,pp.41-43). The universality of a behavioural trait is not necessarily evidence of underlying biological tendencies, operating with equal force on every individual within society. The most that can be said is that it is

evidence of behavioural *capacities*; but whether universal convergence on a particular behaviour implies that this is the only one possible for members of the species, or whether it can be attributed to the effect of cultural factors in similar environments upon what is actually a wide behavioural range, is a question whose answer cannot be unequivocally deduced from the evidence presented above. I will in fact go on to suggest an alternative explanation of the observed phenomena, which I believe will show that the deductions made by the sociobiologists are, to say the least, premature in their claims to explanatory exclusivity.

Secondly, the sociobiological explanation is based on the assumption that our behavioural responses have been and are 'fixed' by our evolutionary past, when we lived in small groups as hunter-gatherers. Not only that, but the genetically heritable tendencies layed down then, under very different social circumstances, continue to reveal themselves through phenotypic behavioural responses in precisely the same way, despite the obvious changes in society. Supposing there were some form of genetic control over our sex-based social behaviour; wouldn't it be surprising if it underwent no changes whatsoever in response to a changing set of social circumstances, and if it continued to reveal itself phenotypically in the same way, despite a different set of social factors impinging environmentally on every developing individual? Again, this criticism is aimed at a specific example of what is in fact a general tendency within sociobiology to make certain kinds of rather important and largely undefended assumptions (see above, pp.69/70). My third objection will constitute what is hopefully a viable alternative explanation of the kibbutzim example, and a general explanation of the appearance within many if not all societies of sex-based role differences.

I take it not to be a disputed fact that there are physiological differences between the sexes in human beings. One such difference of importance is that of the relative physical involvement of the two sexes during pregnancy and birth. This is one area where I believe that there is good reason to expect a link between the morphological and resultant emotional and behavioural characteristics, relative to the two sexes. The physical processes of pregnancy and birth must result in a differential feeling of intimacy between mother and child and father and child respectively. However, as far as the further parental behaviours of everyday child-care and familial responsibilities are concerned (the 'cooking, sewing, washing, etc.' mentioned in such discussions), I see no reason to think that genetically heritable tendencies towards or against such activities should be expected to evolve within the species at any stage.

In a society whose survival depends on some degree of individual specialisation for its maintenance, it is only to be expected that the precise division of roles will reflect to some extent the physiological capacities of the individuals concerned. In an agrarian and largely non-mechanised society, where hunting for food or difficult cultivation of essential resources is largely dependent on attributes of sheer physical strength, there will in my view be an obvious tendency towards male domination of such tasks, the male being on average more physically suited to them. This is *not* to say that there is a 'genetic' tendency towards such sex-based role division, in any sense stronger than the observation that the male of the species is on average more suitably physically equipped for such tasks than the average female. The initial intimacy of mother and child mentioned above, facilitated by the further physical contact and

bond of suckling, would seem to lead naturally to the near-universal adoption in such non-mechanised societies of a sex-based division of roles. This seems to fall naturally along the dividing line of males dominating tasks of the production and control of raw materials, and females *equally* dominant in the areas of transformation of raw materials into goods, and other domestic tasks essential to the continuance of the stable family unit and thus the stability of the basic economic unit of the society. The fact that some tasks are necessarily or more easily carried out within or close to the dwelling site, and some necessarily away from it, leads to further divisions of labour and responsibility naturally falling into particular universal patterns. Such universal division of roles within the species requires no more of a 'biological' element to its explanation than the already-made observations concerning the relative physical strength of the sexes, psycho-physiological differences connected with childbirth and the earliest stages of parenthood, and the physical nature of the natural environment. It is in my view a common characteristic of sociobiological accounts of role division in human society, that they tend to over-emphasise the importance of the male role, because of its supposed dominant features, and under-emphasise the corresponding role of the female. Many writers on the subject of hunter-gatherer societies have more recently stressed that the role of the female in such conditions is at least of equal importance to that of the male, since the regular supply of food is got from the process of gathering, with the occasional addition of meat from a successful hunting party. Surely if the family is the basic unit of the society, then the role of guaranteeing domestic stability and related responsibilities is every bit as important as any other, and within this sphere the female has

just as much right to be spoken of as 'dominant' as the much-publicised male.

What would appear at first sight more difficult to explain is the way in which such initial divisions in physical tasks lead to the differences at higher and more abstract levels of social organisation, such as the male domination of social and political decision-making bodies, and the conspicuous absence of a female voice in most of such general policy-making. Again arguing against any theory of innate psychological dispositions, I would raise the question whether it is at all surprising, given the initial role divisions and the central importance of resource production to the society's life, that the male dominance of resource production and acquisition carries over into the controlling bodies responsible for its maintenance and regulation. This aspect of male domination is stressed in all discussions of the topic: what receives less emphasis is the above-noted female equivalent of domination in other areas. Perhaps because these areas are more individual by nature, rather than being coordinated under any central policy-making bodies (i.e. each household is, in most respects, a law unto itself), the feature of female control does not appear so large or important a factor in social organisation and stability. I would however stress the idea that females carry just the same degree of responsibility in domestic matters and in some related areas of production as males, and that they differ perhaps only in the required degree of coordination and group representation. I grant the fact that in social groups larger than the tribe or small village community it is the institutional bodies at the level of national or international economic and political relations which wield the most significant power, affecting in many ways the lives of every individual

within the community. The male domination in such areas is therefore seen as an overall domination of the society's structure. But even within such societies, the female half of the population continues to play an equally dominant role in other areas more directly associated with the individual lives of family groups. This fact is recognised at the level of economics and manipulated in western society by the practitioners of consumer-targeted advertising, as well as by the many political campaigning groups, even if still not recognised by those who debate the relative status of male and female in human society. (To put it in rather a cliched fashion: the father may regard himself as the bread-winner, but who has hold of the purse-strings?)

Once the initial kind of social organisation that I have suggested above begins to operate within a social group, it will inevitably become precedental, leading to historical constraints affecting further areas of social development. In opposition to this force, as the society develops degrees of independence of factors in the natural environment, via the harnessing of resources through techniques of husbandry and technological means of production, there will arise a concomitant capacity for social change. The decreased need for individual physical strength and the call for a broader range of skills tends to break down the ultimate biological basis suggested above for the male/female division of social roles. Such factors may be accompanied by the release of women from their need to pursue traditional domestic duties, by the introduction of such things as birth control, economic independence and changes in legal rights, the state provision of welfare in areas such as care of infants (creches and kindergartens) and educational reforms. One would expect such changes to result in or accompany other social changes, signalling a growing demand

by and participation of women in areas of social activity previously held to be male preserves. These would cover everything from the male-dominated labour areas (e.g. factory work now requiring skills no longer geared to sheer physical strength), to business management, political and social positions of standing within the community, and a growing emphasis on a sexually egalitarian society. This is, in my opinion, precisely the kind of change which one can observe occurring in post-industrialised western societies, particularly since attitudes towards work were changed rapidly by such events as the second world war. It may be true that the general vision of male and female roles and capacities is deeply entrenched in the history and education of every member of society, and that this entrenchment therefore causes an inertia to change which slows the rate except in times of dire need and general social turmoil. The image of the traditional female role is enshrined in the deep-rooted symbolism of organised religion, for example in the image of the Madonna and child of the Catholic faith. However, one need not seek further than this level of social explanation to account for the prevalence of sex-based role divisions. If it were true that sexual differentiation in society is based upon an underlying 'biopsychological' mechanism revealing itself through the behaviour of every individual, then the kinds of changes which have occurred since the turn of the century in respective male and female roles would simply not have been possible.

Tiger and Shepher actually accept that social changes of the kind I have suggested do occur in many western societies, but turn my argument on its head by suggesting that it is further evidence of (or at least not incompatible with) the existence of an underlying biopsychological element affecting male/female roles:

it appears to be a rule of biogrammar that as part of the broad reproductive process, the sexes tend to create distinctions between themselves, and sometimes in ways that are not directly or indirectly reproductive....Indeed, one may ask whether the current feminist perturbation, its legal, moral and economic thrust aside, is a cognitive expression of a hidden biological process - a species reacting to overpopulation. Meanwhile men and women in the kibbutz have no qualms about bearing children; perhaps that is the reason they divide the sexes and increase their birth rates, acting against the trends of the socially progressive elements of Euro-American society, with which they otherwise often align themselves. (Tiger & Shepher,1975,p.278)

There are at least two revealing points within this quote. The first is that the authors appear to be suggesting the operation of some sort of Wynne-Edwards type group selection mechanism within human society. The suggestion is that on the conscious level there is the phenomenon of female emancipation, which the authors seem to equate with a necessary drop in the birth rate (though I fail to see how this follows of necessity). What is really going on, however, is the operation at some unconscious level ultimately under the control of the genes, of behavioural mechanisms which are reacting to signs of overpopulation, and affecting individual female behaviour in such a way that it effectively reduces individual fitnesses, but thereby also reduces scarcity of resources and other problems for the population as a whole. This process is mirrored in the kibbutzim, where sexual egalitarianism is being reversed, and sexual divisions are (again, apparently with some degree of causal necessity not fully articulated) resulting in a rise in the birth rate. Apart from the problems raised by the apparent group selectionist thesis, which stands with no evidential support whatsoever, and very little to commend it in the way of theoretical argument, I fail to see why the authors think that any of the data they have so far mentioned reveals any kind of necessary connection between sexual social division and the birth rate. And yet a

direct connection between the two is stated in the opening sentence of the quote, and implied throughout. Quite simply, I find such a line of 'reasoning' hard to accept, and can only interpret it as a statement of personal belief rather than as a coherent part of some presented argument. To argue that phenomena which would normally count as evidence against their thesis is in fact - under some possible interpretation - evidence in support of the very same thesis, merely by adding further unfounded suggestions to the original line of argument, is an unacceptable line of reasoning. It renders their thesis unfalsifiable, since they can always accommodate further contrary evidence.

To return to the example of the kibbutzim I would therefore suggest that, far from constituting evidence for a general resistance to change ultimately explicable in terms of our species' 'biogram', the behaviour of the kibbutzniks is an exception when viewed against comparative changes in western society. Viewed in this broader context I would suggest that firstly it does not therefore constitute the kind of hard evidence needed to support the sociobiological thesis of genetically-fixed behaviour regarding sexual differentiation in human society. Secondly, the dismissal of 'purely social' explanations of the kibbutz phenomena is therefore premature, and a misleading appraisal of the possible range and power of sociological argument. I conclude that for various reasons given above, the thesis that sexual differentiation in human society is due to biological factors in the sense meant in the term 'sociobiological' is not proven. An alternative and perhaps more viable explanation can be formulated in purely sociological terms, taking into account the historical dimension of traditional and entrenched ideas of the sexes arising from physiological differences, and the prevalence of entrenched religious, historical and

sociopolitical ideologies. In my view, none of this points towards or stands in need of further explanation in terms of biopsychological forces, and the phenomenon of sexual differentiation has neither the significance nor the permanence which sociobiology suggests.

2.2 Human Aggression

Another of the central issues in sociobiological explanations of human behaviour is that of aggression. Once again, as with the discussion of sexual differentiation, the argument begins with considerations about the role of aggressive behaviour in nature generally, and the likely course of its evolution. Since it is presumed that every population will be competing to some extent both inter- and intra-specifically for a limited supply of available necessary resources, an essential element of every organism's behavioural repertoire will be the capacity for some form of either aggressive or protective behaviour, in response to those around it. Aggression is not normally an end in itself, but often leads to the acquisition or loss of essential resources such as territory, food, supplies, or rank in a dominance hierarchy leading to access to mates. Aggression is therefore one element of an organism's behaviour by which it can further its fitness potential. The display of aggression is often (but not exclusively) more prevalent in the male of the species, who must compete for access to a limited number of receptive females, sometimes indirectly via competition for resources. Like many other features of animal behaviour, the phenomenon of aggression as something which could be systematically analysed and explained was first brought to the attention of a wide audience by the pioneering ethologist Konrad Lorenz

(Lorenz,1963 - translated into English 1966). In the work On Aggression Lorenz chronicles his observations of aggressive behaviour in many species, in order to address the question of the nature of specifically human forms of aggression, their causes and their possible means of reduction. In reaching his conclusions about the nature of man's aggressive behaviour, Lorenz employs the same methodology and kinds of evolutionary deductions as the later sociobiological writers have done, and his work serves as the starting point for what was to follow.

One of the earliest observations made by Lorenz and others such as Niko Tinbergen was that aggressive behaviour appeared to show some relation to the possible costs and advantages involved in any particular encounter. Far from resulting in high rates of mutilation or fatality, encounters often consisted of no more than an exchange of ritualised signalling and response, often falling short of or replacing the act of physical combat. Though initially a surprising fact for some, this feature of animal behaviour is in fact predictable from a sociobiological consideration of the likely evolution of aggression. Such a point is made by both Wilson and Dawkins in their discussion of aggressive behaviour:

The answer is probably that for each species....there exists some optimal level of aggressiveness above which individual fitness is lowered. (Wilson,1975, p.121)

Why is it that animals do not go all out to kill rival members of their species at every opportunity? The general answer to this is that there are costs as well as benefits resulting from outright pugnacity, and not only the obvious costs in time and energy. (Dawkins,1978,p.73)

Disputes between conspecifics are thus settled with the minimum of energy expenditure, and the maximum avoidance of risk to all parties. Aggression,

like other forms of social interaction, therefore involves a degree of behavioural 'adaptation'. An optimum level of fitness is achieved via the advantageous manipulation of the environment - in this case the social environment of interacting conspecifics - involving the sending and receiving of signals. Many 'fights' consist of no more than a show of strength on both sides, escalating through stages of determination and risk, but often sufficient to settle the dispute short of all-out combat. It is therefore postulated on the strength of such evidence, that aggressive behaviour in observed species is subject to fairly strong genetically-heritable forms of control. Unless this were so, there could be no explanation for the sending and receipt of formalised signal patterns, the production of and reaction to which is largely if not wholly innate. Once again, then, an area of significant animal social behaviour appears to be explicable within the confines of evolutionary theory. The logic once again conforms to general deductions which can be made from evolutionary premises. An individual organism whose behavioural responses caused them to become involved in situations of great personal risk and energy expenditure, far in excess of the likely rewards, is not the kind of individual likely to survive and pass on the genes responsible for such behaviour to future generations. On the other hand, the individual whose instinct is 'to have a go', but recognises the signs that mean he is likely to take a serious beating, the consequences of which outweigh the disputed prize, is an individual who lives to fight another day. (And, of course, whose genes live to fight another day and possibly many more days in future generations.) Such an individual may in future meet lesser adversaries whom he can in turn conquer, or become better equipped to challenge the original one on more equal terms.

The precise logic behind such explanations of the evolution of aggression as offered above was given greater plausibility by the application of Game Theory models developed by J Maynard Smith (Maynard Smith, 1973, 1976a & 1976b). From such models, Smith developed the concept of an 'Evolutionary Stable Strategy' (ESS). An ESS is that strategy which, if adopted by most members of the population, cannot be bettered or replaced over time by any alternative strategy. An example in negative terms would be genuine altruism: this is definitely not an ESS, since it would soon be overcome within a population consisting initially of even a few relatively non-altruistic individuals. (This is why, as will be seen in later sections, the question of genuine altruism in our own species has become a topic of heated dispute between sociobiologists and other parties.) The concept of an ESS allowed Smith and others to construct mathematical models of hypothetical encounters between organisms, from which they could predict the likely behaviour within the parameters of the model. The number of variables such models can handle is low in comparison with those operating in the state of nature for any actual species, but some of the more general predictions they produce do seem to match up to observed behavioural strategies in some species.

One such general prediction is that the more heavily 'armed' and therefore potentially lethal a species, the more likelihood that the individuals in it will have strong inhibitions against engaging in all-out combat. Being in the interests of every individual to avoid high-risk engagements, one can predict the evolution of formalised aggressive rituals preceding or wholly replacing actual combat. Such a general prediction is supported by evidence from many species; for example the submissive posture adopted by losers in combat, which triggers a response in the

victor causing them to stop short of actually killing the defeated adversary.

The human species is, in comparison with many others, very poorly equipped for combat: lacking sharp teeth, claws, bulk or protection for use in aggressive encounters. However, our greater intelligence and manual dexterity (which are a much later product of our evolutionary history than our other morphological and behavioural features) give us the ability to manufacture tools and weapons which more than make up for any lack of natural armament. This point was expanded by Lorenz, who believed that it provided the key to understanding the bloody nature of man's recorded past. Lorenz argued that man's move from pre- to post-cultural life and the consequent changes in social organisation and forces was in some ways premature. Man was launched into a complex, novel environment for which - instinctively at least - he was and is poorly equipped:

Knowledge springing from conceptual thought robbed man of the security provided by his well-adapted instincts long, long before it was sufficient to provide him with an equally safe adaptation....Small wonder indeed if the evolution of social instincts and, what is even more important, social inhibitions could not keep pace with the rapid development forced on human society by the growth of traditional culture, particularly material culture. (Lorenz,1966, p.205)

The same view is taken up by Wilson and other sociobiologists in their account of man's aggressive behaviour. They argue that since such weapons as we have are products of relatively recent technical skill, one can predict from sociobiological theory that we will lack the corresponding instinctive inhibitory controls which in other species have evolved side-by-side with the use of arms in aggressive encounters. In our own case, the instinctive emotional controls inhibiting aggressive behaviour are

commensurate to our pre-cultural aggressive capacities. These bear no relation to our present aggressive capabilities, which have advanced as a result of cultural development.

Part of man's problem is that his intergroup responses are still crude and primitive, and inadequate for the extended extraterritorial relationships that civilization has thrust upon him....History is replete with the escalation of this process to the point that the society breaks down or goes to war. No nation has been completely immune. (Wilson,1975,p.563)

The argument is that our crude and inadequate genetically-heritable responses to the demands of contemporary social existence are responsible for the deterioration in personal, group and international relations, resulting in unrest and the threat of global warfare and mutual destruction. Lorenz posited a 'drive-release' mechanism in the psychology of all species alike, whereby aggressive urges would build up pressure over time like a boiling kettle, to be finally released in response to an appropriate stimulus. He stated that the drive and its inevitable build-up were independent of the releasing stimuli, so that there was always the possibility that aggressive displays would occur even in the absence of the appropriate stimuli. The result would be cathartic but gratuitous outbursts of aggressive behaviour in inappropriate circumstances. This is the underlying premise of Lorenz's contention that man has become in effect a 'sick animal' in respect of his present environment, unable to function properly in his social relations as he once did:

in pre-historic times intra-specific selection bred into man a measure of aggression drive for which in the social order of today he finds no adequate outlet. (Lorenz,1966,p.209)

In the absence of an adequate outlet for many of our evolved behavioural dispositions, drives such as that posited for aggression are discharged inappropriately under the control of "instinctive behavioural mechanisms much older than reason and not directly accessible to rational self-observation." (ibid,p.213). The ultimate explanation for supposedly high levels of aggression in contemporary human society is what was once in our evolutionary past a usefully adaptive element of our behaviour. The proximate cause for the triggering of this drive is identified by Lorenz as the present state of overcrowding, and the lack of personal space or territory caused by the changes in population distributions and densities since the advent of husbandry, and more recently the advent of mass urbanisation in post-industrial society.

The increase in number of individuals belonging to the same community is in itself sufficient to upset the balance between the personal bonds and the aggressive drive. (ibid,p.217)

Thus on the local scale, the evolved aggressive response is triggered by intrusion into one's perceived territory, and the minimisation of any sense of territorial rights extending beyond one's house walls. On the level of international relations, it is triggered by disputes over boundaries, and the tendency to classify unfamiliar peoples as 'out-groups', to whom one owes very different allegiances and reacts very differently in comparison with those one classifies as belonging to one's own 'in-group' (for a further discussion of sociobiological explanations of international relations, see below,pp.168-181). This explanation of contemporary levels of intra- and international aggression in terms of an instinctive drive to protect one's territory was taken up and made the basis of another sociobiological theory of human nature in a book entitled The Territorial Imperative by Robert Ardrey (Ardrey,1967). Although not referring

explicitly to the drive-release mechanisms of Lorenzian theory, Ardrey does explain many of the structures and features of human society in terms of the genetically-heritable need to define and defend a territorial area, which as a psychological motivating force may be traced in its origins to the days of our evolutionary ancestors. Thus, we are encouraged to believe, all forms of aggressive behaviour - from work-place squabbling to the nuclear arms race - are seen equally as explicable on the level of genetically inherited and strongly controlled drives towards territorial acquisition and defensive behaviours.

Where contemporary sociobiology parts from the theories of Lorenz and Ardrey is that it is no longer held to be true that there is an aggressive drive within every individual, building up and demanding release even in the absence of appropriate stimuli. An evolutionary argument against the existence of such a mechanism would simply be that individuals indulging in aggressive and potentially dangerous behaviour in an inappropriate and gratuitous fashion will simply be at a disadvantage, in comparison with those who display aggression only when provoked by the appropriate stimuli. Apart from this theoretical consideration, experimentation on many species including humans has shown that an increase in aggressive stimuli lowers the individual's threshold for aggressive behaviour (i.e. they themselves become more aggressive), but that prolonged absence of aggressive stimuli decreases aggressive displays by the individual. In other words, contrary to Lorenz's hydraulic catharsis theory of aggressive outburst, in the absence of stimuli there is a corresponding drop in aggressive displays, rather than their continuance in inappropriate situations.

However, although sociobiologists differ on this point, they still posit the same genetically-heritable tendencies to assert and protect some notion of perceived territory as the ultimate cause and explanation of the prevalence of aggressive behaviour within contemporary society and between nations.

War can be defined as the violent rupture of the intricate and powerful fabric of the territorial taboos observed by social groups. (Wilson,1978,p.113)

As with the earlier theorists quoted above, sociobiologists do not merely point to the possibility of aggressive encounters, but give it the air of *inevitability* contained in Lorenz's now defunct drive-discharge model:

The channels of formalized aggression are deep; culture is likely to turn into one or the other *but not to avoid them completely*. (ibid,p.118 - emphasis added)

The quote captures Wilson's view that one can go beyond asserting the trivially true fact that as a biological species we have the capacity for aggressive behaviour. Rather, it proclaims the stronger thesis that some unspecified level of aggression is an inevitable component of all possible social relations, no matter what the specific environmental conditions influencing individual development and prevalent social structures. Such rationality as we possess is capable of no more than determining to an extent the precise appropriateness of aggressive behaviour and its form: it is not capable of diverting or controlling such genetically-hereditary behavioural traits altogether. Like the famous statement of David Hume, in Wilson's view our rationality is and always will be a slave to our passions, those passions being the somewhat outdated but firmly fixed product of our pre-cultural ancestry.

Although the evidence suggests that the biological nature of humankind launched the evolution of organised aggression and roughly directed its early history across many societies, the eventual outcome of that evolution will be determined by cultural processes brought increasingly under the control of rational thought. The practice of war is a straightforward example of a hypertrophied biological predisposition. (ibid,p.119)

By 'hypertrophied' Wilson means that we have as a species a genetically-heritable disposition towards certain modes of behaviour which, coupled to our ability to rationalise about the means to achieving these instinctive goals, results in evolutionarily unforeseen consequences of the original instinct. Thus the ultimate cause of - say - the nuclear arms race, is the institutionalised use of technology in the service of large organised groups of individuals, who are expressing on a collective level their individual and instinctive tendency towards territoriality. Whether it be guarded threats, cold war tactics or open hostility involving periods of armed combat, though the specific nature of the aggression depends upon choices made by groups on the level of rational appraisal, the driving force and hence ultimate explanation of such action is the non- or pre-rational instinct of territoriality. As one critic has summarised such a view of human behaviour:

they extend the concept of farms or suburban lots to the nation, claiming that men fight to preserve or expand their national boundaries almost as involuntarily as they would their homes. They fight the soldiers of other nations because their remote ancestors once instinctively fought for nesting site, burrow, hunting range or the space which encircles a female. (Carrighar,1967,p.125)

There are several aspects of the sociobiological account of aggressive behaviour in humans which I find inadequately supported by argument or evidence, or else plain misleading. The first is the

simplification of the notion of 'aggression' itself to some unitary form of behaviour. This permits the sociobiological account (invalidly in my opinion) to proceed to make cross-species comparisons of various behavioural manifestations, claiming that they are all expressions of the same basic genetically-fixed behavioural function of aggressiveness - no matter what the particular details of the species or the behaviour in question. This over-simplification of the definition of aggression is coupled to that of 'territoriality', such that anything from a greylag goose spitting at an intruder near the nest site, to human global warfare are explained as equivalent expressions of the instinctive 'territorial' response found in all species. It is not clear that territoriality is such a unitary behaviour, capable of being covered in all instances by a single function-naming term, and then compared across various species. One writer on the subject lists a diverse range of behaviours in many species which defy any attempt to gather them all under a single function-naming term such as 'territoriality'. He concludes:

The very diversity of these mammalian and avian types suggests that any simple statement about the function of territorialism in general is almost bound to be inadequate....The condensation of this range of possibilities into abstract principles such as identity, security and stimulation not only appears premature but neglects the diversity of the phenomena; in different species territories may certainly have functions of widely differing kinds.
(Crook,1968,pp.200/201)

It is questionable if a single aspect of a species' behavioural range can be taken in isolation, and used in such a way for cross-species comparisons, without also taking into account the overall behavioural characteristics of the species, which must have some effect on individual behavioural elements. The specific conditions which provoke aggressive territorial responses vary from species to species, and across time within

species according to their particular life cycles and environments. For example, if the species has a particular mating or breeding season, this is likely to be a time of increased aggressive activity, especially between males competing for access to females. Pregnant females or parents with young and vulnerable offspring may also tend towards higher levels of aggression at such times, from a hormonally controlled instinctive response to shield and protect their young from predators and other dangers. At other times the same species may tolerate territorial encounters in a far more relaxed fashion, since there is no concomitant pressure on the individual to compete for or protect a particular resource. It would appear from such an elementary consideration that the blanket-use of terms such as 'aggression' and 'territoriality' is therefore misleading: it only succeeds (if at all) in building up an image of behavioural continuity and ultimate causal and therefore explanatory continuity by glossing over what would otherwise be important distinctions - both within and between species' behaviours.

The second criticism of the sociobiological account of human aggressive behaviour concerns the supposed 'instinctive' nature of its manifestation. As Lorenz, Tinbergen, Wilson and others have observed, the majority of aggressive encounters between conspecifics of species other than our own do not result in combat: they consist of a mutual exchange of ritualised signals which often suffice to settle the dispute in the absence of actual combat. The sending and receiving of these signals, and the sequence of behavioural responses surrounding them are products of evolution, in the same way as other signal-response modes of behaviour such as recognition of conspecifics. In man, however, the mediation between individuals during encounters does not rely on the instinctive recognition,

exchange and response to ritualised signals, in the way that this occurs in other species. It depends instead on a complex and elaborate system of customs and exchange of true symbols in the form of language and other gestures. These go together to form the rituals which constitute human communication, and which are the product of a culture's idiosyncratic history and the individual's training and awareness of this cultural context. This point is stressed by several critics of the work of Ardrey:

Man is the one species whose social behaviour (except in infancy) does *not* depend on a uniform set of social signals: all our customs, including language, depend on the training we receive in our own family or community. (Barnett,1968,p.81)

What are the critical sign stimuli for the human, which act to release stereotypical adaptive responses? What are the invariances of the cue functions in the human environment which help to release specific motor patterns built up through evolution by natural selection? (Holloway,1968,p.179)

The manifestation and form of aggression in other species is explicable as a response to a set of instinctively recognised signals. If you look at a gorilla 'the wrong way' it will take it as a sign of threat or challenge - no matter what your intentions, and it will respond accordingly. But how does one define looking at a human 'the wrong way' or acting in a way that constitutes 'threatening behaviour'? There is no simple answer to that, since the whole question of the interpretation and meaning of human behaviour is context-relative, and this context is largely the product of a given cultural training and tradition. A famous experiment by Niko Tinbergen succeeded in getting a particular bird to attack a washing-up liquid bottle by associative conditioning of the bird's instinctive aggressive responses to another species. It may well be possible to affect human behaviour in the same way, and obtain equally bizarre results through techniques such as hypnosis and unconscious suggestion, producing

behaviour which is triggered by some aspect of the environment, but which bears no 'meaningful' relation to that environment as it would be considered by most other people. Part of the success of Freudian psychoanalytic method depends on the uncovering of just such forms of apparently 'meaningless' but uncontrollable behavioural associations, which nevertheless reveal a strange logic of their own exposing some form of stimulus-response type link between the circumstances of the behaviour and its involuntary manifestation. However, it would be fallacious to deduce from this that the occurrence of human aggression (and perhaps other forms of behaviour) is under the same control of instinctive disposition to certain stimulus-response mechanisms evolved and genetically inherited by every member of the species, and unaffected by any aspect of culturally-relative socialisation processes. Given this crucial difference in the way that encounters are mediated, it is difficult to see how sociobiology can claim that the occurrence and specific form of aggressive encounters is essentially the same in its origin and explanation for every species, including our own.

Even the empirical side of the sociobiologists' cross-species comparisons seems to come into doubt when looked at more critically. As far as the phylogenetic ancestry of man is concerned, it would seem to point to a diminution of aggressive tendencies, especially as one reaches the level of mammalian and primate species. As one commentator observes, "the peacefulness of gorillas within their troop and between different troops is impressive." (Helmuth,1968,p.97). As Crook goes on to point out in the article referred to above, "Man's nearest relatives among primates are singularly lacking in simple territorial behaviour." (Crook,1968,p.215). Carrighar lists Caribou, Elephants, Sea Otters, Whales, Lemmings and

Gophers as mammalian species who show little or no signs of territorial behaviour, living in an open and communal style of shared resources. Similarly, many primate species display little evidence of the kind of fixed territoriality suggested by sociobiology to be their and our evolutionary lot, and rarely reach the 'red in tooth and claw' stage of aggression often attributed to them in over-simplified behavioural accounts. It would seem that an over-emphasis on the behaviour of a few species phylogenetically distant from our own can have a very distorting effect on the consideration of the origins and form of human behaviour.

Coupled to this emphasis upon the ancestry of human behaviour, and its supposed 'bloodiness' is the suggestion by Wilson and others that there is no hope of changing our basic behavioural instincts. As with sexual differentiation, we are told that aggressive behaviour is an aspect of our nature that we can divert in some ways, but which we cannot hope to avoid altogether.

There is reasonable hope that our moral responsibility may gain control over the primeval drive, but our only hope of it ever doing so rests on the humble recognition of the fact that militant enthusiasm is an instinctive response with a phylogenetically determined releasing mechanism, and that the only point at which intelligent and responsible supervision can get control is in the conditioning of the response to an object which proves to be a genuine value under the scrutiny of the categorical question. (Lorenz, 1966, pp. 233/234 - compare with quote from Wilson, 1978, p. 118 - above, p. 101.)

Unable to eradicate such supposedly fixed behaviours altogether, our best hope is to create social conditions which will divert them into a more positive function. Sociobiological analysis of behaviour therefore becomes an essential element in understanding certain social ills, and in

redirecting the 'hypertrophied' forms of behaviour which have become maladaptive in contemporary conditions of social existence. As an example of the way in which such knowledge of our instinctive drives can be put to a practical use, Lorenz cites the promotion of such international sporting events as The Olympic Games as occasions when "the anthem of one nation can be played without arousing any hostility against another." (Lorenz,1966,p.242). This statement was of course before the staging of Black Power demonstrations, the assassination of members of the Israeli team, and massive boycotts of the games following the USSR's invasion of Afghanistan and South African intransigence over the apartheid system. It would seem that, far from providing a possible occasion for the defusion of international tensions built up in other areas of diplomatic relations, such sporting events merely become a further staging ground for the publicising of national and international grievances. The hopeful proposals of Lorenz, Wilson and others might work if our species reacted in a known stimulus-response type fashion to certain behavioural cues, and if these could then be introduced appropriately to counter the effects of undesirable social phenomena. Whether such a form of mass manipulation is morally justifiable or desirable I leave an open question. However, it would appear from the arguments presented above that it is not in fact possible, as they suggest, to reduce the occurrence of complex social phenomena to such simple mechanistic principles, and to seek solutions to national and international problems in terms of controlling instinctive feelings such as 'territoriality'. To suggest that human behaviour in fact operates in such a fashion, and that problems such as levels of violence in inner cities should be tackled accordingly goes no way at all to understanding the real framework of conditions within which human behaviour is manifested. As the example of The Olympic Games perhaps

illustrates, whatever 'instinctive' feelings are aroused by joint participation in harmless competitive sport, they are not enough to change the overall sociopolitical climate in which such events take place, and which provides the broader scenario of cultural history, ideology and identity within which the thoughts and actions of individuals take on meaning.

In the same way, to draw easy parallels between the behaviour of overcrowded rats and statistics for violence within urban human communities is to ignore the whole range of social and other factors which may not be relevant to the behaviour of a rat, but which if denied or ignored in the human case result in a simplification of explanatory models and solutions that renders their informational content useless in any but a propagandist context. Even if there were evidence and reason to support the idea that the greater part of what appears as 'aggressive' behaviour is in fact instinctive and some evolutionary consequence of our territorial ancestry, it would still not follow that there is no hope of ever changing such tendencies, or that their occurrence can be predicted in any sure way given a knowledge of present environmental conditions. Since *all* behaviour is manifested in response to some given set of environmental conditions, upon which it depends for its precise form, it would appear that accurate prediction of the future responses even of 'instinctive' behavioural patterns is a matter of *a posteriori* induction, and not *a priori* prediction. I therefore conclude that as far as the sociobiological explanation of human aggressive behaviour is concerned, the theory and empirical evidence are inadequate, and insufficient to support the proposed thesis. The alternative to such a programme of explanation and possible social reform would appear to lie within what is traditionally regarded as

areas of 'social' science, which are perhaps more able to accommodate the range and complexity of information and theory needed to deal adequately with such phenomena.

Incidentally, both Wilson and Lorenz suggest that their theories of aggressive territorial behaviour are not merely sufficient to explain man's aggressive interactions, but sail close to the winds of Social Darwinism by suggesting that the same instinctive biological drives are responsible for the general adoption of forms of modern free-market economies:

The biological formula of territorialism translates easily into the rituals of modern property ownership. (Wilson,1978,p.112)

It is self-evident that intra-specific selection is still working in an undesirable direction. There is a high positive selection premium on the instinctive foundation conducive to such traits as the amassing of property, self-assertion,etc,etc. (Lorenz,1966,p.211)

The full title of Ardrey's work -'The Territorial Imperative: A personal inquiry into the animal origins of property and nations' - also suggests that modern capitalist economies and the desire to accumulate wealth and property within a competitive market are also hypertrophies of a genetically heritable behaviour pattern. As such they are to some extent inevitable and constitute a shared behavioural tendency across the whole species, regardless of social circumstances and cultural history. This was precisely the formulation which Herbert Spencer and his eager followers in the late nineteenth century derived from their crude interpretations of Darwin's evolutionary theory. Needless to say, such reasoning as Wilson and others have used to support this view of the 'instinctive' nature of man's economic life stands or falls against the criticisms already offered above, and as another example of the application of biological principles

to explaining human social behaviour I do not think it merits further discussion in its own right.

2.3 Kin Altruism

The question of 'altruistic behaviour' has never been far from the centre of evolutionary theoretical debate. Darwin himself wrestled with the question of why some members of the haplodiploid social insects (the sterile worker castes) could evolve such specialised morphological traits and self-sacrificial behavioural roles for the good of the colony, whilst foregoing individual reproduction. More recently, new theories or expansions of original suggestions have reopened the topic to discussion, especially in the light of sociobiology's own analysis of the supposed evolutionary roots and significance of altruism amongst humans. Since it is the theory of 'kin altruism' which provides the theoretical basis for the sociobiological theories of family behavioural relations, which will be examined in detail in the following sections, one must begin by examining the theory in general, before considering its specific application to explaining various forms of familial relationships.

'Altruism' is normally understood in a biological sense as behaviour which, whilst raising the fitness potential of some conspecific, has the effect of lowering (or leaving unaltered) that of the altruist, as a consequence of the behaviour in question. It is to 'do someone a favour' at one's own expense. The problem posed by such behaviour was how it could ever evolve in the first place, since selection would always appear to be against any altruists within a population. After initial attempts to

explain it by invoking theories of group selection (see above, pp.45-46), a solution was provided by the genic selectionist theory of altruism formulated by W D Hamilton (Hamilton, 1964).

Hamilton argued that since, under a genic selectionist view, individual behaviour becomes adapted by natural selection towards enhancing the survival and replication of the genes, and genealogically related individuals (kin) share some of the same genes, behaviour could be expected to evolve which would enhance the fitness potential of one's kin. A gene which causes its possessor to behave in some way beneficial to its kin is going to increase its chances of survival and replication in future generations more than a gene whose possessor behaves consistently selfishly to all. Thus, though deserving the name 'altruism' when regarded from the point of view of individual behaviour, under the genic selectionist interpretation such behaviour is ultimately 'selfish' from the point of view of the replicating genes. Where kin help each other, the genes help themselves! Thus the terms 'altruism' and 'selfishness' can be applied by sociobiology to the same process, depending upon whether it is being described from the point of view of the individual organism or its genes.

The key point of this chapter is that a gene might be able to assist *replicas* of itself which are sitting in other bodies. If so, this would appear as individual altruism but it would be brought about by gene selfishness. (Dawkins, 1976, p.95)

The importance of the theory of kin altruism was in its ability to explain how social relations involving apparently self-sacrificing behaviour could exist in a natural world where cut-throat competition was the rule of survival. One of the most recent theorists to develop the work of Hamilton

and apply it to all areas of animal social behaviour emphasises the breakthrough which this theory represented:

This turned out to be the most important advance in evolutionary theory since the work of Charles Darwin and Gregor Mendel. (Trivers,1985,p.47)

However, as with much of the application of evolutionary theory to behavioural explanation, there is a price to be paid for such a theoretical breakthrough. It is in the apparent effect which such an explanatory schema has on the evaluation and explanation of human behaviour. Trivers considers Hamilton's work to be of outstanding philosophical import, stretching beyond the bounds of evolutionary theory as a purely scientific discipline:

If Copernicus dethroned us from the center of the universe and Darwin from the center of organic creation, then work on the evolution of altruism has dethroned us once again, making altruism more general than we had appreciated and more deeply self-serving. (Trivers,1985,pp.46/47)

One must therefore examine such a theory closely, to see if it does indeed carry the quasi-metaphysical and rather paradoxical conceptual consequences attributed to it by Trivers.

The theory of kin altruism suggests that in *Homo Sapiens*, as in other species, one can expect on average a higher degree of unselfish, cooperative behaviour between blood relations than between non-relatives. In fact, if one applies evolutionary considerations rigorously, the conclusion is that one should *only* expect and find cooperation and unselfish behaviour amongst relatives - the rest of social intercourse consisting of attempts at mutual manipulation for selfish benefit. (I am

excluding for the moment the possibility of reciprocal altruism, which will be dealt with in a later section covering broader patterns of social interaction. See below, pp.151-167.) Such an interpretation resounds throughout the pages of sociobiological texts:

Individual behavior, including seemingly altruistic acts bestowed on tribe and nation, are directed, sometimes very circuitously, toward the Darwinian advantage of the solitary human being and his closest relatives. The most elaborate forms of social organization, despite their outward appearance, serve ultimately as the vehicle of individual welfare. (Wilson, 1978, p.165)

So what we are in fact being told is that - appearances notwithstanding - all behaviour is ultimately 'selfish' in origin, and when not overtly so is, under the current analysis, seen to be covertly so. One is reminded of Dawkins' introductory statement of his views on human nature:

Be warned that if you wish, as I do, to build a society in which individuals cooperate generously and unselfishly towards a common good, you can expect little help from biological nature. Let us try to teach generosity and altruism, because we are born selfish. (Dawkins, 1976, p.3)

The evolutionary story runs along the following lines: evolution has equipped us all with 'inclusive-fitness calculators', such that whenever we have to make some behavioural decision, we always in fact choose to do the thing which will propagate our genes - either directly (i.e. selfishly) or indirectly (i.e. by helping others in order to help genes identical to our own). According to sociobiology, no other kinds of behaviour could have evolved, since alternative behavioural strategies would have been beaten by more selfish and therefore evolutionarily stable strategies. Such a theory raises several questions in need of further elaboration.

Firstly, it presumes the ability of individual organisms to recognise relatives via some morphological or behavioural cue. The simplest would be spatial proximity - a factor open to abuse by parasitic species such as the Cuckoo, which successfully manipulates the behavioural instincts of other avian species in order to save itself the task of raising its own young. Various mechanisms have been suggested as possible ways in which different organisms might have evolved a form of 'kin detection' device:

The most intriguing of [kin recognition abilities] suggests that many animals, including perhaps ourselves, may literally be able to *smell* kinship, that is, detect it directly by measuring some feature of the other creature and comparing this to a standard that has been learned....Subsequent experiments showed that recognition was achieved by a form of phenotypic matching, in which the appearance of each bee was compared to some learned standard of appearance (in this case sisters). (Trivers,1985,p.129/130)

There is at least one important point to be noted in this quote. The mechanism of kin recognition in species other than our own is, like much behaviour, dependent upon an evolved instinctive response to certain stimuli. In this case, the suggestion is that certain chemosensory cues trigger a response, eliciting altruistic behaviour between conspecifics within a hive of bees. What is interesting is Trivers' suggestion - unbacked by further empirical evidence or argument - that the same system may in fact be operating on a presumably unconscious level in humans. The reason this is interesting is because it specifically helps Trivers to make cases of human kin recognition look as if they depend on some kind of mechanistic reaction to others, as they do in the case of other species. However, as I will go on to argue in more detail below, one of the important factors which distinguishes human behaviour from other species' is that the recognition and definition of 'kin' cannot be separated from

cultural factors and attitudes of individuals coloured by personal experiences throughout their lives. If there is a distinction to be made between human behaviour towards kin and towards non-kin, then the criteria on which such a distinction and subsequent behaviour is based differs from the mechanical process of 'phenotypic matching' or whatever occurs in other species.

However, let us take it for now that there is some truth in the old adage 'Blood is thicker than water', and that we are normally more willing to behave altruistically towards kin than towards non-kin. How does this realisation affect our evaluation of ourselves and altruistic acts? There would appear to be two possible interpretations of the sociobiological perspective. The first comes close to what is known in philosophy as 'psychological egoism', which is the theory that - whatever appears on the surface - when an individual does something for someone else's benefit, they are always getting something out of it for themselves, and this is the real motivation for the act and its explanation. The individual that saves the relatives from the burning house is *really* only doing it for their own satisfaction in some way.

The first response to such a view is to try and clarify exactly what is being claimed. The claim could be that, though the individual genuinely wants to save the other person at some personal risk, nevertheless this is their own desire, and the satisfaction of that desire is therefore what motivates them to action. Ultimately, then, the individual is acting 'for their own good' in that the action satisfies their desires. The answer to such an argument is to say that obviously the individual is satisfying their own desire, but that the desire is not to help themselves in any

way, but to help someone else, moreover at some personal risk. In the sense that they are 'satisfying their own desire', all freely undertaken actions could be thus designated. However, one can still distinguish between those desires which are self-centred, and those which have the welfare of others as their aim.

In reply, it might be claimed that what is really happening is that the hero carries out the act not from genuinely altruistic motivations, but because they could not live with the guilt afterwards if they failed to act, or perhaps they even harbour hopes of the acclaim which such a deed will bring. Such interpretations of behaviour appeal to a sense of post-Freudian sophistication, which has encouraged us to probe beneath the surface of conscious motivation, to the concealed and often less commendable side of our characters. However, there is surely a limit to the range of such interpretations. Supposing that the imaginary individual above shows no signs of harbouring hopes of personal glory from his courageous deed, and only genuine and modest happiness at having saved his companions' lives. Is it still valid to deny that this is the end of the story, and to insist that, despite any and all forms of possible investigation and estimation of his motives, there *must* be some level of motivation at which he is fulfilling a selfish desire? One could take any example of the most selfless behaviour carried out ostensibly for the good of others (Mother Theresa is the most common example in such discussions), and under some logically possible interpretation show it to be the supposed outcome of selfish motives - these motives being posited without the support of any evidence from the subject or from behavioural evidence available to onlookers. But such a behavioural theory fails by being too inclusive: it is capable of constructing theoretical counter-

arguments to any further evidence, and therefore is incapable of refutation. Such a theory is discounted from being a true theory, on the grounds that a true theory must imply counterfactual cases by which it could be proven wrong. If, on all available evidence, it appears that Mother Theresa carries out deeds which are undertaken for the benefit of others, why should I or anyone become convinced on theoretical grounds alone that she is *really* motivated by selfishness, though no evidence from her own testament or that of others who know her could ever be produced in support of such an interpretation? The proof of Freudian interpretations of unconscious motivations is their eventual recognition on a conscious level by the patient, and the hopeful eradication of the psychological disorder associated with them. It is possible to be mistaken about one's motives for acting, or about the apparent motives of someone else: but one cannot deduce from this that one must *always* be wrong in some very fundamental way. (It will be my further contention below that the sociobiological interpretation of behaviour is mistakenly committed to an *a priori* view of human behaviour, such that whatever the evidence for behavioural motivation, the explanation *must* be couched in terms of selfishness. Their radical reinterpretation of human behaviour is thus posited upon a further theoretical premise supposedly deduced from evolutionary considerations.) I therefore conclude from an analysis of the above two lines of argument that neither of them show that genuine altruism is not possible in human behaviour, and that in the absence of contrary evidence, there are many instances of action which we are correct to interpret as being genuinely motivated by care for the welfare of others.

However, there is a further line of argument available to the sociobiologist. This is to argue that there are indeed occasions on which individuals act from purely selfless motives for the good of others, but that the rational and emotional processes involved in such behaviour have evolved because they served to further the inclusive fitness of individuals, and are therefore 'ultimately' selfish in their origins. The motives, on this account, are genuine enough: no amount of Freudian soul-searching would reveal any unconscious elements of selfishness lying beneath those which we take to be the genuine ones on the surface. However, these motives are only possible for us because they have served the 'selfish' ends of the genes in our evolutionary past. So, all behaviour is ultimately selfish.

The answer to such an argument is simply to say 'So what?'. All of our behaviour is the product in an ultimate sense of evolutionary forces (if one accepts a naturalistic account of human life). All of it must therefore have some link with survival functions - though not as strongly as might be thought, since traits which in themselves are not advantageous (or are positively disadvantageous) may nevertheless survive the selection process. Because all behaviour is ultimately subject to the assortative process of natural selection, does this mean that we should suddenly cease to draw distinctions between Mother Theresa and a criminal pursuing a life of crime for personal advantage, merely because both kinds of behaviour are 'the result of evolution' ? This line of argument seems to end in stating an empty fact, which has no necessary relevance to our non-biological ways of classifying and evaluating behaviour in everyday situations. If we are told that altruism is only possible for us because it furthered the struggle for survival in our phylogenetic past, and perhaps

still does so in some cases, I do not see that this is a reason to suddenly cease to draw the distinctions which existed prior to such information.

Perhaps, after all, the sociobiological account is pursuing a harder line than that suggested above. Perhaps we are in fact being told not just that such rationality and emotions as we have are ultimately the product of evolutionary processes which involved kin selection, but that our behaviour is *still necessarily* geared in a very narrow fashion towards the behavioural logic of kin selection. This stronger interpretation of the sociobiological thesis concerning the possibility and limits of altruism in humans is suggested by the sociobiological interpretation of the Japanese kamikaze pilots during the second world war. David Barash mentions their behaviour as an apparent example of extreme self-sacrificial behaviour motivated by altruism, but then goes on to suggest how the real explanation might be in very different terms (Barash,1979,p.168). Barash speculates that apart from expecting a reward in an afterlife, the individuals stood to gain possible sexual privileges, their families gained prestige, and a refusal to go through with the mission meant execution. Thus Barash reinterprets the apparent act of altruism in terms of gains to be made on the personal level, and also by the pilot's kin. The suggestion is that - far from our species having evolved the capacity for genuine altruism - we are still in the firm grip of the relentless evolutionary logic of personal gain, or gain for those with whom we share some of our genetic endowment. The same example is given an even clearer interpretation of supposed kin-directed altruism by Tennant and von Schilcher:

Perhaps the most extreme example that may fall within the scope of kin-directed altruism is the

self-sacrificing behaviour of kamikaze pilots. The modern Japanese population is descended from a few thousand Korean invaders; being therefore more closely related genetically than Europeans and Americans, say, their martial excesses are perhaps more readily understandable. (Tennant & von Schilcher, 1984, p.145)

I see no reason to believe that the supposed evolutionary forces directing organisms towards aiding their kin should constitute anything like a sufficient explanation of the behaviour in this case. The kin-directed altruism explanation relies on the implicit assumption that such evolutionary forces are so strong that they continue to override any form of cultural conditioning, or personal experience on the part of the individual concerned. If this is the case, then why are there not examples of similar behavioural excess occurring in, say, the highlands and islands of Scotland, or the Welsh valleys, where for generations small populations have reproduced in fairly strict isolation from outsiders? Surely here are the conditions for strong genetic relations, and the supposed resultant behavioural tendencies, in a more 'pure' form than the total population of Japan, despite its historical origins. Though there is indeed a sense of identity and community in such rural communities as I have suggested, I do not believe that one can seriously attribute its occurrence to biological factors of genetic relatedness, rather than citing sociocultural factors inherent in the lifestyle of those involved. Likewise, in the case of the Japanese pilots, I believe that a more correct and even plausible explanation for their behaviour could be found in the pervading ideology of the culture; with its history of strict hierarchical society, the divine status of the ruler, and the traditions of warriorship enshrined in the figure and example of the Samurai.

The only reason that human behaviour has to be forced into the explanatory moulds of kin altruism theory is because, as I suggested above, sociobiologists make an initial and largely implicit first premise. This is that, because the cooperative behavioural evolution of other species could only apparently have evolved along the lines of kin-directed altruism, if altruism was to evolve at all, then the behaviour of our own species *must* be geared to the same mechanisms. Since other species are not free to 'choose' their behavioural strategies, neither are we. It is precisely this stronger interpretation of the evolutionary considerations which lies at the bottom of the sociobiological denial of the possibility of genuine altruism in humans.

I accept the fact that the social interaction of other species is largely dependent upon individual responses to cases of one-to-one interaction. It is obviously of advantage to individuals (and to their genes) to be able to distinguish between potential enemies (such as competitors or predators), and those with whom it might be useful to cooperate. Presumably, this process must be carried out largely through the operation of instinctive reflex behaviours, evolved throughout the species' phylogenetic history. The simplest principles (in terms of cost/benefit to the organism) would therefore tend to evolve to govern the evolution of possible forms of social interaction, and kin altruism makes perfect sense as an explanation of the likely course of evolution, and as an explanation for many examples of observed behaviour. However, I would also argue that there are significant differences which alter the validity and application of such reasoning to the human case. Firstly, humans have an ability to communicate in various ways, and to form resultant informed opinions of the actual or likely behaviour of others which are far more complex than

any comparable ability observed in other species. On the strength of such abilities, we commonly make fine distinctions between those individuals with whom we interact, affecting our behavioural reactions in very subtle ways, which are open to constant revision throughout our life due to experiential learning. Our relations to others cannot therefore be reduced to simple mechanisms of recognition such as are captured by terms like 'relative' and ~~'non-relative'~~. Social factors covering a whole range affect how we treat both kin and non-kin, to the extent that we might behave very negatively to some individuals despite their genetic relation to ourselves, whilst reacting very positively and altruistically towards someone who is not related genetically, but whom we nevertheless feel more motivated towards helping. Sociobiology might respond by labelling such behaviour towards non-kin as a 'misfiring' of the genetically-controlled behavioural mechanisms. Once again this is the hard line adopted by Tennant and von Schilcher in their estimation of Mother Theresa:

The biologist's final resort in the face of heart-rendingly pure altruism is to count it as too rare to have evolved by selective force. Saintliness prevails because saints cannot be prevalent. Its occurrence then would be put down to rare genotypic constitution or misfiring in unusual environments. (Tennant & von Schilcher, 1984, p.149)

However, I would argue that such an account takes one large premise for granted: that human behaviour *is in fact* geared in all instances to the same evolutionary logic which controls the evolution of behaviour in other species. The above quote, as a final resort in the face of apparent disinterested altruism, labels it 'freakish' and therefore too rare to be of any consequence. Not only that, but it reveals the underlying premise that all behaviour is in fact under the strict control of genes, since the behaviour of Mother Theresa is due to 'rare genotypic constitution'.

In contrast to the above explanation, I would argue that one does not have to label such behaviour as 'freakish' or 'deviant' at all. Supposing that there were indeed a natural propensity in humans as a result of evolution to favour kin above non-kin. However, supposing further that this was not a case of strong deterministic genetic control: rather, that the human phenotype was capable of behaviour which went against the initial evolutionary forces coded for in the genes, due to effects of individual learning and the overall cultural environment. This would allow one to explain non-selfish individuals and those who behave favourably towards non-relatives *without* recourse to explanations of genes 'misfiring' or particular individuals behaving freakishly. One could admit that their behaviour confers little or no advantage on themselves or their kin, and that they might in fact reduce their inclusive fitness potential through their behaviour, but that because there is not such a strong correlation between genotype and phenotype, the occurrence of such individuals in every generation poses no evolutionary puzzle. It is only if the underlying premise of one's argument is that all behaviour is closely and deterministically geared towards individual genetic advantage, that one is thereby compelled (as the above authors) to label any behaviour going against evolutionary logic as deviant. This is in fact the position of Tennant and von Schilcher concerning human behaviour, as is apparent from their discussion of 'problem' cases of apparently disinterested altruistic behaviour:

The pure disinterested altruists among us - should any exist - form too insignificant a minority, despite the historical potency of their accumulated actions, to call for special confirmation in the evolutionary order. Like the necrophiliacs, the cannibals and the gifted idiots in our midst, they form a deviant residue. The genetic basis of their rare behaviour will itself be rare combinations of certain genes; and the circumstances that witnessed

their rare acts will be evolutionarily rare.
(ibid,p.154)

Such an absurdly extreme characterisation of human behaviour can only arise because of an equally extreme commitment to the theory that all human behaviour is strictly controlled by genetic constitution. Individuals who help non-relatives at their own expense are only freakish against the assumption that helping kin and only kin is the strict behavioural norm. Why should we be inclined to accept that we are as a species governed in our behaviour by the precise evolutionary logic and behavioural tendencies which have been observed and posited for other species? Why not accept that we may indeed have tendencies towards such behaviour, but that the complexity of the human phenotype means that we are capable of being affected by social and other factors, resulting in behaviour which is both genuine in its motivation and yet not geared to the strict evolutionary logic of maximisation of genetic representation. So far there has been no evidence - apart from the insufficient *a priori* deductions from evolution - to support the idea that our behavioural repertoire is incapable of including actions based on principles which go against those of our phylogenetic ancestors.

Given Hamilton's theory, we might well expect humans to show a relatively higher degree of affection or altruism towards their kin in normal circumstances. We might accept that such behaviour reflects a 'norm' of human psychological development, and that this can ultimately be grounded in a general theory of behavioural evolution. But, given the complexity of human psychology, and the capacity for changes due to social factors and individual experiential learning, the occurrence of genuine altruism directed towards non-kin cannot be ruled out *a priori*. At least,

not without further proof and argument to the effect that human behaviour must always conform in some strict deterministic fashion to the narrow dictates of evolutionary logic. Such an interpretation of the evolutionary perspective on human behaviour renders the sociobiological point of view rather less dramatic than supposed by its chief exponents, and reveals little more (if anything) about human behavioural ranges than can be gleaned from the simplest of purely sociological accounts.

The whole question of the relations of human individuals to kin and non-kin respectively appears to be more complex than is credited in the sociobiological account. There is a range of interaction which defies classification by the simple models used by sociobiology to explain the behaviour of other species. Humans do not respond mechanically to cues and signals by which others are then classified: they make decisions on the basis of many varied factors, and then respond within a whole range of possible behaviours. If this ability is ultimately a product of evolution, then so be it. Such a consideration does not make the decisions any less valid, or diminish our ability to assess and compare different individuals' responses, calling some altruistic and others selfish. At least within the sphere of human behaviour, such terms may still retain their normal significance, even if their application to other species must be heavily qualified in the light of evolutionary considerations.

2.4 Parent / Offspring and Sibling Rivalry

The general theory of when one might expect individuals to display altruistic and selfish behaviour has been developed by Trivers and other

theorists, and subsequently applied to explain several specific areas of relationships within the family group. Trivers hoped to explain in terms of personal and inclusive fitness the altruism displayed by parents towards their offspring, as well as the tensions between parent and developing offspring, and the incidence of sibling rivalry at different stages of development (Trivers,1972 & 1974).

Once again, the theory begins with the formulae for genetic relationships first calculated by Hamilton, which states that in sexually reproducing species individual offspring derive on average 50% of their genotype from each of their parents (i.e. the degree of relatedness is expressed by the coefficient $1/2$). Thus, if there is an evolutionary tendency to behave altruistically towards those who share some percentage of genes identical to one's own, it can be expected that parents will invest a proportionate amount of energy and personal resources in altruistic behaviour towards their own offspring. However, since individuals are involved from the moment of birth in life's competitive struggle, Trivers deduces that "an offspring attempting from the very beginning to maximise its reproductive success (RS) would presumably want more investment than the parent is selected to give." (Trivers,1974,p.249). The result of this is a conflict, in which every individual offspring can be expected to behave in ways which attempt to manipulate the parental altruistic responses to their own favour. Thus there will be a constant struggle between parent and offspring concerning the supply and demand of resources. Not only that, but since each offspring will try to dominate parental resources as much as possible, whereas there is no evolutionary reason for parents to discriminate between their offspring (who are all related to them by the same coefficient of $1/2$), there will be conflict

between siblings for the largest share of available resources - 'sibling rivalry'.

However, as with all behavioural strategies, one can predict certain upper limits to their continued development. If any one individual were to elicit too much attention and share of the available familial resources, it would detract from the fitness potential of its siblings, to whom it also bears a coefficient of relation of $1/2$. Any gene for 'all-out attention' would therefore not be as advantageous as might at first sight appear, since individuals possessing such genes would tend to eliminate or be eliminated by their sibling rivals, thus effectively reducing the future representation of replicas of their genes. A gene for a more inclusive and less individual fitness oriented behaviour would have the competitive edge in a breeding population. Selection would therefore seem to favour a balance between outright selfishness and degrees of altruism; but the balance would be expected to remain on the side of selfishness. Such a theoretical deduction from the initial suggestions of Hamilton would seem to square well with typical scenes of familial rivalry, such as the greedy squawking of newly-hatched birds in the nest. Trivers goes on to assert that "it can be shown that, in theory, conflict over the amount of investment that should at each moment be given, is expected throughout the period of PI (Parental Investment)" (ibid,p.252). This is because at all stages of development, the altruistic behaviour of the parent is elicited to an extent in response to signals of demand for attention given by the offspring, reinforced by signals of satisfaction when the demand is met. As Trivers points out:

once such a system has evolved, the offspring can begin to employ it out of context. The offspring can cry not only when it is famished, but also when it

merely wants more food than the parent is selected to give." (ibid,p.257)

It would therefore appear that conflict between parents and offspring, and the incidence of sibling rivalry are also explicable as an inevitable outcome of the tensions created by behavioural strategies in sexually reproducing species under a strong degree of genetic control. The individual (or rather the individual's genes) are torn between the cost/benefit conditions and resultant selective pressures which relate their behavioural outcome to factors of individual fitness, and those which relate their behaviour to factors of inclusive fitness. Selection and subsequent evolution will tend towards some compromise between the two extremes (i.e. there will be evolution towards an ESS). This may result in different periods within the individual's lifetime when their behaviour will tend more towards altruism or selfishness, depending upon factors such as the individual's reproductive status and number and relation of surviving kin with whom they are in contact. Offspring, with a greater future survival and reproductive potential than their parents, will tend towards selfish manipulation of the parent, whilst parents will be prepared to invest to some extent altruistically in their offspring. Siblings will be torn between the altruism due to their 50% average genetic identity with each other, and the selfishness for which every individual is selected in response to the general level of competition in nature. Parents will invest equally in all their offspring (though giving more attention where needed to the young), whereas individual siblings will compete to manipulate parental behaviour for their own individual gain. The longer the period that developing offspring rely on parental investment for their survival, the greater the potential for various stages of conflict, resulting in the evolution of a more complex range of

behavioural interaction between the various protagonists. Once again, sociobiology suggests a theoretically-deduced underpinning from evolutionary principles for the behavioural phenomena of all sexually reproducing species. As this stands, I see no particular reason to criticise the theoretical coherence or observational correlation of such a theory within ethological studies. But I do take issue with the simple application of such a theory to the description and explanation of human familial behaviour, as Trivers believes his theory to be capable:

the arguments presented here are particularly relevant to understanding a species such as the human species in which parental investment is critical to the offspring throughout its entire reproductive life. (Trivers,1974,p.250)

As I have argued above, concerning the simple and general application of kin altruism theory to human behaviour, there seems no reason to believe (and plenty of evidence to the contrary) that human social relations are geared to and under the influence of the same mechanistic processes and narrow evolutionary logic which no doubt governs most other species, and governed the phylogenetic ancestors of our own species. It may be true that in many mammalian species, as Trivers points out, there are natural constraints upon the optimum length of time that the mother spends in suckling and otherwise nurturing her young, leading to selective pressures upon genotypes to 'fix' this stage of behaviour by strong genetic control. This may lead to conflict between the mother and the young, who will be selected to demand a longer period of suckling than the mother gives. It may also be true that there is a likelihood of selective pressure to genetically 'fix' parental responses, such that more energy is expended upon the younger offspring, but otherwise all offspring are

treated equally, leading to a state of sibling rivalry for manipulation of available parental resources.

In species where there is little capacity for communication and recognition of distinguishable individuals, the information relevant to the control and development of social interaction must be largely innate - i.e. behavioural strategies cannot be learned and developed in response to ongoing interaction during individual lifetimes, so they must be coded for in the individual's genes. There will therefore be a strong correlation (i.e. 'closed' relation) of genotype to phenotype. Not only that, but the pressures exerted by the need to compete within a natural environment of limited resources, and the purely material relationship of parent to offspring in terms of food, shelter and protection, would be expected to result in selection for and evolution of behaviour tied very closely to the strict logic of maximisation of energy investment and resource availability. There is no reason to think that the pressures upon human relations, and the capacities for human social interaction are of the same kind, and that there is therefore selective pressure for the evolution of similar genetically controlled behavioural dispositions.

In the human case, allegiances, favouritism, family feuding and a whole range of social interactions lying along a continuous scale define human social relations, and are ascribable to factors of experiential learning within a cultural context which are conceptually too complex to be instinctive. Before one is introduced to one's relatives, one has no instinctive response towards them whatsoever (at least, not one that conforms to some biologically-predictable pattern). Relations are built and continually alter, open to revision throughout individual lifetimes, due to

learning processes, and the complex customs, rituals and mannerisms which make up daily social intercourse. How could selection ever begin to operate upon the individual competing genomes, to differentially sort out those genotypes with the most advantageous phenotypic expressions, when the behavioural range is so complex and dependent upon such a range of factors, and the social environment in which such genotypes must express themselves is so unstable? What are the social characteristics which can afford to become genetically 'fixed' within individuals by selection, and which will guarantee higher degrees of inclusive fitness in future social environments?

The period of maturation to adulthood in humans is normally longer than in any other species, both requiring and producing a level of social interaction far more complex than the purely material relationship of other species described above in terms of provision of resources and protection. Except in the most tenuous subsistence environments, the comparative weakness and greater demands of certain individuals by virtue of age or illness will be more than compensated for by the capacity of relatives and even friends to produce more in the way of resources than they need for their own consumption alone. In short, humans exist for the most part in some sort of shared 'economy', the nature of which compensates for and largely reduces the effect of the kinds of 'resource thresholds' which dictate the daily behaviour of other species. This factor of human existence, coupled to the above points about the nature of human social relations, effectively frees the human individual from the kinds of environmental pressures and related behavioural decisions towards which Trivers' calculations suggest that they are motivated, and for which it is suggested that controlling genetic mechanisms have been selected.

Such complex factors of social development and relationship, brought about by the fact that humans inhabit not just a physical and biological, but also a cultural environment of conscious concerns and attitudes, would seem to reduce the importance and application of Trivers' speculations on familial relations. This is not to deny that evolutionarily-developed and genetically-heritable behavioural tendencies may play some role in human affairs, especially in guiding the emotional responses and behaviour in early stages of growth, thereby ensuring an advantageous bond between parent and offspring. But whereas for a fledgling one can define stages of development purely in terms of growth and attendant physical capabilities such as flight and feeding independence, in the case of human development the precise conditions and recognisable stages will differ from one society to another, and from family to family. Except for stages common to all such as weaning (a relationship which in humans is not even necessarily between the biological mother and the child), it would therefore seem impossible to predict from biological considerations alone at what stage or in what form conflict is likely to occur. The stages of individual development in human society are largely marked by cultural ceremonies known as 'rites of passage', which serve to define in symbolic form the changes in social status of individuals within the cultural group. Since obvious physical signs of development are lacking or ambiguous, such practices serve to prevent or ease the kinds of tension and conflict which might otherwise ensue. Perhaps in reply to this Trivers would assert that these are merely the social embellishment of underlying biological mechanisms and genetically heritable behavioural tendencies, and therefore coincide with the predictions of his theory concerning human social behaviour. In reply I would say that though they may serve some such function, their precise form, significance and timing is a matter of

culturally-conditioned factors, rather than any underlying genetically controlled behavioural disposition, and that to attempt to reduce their description and explanation to such a level would be to severely restrict our understanding of them.

Given the above arguments, does the sociobiological perspective on family conflict from the predictions of kin altruism theory really offer much more in the way of explanation or prediction of human behaviour than can already be gained from a mixture of psychology and sociology? I believe that the answer must be negative, and that the failure to do so is due largely to an insistence on the misleading use of comparative behavioural models from other species, and the generalisation of behavioural predictions which fails to take into account the unique attributions of our own species for certain kinds of behavioural interaction. It may be the case that our species' ancestors were once limited to the forms of social interaction dictated by the genic selectionist logic in our distant evolutionary past, but it is invalid to argue, as Trivers implicitly does, that we are necessarily still tied to the same behavioural constraints, and that evolution has not given us capacities for behavioural interaction which are no longer bounded by the narrow logic of genetic advantage.

2.5 Homosexuality

Another of the areas of human behaviour to which the kin altruism theory has been applied is the explanation of the phenomenon of homosexuality. The reason for this area of behaviour coming in for such

scrutiny is that, as with genuine altruists, homosexuals apparently contradict evolutionary logic and thereby pose a puzzle. The opening premise of the sociobiological account of human homosexuality is that homosexuals produce no offspring, or certainly fewer than the average heterosexual in the population. As a result of this, the sociobiologist must find a way of accounting for the persistence of homosexuality across generations, since by conferring lower or nonexistent reproductive potential on those individuals exhibiting homosexual behaviour, it would appear that such individuals and their descendants ought to have been selected out of the species long ago in our evolutionary past.

This points to the second underlying premise of the sociobiological account, which is that homosexuality is a behavioural disposition for which there is a particular genotype, and that the likelihood of any individual displaying homosexuality is therefore raised or lowered according to the individual's genetic constitution. In other words, sociobiology assumes that there is a strong degree of genetic heritability for homosexuality. The 'solution' to the evolutionary puzzle posed by the persistence of homosexual behaviour within human society finds its fullest statement in the writing of E O Wilson:

Freed from the special obligations of parental duties, they would have been in a position to operate with special efficiency in assisting close relatives. They might further have taken the roles of seers, shamans, artists, and keepers of tribal knowledge. If the relatives....were benefitted by higher survival and reproduction rates, the genes these individuals shared with the homosexual specialists would have increased at the expense of alternative genes....This conception can be called the 'kin selection hypothesis' of the origin of homosexuality. (Wilson,1978,p.150)

Wilson's reasoning seems to answer the evolutionary puzzle of homosexuality in three stages. Firstly it suggests (as noted above) that homosexuals tend on the whole not to reproduce themselves, thereby freeing them to act as 'aunt' or 'uncle' to their kin, and assist in the raising of nephews and nieces (or perhaps even younger siblings), with whom they share some degree of genetic identity (a coefficient of genetic relationship equal to $1/2$ in the case of siblings, or in the case of nephews and nieces, $1/4$). Thus, though possessing a low or nonexistent individual fitness potential, by virtue of their homosexuality they are able to raise their inclusive fitness potential by aiding their kin to successfully reproduce. (There is even the suggestion that in some cases, a mother might recognise potential homosexuality in her offspring, and purposely 'manipulate' their behaviour in order to provide the family with a non-reproducing helper.. However, this line of argument is too implausible in the human case to be considered in detail.) The second suggestion, which is particularly vague, is that homosexuals might in some way be more able or suited than others to take up roles of relative importance and influence within the community, such as religious leaders, counsellors, artists or some other position of relative authority. Again, this would not enhance their own reproductive potential, but would in some way raise the status within the community of their kin and their kin's offspring. The third stage of the explanation is again a little vague, but suggests that, though homosexuals do not themselves pass on the genes for homosexuality since their reproductive potential is at best low, nevertheless their relatives and their relatives' offspring may carry some of the same genes. Thus by helping them to survive and gain successful status within the community, they indirectly spread the genes for homosexuality through their kin.

It seems to me that the sociobiological account of homosexuality is flawed in every one of its major premises, and from the general assumptions it makes concerning the phenomena in question. Firstly, it assumes that homosexuality is under fairly strict genetic control, and therefore genetically heritable (otherwise its occurrence would pose no particular evolutionary puzzle). The evidence for this is from studies of human monozygotic and dizygotic twins (see Heston & Shields, 1968 - cited in Wilson, 1978, p. 151). Even Wilson admits that such an empirical basis for his theory is not beyond question. The problem with twin studies is that they are supposed to show the relative effects of genetic inheritance versus cultural conditioning on some trait in the individual. However, it is notoriously difficult to draw a clear demarcation between the two in practice, since identical twins tend to be treated by others in a more similar fashion than non-identical twins or ordinary siblings. Thus, one cannot point to a feature of their behaviour and say decisively that it is due to genetic rather than cultural factors of development, since the two are interdependent. Bearing a physical similarity and coincidence of age and therefore of developmental stages throughout growth affects the cultural environment as well. If there is a correlation between sexual behaviour and monozygosity in twins (and one research paper to that effect hardly constitutes a proven fact by anyone's standards), this could be due to genetic factors, or cultural factors, or a mixture of the two together. To take it, as Wilson does, that there is a sufficient empirical basis to support such a crucial premise of his argument is to draw conclusions which are biased far in excess of the available evidence.

The next major assumption is that homosexuals necessarily have a lower reproductive potential than heterosexuals within the community. This

is taken for granted, revealing the underlying assumption that homosexuality is a single, simply definable behavioural type, manifesting itself in all individuals in the same way, with the same effects on individual reproduction. This too is, in my view, a rather grand assumption to be making, since it is not at all clear how one defines homosexuality, or that it constitutes a single definable behaviour pattern with the same effect on reproductive potential in every individual. The recent investigations and flurry of interest in human sexual behaviour, and in particular homosexual behaviour, occasioned by the need to try and curtail the spread of the AIDS virus has revealed evidence that a significant number of individuals practice both heterosexual and homosexual relationships, often without the knowledge of their married partner, with whom they have a family. Even in the absence of such reports, it would still seem to be a rather simplistic view of things to talk of 'homosexuality' and 'heterosexuality' as if they were mutually exclusive practices, with totally separable outcomes in terms of likely reproduction. How would the supposed sociobiological account of homosexuality deal with the phenomenon of women who, after many years as wives and mothers, make a decision to leave their family and enter into a homosexual relationship as part of a general change in attitudes which are as much socially and politically motivated as to do with preferences in expression of sexual behaviour? Are we to understand such people as responding to underlying genetic dictates, or is sexuality inseparable from a whole range of social behaviours, through which individuals define and express their identity in a way which defies a process of simple atomistic analysis? Once again, I find the sociobiological assumptions concerning the necessary correlation of sexuality with reproductive potential, and the

simple and mutually exclusive definitions of sexuality damaging to their position, and a misleading presentation of the phenomena under discussion.

The next stage in the sociobiological argument is Wilson's suggestion that homosexuals are more likely to become 'seers, shamans, artists, and keepers of tribal knowledge', thereby attaining some degree of prestige or status within the community and indirectly aiding their kin. This is perhaps the most curious of all the sociobiological positions, since it is unsupported by even a hint of further argument, except for a reference to scant anthropological evidence concerning the role of male homosexuals in some primitive tribal societies, and the suggestion that in Western societies male homosexuals perform better than heterosexuals on intelligence tests (see Wilson, 1978, p. 152 for further references). I can only surmise that Wilson is putting forward what might be called an 'Oscar Wilde Thesis', to the effect that homosexuals are in some way more artistically gifted or sensitive, or have unusual gifts of social importance which give them a potential for high regard in a society which values such things. Thus the lack of reproductive potential is compensated for by an attendant rise in 'sensibility', which again affects the standing of relatives within the community. However, what is utterly lacking is even a cursory discussion of the supposed genetic basis for such correlations of behavioural attributes. Are we to understand that there exists some kind of genotypical pleiotropy - i.e. that there is a genetic linkage between genes for homosexuality and genes for artistic sensitivity, or is the development of artistic abilities and higher IQ ratings also a result of the individual's being 'freed from the special obligations of parental duties', thus giving them more time to perfect social roles which are

perhaps more demanding? On this particular point of detail the sociobiological account is silent.

The final point I wish to criticise is the sociobiological suggestion that, though individual homosexuals are unlikely to pass on genes for homosexuality directly, such genes (if they exist at all) may be passed on by close relatives, who are thus 'carriers' of the genes, but who do not express the sexual behaviour associated with such genes. Again, Wilson is not forthcoming about the precise nature of this genetic process, though I can only see two possible interpretations of how it might supposedly operate. The first is supported by Wilson's own comment that:

Like many other human traits more confidently known to be under genetic influence, the hereditary predisposition toward homosexuality need not be absolute. Its expression depends on the family environment and early sexual experience of the child. What is inherited by an individual is the greater probability of acquiring homophilia under the conditions permitting its development. (Wilson, 1978, p.151)

Such a statement would seem to suggest that genes for homosexuality may be passed on by individuals who, although possessing them, do not express them in overt behaviour, since their sexual development has been affected by the social environmental conditions of their upbringing and early experience. Thus, genes for homosexuality could be present in two siblings, and expressed in one but not the other, being eventually passed on to future generations by the individual who in fact behaved heterosexually. Such an interpretation would seem to raise doubts about the importance of any genetic element of an account of sexuality, since it suggests that human sexual expression has as much if not more to do with the environmental effects on individuals of early social conditioning, as it

has to do with relative genetic constitution. The second possible interpretation of the precise mechanism by which genes for homosexuality are passed on relies on the idea that the behavioural expression of homosexuality is dependent upon whether the individual is homozygotic or heterozygotic for the genes in question. Such a line of reasoning is suggested by Tennant and von Schilcher's discussion of human homosexuality, where they state that: "it is ~~not only~~ possible that genes for homosexuality prevail because of superior heterozygote fitness." (Tennant & von Schilcher, 1984, p.141). The suggestion, again unbacked by any further discussion or elaboration, appears to be that individuals who are heterozygous for 'homosexual genes' may be in some unexplained way 'fitter' than individuals who either lack the gene altogether, or who are homozygous for the gene (and who presumably display homosexual behaviour). However, I am merely attempting to develop the line of argument on behalf of the sociobiologists, since it is not possible to gain a clear idea of their precise position merely from their own statements on the matter, which are at times little better than passing comments.

In conclusion, I can only say that I find the whole treatment of human homosexuality by sociobiologists a web of confusion and invalid deductions from scant or nonexistent empirical evidence. The underlying premises of their argument are once again far from empirically or otherwise proven, and behavioural definitions are manipulated in ways which simplify the phenomena into a form which conveniently fits the flow of the argument. No attention is paid to the complexity of the phenomena under appraisal, and detailed accounts of the supposed mechanisms in operation are lacking, or at best merely hinted at. In the case of human homosexuality, I can only conclude that sociobiology has not begun to fully recognise the subject of

its investigations, let alone offering any insight into the nature of that subject.

2.6 Incest

One of the traits of human familial behaviour most often cited in sociobiological literature as being under a strong degree of genetic control is the recognition and practice of incest taboos. The sociobiological explanation is in terms of a genetically heritable biopsychological mechanism, which affects conscious behavioural choices by individuals:

The biological hypothesis states that individuals with a genetic predisposition for bond exclusion and incest avoidance contribute more genes to the next generation. Natural selection has probably ground away along these lines for thousands of generations, and for that reason human beings intuitively avoid incest through the simple, automatic rule of bond exclusion. (Wilson,1978,p.40)

The question of how such a mechanism could function, since it implies an ability to recognise one's kin 'intuitively', is answered by the 'spatial proximity' hypothesis. There is no need for there to be an intuitive ability to 'recognise kin' as such: all that is necessary is some mechanism which results in a tendency to avoid sexual relations with those who are very likely to be one's kin, and those are normally the people with whom one has been raised in childhood. Though not foolproof, such an evolved mechanism would in most cases serve the same function as a kin recognition faculty, and might explain how incest taboos and incest avoidance by individuals are such widespread if not universal phenomena in human society.

When two persons form one kind of strong bond between themselves, they find it emotionally difficult to join in certain other kinds. (ibid,p.71)

The empirical evidence for the sociobiological thesis comes from two examples of societies where non-genetically related individuals have been brought up in close proximity throughout childhood, resulting in an apparent reluctance to indulge in sexual relations in adulthood, despite the lack of actual genetic relation. This behaviour is interpreted by the sociobiologists as evidence in favour of the idea that there is an instinctive biopsychological disposition in operation which prevents individuals from becoming sexually attracted to one another, because on some unconscious level they regard each other as if they were true siblings. One of these examples is that of Taiwanese 'minor marriage' customs (see Lumsden & Wilson,1983,pp.133-139 for an account), but I will concentrate my argument on the more commonly cited example of exogamy in the Israeli kibbutzim.

It had been noted by researchers that children raised together in the same 'family unit' or hut community on the kibbutzim tended almost exclusively to choose marriage partners from another hut or community in adulthood. There was, in effect, an almost universal adherence to an apparently self-imposed system of localised group exogamy. The phenomenon was explained by some researchers in terms of unconscious psychological mechanisms geared towards the instinctive avoidance of incest, operating proximately via cues such as spatial and behavioural proximity in early childhood (see Shepher,1971 for this explanatory account). This example from human behaviour was linked to examples of instinctive incest avoidance in other species, plus the universality of human cultural taboos

against incest, to formulate a general sociobiological theory concerning the evolution and operation of genetically heritable incest-avoidance behaviour. The final story is that all sexually reproducing species have some degree of instinctive behavioural tendency to avoid incestuous couplings, which has evolved as a negative response to the consequences of incestuous behaviour, since the offspring of incestuous couplings are more likely to be homozygotic for deleterious or lethal recessive genes. Since incest is therefore likely to lower inclusive fitness potential, sociobiology states that there has probably been selection for genes which control individual behaviour in such a way that humans instinctively avoid sexual relations with those with whom they have been raised in childhood (who are likely to be kin).

On the face of it, the sociobiological explanation of this form of behaviour seems to have some plausibility. However, there are some important critical points to be noted. Firstly, there is in this case, as has been noted in other examples of sociobiological discussion of human behaviour, very little or no discussion of what actually constitutes the behaviour in question. Is 'incest' to be translated as referring to cases of full sexual intercourse between close relatives, or does it also include such things as kissing, petting and other related sexual activities, or even cases of mere sexual curiosity directed towards close relatives such as peeping through key-holes? The sociobiological account makes no attempt to separate the existence and enforcement of taboos against actual physical relationships, and individual thoughts and feelings concerning possible or fantasised relationships. It is presumed from the start that there is no significant distinction to be made between different kinds of physical behaviour, or between actual physical behaviour and individuals'

behavioural intentions, desires, fantasies, etc. I would argue that if sociobiology is going to offer a coherent account of such phenomena, then it must begin by attending to such definitional details. There would appear to be no necessary connection between individual psychological states, and public or covert behavioural actions, and yet the sociobiological theory begins by taking examples of publicly-observed action as evidence for supposedly corresponding underlying psychological states of the individuals concerned. In a society where certain forms of behaviour incur strong public condemnation, with attendant publicly-sanctioned punishments (e.g. a death penalty), one would expect to find a relatively low incidence of the behaviour in question. However, it would be simplistic and false to deduce from behavioural evidence alone that individuals in that society had an instinctive psychological aversion to the behaviour in question. Yet this is precisely the model of reasoning used by the sociobiologists in the case of incest avoidance.

The sociobiologist could claim that my above argument presents a false analogy to their own, since the evidence from the kibbutzim shows public behaviour for which there is no corresponding sanction (since individuals are not avoiding incest - they are avoiding marrying non-related individuals, against which there are no public sanctions at all). Since the behaviour has nothing to do with public sanctions, they might argue, it must therefore be due to the function of some instinctive behavioural response. In reply I would argue that the behaviour in question can in fact be shown to be a possible reaction to public sanctions and codes of behavioural conduct, and that until this source of explanation has been shown to be invalid or irrelevant for some reason, it is premature to

insist that the true explanation must be couched in terms of some biopsychological mechanism.

The evidence for an alternative explanation of the phenomena in question comes from a more recent report on sexual behaviour in the kibbutzim (Kaffman,1977). In this paper Kaffman reports that there has been a change in sexual behaviour since the mid-seventies, such that the phenomena of sexual behaviour upon which Shepher and other earlier researchers based their assessment of kibbutz lifestyles in the 1960's is no longer accurate, and belongs to a special era of kibbutz development. According to Kaffman:

Until the mid-sixties, the sexual and valuation education in the kibbutz preached abstention from sexual intercourse until after high school, and stressed the need for "sublimation" of sexual urges. (Kaffman,1977,p.208)

This 'puritanical era' of the kibbutzim development can be characterised by the strong leadership and inculcation of educational and moral standards, with an emphasis on such things as sexual equality at all stages, and discouragement of sexual relations between individuals below eighteen years of age. Such standards were internalised to a great degree by the children of the kibbutz, to the extent that "These youngsters regarded [sexual] abstinence as a positive value, and supported their attitudes with arguments similar to those of their educators" (ibid,pp208/209). Since the mid-seventies, however, values within the kibbutzim, the state of Israel and even the Western world regarding sexual freedom have undergone a considerable degree of change. Kaffman reports that these changes in moral valuations and attitudes towards sexual relations have had radical effects upon the sexual behaviour even

within the kibbutz, though there is no correlation between this change and the continuing living conditions which (according to the sociobiological theory) are responsible for the children's attitudes towards sex.

Without any significant change in collective education methods, there has been a rapid shift toward removing the taboos on sexual intercourse among those at the secondary school level. Today, the norm is one of marked sexual freedom, though in a large part of the kibbutzim the coeducational child rearing approach has not changed; toddlers of both sexes continue sitting on their pots close to each other, children shower together, and boys and girls go on living in the same quarters from infancy through adolescence. (ibid,p210)

It would appear from such evidence that there is no general connection to be induced from the facts of childhood proximity in kibbutz living arrangements, and general attitudes towards sexual relations. Further to this, Kaffman looks at the evidence for a specific biospsychological incest taboo mechanism, concluding that the evidence in this case too is insufficient, and that simpler explanations of a purely sociological nature exist. The alternative explanations are: the smallness of communal groups, which forces individuals to look to other groups for variety and the chance to find a suitable sexual partner; the tendency to maintain peer group relationships into adulthood on the same footing; and finally the simple fact that romantic associations are unlikely to develop between individuals who know each other intimately from early childhood, when the glimpse of a stranger is enough to arouse curiosity and excitement. Added to this is the doubt that the purported exogamy was really such an absolute behavioural practice to begin with, as Shepher and the subsequent sociobiological reports have made it out to be:

The suggestion of absolute sexual abstinence within the group is less than exact. Mutual sexual attraction, even sexual relations, between boys and

girls from the same group may not be typical, but it is not all that rare either. (ibid,p216)

In the light of such evidence, it would appear that the sociobiological argument from the kibbutzim is, to say the least, less than compelling in its deductions. There are also further queries which can be raised against the sociobiological account of incest avoidance and the existence of taboos. For example, the supposed effectiveness of the instinctive avoidance of incest must correlate in some way to the degree of genetic relatedness of the individuals involved. That is, one would expect the strength of any instinctive behavioural response to differ with the genetic relationship involved - i.e. siblings should feel a stronger aversion towards incest than first cousins, who should feel a weaker aversion than uncles or aunts and their respective nephews or nieces (since the coefficients of relationship are $1/2$ for siblings, but only $1/8$ for first cousins, and $1/4$ for uncles and nephews, etc.). There is no evidence to suggest that such correlations of relationship and subsequent behaviour actually occur: in fact, such a theory presents a further problem for sociobiology. In the case of the most common forms of incest - those of father/daughter, sibling and mother/son - the coefficient of genetic relation is equal, being in each case $1/2$. However, the sociological evidence reveals that the incidence of father/daughter incest is higher than that of sibling incest, which in turn is higher than that of mother/son incest. If the psychological response is in each case due to the evolution of an instinctive mechanism linked to specific genes, then why doesn't it manifest itself with equal strength where the genetic relatedness is equal? One can perhaps formulate answers to such problems on behalf of the sociobiologists in terms of relative reproductive potential of the individuals concerned, coupled to their relative status

and degree of influence over others within the family group. However, even if such elaborations are logically possible, they are not forwarded by the sociobiological theorists themselves, who seem content to furnish the merest outlines of a theory, without regard for the complexity of detail involved in actual cases. If the differential incidence of different forms of incest is due to further factors, are these too deducible from evolutionary considerations of human biology, or are they such that their explanation demands a framework of social theory and explanation?

A final problem for the sociobiological explanation of human incestuous behaviour is in explaining the nature of the link between supposed instinctive behaviour on the part of the individual, and the manifestation of social sanctions and group morality governing public attitudes towards incest. Throughout the sociobiological discussion, it is taken for granted that these two aspects of human behaviour are functionally equivalent, and therefore to explain the one is *ipso facto* to have explained the other. It is, however, far from obvious that an instinctive aversion to incest occurring at the level of individual psychology should result in or have any simple causal correlation with group manifestations, such as displays of public disapproval, backed in many cases by legal sanctions. The question which most readily springs to mind is why there should be publicly ordained taboos governing incestuous behaviour, if everyone indeed has some instinctive aversion towards it. Does evolution result in 'belt and braces' behaviour, which governs both individual and group behaviour, and if so, why is there so much cultural diversity as to the severity with which cases of incest are dealt with at the public level? (An extended discussion of this particular aspect of the

sociobiological account of incest taboos is given in the later section on coevolutionary theory - see below,pp.235-237)

In conclusion, I would argue that the sociobiological account of incest in humans raises more questions than providing answers. It fails to define the precise nature of its subject matter, and to account for details of behaviour which would normally be regarded as significant and in need of a further explanation. It takes a definitive line of explanation in the case of the kibbutzim example, where there are alternative explanations available which are not considered, and where even the evidence itself is not beyond dispute. Finally, it fails to answer problems raised by its own theoretical premises, thus displaying a lack of explanatory depth, and a relative inconsistency in comparison with viable alternatives such as the explanations of phenomena offered by Kaffman and others, who use a purely sociological framework for their discussions.

Chapter Three: Beyond the Family

3.1 Reciprocal Altruism

So far the explanations of behaviour have applied to the analysis of individuals and their nearest relatives. When one moves from this level to that of social groups, the models and theories become correspondingly more complex. However, the basic mode of analysis remains the same, geared to explanation of social structures and behaviour from a stance of methodological individualism. The justification for this is to be found in the sociobiological adherence to the arguments concerning the unit of selection, and the emphasis on what are taken to be a *priori* necessary features of organisms' behaviour deduced from evolutionary theory and concepts such as that of 'Evolutionary Stable Strategy'. All structures, from single cellular organisms to the social structures of human society, must be analysed eventually from a functionalist stance, in terms of the possible advantage they confer on competing genes within the gene pool.

Contrary to theories which posit individual human social development as to some extent a process of gradual internalisation of already-existing customs, habits, fashions, etc. in society (e.g. Piaget, 1932 & 1966), the sociobiological perspective insists that the features and structures of social groups must be attributable to the largely pre-existing behavioural propensities of the constituent individuals. Again, there is an insistence on the explanatory priority of genetically heritable behavioural propensities over learned behaviour due to environmental experience. Even small differences in individual instinctive behavioural constitutions can,

according to Wilson, result in a 'multiplier effect', whereby they reverberate and become magnified throughout the social group resulting in large-scale effects on the precise social structure.

Social organization is the class of phenotypes furthest removed from the genes. It is derived jointly from the behavior of individuals and the demographic properties of the population, both of which are themselves highly synthetic properties. A small evolutionary change in the behavior pattern of individuals can be amplified into a major social effect by the expanding upward distribution of the change into multiple facets of social life. This phenomenon can be referred to as the multiplier effect. (Wilson,1975,p.9)

It would therefore appear that not only are the social interactions of individuals and their closest relatives explicable under a sociobiological analysis, but the social structures of the whole of human society as well, subject once again to the machinations of the genes - albeit in a 'removed' fashion. Such a fundamental theoretical stance runs counter to that of Marxism, for example, which posits the structure and changes of society as prior to the behavioural inclinations of the individuals comprising it. For the Marxist, it is the society which produces the specific individual behavioural types, and not vice versa. Changes in the behaviour of individuals are brought about by pressures to conform in various ways to the governing ideological structures, which are in turn ultimately a product of the necessity to regulate and uphold a given set of economic relations. The two theoretical stances are thus fundamentally opposed, beginning as it were from opposite ends of the structural scale: sociobiology at the level of the pre-social individual, and Marxism at the level of the pre-existing social structure. It is perhaps no coincidence that the strongest attacks on sociobiology have come from self-professed socialist thinkers, who see in sociobiology a scientifically-guised threat

to the theoretical premises of their own political ideologies. (For a fuller discussion of this aspect of theorising on human nature, see the later section pages 181-194 below.)

The first step in explaining the evolution of human society was to establish the possibility of some form of cooperative or 'altruistic' behaviour. This was achieved by the theories of Hamilton, and the extension of the concept of individual fitness to the new broader concept of 'inclusive fitness'. Having established the theoretical possibility of the evolution of a limited form of altruism directed towards kin, a further form of altruism was proposed called 'reciprocal altruism' (see Trivers, 1971). This form of behaviour is not limited to individuals and their genetic relatives, but to any conspecific with whom the individual shares a degree of social contact over a sufficient period of time. (Trivers also extends the term to cover examples of what would normally be called 'symbiotic relationships', existing between members of different species. However, as such individuals are not competing directly with each other for resources and differential representation in future generations, I do not think that such a use of the term is appropriate or constitutes what one would normally regard as 'social' relationships. I will therefore limit my own discussion to examples of altruistic behaviour directed at conspecifics.) As the name suggests, it explains behaviour which enhances the fitness of some other individual at personal cost, but which in so doing raises the probability that the recipient of the altruism will at some future time return the favour. It is summed up in the saying 'You scratch my back, and I'll scratch yours!'. Because of the nature of the behavioural interaction involved, it can only be expected to evolve in certain species which possess prerequisite characteristics. Firstly, the

members of such species must be able to recognise and distinguish individuals, and to retain memories of individuals for long enough to carry out the acts of reciprocation. This leads to the probability that groups practising reciprocal altruistic behaviour will be fairly small, and their membership relatively stable, since there is obviously an upper limit to the number of individuals that can sustain relationships which rely on mutual recognition and memories of previous behavioural characteristics. Lastly, there must be some method of communication which goes beyond the simple mechanistic and largely instinctive repertoire of many species, since this would be insufficient to sustain the level of cooperation and updating of information needed for a reciprocal relationship to be achieved. Thus, one can predict that species living mainly in groups which have a fluctuating membership, a low degree of individual informational exchange, and where individuals are not recognised as such, is not likely to evolve reciprocal altruistic relationships. In fact, such behaviour has only so far been observed outside our own species in some of the ape species, and some social mammals such as wolves. The benefit of reciprocal altruism to the individual might be anything from grooming an otherwise inaccessible part of the body, to having one's life saved. Such behaviour is maintained against intrusion or abuse by selfish individuals by their exclusion from future benefits. Anyone noticed taking advantage without paying back their share will be ostracised in future. Such 'cheats' stand to lose the benefits afforded to the other cooperative members of the group, some of which may be necessary for survival.

In a society with a high degree of interpersonal communication and mutual benefit from social exchanges of all kinds, such as is the case in our own species, one would expect a correspondingly high proliferation and

diversity of reciprocal behaviour to evolve. However, as in the case of kin altruism, the term 'altruism' is somewhat of a misnomer. The altruist in fact stands to gain at some future time as a result of their behaviour, and the 'favour' they carry out is really no more than a 'loan'. Once again, in line with the deductions from evolutionary theory, there can be no behavioural exchange which is genuinely 'altruistic' in its everyday sense. Reciprocal altruism, like kin altruism, is essentially self-seeking, since by doing someone a favour now, the individual is only really increasing the chances of reciprocal help in the future when they may be in need of it. One comes back once again to the insistence of the sociobiological perspective that "a predominant quality to be expected in a successful gene is ruthless selfishness. This gene selfishness will usually give rise to selfishness in individual behaviour." (Dawkins,1978,p.2). Where individuals are not apparently selfish, one should not therefore presume that they are in fact behaving altruistically in its normal sense.

E O Wilson's discussion of 'altruistic' behaviour follows the same lines as that above (see Wilson,1978,pp.155-175). Wilson distinguishes two forms of altruism he labels 'hard-core' and 'soft-core'. 'Hard-core' in Wilson's terminology corresponds to kin altruism, dealt with in the previous chapter. He states that one would "expect hard-core altruism to serve the altruist's closest relatives and to decline in frequency and intensity as relationship becomes more distant." (ibid,p.162). He contrasts this with soft-core altruism which "in contrast, is ultimately selfish." (ibid,p.162). Once again, the individual need not even be aware of the 'ultimately selfish' nature of their behaviour, believing falsely that they are behaving in a genuinely altruistic way (just how this is possible within an evolutionary framework will be examined in detail below).

Somewhat ironically, it is these forms of behaviour - ultimately and necessarily selfish so Wilson and others tell us - which form the cement of human society, involving as it does many forms of social intercourse, ritual and performance.

But in human beings soft-core altruism has been carried to elaborate extremes. Reciprocation among distantly related or unrelated individuals is the key to human society. The perfection of the social contract has broken the ancient vertebrate constraints imposed by rigid kin selection. Through the convention of reciprocation, combined with a flexible, endlessly productive language and a genius for verbal classification, human beings fashion long-remembered agreements upon which cultures and civilization can be built. (ibid,p.163)

X

The same view of reciprocal altruism as forming the essential core of human society is echoed in much of the sociobiological literature, notably in the works of the man most responsible for developing the notion, R L Trivers:

There can hardly be any doubt that reciprocal altruism has been a potent force in human evolution. The emotions of friendship, moralistic aggression, gratitude, and sympathy, as well as our sense of fairness, probably arose as mechanisms to regulate reciprocal altruism. (Trivers,1985,p.393).

Both Dawkins and Wilson also suggest that, whatever their specific forms of regularised exchange and complexity, human economic systems may also be seen as quantifications of the human behavioural propensity for reciprocal altruism:

As a result only man has an economy. His high intelligence and symbolizing ability make true barter possible. Intelligence also permits the exchanges to be stretched out in time, converting them into acts of reciprocal altruism. (Wilson,1978,p.551)

Money is a formal token of delayed reciprocal altruism. (Dawkins,1978,p.202)

According to the sociobiological account, hard-core altruism imposes limits on individual cooperative behaviour along the lines of genealogy. In order for individuals to go beyond such limits, a mechanism must have evolved which, although still ultimately self-serving, at least gives the opportunity for an exchange of cooperative acts between non-kin. Such a mechanism, we are told, operates via the genetic inheritance by every individual of emotional and general cognitive responses which guide behaviour towards the 'calculation' (be it conscious or otherwise) of reciprocal relationships with others. Thus, the ultimately selfish individual may, to an extent, be prepared to indulge in cooperative behaviour towards certain other individuals, provided that such acts eventually secure some degree of advantage (in the sense of being 'owed' some future favour). As for those not coming even within this limited range of recognition and reciprocation, they will be subject to behaviour governed by the unsympathetic and self-protective reactions of territoriality and xenophobia. It is no wonder that Wilson describes the collective state of an individual's social relations as "a melange of ambivalence, deceit, and guilt that continuously troubles the individual mind." (Wilson,1978,p.166)! Seemingly trapped within a schema where all behaviour is tied to the evolutionary logic of self-serving individuals, Wilson bites the bullet and asks the most important question about human behaviour:

Can the cultural evolution of higher ethical values gain a direction and a momentum of its own and completely replace genetic evolution? (ibid,p.175)

Perhaps not surprisingly, his answer is in the negative, reflecting once more the premise of sociobiological reasoning that genetically heritable

limitations on human behaviour overrule the possibility of change due to factors of social environment, the self-questioning nature of human reasoning, or man's apparently unique ability to create his own goals, ethical values and standards.

The genes hold culture on a leash. The leash is very long, but inevitably values will be constrained in accordance with their effects on the human gene pool.
(ibid,p.175)

Any utopian vision of a society in which individuals regularly perform acts of a genuinely altruistic nature is, in the sociobiologist's view, not a possible reality for human beings. There is just no room for the use of the term 'altruism' in its literal everyday sense. Once again, we are told that saintliness, or even simple charitableness, are either an illusion, or else exist in such scarcity as to be relegated in importance to the level of behavioural exhibits in some freak show (as Tennant & von Schilcher suggest, somewhere between the cannibals and the necrophiliacs).

The sociobiological discussion of human relations in terms of reciprocal altruism obviously has strong similarities to the earlier discussion of kin altruism. Once again, it is argued *a priori* from evolutionary theory that there is no such thing as genuine altruism, therefore all appearances of altruism must be reinterpreted in the light of the theoretical premise. All behaviour is to be explained as ultimately selfish, even contrary to the claims of the agent involved, since they are not necessarily aware of the genuine motivations or the functions ultimately served by their actions. Once again, the same counter-claims against versions of psychological egoism can be made, and are made by Mary Midgley in an attack on the theory of reciprocal altruism in the human context (see Midgley,1979,pp.117-142). Midgley also claims that

individuals involved in behavioural decisions concerning potential altruistic behaviour (e.g. in the 'drowning man' scenario) do not have time to make the calculations of profit and loss necessary if actions are really to be undertaken in order to secure consequential advantage for the agent. Such a calculating species might have evolved, she says, but we are not it. However, I do not think that Midgley's argument goes far enough to combat the sociobiologist. She may be right to claim that actions such as jumping in to save a drowning person are too instinctive to be the result of cool calculation of the situation, but this is precisely where the sociobiologist would claim that the evolutionary aspect of behaviour comes into play. The individual need not consciously calculate anything at all for their resultant behaviour to coincide with the expectation of such calculations, were they to be made. Their reaction is supposedly the genetically heritable result of the selection of their forefathers' reactions, and by the logic of selection and the evolutionary process, only the more 'advantageous' reactions survive to be passed on to future generations. Those individuals who by nature tended to jump in to save anyone, regardless of possible return of favours, would also tend to be eliminated in competition with those whose behaviour coincided more with the calculable but not necessarily calculated expectation of return of favour. So what appears as a largely instinctive, uncalculated response by an individual is in fact the result of selective inheritance of accumulated ancestral 'wisdom'. No one individual need carry out any particular calculation, as Midgley seems to think is being suggested, for the end result of the natural historical process of evolution by natural selection to be identical or very nearly so to the product of conscious calculation. No honey bee ever sat down and calculated that the figure of the regular hexagon was the answer to the problems posed by hive construction. It was

an answer reached not by calculation but by evolution via random mutation and natural selection. Exactly the same argument applies to the evolution of human behaviour, and undermines Midgley's objection that individuals cannot be acting for their own consequential advantage because this would involve too much calculation, whereas classic examples of human 'altruism' show reactions to be largely instinctive.

If the position of egoism which Midgley is attacking depends on behaviour being defined as consciously or intentionally selfish, then her objection to the sociobiologists would indeed stand, since it is not necessarily the sociobiologists' claim that all behaviour is selfish in an intentional way. This would appear to be the basis of Midgley's objection to Dawkins' use of the word 'selfish' in describing the non-conscious behaviour of entities such as genes. Under such a definition, behaviour could be both self-serving (being the product of evolutionary forces which necessarily make it so) and yet at the same time not be defined as selfish, since it is not intentionally so. However, such a dispute would seem ineffectual, since it merely turns on the definition and correct use of the term 'selfish' , without affecting the fundamental evolutionary view of human behavioural limitations, or the denial by others that human behaviour is no longer subject to the dictates of evolutionary logic. In contrast to Midgley's line of attack on the sociobiological stance on human altruism, I would therefore propose an alternative line of argument, which actually incorporates the sociobiological insistence on evolutionary considerations.

I would begin by agreeing with the claim that where behavioural response and regulation is tied to the factors of resource acquisition and

competition with other organisms, altruism in its literal sense contradicts evolutionary logic. Where behaviour is a matter of instinct plus rule-governed learning capacities, there is no place for behaviour which is other than selfishly advantageous. Is there any reason to think that human behaviour is to be discriminated from such a general model? I think that there is, and my argument rests on the human faculty for self-conscious reflection, and the related capacities for reasoning, conceptualisation and linguistic ability.

Humans do not appear to be in the same position as other species, in merely being presented with a world of perceptions through sense awareness, plus a set of more or less specific instinctive guides to behavioural reactions. Humans have the further capacity for self-awareness, for which I believe there is no evidence in other species. Given a certain situation, we may examine our own initial reactions, feelings and impressions before acting. We may look back on our own past behaviour almost as a third person observer, and examine our behavioural tendencies with a view to altering or at least acting with an awareness of them. In this way, present choices appear to be under a greater degree of control, and stand in a more autonomous relation to past behaviours than in other species, where present behaviour is more simply and obviously traceable to a previous succession or pattern of events. On top of this, we can use imagination and other conceptual faculties such as abstraction to create goals towards which behaviour may be directed, rather than it being a simple product of a previous chain of behavioural reinforcement. All of these modes of thought are possible intermediary stages between initial perception of a situation and action, and may be further drawn out by

communication with others, and informational exchange with resultant effects on decision making.

Presumably, since no other species shows signs of such capacities, they are a later product of evolution than the faculties employed in simpler stimulus-response type behaviours, which we share in common with other species. However, the sociobiological analysis of such faculties renders them useless or functionally impotent, since the insistence is that despite such capacities, our behaviour is still chained to the limitations and logic of the phylogenetically earlier non-self-reflective forms of behaviour. The additional attributes of self-consciousness, reasoning and conceptual communication with ourselves and others furnish us with no behavioural capacities beyond those already enjoyed by the rest of the natural world of organisms that lack such attributes. Any theory to the contrary, we are told, is the product of ignorance or self-delusion.

It is at this point that I feel obliged to raise the question: given the usual functional economy of nature, why the unlikely evolution of a complex structure like the human brain and its attendant faculties if, so we are led to believe, much of it is in effect redundant? (This question will appear again in my discussion of theories of mind and especially epiphenomenalism. See below, pp.279-291.) After all, we certainly have the subjective experience of being able, within limits, to reflect on our past and attune our future behaviour to goals of our own making which again have at least the appearance of being in some instances genuinely altruistic. This is backed up by the common observation that organisms appearing later in the phylogenetic history of life on this planet typically display behaviour which is more 'open' and geared to flexibility

of response, rather than being fixed to a rigid pattern of reflexive responses to given stimuli. If self-consciousness and the attendant faculties mentioned above cannot supercede or expand upon the behavioural goals and limits set by mere sentience alone, then what possible purpose can such complex capacities serve, and why do they provide us with the illusion of abilities which, we are told, we do not and cannot possess?

One possible line of reply to this is to claim that the subjective experience or illusion of genuinely altruistic forms of behaviour which are contrary to the self-serving manipulations of the genes is in itself a sufficient explanation of its own existence. Such an illusion furthers group cooperation and instances of 'altruism' which - though in reality self-serving - have the effect of raising overall fitness levels in the group. Thus, the illusion of altruism is still more adaptively advantageous than comparable states of awareness which lack the illusion and resultant behaviour. Such a line of argument is pursued in an article by Joseph Lopreato (Lopreato,1981). He begins by stating that he does not have an evolutionary answer to the question of why the capacity for self-deception should have arisen in the first place, declining to speculate on its function.

I do not hypothesize about the evolutionary circumstances that led to self-deception. But it is reasonable to argue that, whatever the origin of self-fraud, once it was evolved, it developed a rich repertoire of mischievous tricks. (Lopreato,1981, p.121)

However, despite this unpromising start, Lopreato does in fact go on to suggest a functional explanation for the evolution of self-deception and illusions about our own behaviour:

As consciousness reached higher levels of complexity, the curiosity instinct...became thoroughly restless. Having pried into the nature of external things, it gradually turned on the mind itself to inquire about the nature of thoughts, desires, fears, self-centeredness....Could introspection, under the influence of the need for approval, have threatened to reveal that the "evil" of others was in fact lodged in one's own being? (ibid,pp.121/122)

Like the fall of Adam and Eve brought about by the knowledge of good and evil, the evolution of self-consciousness brought with it the potential for recognition of our own imperfections and the fundamentally selfish nature underlying all our behaviour: in order to survive such knowledge a further device - that of self-deception - evolved to cope with it. Lopreato goes on to suggest how the ability for self-deceit could in fact have become the starting point for genuine cases of altruistic behaviour, in which the selfish manipulations of the genes become perverted by aspects of human culture, and finally lose their original goal of maximising inclusive fitness levels. However, all of this is based on the original premise that the subjective experience of our own behaviour, and our readiness to attribute to others qualities of altruism is in fact an illusory trick, played on us by our evolved cognitive structures to protect us from the truth of our own and others' essentially 'evil' nature. Thus, sociobiology provides an explanation of the last stumbling block to acceptance of their theory: those who deny the 'selfish' nature of all behaviour on the grounds that we at least have the subjective experience and concept of altruism can now see why such an experience evolved, and how it falls short of constituting a counter-argument to the sociobiological thesis.

Such an argument may have an appeal of sorts, in that it appears to be facing up bravely to a harsh reality - that of the essential and not very commendable nature of human behaviour. In so doing, it automatically

labels any counter-argument an instance of self-deception or fantasy, whilst satisfying itself with the fact that it has even provided an explanation for the occurrence of opposing arguments, and the reluctance to accept the 'truth' of its own conclusions. However, I believe that the above position on the possibility of genuine altruism in human behaviour and the explanation for the evolution of self-deceit raises more questions and confusion than it provides answers.

Firstly, why should self-deception evolve in the way in which Lopreato suggests? Given that all species have evolved behaviour which is essentially and necessarily self-centred, why should individuals develop a psychological mechanism whereby they can pretend that - in their own case at least - behaviour is sometimes motivated from genuine altruism? Where and why did the concept of 'altruism' arise for it to be held as a standard, against which the ancestors of modern-day egos were supposedly forced to contemplate the essential 'evil' nature of others and their own being? Deception implies at least a duality of concepts, if not of actually possible behaviour - but we are offered no explanation as to why the concept of genuine altruism should have evolved within a 'selfish' species, thereby bringing about the desire to mask one's true nature even from oneself, by pretending that it conformed to one description rather than another. Why should a concept such as altruism ever come about in the total and necessary absence of any possible examples of such behaviour?

Instead of the above questions which remain unanswered by the sociobiological story, one can imagine a different account of the evolution of human social behaviour. Rather than labelling the subjective experience of genuinely altruistic motivation a case of 'self-deceit', and constructing

ever more complex explanatory devices for incorporating 'supposed examples' of altruism within a schema which designates all behaviour selfish from an *a priori* standpoint, why not just allow that sometimes humans do in fact carry out actions which are genuinely altruistic? The sociobiologist admits that some actions *appear* to be altruistic, but then remembers his fundamental premise about the possible evolution of certain kinds of behaviour, and quickly constructs a further piece of theory to accommodate the new observational data within the original framework. However, as I have pointed out above, the sociobiological account raises more questions than it provides answers, whilst leaving totally unanswered the question concerning the evolution of a complex central nervous system, providing capacities for behavioural reactions which would appear to be 'impotent' in the face of the all-controlling logic of the selfish genes. The reader is asked to believe that not only is all behaviour geared to the 'ruthlessly selfish' logic of genetic competition, but that the primary function of the evolution of the human brain and nervous system, with all its attendant faculties, is to provide us all with comforting but wholly false illusions about ourselves and others, so that we feel better in our daily existence and go about our selfish lives in a more reciprocally manipulative and therefore overall advantageous way.

Once again, the sociobiological account of human behaviour rests on a dogged adherence to one or two basic premises, whilst refusing to allow that perhaps not all instances of behaviour are tied to or can be described within the framework of such premises. Instances of behaviour such as help offered by individuals under circumstances where there can be no possible expectation or opportunity of reciprocal benefit are dismissed as 'freakish' or given an unproven and unprovable Freudian-type analysis

in terms of unconscious motivations. Questions about why we entertain concepts which, we are told, have no possible reference in reality, are given partial and unsatisfactory answers in terms which have the effect (even if not by design) of raising *ad hominem* doubts about the willingness or capacity of the questioner to face up to the reality of true self-knowledge. Finally, the reader is asked to believe that self-awareness, surely one of the latest and most complex products of evolutionary development, evolved to provide the individual with a view of themselves and others which is systematically distorted in its essential categories of behavioural assessment, and that it is only through an appreciation of this state of affairs that one can come to a true evaluation of the nature and significance of human behavioural relations. In the face of such theorising, I can only appeal to alternative explanations which begin by accepting that at least some instances of behavioural assessment, such as are commonly made, are in fact correct, and that one need not construct a complex and (in my view) at times wholly implausible explanatory framework in order to 'explain away' the host of examples which do not appear reconcilable with the fundamental premise of one's position. It is not perhaps a kind of 'cowardice' which prevents many people from accepting the view of human nature presented by sociobiology; it is perhaps an adherence to 'reasonableness' which, in the absence of concrete evidence or more coherent and convincing argument, dismisses the fundamental explanatory views of sociobiology as unnecessarily limited and distorting when applied to the field of human social behaviour.

3.2 Power Politics and the Evolution of the Selfish State

Many sociobiological texts claim that their theories can be used to shed light on the processes involved in the sphere of human international relations, and the political institutions which govern this area of sociopolitical behaviour. The basis for their claim comes from the observed tendency of individuals to discriminate others and classify them according to some criterion of 'in-group/out-group' membership. This is supposedly a basic psychological trait of cognition common to all individuals, subsequently affecting many aspects of social relationships. Having thus divided one's perception of others, the two groups are treated according to very different behavioural standards, leading to behaviour which is described by the related terms of 'ethnocentricity' and 'xenophobia'. Wilson, for example, forwards just such a view of human behaviour in his discussion of the supposed universality of organised warfare:

The force behind most warlike policies is ethnocentrism, the irrationally exaggerated allegiance of individuals to their kin and fellow tribesmen. In general, primitive men divide the world into two tangible parts. (Wilson, 1978, p.114)

In a collection of papers dedicated to exploring the implications of this aspect of sociobiological theory, entitled The Sociobiology of Ethnocentrism (Reynolds, Falger & Vine, 1987), the general point forwarded by Wilson above is developed at some length. Falger states the main issue raised by a sociobiological approach to the analysis of international political relations in his paper entitled "From Xenophobia to Xenobiosis?":

The purpose of this chapter is to deal with the question why deterrence exists at all - quite a standard question in international relations theory - and whether this typical group-behaviour pattern has

a longer history than the period since the first states were formed; this is a question which is most likely to be answered using theories and hypotheses developed in evolutionary biology. (Falger,1987,p.235)

This kind of approach to the topic does not hope to elucidate any particular state of affairs; that can best be achieved by reference to proximate factors of the history and culture of the parties involved. Rather, the aim is to discover if there are any behavioural patterns and identifiable 'forces' of an ultimate and more general kind, which underlie and shape to some extent the nature and form of all political relations, no matter what the differences at the level of localised proximate factors. I believe that there are two senses in which Falger's arguments attempt to form a link between theories of international political relations and aspects of evolutionary theory.

The first sense is that in which the system of international relations, and the processes which bring about the changes in such relations are seen as constituting an evolutionary system, essentially analogous to that which operates at the level of the biological evolution of organisms and species. Falger begins his analysis by expounding a realistic theory of international relations, in which the absence of any international governing body results in a state of semi-anarchy between nations. Every national group involved in such a situation attempts to secure and maintain its own status within a competitive and potentially hostile environment of equally self-motivated contenders:

In any self-help system, units worry about survival, and considerations of security will subordinate economic gain to political interests. (Marxists see it just the other way around.) (ibid,p.237)

This picture of selfishly competing international groups aligns the balance of power theory of international relations with evolutionary theory. The analogy between the biological system of evolution by natural selection and power politics is made even more compelling when we are told that the development of international relations is, from one perspective at least, more due to non-conscious forces of competition and their outcome than to the conscious planning of individuals and political groups. Changes at the level of international political relations appear to be as much a result of 'blind' forces of natural selection within a competitive environment as the biological evolutionary changes occurring within and between competing species:

This theoretical perspective distinguishes and dismisses itself from the common-sense conviction that international politics is the outcome of an ideological struggle and political will. In terms of the foreign policy of a national state, political leaders and their self-ascribed power are not unimportant at all. But seen from a longer historical perspective their influence appears to be more bound up with the internal and external structure of power relations, in which every (competitive) interaction among social groups takes place, than the other way around. (ibid,p.240)

Falger goes on to expound and support the theories of R D Alexander (Alexander,1979), in which it was argued that the first nation-states evolved as a result of a natural process (i.e. with no need for conscious foresight or planning from political leaders), changing from smaller to larger and more complex groups. This process of evolution towards increased size and complexity of internal and external relations supposedly came about as a result of a combination of two basic and opposing forces. The first was ethnocentrism, which became more acute because of the increased need for solidarity and group strength and identity in the face of external competition and threat. The second force

was that of xenophobia, the growth of which is an obvious counterpart to the increase of ethnocentricity, and accounted for changes in the relations between competing groups. Thus, at the level of intergroup relations, the view supported by Alexander and Falger is that of an autocatalytic process of change, subject to forces of increasing competition, intergroup diversification, and differential survival by natural selection. The increasing pressure of defence needs and stability within a competitive environment, building upon the supposed genetically heritable tendencies towards ethnocentricity/xenophobia, lead naturally to the formation of warlike groups and an emphasis on organised defensive strategies of behaviour, thus maintaining an uneasy balance of power.

Such self-reinforcing processes, in combination with suitable ecological environments in several regions in the Middle East, and evolving technological capabilities, gave birth to the first nation-states. (ibid,p.244)

Such a view of the evolution of international relations between the earliest human civilisations, and its subsequent effect on the continuing course of human history, is also forwarded in very similar terms by Wilson, who explicitly acknowledges the extension of evolutionary theory to explaining the origins and continuing phenomena of political relations:

Primitive men cleaved their universe into friends and enemies and responded with quick, deep emotion to even the mildest threats emanating from outside the arbitrary boundary. With the rise of chiefdoms and states, this tendency became institutionalised, war was adopted as an instrument of policy of some of the new societies, and those that employed it best became - tragically - the most successful. The evolution of warfare was an autocatalytic reaction that could not be halted by any people, because to attempt to reverse the process unilaterally was to fall victim. A new mode of natural selection was operating at the level of entire societies. (Wilson,1978,p.119)

The first sense in which the rise and relationships of nation states is a supposedly suitable topic for investigation by evolutionary theorists is therefore by analogy to the processes of biological evolution by natural selection. The historical process of state formation, and the subsequent relations between states can be captured and explained using models and theories already developed to describe biological evolution. However, this is not the only sense in which evolutionary theory may be thought to have some relevance to theories of international relations.

There is a further, and in my view more contentious sense in which political theory and the biological sciences may be linked. This is in the above-mentioned reference to human individuals having a genetically heritable tendency towards classification of others, and resultant ethnocentric and xenophobic behavioural reactions, which supposedly lie at the root of all group formation and the identification of others as 'outsiders' and therefore potential enemies. This is the behavioural premise upon which the theory of the natural evolution of warring states is based:

international politics is one prominent area where in-group/out-group manifestations are overwhelmingly present, and, [] it will be argued that it is here that the connection between biology and the study of international politics is most promising. (Falger, 1987,p.240)

When this view of human behaviour is added to the existing sociobiological descriptions of the supposed limits to human cooperation - the biologically-dictated restrictions and true significance of 'altruism' - the resultant picture of human potential in the international arena is not exactly promising. The references to the continuing existence of arms races, mistrust, and the often limited and explicitly reciprocal nature of

any international agreements are all used as evidence in support of the view that we are as a species limited to just such forms of behavioural relations, not because of any lack of good will on our part, but because of our genetically endowed behavioural range. This is coupled to the emphasis on the 'autocatalytic' nature of power struggles, which reinforces a common theme in sociobiological writing - that of the insignificance of consciously-motivated action in a world where most changes are really brought about by forces which are non-conscious, and beyond any kind of conscious manipulation by ourselves. The development of human history is largely seen from the perspective of a nexus of impersonal forces operating on and within human society, resulting in changes which individuals can do little to alter or even comprehend.

Falger does not, however, accept the supposed inherently xenophobic nature of man's behaviour without some discussion of the grounds for such a view. This involves posing the question of whether xenophobia is a product or a precursor of post-agricultural society. Did the nation states emerge because of tendencies towards xenophobia, or vice versa? Some theorists (e.g. Montagu, 1976 and Reynolds, 1980) have argued that there is no archaeological evidence for high levels of violence in pre-cultural man. This argument is backed up by further anthropological evidence of the peacefulness of many contemporary hunter-gatherer societies. However, Alexander and Falger both take the opposite view in this debate. They argue that unless there is positive evidence of a lack of warlike violence in pre-cultural man (though one wonders what such evidence would look like), it remains in their view likely that modern behavioural norms can be extrapolated backwards into our pre-cultural history. They conclude that it is 'unlikely' that xenophobia is a product of cultural existence,

and that it pre-dates and explains the emergence and continuing relations of states and national groups.

So, although it will never be possible to prove it definitely, the model of in-group/out-group behaviour is so extremely widespread and tied up with competition, conflict and cooperation that one can safely use it as an axiom in theories about international politics of past and present. (Falger,1987,p.245)

The theory does not stop at using the supposed genetically heritable characteristics of ethnocentricity and xenophobia to explain the sphere of international relations: Falger believes that such characteristics also explain many of the features common to the internal social structures of human groups, thus providing a possible link between the natural and the social sciences:

Alexander's hypothesis on the rise of the nation-state - depending on intergroup competition *and* on the maintenance of balances of power between human groups with the increase of those groups - seems to be the most powerful explanation bridging the factual and theoretical gap between present time and man's long prehistory, and between the realm of the social and evolutionary-biological sciences. (ibid,p.246)

Thus, after a brief comparison of the contending theories, Falger answers the question he posed at the start of his paper: in his view international deterrence strategies do in fact pre-date the historical emergence of the first nation-states, and they conform to the basic behavioural characteristics of ethnocentricity and xenophobia which are supposedly genetically heritable behavioural patterns in humans. This view is also the underlying premise of Alexander's theory of human history. Even if the word 'cooperation' is brought into Falger's discussion of political relations, the arguments already presented by Wilson and others would immediately translate it into 'reciprocal' or 'kin' altruism, leaving one

with the view that international relations are at best a process of mutual exploitation, and at worst an inevitable arena of threat, suspicion, counter-threat and irrational aggression, necessarily beyond any individual's comprehension or control.

In the light of the above, it is somewhat surprising to find Falger stating his hope that, through an appreciation of the true nature of our behaviour, we may come to change it via rational means of control. He looks forward to the possibility of groups mingling and sharing one another's resources - a utopian state of human relations which he compares with the cooperative relations found between some species called 'xenobiosis'. Such a surprisingly optimistic coda is to be found in other texts which reach similar conclusions to Falger, and raises important questions about just what is being stated in the final analysis.

Even if ethnocentrism and group conflict have their origins in our evolutionary past, and even if our genes predispose us to make in-group/out-group distinctions and behave accordingly, we can hope at least that through greater knowledge and understanding we can substantially overcome these tendencies. (Falger,1987,p.271)

The obvious conclusion is that love and friendship should embrace all humanity....nevertheless, made as we are, we are unable to obey it [this commandment]. We can feel the full, warm emotion of friendship and love only for individuals, and the utmost exertion of willpower cannot alter this fact. But the great constructors can, and I believe they will. I believe in the power of human reason, as I believe in the power of natural selection. I believe that reason can and will exert a selection pressure in the right direction. (Lorenz,1963,p.258 - entitled 'Avowal of Optimism')

We are forced to choose among the elements of human nature by reference to value systems which these same elements created in an evolutionary age now long vanished...Fortunately, this circularity of the human predicament is not so tight that it cannot be broken through an exercise of will. (Wilson,1978,p.203 - entitled 'Hope')

All of the above three statements seem to raise serious doubts as to the conclusion of the sociobiological analysis of human social relations. I would argue that, given their accounts of man's tendency towards xenophobia and aggression, to which "our genes predispose us" (Falger), added to the historical dimension of culturally entrenched identities and perceptions of national groups and boundaries, it is difficult to see how one can still have hopes of enlightened behaviour and social change brought about through rational control. Surely, from their own premises concerning human behavioural tendencies, it must be doubtful (to say the least) that anyone would feel a sufficient natural urge to invest their energies in a project of group reconciliation. We have also been forcefully reminded of the likely consequences of a unilateral lowering of one's defensive strategy, which in a balance of power situation is tantamount to national suicide.

To hope that enlightened action can transcend the evolutionary roots of human behaviour as they have been described is, in my view, to attempt a U-turn on their own previous pronouncements. At other places in the writings of Wilson we are told that our rationality is slave to our instincts, and that our instincts are tuned over millions of years of evolution to the balance scales of selfish advantage. Lorenz also sides with Hume against Kant in the debate over the strengths and relative functions of the passions and reason. Dawkins has told us "We are born selfish". Where then in all this system is the genuine motivation or capacity for escape from selfish competitive rivalry supposed to originate and gain a secure foothold, at any level of behaviour from individuals to national political consciousness? Falger begins to visibly falter in his

conclusions as to the practical relevance of his theoretical arguments about the origins and nature of national consciousness and aggression in human society:

However, to be completely honest: in explaining *today's* existence of 'mutual deterrence' or the 'balance of terror', we do not need evolutionary biology. The structural theory of power politics itself contains enough to do that job. (Falger, 1987, p.247)

This is juxtaposed with a passage on the same page which would appear to present a different kind of conclusion:

In general, a more biologically oriented political science could contribute substantially to a more fundamental analysis of political life. The emergence of *biopolitics* is promising in this respect. (ibid, 1987, p.247)

I believe that Falger, Wilson, Lorenz and others have created a paradoxical situation by their theories, from which they wish but find it very difficult to extricate themselves. If sociobiological theory is false, or does not imply the kind of strength of heritable behavioural dispositions which have so far been implied, then it can have little or no relevance to possible future changes in society. As a false or partial theory whose predictions can be overturned by effects described in some other area such as social history, its pragmatic value is minimal or nill. If, however, it is a true theory whose predictions can be made largely or wholly without regard to factors such as those cited in the traditional social sciences, then it would appear to offer a vision of a species which, short of a Baron von Munchhausen feat of self-preservation, is incapable of escaping the behavioural dictates of its evolutionary heritage. Thus, the truth of sociobiology would also seem to render its pragmatic value nill. Such a conclusion is at odds with the professions of Wilson, Lorenz and Falger,

who all try to assert that despite their own rather bleak characterisation of the necessary nature of human behaviour, they can still offer a degree of optimism for the future.

In my view there is a way out of the apparent dilemma above, based on my discussions of the human capacity for altruism. The sociobiological theory of human relations within and between societies is based partly on the theory of supposed instinctive tendencies towards ethnocentricity and xenophobia, and partly on the supposed limitations to genuine cooperation described by the theories of kin and reciprocal altruism. The theory that xenophobia is an instinctive predisposition, rather than a product of the social climate produced by the rise and establishment of nation states is, according to Alexander and Falger, established by a *lack* of evidence to the contrary. This, in my opinion, will surely not do as an acceptable argument on which to base their theory. Firstly, what would positive evidence of a 'lack' of warlike behaviour in our pre-cultural ancestors look like? The existence of war and battle tends to leave more of a visible trail than its peaceful counterpart. A lack of evidence sufficient to decide the matter must mean just that: it cannot be used as an empirical premise to establishing the sociobiological theory. If the alternative theory states that present behaviour is the product and not the precursor of cultural society, then that same behaviour cannot be cited as evidence for its existence prior to the emergence of culture, by using 'extrapolation' when it is the very validity of such extrapolation which is in question. To argue in such a way, as Alexander and Falger do, is to commit a very basic and in my view obviously circular error in reasoning, and one which must cast doubt on the presumed impartiality of their research.

Even if one were to accept some sort of genetically heritable behavioural predisposition towards xenophobia, this might be amenable to change via the avenue of altruism and cooperative behaviour, leading to a breakdown of the instinctive interpersonal barriers. But, of course, this avenue is a cul-de-sac for sociobiologists: all altruism is ultimately selfish, at best an exchange of favours surrounded by deceit and mutual manipulation. If, as I argued in the sections on altruism, this were not in fact the case and genuine altruism were a possible behaviour for our species, then the inescapability and urgency of sociobiology's characterisation of the necessary nature of human inter- and intra- group relations would be to a large extent diminished. One might accept that in the early history of mankind, international relations developed according to an unplanned system of anarchic balances of power, subject to evolution by natural selection of aggressive dominance. One might even accept that there is an instinctive tendency towards favouring the group with whom one identifies, and that all foreigners are at first an object of suspicion. However, none of this implies that history must continue to develop in such a way, given the improvements in international means of communication, migration, transport and factors such as economic interdependence. Neither does it imply that any tendencies towards xenophobia are ineradicable, since the converse behaviour is ethnocentrism, and all that divides the 'friend' from the 'foe' is their identification. Improvements in this area can be brought about through education, and once again international means of communication and travel may play a major role in bringing peoples to recognise each other on a friendly basis. As already noted, the sociobiological account seems to suggest that identification of others and the resultant behaviour is largely an instinctive process, rather than taking into account the possibilities of

change through learning: it posits once again behavioural characteristics which are so deeply embedded in our nature that they are not amenable to the kinds of change I have suggested. And this is despite the authors' unanimous efforts to inject a little optimism into the concluding part of their writings.

If one accepts the above arguments I have presented against the necessity of the past and present climate of human relations, it would follow that a true and complete understanding of them must be formulated not in terms of necessary ultimate forces and their results, but rather in terms of contingently historical proximate forces. Such a view may still not assert the complete freedom in a libertarian sense of individuals and groups, since it may well identify limiting forces which are the product of social history itself. (Such is the view of traditional Marxism to be discussed in the next section.) However, even if individuals and groups were seen to be under a degree of limitation from pervading sociohistorical forces, the emphasis of explanatory principles would still be shifted from that of ultimate biological mechanisms incapable of rapid change, to that of proximate sociohistorical forces, whose heritability is due to different mechanisms and which are subject to more rapid changes. This is not to deny that there are features of human psychology which we owe to the evolutionary history of our species. It is, however, to deny that such factors can play anything more than a peripheral and perhaps pragmatically empty role in questions of present sociopolitical relations, and the possibility of future change in such states of affairs. This conclusion is at odds with that initially presented by the sociobiologists, though it does seem to come closer to their concluding remarks, suggesting that perhaps they too see a greater role for genuine altruism and

cooperation guided by reason, despite their earlier denials of such a possibility.

3.3 The Debate between Sociobiology and Marxism

Sociobiology has received a great deal of critical interest and comment from representatives of many different disciplines and backgrounds. Perhaps the most widely publicised of these have come from self-professed left-wing political activists, whose form of attack has ranged from articles in journals, books and the popular media, to personal verbal and even physical abuse of the leading proponents of sociobiological theory. An example of an extended critique of sociobiology and related topics, written from a sociopolitical perspective is the book Not In Our Genes (Rose, et al,1984). The authors of this work, who proclaim their political sympathies in the introduction, carry out a Kuhnian-type analysis of the sociopolitical climate surrounding the conception, development and reaction to sociobiological theory. Their aim is to discredit its scientific credentials by proving its status as a socially divisive misuse of science, which is upheld more by its furtherance of a given sociopolitical ideology than by any actual scientific evidence or theory. In their view sociobiology is an integral part of a broader philosophical and political viewpoint - a viewpoint they label 'The New Right' - and to which as committed socialists they are in total opposition.

From a historical point of view, it is of course no surprise that sociobiology should be regarded by many as an inherently suspicious line

of thought. Evolutionary ideas applied to human society and social ideologies were, after all, used in a crude form to give pseudo-scientific justification for right-wing ideologies of Social Darwinism in America and Europe from the last quarter of the nineteenth century onwards. Some commentators on this topic have even suggested that the major part of the theory of National Socialism in Hitler's Germany was already provided, prior to the rise to power of The Third Reich, by members of the German scientific community, such as Ernst Haeckel. The horrors of the Nazi racist policies were the enactment at the political level of a largely pre-existing theory, supposedly rooted in evolutionary premises which gave an ethical justification from natural scientific grounds for what followed.

It would of course be a gross exaggeration to claim that the German scientific community, especially the members of the biological science faculties, were directly responsible for national socialism.... Nevertheless....It is simply true historically that German academics and scientists did, in fact, contribute to the development and eventual success of national socialism, both directly through their efforts as scientists and indirectly through the popularisation or vulgarisation of their scientific work. (Stein,1987,pp.266/267)

Whether or not one concurs with the view that sociobiological explanations of human behaviour are best seen as a "political manifestation of biological determinism....directly opposed to the political and social demands of those without power." (Rose,et al,1984,p.21), I believe that one can see how sociobiology poses a direct threat to the theoretical basis and practical aims of Marxist Socialism. In this section I intend to show that, even if there had been no such historical link between evolutionary theories of human nature and right-wing political ideologies, sociobiology would for reasons of fundamental theoretical

opposition alone continue to draw heavy fire from the artillery of left-wing commentators.

To appreciate the basis of the opposition between sociobiology and Marxism, it is necessary to examine and attempt to clarify Marx's own view of human nature and society. To begin with, it is perhaps interesting to note Marx's own reaction to Darwin's publication of the Origin:

Darwin's book is very important and serves me as a natural-scientific basis for the class struggle in history. One has to put up with the crude English method of development, of course. Despite all deficiencies, not only is the death-blow dealt here for the first time to 'teleology' in the natural sciences but its rational meaning is empirically explained. (McClellan,1977,p.525)

Marx clearly greeted Darwin's theory as providing a non-teleological materialist explanation of the origin of human life, thus coinciding with and complementing the overall philosophical perspective of his own theories of human social evolution. However, his interest in Darwin's work appears to have stopped just there: there is no suggestion that Marx considered biological evolution and its laws to have any further relevance to the explanation of man's social history, other than providing a base upon which such further explanation might stand. Darwinian theory provides a materialist account of the origin and possibility of human social history: Marx saw himself as charting the route that possibility then takes, under the control of new forces, properties and laws. As one recent commentator on the relation of Darwin to Marx has said:

Thus, while Marxism recognises the essence of man deriving from his membership in the human species, it draws the attention of researchers to the most important element, namely a concrete-historical analysis of man, and to the identification of specific traits of social relations such as the nature of labour activities. (Frolov,1986,p.91)

Clearly, such a view differs from that of sociobiology. Where Marxism looks for the social traits of human nature in the historical record of human society, the sociobiological analysis of social relations is derived from theories which emphasise properties of man as a particular biological species, and which theoretically precede any actual social history.

On the topic of human nature, as with much of Marx's theorising, it is a difficult and controversial task to attempt a definitive account of what Marx's views actually amounted to. This is partly to do with the fact that the body of Marx's writings covers a wide historical span, and particular topics are often given piecemeal treatment in many manuscripts, requiring a textual collage technique on the part of the reader to gain as complete as possible a picture of the overall view. It is also perhaps difficult in the particular case of assessing Marx's views on the 'essence' of human nature, because his theories amounted to a denial of any such 'essence', if by that term one is to understand some fixed, universal, identifiable quality by which human behaviour can be explained and defined. One of the main sources for the Marxist view of human nature is to be found in the sixth and seventh sections of Marx's Theses on Feuerbach:

Feuerbach resolves the religious essence into the human essence. But the human essence is no abstraction inherent in each single individual. In its reality it is the ensemble of the social relations.

Feuerbach, consequently, does not see that the 'religious sentiment' is itself a social product, and that the abstract individual whom he analyses belongs to a particular form of society. (Elster,1986,p.22)

Marx's criticism of Feuerbach reveals two main elements of his own theory of human nature, and the means by which in his view it can be properly investigated. Firstly, there is no 'essence' of human nature as such to be discovered within any given individual, taken as an isolated member of the species *Homo Sapiens*. Rather, the essence of human nature only appears and can only be discovered as a relational phenomenon: the essence of human nature as that thing which distinguishes humans from other species is to be found in the specific forms of social relations which groups of humans naturally develop. The second point is logically connected to the first: namely that analyses of human nature cannot be undertaken via a process based on methodological individualism. Such a methodology severs or ignores the social relations actually in existence between individuals, thereby theoretically destroying the very processes and locations at which - according to Marx - man's essential nature reveals itself. Human nature cannot therefore be deduced as a set of properties inherent in each and every individual seen from a biological point of view: it can only be discovered by the method of historical induction from observation of actual social groups and their particular activities and relational structures. This amounts to a firm denial of any theory of human nature which attempts, as sociobiology does, to derive in a reductive fashion an account of essential social characteristics of humans from a knowledge of individual psychology.

it should be emphasised that Marxist-Leninist theory has not simply shown the significance of social factors operating *together* with biological ones.... Instead, Marxist-Leninist methodology determines the significance of social methods for studying man, thus opposing biological tendencies in the course of which a scientifically unjustified reduction takes place, in which essential properties of man that appear in his integrated biosocial existence are reduced to individual aspects of man as a living tangible and sensing being. (Frolov,1986,p.100)

Because of the wealth of interpretation and revision of Marx's original ideas which has been carried out by successive political reformists and theorists, there is a great deal of debate within the social and political sciences as to whether Marx really intended to deny altogether the existence and use of 'human nature' as a meaningful concept *per se*, or whether he merely intended to outlaw the use of the term to mean some set of non-relational, non-historical, abstracted and fixed tendencies of humans regarded in isolation. (For a defence of the latter, perhaps less crude interpretation of Marx on human nature, see Geras, 1983). What is clear is that Marx's analytical approach to the phenomenon of human social development did not include a regard for biological features of the species as relevant to the explanation of human history in the way in which the sociobiological approach to the same topic does. The superstructure of any given society (which includes political, legal, religious and moral systems, as well as aesthetic and other commonly-held values) is, according to Marx, the result of and in a dialectical relation with the economic base of that society. This in turn consists of the available raw materials, the means of production by which these are converted into usable goods, and the labour relations demanded by such production, including the means of distribution, markets and consumption rates, etc. The vulgar interpretation of Marxism labels such an analysis of society 'Economic Fundamentalism', accusing it of presenting an oversimplified and biased account of social structures by exaggeration of the importance and driving force of the economic base as merely one determinant among many in society. More generous interpretations emphasise the dialectical interactive nature of the relation between base and superstructure, giving the Marxist model a more reasonable appearance.

The main weight of our argument so far has gone into demonstrating that the superstructure depends on its economic foundations. But it is necessary to emphasise the fact that the superstructure operates retroactively on its base, and that both spheres therefore determine each other. (Jakubowski,1976,p.57)

(For a further defence of Marxism against the charge of economic fundamentalism and over-simplicity in relating economic to social and ideological factors of human society, see Tucker,1980,especially pp.46-49.)

Whether one sees the Marxist analysis of human society as inherently biased and crude or not, it is clear from his work that individuals cannot be described except within a given sociohistorical context, from which they derive their particular natures. The most important feature of this context is the structure which controls and maintains the material production of the society, and the relations between individuals which such production necessitates.

As individuals express their life, so they are. What they are, therefore, coincides with their production, both with *what* they produce and with *how* they produce. Hence what individuals are depends on the material conditions of their production.

This production only makes its appearance with the *increase of population*. In its turn this presupposes the *intercourse* of individuals with one another. The form of this intercourse is again determined by production. (from The German Ideology, Elster,1986,p.26)

Beyond the fact that man is seen as essentially a 'social' animal, that naturally forms social bonds and groups, and also naturally and creatively produces goods made from the raw materials of his habitat, Marx does not regard man as having an 'essential nature' in the sociobiological sense of behavioural capacities and limitations shaped by biological evolution and genetically heritable on an individual basis. Rather, the behaviour of

individuals is seen as - to a debatable degree - the product of the society in which they grow up and live, since it is the structure of the social group which determines the possible behavioural tendencies of the constituent individuals. The overall structures of the group are, in turn, ultimately dependent upon the processes and relations involved in the extraction of raw materials and the means of production. As far as Marxist theory is concerned, therefore, the social behaviour of individuals cannot be explained or predicted in abstraction from the given society in which those individuals live, since it is only in the context of social relations that individuals express their nature at all. This point is indisputably asserted by Marxist theorists:

Man's consciousness and thought emerge as a social product, and accordingly are secondary in relation to his social existence. (Frolov,1986,p.92)

Frolov's statement strongly echoes an aphoristic line of Marx's own writing, once again from The German Ideology: "It is not consciousness that determines life, but life that determines consciousness." (Elster,1986,p.28).

To sum up, Marxist doctrine is at odds with that of sociobiology in its major theoretical and methodological premises concerning what one can know about human social behaviour. Sociobiology is committed to methodological individualism in its approach, deducing supposedly *a priori* facts about human nature from evolutionary considerations, from which the macrostructures of society are then both deduced and interpreted. For Marx, however, the social individual does not and cannot exist even in theory except when seen in a given sociohistorical context, the nature of

which can only be known through a historical analysis by a process of inductive observation.

In spite of such fundamental differences between sociobiology and Marxism, they also share certain similarities as theories of human nature. To begin with, as noted in the quote from Marx above, they share an anti-transcendental and non-teleological theoretical stance. They both account for the course of human history with great emphasis on the effects of unconscious material forces, which precede and encompass the development and actions of any particular individual. In Marx these forces are identified as being ultimately economic, whilst in sociobiology they are ultimately the result of biological evolution. Both theories thus picture the individual or social group as developing and acting according to a pre-existing framework of restrictive forces which check and shape subsequent capacities and tendencies. Both theories insist that social planners and political reformers must look to another area - economic history or evolutionary biology - to discover the necessary knowledge prior to analysing or attempting to consciously alter any aspect of human society. To act without regard for such areas would be, in their view, to ignore the factors which are of most significance for the course of human history. Both theories thus place a theoretical limit on the amount of true 'freedom' left for genuinely innovative thought or behaviour by individuals, given their premises concerning the operation upon individual development of impersonal forces which precede and shape individual existence. In this respect, both theories entail somewhat of a paradox as far as the possibility of social change through conscious human endeavour is concerned. This point, already raised in my analysis of sociobiological theory (see pages 177-180 above), is also a prominent feature of

discussions of Marxist theory, where it is argued by some that the Marxist emphasis on economic forces in shaping the history of social change risks reducing the consciousness and actions of individual leaders and groups to mere products of a given deterministic environment. For Marxism as for sociobiology, the accusation of 'determinism' is never far away.

Where the two theories disagree violently is in their respective view of the capacity for human society to undergo radical social change. For Marxists, aspects of contemporary Western society such as inter- and intra-national aggression; nationalism; hierarchical divisions of society according to sex, race and class; paternalism and sexual role divisions; acquisitiveness and material possessiveness, and other forms of social inequality and causes of unrest are all aspects of a particular stage in possible economic and social development - namely Capitalism. Since the superstructure is formed and regulated in accordance with the economic basis of the society, such aspects of social inequality and alienation are seen as phenomena produced and dictated by the prevalent relations of a capitalist labour structure. In the event of Capitalism giving way to an alternative economic structure such as Socialism (a change which traditional Marxists think not merely possible but inevitable) the social relations between individuals would also change. A given social climate does not exist because of the aggregation of individual behavioural psychologies: rather, individual psychology is produced by nurturing within a given social climate. For sociobiologists, on the other hand, the very qualities of human nature which Marxists take to be the transient products of a given socioeconomic epoch, are instead instinctively present in every individual's nature, as a result of our species' evolutionary history and our individual biological heritage. With regard to any particular social

epoch, they constitute a behavioural foundation for social relations, rather than a product of such relations. Whereas for sociobiology the major features of social behaviour are genetically heritable, for Marxism they are socially heritable.

This theoretical division leads to further conflict between the two views of human nature. Marxists are committed by their ideology to creating a society in which the social inequalities and injustices inherent in the capitalist phase of economic development are overcome, and finally transformed into relations of equality and mutual respect. This aim is regarded by them as not merely desirable in a utopian sense, but also practically possible. However, this project of envisioned social reform is denounced by sociobiology, the central dogmas of which are the impossibility of genuine altruism and the inevitability of egotistic competition. Sociobiology constitutes a refutation of the possibility of ever attaining a socialist state of genuine equality and cooperative existence. Sociobiology thus poses a double threat to the ideology of Marxist socialism. Firstly, it undermines the theoretical basis of the Marxist analysis of social relations, by tracing the origin and necessary features of individual behaviour to our species' evolutionary past, and asserting that behavioural tendencies of a fairly inflexible nature were laid down then, prior to the existence or possible influence of any economic forces. Any further social development is therefore constrained within the behavioural boundaries of this pre-economic epoch in the history of our species. Secondly, it denies the possibility of any socialist-minded programme of socioeconomic reform, since the avowed aims of socialists contradict the biologically inherited norms of human social behaviour. In the light of this, it is hardly surprising that left-wing

commentators have denounced sociobiological theory as offering pseudo-scientific support to aspects of the status quo of many present-day capitalist societies. Whether or not such a direct claim can be made as it is by writers such as Stephen Rose, it is certainly true that the conclusions of sociobiology, if not overtly supportive of the status quo, are in fundamental opposition to the theory and practical aims of socialist reform.

With regard to the political and ethical stance of sociobiology, Wilson and other proponents of the theory have made claims on several occasions to the effect that they are engaged in work of a purely objective scientific nature which is, in their view, undertaken in an atmosphere free of any political or normative intentions. However, in the light of the above analysis, it is difficult to see how sociobiology can help but enter the field of normative social theory, since its own conclusions make it a theoretical and practical opponent of one of the most influential sociopolitical theories at work in contemporary society. If 'ought' implies 'can', and sociobiological theory places limits on the range of what humans can and cannot do, then it has already entered the arena of normative sociopolitical theory - willingly or not. In the conclusion of his article on the origins of National Socialist ideology, Stein gives a stern reminder to those scientists who insist that their work can be carried out in some kind of ethical and sociopolitical vacuum:

must we not admit that the scientific findings of the natural science of sociobiology or the social science of biopolitics are as likely to be appropriated by interested parties, even scientists, to serve political ends as were the scientific findings of the German social Darwinists, racial anthropologists and eugenicists? The history of scientific racism, ethnocentrism, and nationalist xenophobia suggests

that this is no mere academic question. (Stein, 1987, p.267)

To claim, as Wilson and others have done, that their work is 'purely' scientific is to beg a little too much indulgence from their audience. In a post world war technology, even the simplest account of scientific history allows for social and political factors as being of importance to the state and progress of any particular line of research. No work is carried out without funding, or at least governmental knowledge of its contents, so the claim that one is 'just doing science' cannot be taken seriously. Sociobiology contains within its theory - implicitly or explicitly - statements about human nature; the meaning of social behaviours; the limits of human behaviour and feeling; and guidelines for would-be social planners. Moreover, these ideas have been presented both in 'scientific' form for the academic community, and in 'popular' versions (Wilson,1978; Lumsden and Wilson,1983; Dawkins,1978), specifically aimed at a general market of readers, with a view to disseminating the ideas of sociobiology across a wide audience. Given that individuals are going to evaluate to some extent their ideas about human nature and society in the light of such books, how can the authors then claim that they are not involved in the business of sociopolitical debate or influence, and dodge issues such as moral responsibility for the possible effects of their work?

Whatever their wishes to the contrary, the proponents of sociobiology will not escape attention from politically-minded theorists, who will inevitably decypher the normative implications of their work. As I have shown in this section, there is good reason to expect that most of this attention will naturally come from members of the political left-wing,

whose ideology and social aims are most threatened by sociobiological pronouncements.

3.4 Religion and the New Replicators

The topic of religious belief and the practices of organised religious groups, perhaps more than any other aspect of human society, reveals deep divisions in contemporary western social attitudes and theory. Historically this was not always the case. Although difficult to determine with any strong degree of consensus, one could say that until the latter half of the nineteenth century most discussion of religion was of a 'theological' nature. By that I mean that the interested parties disputed the meaning and relative status of various contending beliefs, alternative faiths, or alternative interpretations of received religious texts or articles of faith. The status of religious belief and practice *per se* was not openly put into question, except by a small number of professed agnostics or atheists who stood to pay dearly for their scepticism. Various factors combined towards the middle of the last century to alter this state of affairs, resulting in the present climate of debate which offers a wider range of problems and questions concerning the phenomena of religious belief and practice.

It was the founders of modern sociology who first formulated a new way of looking at religious belief and practice as a social phenomenon, rather than entering into the field of traditional theological dispute between contending religious factions (for example: Weber, 1922 & Durkheim, 1915). The novelty of Weber and Durkheim's approach to the

subject was in effect a philosophical change of stance: rather than accepting the literal significance of religious experience, statements and practices, they applied a functionalist analysis in order to relate them as social phenomena into a broader conception of the overall functions of social structures. Their method thus involved a move from the mere description and comparison of the subjective accounts of religious practitioners, to the adoption of a more 'objective' stance, where such phenomena were reinterpreted as serving a social function not necessarily perceived by the involved participants. The subjective accounts of belief and practice were translated into the language of functional social structures, and as such could be accommodated within a rationalistic appraisal of society. By such a change of methodological stance, religion was to be brought within the cognitive framework of the rationalist positivist sciences, and any references to transcendental phenomena or meaning reduced via the functionalist approach to objectively verifiable structures of human society. (The later dominance of Logical Positivism in British philosophy around the 1930's took an even tougher stance as regards religious belief, declaring that religious along with all other classes of statements which could not be assigned either analytic or empirical content were literally meaningless, and therefore not worth the expenditure of further intellectual effort. Ironically, as will be shown below, Wilson and others have argued that the 'meaninglessness' of religious doctrine is perhaps the clue to its origin and importance in human society.)

It is perhaps no coincidence that Weber and Durkheim's revolutionary analysis of religion came just when it did, as there were a number of contributory or related factors of change in other areas of intellectual

thought which occurred around the same era. For example, the rising success of the empirical sciences in explaining phenomena under an exclusively materialist conception of the universe, replacing previous religious or metaphysical accounts; Darwin's naturalistic account of man's own origins; the development of the psychoanalytic approach to the meanings and origins of various forms of behaviour and states of consciousness; sociopolitical changes towards the secularisation of social controls, in favour of increasing state legislation and a clear demarcation of the power and function of church and state respectively; the dedeification of the teleological theory of human historical development central to many religious or idealistic philosophies (specifically the Idealism of Hegel), carried out by such philosophers as Feuerbach, and its replacement by a materialist concept of historical change; the rise of historical analysis and questioning of the Bible and other sources of religious doctrine; and finally the explicit philosophical questioning of the status of religious experience and organised religion exemplified by writers such as Nietzsche. It would be unwise to attribute the change of stance characterised by the functionalist approach to religion to any particular factor or factors, but perhaps when the above-mentioned are taken into account it becomes more easily seen as part of a general change, which might be described as embodying a philosophical move towards a more positivist analysis of all phenomena, and their subsequent incorporation into the expanding explanatory range of a materialist cognitive framework exemplified in the theory and method of the natural sciences.

Weber and Durkheim thus laid the foundation for contemporary sociological analyses of religion, which still adopt the functionalist

perspective. In a chapter entitled 'The Functions of Religion in Contemporary Society' (Wilson,1982), Bryan Wilson lists among the commonly attributed functions of religion: individual psychological reassurance; provision of social solidarity; substantiation of ethical and legal statutes; explanations of ultimate origins of life and promises of future continuation of life in some form; justification for waging war on other groups; the regulation of and a ritual platform for emotional release; the provision of symbols and rituals for encouraging group identity, and the converse identification of outsider groups. Such a list typifies the sociological approach to religion, and provides the starting point for the sociobiological account.

Where the sociobiological account of religion differs from that given above is to suggest and develop a further level of explanation which claims priority over that of the social sciences, by relating social structures and functions to ultimately biological structures and processes, described in terms of evolutionary theory and population genetics. In other words, where the social sciences offer a reductive account of religion in which beliefs and practices are cashed out at the level of social structure and function, sociobiology takes the social phenomena and relates them to their further function in terms of biological mechanisms describable within the framework of the synthetic theory of evolution.

Although the manifestations of the religious experiences are resplendent and multidimensional and so complicated that the finest of psychoanalysts and philosophers get lost in their labyrinth, I believe that religious practices can be mapped onto the two dimensions of genetic advantage and evolutionary change. (Wilson,1978,p.179)

The sociobiological as opposed to the sociological account of religion thus involves two stages of reduction: firstly, from the literal meaning of religious experience and practice to social structures and functions which promote in some way the 'organic health and unity' of the social group. Secondly, how such social functions ultimately relate to effects on inclusive fitness levels possessed by members of the social group, also ~~relative to~~ members of other social groups with whom there may be competition for resources. As I will go on to point out, this second level of reduction is not approached identically by all sociobiological theorists, as there is disagreement over just how closely social structures are tied to factors of genetic competition. However, I doubt if any two social scientists agree entirely on the functional purpose of many social structures, so the disagreement within sociobiology is not in itself a serious detraction from the plausibility of their theoretical approach.

Since E O Wilson believes that group selection is a possible and relevant factor in evolutionary change, he explains the ultimate biological function of religion as contributing to the maintenance or raising the fitness level of the group concerned, relative to other groups or sub-groups with whom the initial group is involved in some form of competitive interaction. Thus, for example, though an individual may die in battle or perform some act of lesser sacrifice in the name of religious conviction, the net effect of such behaviour is likely to result in an overall raising of the inclusive fitness quota for the group as a whole, though obviously not for certain individuals within the group. Clearly there would be natural limits to the optimality of any particular form of behaviour in terms of its benefit to the group: unreserved mutual self-sacrifice by every member of the group is one extreme of the behavioural

range which would be maladaptive, as would unreserved egotism. As with other features of biological systems, it can be predicted that the relative ratios of particular behavioural forms would be subject to a certain amount of random fluctuation, but nevertheless - given a relatively stable environment - they would display a definite degree of homeostasis around a point of optimal advantage for the given environment.

religions are like other human institutions in that they evolve in directions that enhance the welfare of the practitioners. Because this demographic benefit must accrue to the group as a whole, it can be gained partly by altruism and partly by exploitation, with certain sectors profiting at the expense of others. Alternatively the benefit can arise as the sum of the generally increased fitness of all the members. (Wilson,1978,p.182)

The first sense in which religion is seen as an ultimately 'biological' phenomenon is thus in its effects on group and individual fitness levels. However, there is a further sense in which Wilson believes that religion is an appropriate subject matter for the biological sciences. This is forwarded in Wilson's argument that there could be selection for readiness on the part of individuals to be religiously indoctrinated - i.e. that there could be selection for genetically heritable tendencies towards religious experience and expression. Wilson's argument is quoted below at some length:

The hypothesis before us is that some gene frequencies are changed in consistent ways by ecclesiastic selection....Incest taboos, taboos in general, xenophobia, the dichotomisation of objects into the sacred and profane, noism, hierarchical dominance systems, intense attention towards leaders, charisma, trophyism, and trance-induction are among the elements of religious behaviour most likely to be shaped by developmental programs and learning rules. All of these processes act to circumscribe a social group and bind its members together in unquestioning allegiance. Our hypothesis requires that such constraints exist, that they have a psychological

basis, and that the psychological basis in turn has a genetic origin. (Wilson,1978,pp184/185)

I will ignore the possible lines of criticism which one could raise at this point, such as the ambiguity of psychological features having 'a genetic origin' as Wilson puts it, and the question of whether there is any empirical evidence for the theory he is forwarding. I will instead accept that Wilson thinks that there could genuinely have been some kind of evolutionary process of selection for individuals whose genetically heritable behaviour tends them towards the formation of and participation in organised religious activities. Such a suggestion is of course welcomed by some as providing an ultimate biological explanation for the frequent sociological observation that religion of some form is common to every known human society, and appears to be in some way an instinctive behavioural feature of our species.

Wilson's theory of the genetic heritability and resultant selection for religious behaviour is, as Wilson acknowledges, suggested in the context of a linguistic theory by R A Rappaport (Rappaport,1971). (It is also perhaps hinted at in a remark made by Henri Bergson.) The suggestion made by Rappaport, endorsed by Wilson, is that the existence and use of symbolic language brings with it the occurrence of semantic ambiguity, and the possibility of intentional deception by any party or parties. This novel, socially complex and potentially divisive state of affairs must be countered and held in check by some non-meaningful (and therefore unquestionable) but nevertheless respected and powerful concept, resulting in a social institution and a set of governing behavioural codes. Rappaport suggests that in order to combat "the dissolving power of the intellect" as Bergson put it, there evolved the idea of 'the sacred', which

forms the essential core of every form of religious observance. The judicious use of such a concept could fulfill the desired social function by sanctifying those aspects of social order and cultural tradition which were deemed to be essential to the continued survival of the group.

Propositions such as these sacred sentences are peculiar. Since their terms have no material referents, they are not amenable to verification, but neither are they vulnerable to falsification. They are, in a strict logical positivist sense, nonsense.... The sanctification of such sentences, however, assures the recipient that they are sufficiently reliable to act upon. (Rappaport,1971,pp.29/30)

The first question which such a theory raises is the degree to which such an 'evolutionary process' is meant to be the product of human intention, and how much a process of non-intentional blind selection. It is not clear from Rappaport's or Wilson's account whether it is the first or the second of these two, though in my view the difference is not insignificant. Are we to understand that certain individuals in positions of power took the opportunity to establish further power by manipulating an 'instinctive' readiness to be indoctrinated present in the population, or is the story supposed to be that as a result of the readiness to be indoctrinated, certain individuals 'rose' to fill positions of power, just as nature tends to 'fill' available ecological niches? Whereas I would accept the second case as an example of a process of evolution by natural selection, occurring at the level of social phenomena but analogous to processes which occur within biological evolution, the former scenario does not appear to me to be analogous in the same way. The element of human intention changes the forces involved in the process, since the structures formed in society as a result of human intention have a different origin, history and significance. Though a structure which has evolved by natural selection may resemble perfectly another structure which is the product of

human design, the two are surely not describable in precisely the same terms, just because as end products they fulfill the same function. The difference in origin and history of development between the 'natural evolution' of religious structures and the 'intentional creation' of such structures is blurred or ignored in the sociobiological account, which is merely content to relate the end products to a functionalist account, and to draw comparisons of a very general nature between them. Distinctions which would make all the difference as far as deciding if a process is to be analysed in a certain way or not - such as pointed out above - are not drawn by the sociobiologists, and in my view such an omission on their part undermines the credibility of their analytical method.

The plausibility of the line of argument put forward by Rappaport and Wilson is also open to another criticism. They argue generally that there is or has been selection for religious attitudes or behavioural inclinations. These must therefore have appeared initially as genetic mutations affecting behaviour, and come under selective pressure in competition with allelic rivals. Their supposed advantage over competing behavioural forms ensured their positive selection, until they became general species-specific traits. What began as a randomly produced trait affecting social behaviour and ultimately inclusive fitness quotas spread to fixity in the population (in this case, the whole species), and became a genetically heritable social trait. However, the specific behavioural characteristics mentioned by Wilson in the above quote (incest taboos, xenophobia, etc.) as constitutive or at least representative of religious behaviour could and have been explained individually in terms of serving some socially and perhaps ultimately biologically advantageous function. Although it is by no means clear, Wilson appears to be saying that all

these characteristics have become genetically heritable as a result of selective pressures for the overall religiously inclined mentality of individuals, of which these characteristics are severally and partially constitutive.

On the other hand, it seems to me equally if not more plausible that each of these characteristics on its own could have been selected for, and that religious mentality is a general manifestation of a whole range of aspects of human society, whether genetically or socially heritable. The function of religion could be identified foremost or exclusively at the social level, where it functions to organise and unify various contending elements of human experience and relations under a single but complex conceptualisation. One wonders just how Wilson envisions the process of selection for 'religiosity' if it encompasses such a wide and complex range of behavioural phenomena. Is there supposed to be a convergent selective force acting on many genetic loci with a single overall result in behavioural responses, or is 'religiosity' controlled in its expression by just a few loci, upon which a single selective pressure could act? Once again, the details are not provided by the sociobiological theory. It seems more plausible to me to posit such complex social phenomena as religious belief and practice as being social 'macro-properties', which combine and involve many constituent elements of behaviour. If this were so, then one could not posit the existence of natural selection for 'religious belief and practice' *per se*, since this complex behavioural phenomenon does not appear as a unified trait capable of coming under selective pressure either at a genetic or a social level. It must therefore be seen as a complex product of certain underlying component factors of behaviour, which may or may not be subject to selective pressures of some sort. Being

a product, however, it is not correct to say that it is or could be itself 'selected' in some way.

The above points do, in my view, somewhat diminish the plausibility of Wilson and Rappaport's claim that religion is a genetically heritable trait. However, Wilson goes on to consider other aspects of religious behaviour, and in particular one area in which I believe there is an interesting link between human psychology and a traditional problem in philosophy. Wilson echoes previous social theorists (such as Julian Huxley) who question whether religion is endangered by the growth of secular social structures, and the rise of a scientific materialist conception of the universe and man's place in it. Wilson argues that, since religious fervour appeals more to emotional rather than rational responses (its doctrines and practices relying on the use of mystical symbolism and the communication of shared intuitive response, rather than an appeal to empirically ascertainable or logically deducible facts), he doubts whether a form of scientific secular humanism can ever functionally replace the older form of established religions:

But religion itself will endure for a long time as a vital force in society. Like the mythical giant Antaeus who drew energy from his mother the earth, religion cannot be defeated by those who merely cast it down. The spiritual weakness of scientific materialism is due to the fact that it has no such primal source of power...Humanists will never enjoy the hot pleasures of spiritual conversion and self-surrender; scientists cannot in all honesty serve as priests. (Wilson,1978,p.201)

This worry of Wilson's is, I think, part of a larger problem posed by any rationalistic approach to questions of human behaviour and prescriptive theories of social organisation. Traditionally, ethical dictates have found their ultimate foundation in some transcendent, non-naturalistic area of

justification - such as the will or decree of a deity, or else an intuited absolute moral truth. A rationalist approach to ethics attempts to construct a system of moral justification based on generally accepted rules of logical inference, which link relevant facts to deduceable behavioural prescriptions. However, although such a system may provide one with prescriptions as to how to attain certain goals, it would appear that ultimate goals themselves cannot be anything but non-rationally justified. To borrow a phrase from Wittgenstein, eventually one just hits the 'bedrock' in terms of further possible rational justification, and one can go no further using rational inference alone. Even the most widely debated attempt to construct a plausible rationalist ethics - Utilitarianism - relies ultimately on acceptance of the foundational premise that the end of all morally correct action is promotion of happiness or pleasure and the converse avoidance of pain. Rationality may indicate the means, but it seems incapable of giving sole moral justification for any professed end.

What Wilson refers to as a conflict on the psychological level, between the cold rationality of science, and the 'primal source of power' tapped by religion may be a reflection in psychological terms of the logical structure of goal-directed actions. Rationality provides us with the means to calculate the best route to attaining certain ends; it may even help us to choose between competing ends. But in itself it cannot fully justify those ends. The ultimate source of conviction for belief in any moral system cannot reside within that same moral system, or else the justification is circular and invalid. It cannot therefore be a 'rational' impulse; ultimate justification must be sought in a sphere of understanding which transcends the rational - an intuited feeling either from one's own being, or else attributed to an active external source such as a deity or

impersonal forces in society which transcend or impinge upon the thoughts and feelings of the individual. Social structures may indeed arise in the manner suggested by Rappaport, which serve to protect or bolster the cherished non- or pre-rational foundation from the 'dissolving power' of intellectual questioning and debate. In this way, religion might be seen as serving a socially functional purpose along the lines suggested above, as do other features of human society, by providing a psychologically supportive structure within which individuals are freed from the strain of fundamental doubts, and are therefore able to carry out aims and intentions without further worry for the ultimate justification of their ethical system. It is not, however, necessary that the psychological traits connected with religious belief and observance must be posited as genetically heritable; on the contrary, my own analysis reveals that religion is possibly a socially constructed product aimed at a solution of problems inherent in decision making *per se*, and its universality is explicable as a consequence of the universal predicament created by the structure of rational systems of thought and action. One does not have to suppose that there is some positive force compelling individuals towards religion, as Wilson suggests in his genetic account of the 'religious impulse'. It could equally be a universal but nevertheless social reaction to a negative aspect of human psychology. After all, the supposed angst of the true existentialist is not felt in reaction to some positive truth or value: it is rather a realisation of something negative or non-existent. It is the nausea which rises in response to the realisation that there are no ultimate moral truths, and therefore no ultimate and self-transcendent guides to action. It is the vertigo caused by standing on the brink of action and peering into the abyss of absolute freedom.

In the light of the above comments, I therefore reject the sociobiological account of religion offered by Wilson, and his attempt to capture the phenomena of religious observance within some description that is amenable to biological analysis. In contrast, Richard Dawkins attempts to give an account of religion which differs significantly both from that of Wilson and from that traditionally offered by the social sciences. Since Dawkins is at pains in his work to play down the existence of selection occurring at levels more complex than that of the individual genes, he is unwilling and unable to account for religion in the same way as Wilson, by reference to factors of group selection and inclusive fitness levels. Most surprisingly, considering all that he has so far expounded, Dawkins denies that religion can be explained by reference to its serving some function ultimately advantageous to the genes at all. Instead, at this point he introduces his own novel concept of a 'meme', which is supposedly the cultural equivalent of a gene.

I think that a new kind of replicator has recently emerged on this very planet....We need a name for the new replicator, a noun which conveys the idea of a unit of cultural transmission, or a unit of *imitation*. 'Mimeme' comes from a suitable Greek root, but I want a monosyllable that sounds a bit like 'gene'. I hope my classicist friends will forgive me if I abbreviate mimeme to *meme*. (Dawkins,1978,p.206)

The new entities are supposedly analogous in certain respects to the genetic replicators, but a clear idea of just how far one can truly draw such an analogy is difficult to attain. They appear to be 'ideas', 'concepts' or even behaviour patterns, with examples such as "tunes, ideas, catch-phrases, clothes, fashions, ways of making pots or of building arches." (ibid,p.206). Even the concept of 'God' appears to count as a meme. Although Dawkins does not enter into any discussion of the precise philosophical dimensions of these new entities, it would appear that their

primary existence is as ideas, and that their continued existence is dependent upon their relative success in being accepted by and affecting the subsequent behaviour patterns of individuals and collective groups.

Dawkins' introduction of this new concept does, in my view, raise several problems for his own work in general, and specifically for his account of religion as a human social phenomenon. Firstly, there is the supposed analogous relation of memes to genes. Memes obviously differ in their origin: whereas genes are the result of mutations or recombinations of existing genetic material whose properties are random in respect of the environmental demands made upon the organism, the production of a meme would not normally appear to be a random process at all. Presumably the designer of stiletto heels was at least concerned with solving some problem posed by the existing state of the shoe fashion market, rather than working at some problem in aeronautical engineering. Likewise, the melody of Beethoven's Fifth Symphony was the product of a trained and gifted musician intent on the business of composing a major work of art. It did not occur in the thoughts of a member of the Prussian Guard on night duty, thinking of his wife and family at home in bed. It would seem that, far from having a random relation to the environment with respect to their conception, memes are very much intentional products in response to an existing environment of demands and concerns, which may shape them in fairly specific ways. This is not to deny the fact that individuals may on occasion have blinding insights and find solutions to problems at the most unusual moments, such as when driving a car or playing golf or some other unconnected activity. What it does point out is that this is not the norm for 'memetic' production, whereas randomness with respect to present and

future environmental conditions is one of the defining characteristics of neo-Darwinian evolutionary theory.

As regards reproduction or transmission of memes, it is Lamarckian as opposed to Darwinian. An idea or fashion or whatever can obviously be adopted or copied by any individual during their lifetime, and passed on further to others during their respective lifetimes. There is not only transmission across but within generations, between individuals who bear no genetic relation to one another. This form of reproduction is something which cannot possibly occur in the process of germ-line biological transmission and replication of genetic information. The contribution of Gregor Mendel to the Darwinian theory of evolution was to show how the mechanism of inheritance actually functioned. One of the key issues solved by Mendel's experiments was whether genetic inheritance of characteristics was particulate, or whether genetic information was blended during reproduction. Mendel showed that for the biological process of reproduction, the genetic inheritance of traits is particulate, and no blending occurs. However, once again the so-called process of memetic evolution differs, in that memes can be inherited intact, or else they can be blended with other existing memes, or altered slightly by an individual for their own purpose. As far as memes are concerned, the process of inheritance is not necessarily particulate, though it may actually be on occasion. Finally, whereas genes are necessarily physical objects, whose properties can in theory be captured within a natural science description, it is not clear what status memes are supposed to have. They appear to be ideal - at least initially - and then they become in some sense 'physically instantiated' in the world. Whether memes are the proper study of the social or the natural sciences, or some new hybrid of the two which

combines both disciplines is left unclear in Dawkins account, as is the precise ontological status of the new replicators.

Such fundamentally significant distinctions between genes and memes raise the question of whether Dawkins can introduce his new concept and establish its status by reference to 'analogy' at all. How far can one go in claiming that something is 'analogous', despite the fact that many of its essential features differ radically from those of the supposed analogue? Dawkins is not claiming that human social development can be described as in some sense an 'evolutionary process'. He is claiming that it is an evolutionary process which is strongly analogous to and therefore can be described using the same terms and theories as those used for the description and explanation of biological evolutionary processes. However, as I have pointed out above, the process of 'memetic evolution' differs from that of genetic evolution in every one of its major characteristics, so it can hardly be claimed that the two processes are describable under the same neo-Darwinian synthetic theory of evolution by natural selection.

Having proposed the new concept of a meme, Dawkins then goes on to make a surprising statement about them: he says that they are not necessarily subject to the evolutionary demands and strategies of the selfish genes.

Whenever conditions arise in which a new kind of replicator *can* make copies of itself, the new replicator *will* tend to take over, and start a new kind of evolution of their own. Once this new evolution begins, it will in no necessary sense be subservient to the old. (Dawkins,1978,p.208)

I regard this statement as surprising because, given everything which Dawkins has so far claimed on behalf of the genes and the behaviour which

they produce - directing capacities from sheer physical growth to patterns and limitations to learning - it would seem at the very least problematic to claim such autonomy on behalf of memes as the cultural products of a given biological species. Since memes come into existence as the thoughts and actions of human individuals and social groups, it would seem obvious that a necessary condition for their individual success and survival would be the effect they have on the fitness quotas of the groups in which they are held as beliefs or adhered to as practices. Any meme which put the group at risk (e.g. a meme for gratuitous acts of violence against fellow group members, perhaps coupled to some concept of personal prowess) would tend to eliminate individuals in contact with it, and in time the whole population. Contrary to Dawkins' statement that memes and genes are not functionally connected, I would argue that the general features of any social system must conform to - or at least not come into serious conflict with - the functional requirements and dictates of the group as a biological and social system, which includes effects on the gene pool. Until this point, Dawkins' whole thesis has been dedicated to the task of convincing the reader of the necessary features of the evolved 'hardware' underlying our mental and general behavioural capacities and tendencies. He has been at pains to point out his view that genes are necessarily selfish, and that this will tend to create a world of selfish organisms, even selfish groups. This, we have been told, is due to the evolved structures of the brain and CNS, which result in our own case in the range of reflexive, emotional and even rational capacities. In view of such a thesis, I now find it odd that Dawkins claims that, given the evolution of such hardware, nevertheless at the level of culture we are capable of producing enduring concepts and behaviours which functionally have little or nothing to do with our genetic evolutionary past or future.

The whole idea of memes and memetic evolution seems to raise problems for Dawkins' version of sociobiology. The value of the sociobiological approach was supposedly in its ability to assess to some degree the genetic heritability and biological functional origin of at least some forms of social behaviour. Dawkins now tells us that there are at least two forms of replicator: genes and memes. There are presumably therefore two forms of advantage or kinds of fitness to be calculated: genetic and memetic. However, the two replicators share the same vehicle - i.e. active individuals. It now looks as if, given any particular behaviour, one will have to decide by some method whether it has evolved as the result of its conferring advantage on the genes or on the memes or, despite their supposed independence from one another, on both. How such problems of behavioural interpretation are to be dealt with is not clear or even apparently foreseen in Dawkins' account. What does seem clear is that, far from furthering his case, Dawkins' introduction of memes into the argument seems to cast doubt on the whole of his thesis so far. Dawkins, like other sociobiological thinkers, has insisted on a reductive account of social phenomena from an evolutionary biological stance. The existence of behavioural functions which are not related to biological features of the organism, and thus ultimately to genetic advantage, casts doubt on the feasibility and purpose of the whole reductive enterprise. With the introduction of memes we are once again in the position of having two separate spheres of functional explanation: one biological (genetic) and the other social (memetic). Social behaviour is no longer necessarily linked to biological processes via the factors of inclusive fitness and advantage. The only sense in which the two kinds of evolution are linked is that of 'analogy', and this sense has been shown above to be in serious question. It would appear that with the introduction of memes, Dawkins has

only succeeded in separating the 'socio' from the 'biology' once again, after expending so much effort in attempting to show that the two areas must be seen from a single unified perspective. I can only therefore conclude that, far from furthering his argument, Dawkins' introduction of the concept of 'memes' in order to explain religious and other forms of activity is more damaging to the validity of his own thesis than many of his critics' own objections.

Perhaps, in spite of the above points, it is possible and revealing to try and see the reasons why Dawkins introduces the concept of memes at the closing stages of his argument. To begin with, extremes of religiously-motivated behaviour and emotional experience are not easily accommodated into the sociobiological account of ultimately selfish behavioural motivation and consequence, and the supposed impossibility of widespread or recurrent genuine altruism. Wilson accounts for such behaviour in terms of a group-selectionist theory of behavioural evolution, but Dawkins' own insistence on the primacy of the gene as the unit of selection in biological evolution does not permit him to entertain any such notion. Perhaps he also sees that he has left little room in his account for traditional notions of 'free will' or 'human agency' as capable of escaping the mechanistic dictates of the selfish genes. His coining of the term 'meme' certainly seems to introduce a sudden and unexpected degree of behavioural freedom and novelty into what was predominantly a deterministic-type account of human behavioural capacities. I do not intend to impute Dawkins with having portrayed humans as totally determined in its strictest philosophical sense, though many critics of his work have labelled it a form of 'genetic determinism'. I merely wish to point out that, until the introduction of the concept of 'memes', the origin,

evolution and 'purpose' of behaviour in a functionalist sense were all directly linked to the existence and necessary characteristics of the genes. The introduction of memes and Dawkins' explanation of them as a new form of replicator not necessarily tied functionally to the old seems to open the way for a range of behaviours denied as impossible or freakish under the previous biological dictates. The effect of this is nowhere more evident than in the following passage:

We have the power to defy the selfish genes of our birth and, if necessary, the selfish memes of our indoctrination. We can even discuss ways of deliberately cultivating and nurturing pure, disinterested altruism - something that has no place in nature, something that has never existed before in the whole history of the world. (Dawkins,1978,p.215)

This statement totally overthrows the insistences made throughout Dawkins' work that, reasoning from principles of evolutionary theory, genuine altruism is just not possible. The only possible forms of 'altruism' are the limited forms of kin and reciprocal altruism. Where, one is forced to ask, is this 'power' to come from, which will enable us to escape the dictates both of our biological heritage, and our social environment and its influences? Can there exist an element of our nature which does not derive its form and origin from either or both of these factors? Is Dawkins referring to anything more than some vague sense of transcendent freedom of the will, which as a serious philosophical doctrine would take a lot more explaining than he has so far offered? If we can indeed 'defy the selfish genes of our birth' to cultivate genuine altruism, then why not as regards other aspects of our behaviour as well? If this is in fact possible, then the 'biological' side of our natures would seem to have very little relevance, since we have the power to defy its dictates. Once again, it appears that the introduction of 'memes' into the dicussion only ends up

by taking the 'biology' out of 'sociobiology', to leave one with the social sciences much as they were before the controversy began. The concept of 'memes' seems to provide Dawkins with some sort of escape route from the closed behavioural framework so far outlined in his work, and enables him to introduce a note of optimism for the future of the species into his concluding remarks. If my criticisms are valid, however, Dawkins' conclusion is no more than a rather carelessly thrown crumb of comfort, cast in order to placate his harshest critics, and perhaps encouraged by personal intuitions which run counter to the direction suggested by his own scientific arguments.

It was noted above that the concluding sections of both Wilson's and Lorenz's works also displayed a note of optimism which was, however, paradoxically at odds with the main thrust of their previous arguments (see above, pp.175-176). I would suggest that the change in tone displayed by the authors has very little to do with any given scientific theory or observation made during their research. Rather, it constitutes an affirmation - contrary to much of their own argument - of a particular metaphysical view as regards human nature and the way in which we commonly think about and explain human behaviour. The basic and seemingly ineradicable metaphysical premise which I believe underlies the above authors' conclusions is that - within certain limits - human behaviour and choice is in some sense of the term 'free', and that in some cases at least it is not amenable to explanatory reduction to the terms and theories which apply to the rest of the animal and plant world. It would appear that, despite their commitment to the expanding use of a mechanistic scientific model of explanation, they are unable in the last stage to draw the conclusions which the stance of the natural sciences would seem to

imply as regards human thought and action. This is not to suggest that a commitment to materialism necessarily implies an equal commitment to a thesis of strict determinism and explanatory reduction. However, in the light of all that has been said concerning the millions of years of evolution during which our basic behavioural tendencies were laid down, and the necessarily selfish aspect of all organisms' behaviour, it is somewhat surprising and confusing to be told that humans can break out of this behavioural mould and choose to become otherwise. In the light of all the evolutionary arguments so far presented, which tell us that even our rationality is the end-product of gene-centred natural selection processes, it becomes difficult to see how there could be much room left for a view of human behaviour as rationally autonomous from the genetic mechanisms for whose unconscious ends it came into being in the first place. Yet this is what we are being asked to believe.

Perhaps after all there is something ineliminable about our concept of free will as regards our own behaviour, to the extent that intuitions take precedence over the arguments of even the most hard-headed scientific perspective. As one writer has suggested in a slightly different context:

Until now, the image of man which has prevailed in all images of 'god' is that man is neither totally free nor wholly determined. Man is conceived of as possessing always at least a modicum of free will, but in all images there is also an element of determinism, sometimes larger, sometimes smaller....But within the framework...some room is left for man to decide for himself. (Glock,1973,p.301)

One might say, to borrow the title of a philosophical work on this very question, that in most images of human behaviour, however mechanistically determined the picture they offer of ourselves, there is always a little 'elbow room' left as that area over which we are deemed to have a degree

of genuine control, and the freedom to exercise rational choice. It seems that such a view is held in the end even by the proponents of the sociobiological theories of human social behaviour, though the source of such a view cannot be the sociobiological theory itself. It was perhaps the dissatisfaction with the tenor of his concluding remarks, so different from what had gone before, that spurred Wilson to attempt a reworking of sociobiological theory in its specific application to human behaviour, which will be examined in the next section.

Chapter Four: Coevolutionary Theory: The New Start?

In chapter two of Promethean Fire, Lumsden and Wilson describe the controversy which surrounded the initial publication of sociobiological theorising about human behaviour. The vehement attacks made by politically-motivated groups such as Science for the People (New York Review of Books, 1975) were later followed by criticisms which pointed to doubts and weaknesses within the structure of sociobiological theory itself, rather than to the theory's potential sociopolitical dimensions. These criticisms forced the confession from Wilson that:

The questions of importance in the social sciences - of mind, self, culture, and history - are beyond the reach of sociobiology as that subject was originally formulated.

These criticisms of human sociobiology, also forcefully argued by Science for the People, were largely correct. (Lumsden & Wilson, 1983, p.45)

With the added impetus of a new researcher - Charles Lumsden - Wilson proceeded to reformulate his project of merging the social sciences with biological theory, describing the new approach to human behavioural analysis in the introduction to Genes, Mind, and Culture:

Behavior is not explicit in the genes, and mind cannot be treated as a mere replica of behavioral traits. In this book we propose a very different view in which the genes prescribe a set of biological processes, which we call epigenetic rules, that direct the assembly of the mind.....culture is the translation of the epigenetic rules into mass patterns of mental activity and behavior. (Lumsden & Wilson, 1981, p.2)

The added dimension of culture in some form of interplay with inherited genetic structures and the resultant behavioural traits required Wilson to expand on his original characterisation of human and animal

behaviour. He proceeded to develop a new scheme of behavioural classification which would allow him to incorporate this new element in his theorising. The new classification divided species according to their possession of and ability to acquire cultural traits by means of four basic processes, labelled: Simple Learning; Imitation; Teaching and Reification. The last of the terms is defined as "the construction of symbols and other abstract representations of the environment." (L&W,1981,p.3). Species are called 'acultural' if they display none of the above four methods of acquiring cultural behaviour, and 'eucultural' if they display all four. The only species falling within the category of 'eucultural' is *Homo Sapiens*: the next group (labelled 'Protocultural II' and capable of learning, imitation and teaching) contains wolves, dogs, lions, elephants and apes.

Such a scheme of categorisation provides Wilson with a justification for his general claim that humans differ from other species only in the degree of complexity of displayed behaviour, behavioural acquisition and development techniques, and not in kind:

Although recognising the preeminence of symbols in human culture, we do not agree with Geertz, Schneider and some other social scientists in considering them to be exclusively diagnostic of culture....In short, human beings differ quantitatively from animals in the magnitude of the enculturation process. (L & W, 1981,pp.3 & 5).

However, the increase in behavioural complexity made possible by the attainment of the fourth level of the evolution of cultural traits is clearly an important factor when analysing human behaviour. The stated recognition of this fact by Lumsden and Wilson at the start of their work demands a new and more complex range of terminology and behavioural

modelling if sociobiology is to do justice to its own revisionary programme. The aim of this chapter is to assess this 'new start' in the light of the criticisms and objections already raised, and to see if the sociobiological approach to human behavioural analysis and explanation has finally come of age, as promised by its most eloquent proponents.

One of the most common lines of criticism pursued against Wilson's and others' earlier accounts of human behaviour was that it offered a very rigid, one-way deterministic model. The picture was very much one of genes prescribing definite phenotypic traits in an inflexible way with respect to environmental factors, resulting in individuals who, as collective social groups, also displayed social behaviours which were directly explicable in terms of the genes or genotypes present in the population, irrespective of prevalent environmental factors (see above, p.68). The overall model could be represented by the use of single arrows thus: Genes → Individual Behaviour → Culture. In contrast to this characterisation, Wilson promised to redress the balance by giving a more effective role to cultural factors in his new account, offering a more dynamically interactive model of the relationship of genes and culture:

The conception that began to emerge is that genes and culture are held together by an elastic but unbreakable leash. As culture surges forward by means of innovation and the introduction of new ideas and artifacts from the outside, it is constrained and directed to some extent by the genes. At the same time, the pressure of cultural innovation affects the survival of the genes and ultimately alters the strength and torque of the genetic leash. (L & W, 1983, p60).

The new model would appear to contain interactive forces of relation which are capable of operating in two directions, characterised by double-headed arrows: Genes ↔ Individual Behaviour ↔ Culture. In order to analyse and

fully articulate the precise nature of these relations between the 'biological' and the 'cultural' aspects of human social behaviour, Wilson invents a new terminology, of which the two key terms are 'culturgen' and 'epigenetic rules'. The first term denotes a given cultural unit or artifact, the most precise definition of which is given as:

a relatively homogeneous set of artifacts, behaviors or mentifacts (mental constructs having little or no direct correspondence with reality) that either share without exception one or more attribute states selected for their functional importance or at least share a consistently recurrent range of such attribute states within a given polythetic set. (L & W,1981,p.27).

Though it is difficult to extract from the text typical examples of what might count as 'culturgens' further to the above definition, Wilson does go on to say that they might be such things as "an assortment of food items, an array of carpenter's tools, a variety of alternative marriage customs to be adopted or discarded, or any comparable array of choices." (L & W,1981, p.7). The concept of a 'culturgen' is likened by Wilson to similar ideas proposed by other social theorists, but most notably (in the present context) that of 'memes' - a concept coined by Richard Dawkins and discussed at some length in a previous chapter of this thesis (see above, pp.207-213). An important difference between Dawkins' memes and Wilson's culturgens apparently (and surprisingly) not noticed or thought relevant by Wilson is that whereas Dawkins explicitly states that such cultural units, once in operation, are no longer necessarily restricted by factors such as their conferring or detracting from individuals' biological (i.e. reproductive) fitness potential, Wilson's culturgens are always portrayed as functioning within a process of dynamic interaction of genes and culture. In other words, the prevalence of any particular cultural elements is ultimately and always dependent upon their adaptive significance, and

the existence of corresponding genotypical traits which tend to prescribe such behavioural choices. Whereas for Wilson culturgens are ultimately linked to genes (the 'leash principle'), for Dawkins memes may and perhaps inevitably do take on a life of their own (as 'the New Replicators').

Having suggested a new term for characterising the element of cultural diversity in human social behaviour, Wilson goes on to provide the all-important link in his schema (the 'leash') between the genes at one end and the culturgens at the other. This is what he calls the 'epigenetic rules' defined as:

the genetically determined procedures that direct the assembly of the mind, including the screening of stimuli by peripheral sensory filters, the internuncial cellular organizing processes, and the deeper processes of directed cognition. The rules comprise the restraints that the genes place on development (hence the expression "epigenetic") and they affect the probability of using one culturgen as opposed to another. (L & W,1981,p.7).

These rules governing the course of individual development in interaction with environmental factors can be further divided into two groups: the primary and the secondary epigenetic rules:

Primary epigenetic rules are the more automatic processes that lead from sensory filtering to perception. Their consequences are the least subject to variation due to learning and other higher cortical processes.....

The secondary epigenetic rules act on color and all other information displayed in the perceptual fields. They include the evaluation of perception through the processes of memory, emotional response, and decision making through which individuals are predisposed to use certain culturgens in preference to others. (L & W,1981,p.36).

Wilson cites empirical evidence in support of his model of the processes of individual development with such examples as the universality of

linguistic schemes of colour classification, which divide the spectrum according to the four prime colours. This is an example of a primary and therefore relatively inflexible epigenetic rule, where the individual cognition and behaviour is severely restricted by the structures and operation of the cognitive apparatus itself. Examples of secondary epigenetic rules are such things as the reaction of babies to attention from other humans (facial recognition); visual complexity recognition in infants; nonverbal communication, and such things as fears and phobias. These characteristic patterns and types of human behaviour are, according to Wilson's new theory, to some extent under heritable genetic control. That is, differences and similarities between individuals displaying such behaviour are ascribable to differences and similarities between those individuals' genotypes, rather than to the effects of their individual experience of the environment and behavioural novelty produced by genuine learning. According to which particular aspect of behaviour one is addressing, the prescriptive effect of the genotype may be more or less amenable to change due to environmental forces acting on the individual at different stages of development. Secondary epigenetic rules are more amenable to such forces than the primary ones, but still prescribe a limited range of behaviour beyond which the individual cannot respond: these parameters are heritable and remain fixed for the individual throughout their lifetime.

This idea of human reasoning and behaviour being explicable in terms of the supposed influence of genetically heritable rules has been taken up enthusiastically, and developed into an overall approach to philosophical questions by Michael Ruse (see Ruse, 1986a & 1986b). The two main areas

which he believes fit within the explanatory range of Darwinian evolutionary theory are epistemology and ethics:

There are rules for approval of *modus ponens* and consiliences, no less than there is a rule setting up incest barriers. This is the hypothesis. (Ruse,1986a, p.161)

He also uses the concept of 'epigenetic rules' as genetically heritable traits affecting individual psychology in every major aspect of human life.

In short, I argue that the principles which guide and mould science are rooted in our biology, as mediated by our epigenetic rules. (ibid,p.155)

It is in the case of the supposed operations of secondary epigenetic rules that sociobiological theory becomes both interesting and controversial. Although the secondary rules are supposed to be more 'flexible' i.e. amenable to developmental change in response to interaction of the individual with prevalent environmental forces, there is, as I said above, a limit to their flexibility. Such a thesis might go unnoticed if it limited its conjectures to such things as patterns of facial recognition in infants, but the epigenetic rules envisaged by Wilson affect everything from infantile visual perception to adult sexuality, role adoption, aggression, and all the behaviours familiar to readers of the earlier sociobiological claims about human behaviour. If the new sociobiology were to limit its claims to the explanation of behavioural functions supposedly governed by the primary epigenetic rules, it would cease to be interesting or novel, since it would merely tell us that *Homo Sapiens* as a species has a cognitive and behavioural range unique to it as a species. Such a pronouncement would be merely to repeat the findings of the early comparative ethologists such as Lorenz and Tinbergen. Lumsden and Wilson

wish to go further than their predecessors, however, in order to make specific pronouncements on particular aspects of human behaviour and society, and to discuss the origins of the differences and similarities between individuals within the same species. It is at this point that sociobiology exceeds the theoretical scope of the foundational works upon which it was built, and at this point that its claims become novel, controversial and worthy of close critical scrutiny. Since the theory is largely built upon the two newly-coined concepts of 'culturgens' and 'epigenetic rules', forming what the authors refer to as the 'gene-culture cycle', it is these ideas which I propose to evaluate in the following pages, in order to see if the account which they offer of human social behaviour is any more tenable than that previously offered by the early sociobiological theorising.

In the previous sections of this thesis one of the principle criticisms raised against the sociobiological account of behavioural evolution was that the account it offered of the relation of genotypical to phenotypical traits of an organism was far too crude. Writers spoke of 'genes for' this or that trait, suggesting that isolated sections of DNA could be treated in theory as if they were responsible for isolated phenotypic traits, with no regard for other present genetic factors or the organism's interaction with the environment (see above, pp.21-24). This kind of 'genetic atomism' was labelled 'bean-bag theorising' by its critics, and dismissed as gross simplification of a more complex and holistic process.

I believe that much the same criticism can be made of the concept of 'culturgens' and the way in which it figures in Wilson's account of human society and its relation to the developing individual. Culturgens are

presented as isolated units of the total cultural web of society, and are therefore supportive of an atomistic analysis of culture. Culture itself is defined as the sum total of existing culturgens. This atomistic approach to social analysis is made explicit in the closing paragraph of Genes, Mind, and Culture where one of the stated aims of a unified science is "The decomposition of social behaviour into objective functional units" (L & W,1981,p.362). In reply to criticisms of the stance adopted in this work regarding the theoretical analysis of culture, the authors state:

Moreover, it is of great theoretical and practical importance that some culturgens are discrete and readily conceptualized in the procedure followed in our elementary test models. (L & W,1982,p.31).

It is important to note in the above quote that culturgens are said to be not merely isolatable for the purposes of constructing theoretical descriptive models of human social behaviour. Individuals are supposed to be making actual choices between perceived alternative culturgens, guided by their possession of and response to the inherited epigenetic rules of decision. In order for the mechanisms prescribed by the epigenetic rules to be operant in individual behaviour, it is therefore necessary that culturgens exist as perceptible atomistic units within the totality of culture as it is perceived by any individual at one time. If there are to be rules guiding my choices, then there must be definable units over which such rules may operate. It is, however, far from obvious that the totality of 'culture' as it presents itself to individual perception can in theory or in practice be divided into the kind of discrete units suggested by the concept of culturgen. Further to this, the notion of 'choice' in human behaviour is typically presented as being a simple matter of binary choice between two competing alternatives. In order for there to evolve a set of 'rules' guiding such choices, it is necessary that the outcomes of previous

similar choices can be systematically represented in some way, in order for comparisons and evaluations to be made. In the case of human social behaviour, however, it is clear that any particular choice or aspect of individual behaviour is carried out within a complex social context, the precise elements of which will alter from one occasion to another. This has the effect of adding a further degree of complexity to the process and possibility of 'rule' formation.

According to the account of culture being offered, culturgens are supposed to increase or decrease in popularity according to the genetic fitness potential which their adoption or rejection confers on actively choosing individuals. But just as the fitness potential of a gene or genotype can only be stated as relative to the values of competing genetic units in the available gene pool, so the fitness potential conferred by the adoption or otherwise of a culturgen can only be calculated as a value relative to that conferred by competing alternatives. From this observation I believe that one can deduce two important conclusions regarding the concepts of culturgens and epigenetic rules. Firstly, any model which portrays and describes particular culturgens as advantageous in a discrete, atomistic sense - as opposed to viewing them as competing constituents of a continuous holistic type system - is a misleading simplification of reality. It is very rarely that a choice can be represented in a simple binary fashion, but this is clearly the model on which Wilson continues to base his theory of human behaviour.

Gene-culture coevolutionary analysis runs counter to the organicist conception of many social scientists, which views culture as a virtually independent entity that grows, proliferates, and bends the members of the society to its own imperatives. (L & W, 1981, p.176)

There are in fact three steps: from genes to epigenesis, from epigenesis to individual behaviour, and from individual behaviour to culture. (L & W, 1981,p.343).

Secondly, supposing that evolution did favour the development of the kinds of structures that Wilson says are prescribed by the genes and govern behavioural choice, it would seem that any form of 'rigid' or 'inflexible' rules would not be of much use to the organism in question. The reason for this is that the outcome of choices in terms of maximisation of individual advantage is always dependent on the ever-changing factors present in the total social-environmental climate in which any particular choice is made. The presence of inflexible rules governing choice would therefore defeat the object of their own existence, since they would not be capable of taking into account the necessary changes of surrounding environment which provide the evaluative context in which any particular behaviour is measured. In short, if rules evolve at all, it would appear that they would tend towards increasing non-specificity in their heritable element, thus allowing for a greater degree of individual behavioural flexibility in response to novel and contemporary environmental factors.

This leads one on to the second of the two new concepts introduced by Wilson on which his new theory is based - that of epigenetic rules. These are supposedly psychological characteristics of individuals (though not necessarily in a conscious form) which have some specific neurophysiological basis ultimately due to the individual's possession of a particular genotype. An example of primary epigenetic rules are those apparently governing human colour perception. Certain genetic structures carry the information which results in the fairly uniform construction and operation in each individual of the occipital and attendant neuronal

apparatus of sight. Deviations from the norm of visual perceptual ability, such as forms of colour blindness, are traceable to heritable alternative alleles affecting precise development of the sight-related structures. The general uniformity of heritable visual perceptive structures is further reflected by a uniformity of linguistic usage, resulting in the observed universal features of the vocabulary of colour classification in natural languages. To present such a thesis is neither novel nor, I think, far-reaching in its consequences for behavioural analysis and explanation. It is merely to reiterate with new phraseology an already-established observation-based theory. As far as everyday views of human knowledge and behaviour are concerned, the theory that our general range and acuity of sense-perception is species-specific and governed by some fairly universal and inflexible components of the human genotype should no longer come as a revelation, and does not in my view present any grounds for significant controversy. Where the theory becomes genuinely interesting and controversial is in its claim that there are secondary epigenetic rules. These are supposedly of a more general nature, but still with definite and far-reaching consequences for individual behaviour and the resultant structures of human societies:

our interpretation of the evidence from cognitive and developmental psychology indicates the presence of epigenetic rules that have sufficiently great specificity to channel the acquisition of rules of inference and decision to a substantial degree. This process of mental canalization in turn shapes the trajectories of cultural evolution. (L & W,1981,p.56)

Whereas such things as colour, smell and taste perception in humans are both well-documented by empirical experiment and fairly uncontroversial in their implications for social theorising, Lumsden and Wilson go on to say

that genetically-accountable differences and similarities between individuals exist for such things as the following:

spelling, sentence construction, perceptual skill, psychomotor skill, extroversion/introversion, homosexuality, proneness to alcoholism, age of first sexual activity, timing of Piagetian stages of development, some phobias, certain forms of neurosis and psychosis, including manic-depressive behavior and schyzophrenia, and others. (L & W,1981,p.16)

One can add to this list the earlier sociobiological favourites such as male/female role differentiation, aggression, incest taboos, altruism and other features of human behaviour regarded as under a significant influence from heritable predispositions. Unlike the examples of various forms of sense-perception, these latter forms of cognition and behaviour are far from uncontroversial. With such things as 'incest' and 'homosexuality' it is not clear that a single, simple definition of what constitutes such behaviour is possible (see above,pp.134-150). If this is so, then it becomes increasingly difficult to see how there could be specific rules innate to every individual controlling the expression of attitudes towards such behaviour, since the object of the rule would appear to be complex rather than simple. Nowhere is Lumsden and Wilson's disregard of this factor of complexity more in evidence than in their discussion of the supposed innate aversion towards incestuous relationships in human beings:

Culturgen categories range all the way from those in which the variation consists in two obvious states, such as the acceptance or rejection of incest, to much more subtle and complex phenomena that must be subdivided in an arbitrary manner. (L & W,1981,p.30)

Apart from the problem with the first part of this quote - viz the supposed 'obvious' nature of the behavioural choices involved in such

things as incestuous behaviour - the second part of the quote also seems problematic in view of my above point about the probable and possible evolution of rules which govern vague categories of choice. If the phenomenal categories furnished by cognition are themselves 'arbitrary', in what sense can it be claimed that there has been evolution for universal or near-universal 'rules' governing the perception of and behavioural reaction to such phenomena?

A further criticism of the picture of human development being offered here is that, contrary to Wilson's opening remarks about the previous sociobiological model being replaced by one which incorporates a far greater degree of complexity and scope for individual behavioural development and change, I would argue that the model is still one of fairly static individual capabilities. We are told that any particular individual is born with a set of heritable prescriptive rules, coded for in some way in their genotype. These govern a range of possible behavioural development, within which there is scope for assimilation of environmental influence and resultant change. However, the scope for change is still fixed at the moment of conception and formation of the zygote: i.e. the behavioural parameters are not presented as capable of further development or change during the course of the individual's lifetime. The true 'flexibility' built into the new coevolutionary picture is one which occurs *across* but not *within* generations. It is presumed that for any behaviour for which there exists an epigenetic rule, there will be variation between individuals for that rule and for resultant behaviour. As a result of this, there will occur a process of natural selection within populations, by which those individuals who manifest the most 'advantageous' behaviour due to their possession of particular genotypes will survive and flourish, to

pass elements of their heritable characteristics on to the next generation. This means that epigenetic rules can only undergo 'change' in the sense of their evolving across generations by natural selection, presuming that there exists sufficient variation between individuals, and a corresponding selective pressure. The flexibility is therefore confined to evolution across generations, and does not have as much to do with the flexibility of development of particular individuals as it might at first sight appear from Lumsden and Wilson's opening remarks.

As a starting point in assessing the general status of the new theory, Lumsden and Wilson themselves provide four prerequisite conditions which must be met in order to provide minimal justification for their theory. The first of these is that epigenetic rules must be shown to be commonplace in human societies in forms amenable to analysis of the theory. Secondly, there must be demonstrable genetic variance for such rules between individuals within populations. Thirdly, there must be a verifiable link between cultural practices and genetic fitness - i.e. cultural practice must be reflected by changes in the proportional representation of particular genotypes within the population's gene pool, and vice versa. Lastly, there must be a verifiable link between genes and cellular structures or ontogenetic processes resulting in significant forms of cognitive structure and process. It will be my contention that, apart from the above doubts concerning the terminology of the new model, coevolutionary theory as it is presented in the works of Lumsden and Wilson fails to meet the four requirements which they proclaim themselves to be minimal for its acceptance. I will deal with them in turn in the order in which they are mentioned above.

I think that it is fairly uncontroversial to state that some aspects of human cognition are uniform within and across various apparently isolated human cultural groups. As I suggested earlier, information such as that all children show a preference for food with a relatively high sugar concentration (i.e. high calorific value) would not come as a surprise to most people, and carries no great controversial implications. The universality of other forms of human behaviour is far from uncontroversial. Incest is a good example of behaviour which Wilson takes to be under some form of universal species-specific heritable control, supported by the evidence that its occurrence is relatively low in all known human societies, and there exists some form of corresponding taboo against its practice. The first of the prerequisites for proving the gene-culture coevolutionary thesis was the production of evidence independent of actual social practice that underlying epigenetic rules governing certain behaviours do in fact exist. It is obvious from the writing of Lumsden and Wilson that they do not appreciate the difficulty of substantiating their claims in this way. The evidence which they actually present is to refer to various cross-cultural similarities as proof of underlying fairly rigid heritable behavioural rules, operating on all individuals regardless of specific details of their cultural upbringing and personal development. However, a moment's thought suffices to come up with the objection that such cross-cultural 'universals' could also be due to cultural convergence or 'cultural parallelism', due to similar environmental effects on species-specific but nevertheless flexible cognitive structures. The fact of certain kinds of cultural uniformity does not prove that alternative forms of behaviour lie outside the possible behavioural scope of the species, or that the uniformity itself is not due to similar environmental effects on basic capabilities. For there to be a serious

empirical basis for this aspect of the coevolutionary theory, Wilson would have to offer independent evidence of the hypothetical genetic structures, and from psychology or neurophysiology of the effect of such structures on individual cognitive development. In fact, the only evidence of this kind offered by Lumsden and Wilson is of the fairly uncontroversial kind already mentioned, concerning such things as colour vision or infantile dietary dispositions.

Further to this, Lumsden and Wilson appear to offer a circular argument in support of the existence of underlying epigenetic rules governing individual behaviour. Their mistake appears to be due to the 'double aspect' of epigenetic rules, and the role which this concept plays in their coevolutionary model. I say 'double aspect' since epigenetic rules are supposedly both the *results* of heritable genetic structures and the *causes* of corresponding social structures. By their influence on the individual, they produce mass patterns of choice between competing available culturgens. The supposed 'proof' of the existence of epigenetic rules is to work backwards from the observed to the unobserved; from the empirical to the hypothetical side of the model. The inference is from the existence of certain visible social structures, to the existence of certain invisible genetic structures. Since these invisible structures have been posited in the first place in order to explain the observed evidence of social practice, it is circular and invalid to argue, as Wilson does, that the evidence from social practice is 'proof' of the existence of the underlying hypothetical genetic structures. The existence of uniform social practice is evidence of nothing but itself: it only becomes evidence of the existence of underlying genetic structures if such structures are known to exist and have such effects, and there is no alternative explanation which

accounts for the uniformity of the social structures - and this is the very point at issue. Such an error is, in my view, grave in its implications not only for this particular aspect of the discussion of the coevolutionary theory, but also since it implies a disturbing lack of rigour in the reasoning used to justify the authors' theories in general.

A further problem for acceptance of the theory is in the way it deals with particular phenomena such as incest taboos. As pointed out by many critics of the theory, the terminology of the 'culturgen' allows Lumsden and Wilson to lump together for theoretical explanatory purposes a range of significantly differing behaviours which, to the discerning eye of the social scientist, are far from uniform. The point is most graphically illustrated by Kitcher's discussion of incest taboos and their coevolutionary explanation (Kitcher, 1985, pp. 344 - 346). Kitcher proposes three hypothetical societies called respectively the 'Shunsib', the 'Moralmaj' and the 'Tabuit'. As the name suggests, the Shunsib people exhibit a high degree of incest aversion, but there is no social institution reflecting this individual behaviour. In the Moralmaj society, there is also a high degree of incest avoidance, but they also educate their children in such a way as to condemn incestuous practices, though without ever making such condemnation a public affair. In the Tabuit society, there is not only a high degree of aversion to incest, but its occurrence is expressly forbidden by their civil and/or religious codes. Its occurrence carries severe publicly-ordained penalties for those convicted. Since Lumsden and Wilson only ever talk of incest taboos and the practice of incest as a single, indivisible culturgen, it would appear that their analysis of these three societies would render them all equivalent, since in each of them there is an avoidance of incest. However,

it seems obvious that to the social scientist the three societies represent very different social phenomena. Wilson could try to differentiate between them by saying that the Shunsib and the Moralmaj are separated by their different use of a culturgen for 'criticizing the behaviour of others'. In the Shunsib tribe this culturgen is low in frequency or nonexistent, whereas in the Moralmaj society it has a high frequency of use. However, it now seems as if there is a distinction between the simple fact of individual incest avoidance, and the existence of a propensity for instructing one's children that such practices are wrong - a distinction which does not appear in Lumsden and Wilson's account, where the avoidance of incest and the existence of some rule governing one's propensity to instruct others in one's opinion are never treated as separate phenomena. If they are separate phenomena, what is the nature of the psychological link between them, and how is this controlled, since there now appear to be two culturgens: one corresponding to individual behaviour, and the other corresponding to one's moral approbation of others? The problem is further complicated when one considers the Tabuit society, since they have not only the phenomenon of private practice, but also the existence of a public system of sanctions and punishments. The problem here is how to account for the public ceremony of punishment, since if this is also held to be a culturgen, then it must be a joint manifestation of individual behavioural propensities. One might be able to envisage how individuals could have behavioural propensities guiding their personal reaction to potential occurrences of incest, but what would a genetically heritable propensity for condoning and taking part in public ceremonies related to incest look like? As Kitcher intended, the more one begins to try and offer expanded versions of the Lumsden and Wilson arguments in order to deal with the examples, the more unlikely and incomprehensible the explanations become.

Kitcher concludes the section by stating that Lumsden and Wilson have failed to respond to the critics of the earlier sociobiological theories, and that in order to do so:

Lumsden and Wilson will need a theory that accounts for the presence of social institutions, public ways of regulating the behaviour of individuals. The challenge will be to specify the group property of having a social institution in terms of the frequency of the use of culturgen, conceived as things that individuals can adopt. Unless that challenge is met, there will be no reason to think that Lumsden and Wilson have solved the problem they set for themselves. (Kitcher,1985,p.346).

It would appear from the above analysis that Lumsden and Wilson have indeed failed to fulfill the first of their required proofs for the coevolutionary theory, and that far from clarifying the original proposals of sociobiological theory, it has led to further confusions.

The second criterion for establishing the truth of the coevolutionary theory was to show that "genetic variance in epigenetic rules must exist within human populations." (L & W,1981,p.16). The evidence in support of this element of their theory comes from studies of identical and non-identical twins. However, such data is notoriously problematic if used as evidence in the way here required, and limited case studies of fraternal and identical twins will not yield sufficiently independent evidence to decide the issue (see above,pp137/139 for criticism of the use of twin studies in relation to the analysis and explanation of homosexuality). The argument forwarded by the authors is that case studies of twins shows evidence of greater behavioural similarities between identical as opposed to non-identical twins. The argument then proposes that this greater degree of similarity is due to the identical twins' possession of identical genotypes, rather than to any environmental factors, on the grounds that

the non-identical (dizygotic) twins share a similar environment, yet do not display the same degree of behavioural similarity. However, what such evidence does not take into account is the precise details of the environmental effects in the two types of case. It can obviously be argued that identical twins share a much more 'identical' environment than non-identical twins. They are treated more equally by their peers and others, they are even confused for each other or able to swap places deliberately. Parents will tend to expect and react to displays of different stages of growth and development identically. Perhaps the sheer fact of their similar physical appearance is enough to ensure that their treatment by others is bound to be more identical than in the case of fraternal twins. Subsequently, one would expect their emotional and general cognitive development to be more similar than in the case of non-identical twins. In this case, it is not at all easy to separate the fact of genetic identity from the prevalent facts concerning relative identity of environmental forces, which is what is required if such cases are to stand as evidence of the relation of genetic to environmental factors in individual development. Since it is impossible to be sure that any particular traits displayed by identical twins are due to genetically heritable factors rather than to environmental ones, to cite such cases as Lumsden and Wilson do in support of their theory is an insufficient proof of their contentions. Once again the authors argue from scant evidence of the similarity or dissimilarity of behaviours in particular cases to the existence of underlying genetic causes for such behavioural similarity. However, the genetic factor used in the explanation is the very thing in need of independent evidential support, since the supposed evidence can be equally interpreted as supportive of a viable alternative thesis. Once again, I would argue that the authors have failed to provide convincing

evidence in support of their theory, and have merely shown how their theory would fit in with and not contradict existing observations which, however, permit of other equally viable explanations. (A lengthy discussion of the problems raised by the citing of case studies of twins in relation to questions of genetic heritability is to be found in chapter five of Not in our Genes by Rose, Kamin and Lewontin.)

The third area of evidential support for the coevolutionary theory is in the question of a positive link between cultural practices and genetic (i.e. reproductive) fitness within (and presumably between) different cultural groups. The examples of cultural elements which have these effects are such things as the following:

For example, certain practices in tattooing and other modes of body marking, as well as circumcision, treatment of menstrual and afterbirth blood, and diet, are known to transmit viruses and other infectious agents that profoundly affect mortality, birth rate and even sex ratio. (L & W, 1981, p.17)

The effects of such practices on the health of individuals and on demographic features of the cultural group in general are claimed as proof of a direct link between culture and genetic fitness potentials. One must, however, analyse such a claim to extract its precise implications. It is true that tattooing, circumcision, obstetrical practices and other customs involving exposure of or damage to body tissues all involve an increased risk of viral infection and mortality rate changes due to insufficient medical care or knowledge. This is too obvious and trivial a point to be the one intended by the authors. The clue to the real purpose of such examples is given in the next few pages of their text, where Lumsden and Wilson go on to discuss two examples of dietary customs.

The first case has to do with the special method of alkali cooking which considerably raises the nutritional value of maize, compared with alternative methods of preparation. Wilson states that in the Americas:

there exists a strong positive relation between the intensity of maize cultivation, the use of alkali cooking, population density, and the complexity of social organization. (L & W,1981,p.17)

If the population is one for which maize consumption is a major factor in the diet, and if the variation in cooking methods causes large changes in the nutritional value of the end product, then it comes of no surprise to find that demographic features such as comparative population density show some degree of correlation to cullinary techniques. However, it becomes clear from the sentences which follow that the authors' example is meant to indicate something far stronger than this simple correlation of demographic and dietary factors: X

It seems unlikely that the many New World societies adopting alkali cooking could have directly perceived this procedure as the solution to their biochemical shortfall and hence as a requisite for further population growth and social evolution. (L & W, 1981,p.17)

What is being darkly but definitely hinted at here is the suggestion that the present-day prevalence of societies using the alkali cooking method cannot be due to factors of conscious decision making by any individuals or groups thereof. The necessary knowledge for such a decision was until comparatively recently lacking, and could therefore have played no part in the history of the cooking methods. The obvious but unstated implication is that the present state of affairs is the result of some process of evolution by natural selection: those groups adopting alkali cooking methods thrived, and those which didn't are not extant in sufficient

numbers to tell the tale. But if this is all that is being implied, it is perhaps not too controversial a thesis, but nor does it constitute evidence in support of the coevolutionary thesis as defined by the authors. Before spelling out why this is so, I will go on to their second example which shares similar characteristics.

The second example is also of dietary customs, and has to do with the consumption of a particular foodstuff called fava beans. These can have serious deleterious effects, even fatal, if consumed by individuals who possess a certain sex-linked recessive gene labelled G6PD⁻. It also happens that in many Mediterranean societies there exist taboos and special rituals regarding the preparation and consumption of the fava bean. Lumsden and Wilson's concluding comment on these phenomena is once again suggestive of what they regard their examples as proving:

Yet there is little evidence that the peoples affected made any direct, rational connection between their beliefs and what is now perceived to be the true nature and cause of favism. (L & W,1981,p.18)

This example, along with the first one above, is meant to be evidence of a definite 'coevolutionary link' between certain aspects of culture and genes. But just what is meant here by 'coevolution'? To return to the first example, one might accept the suggested explanation that, as a result of alternative cooking methods, cultural groups thrived or dwindled accordingly. Perhaps, as the authors seem to be suggesting, those groups that 'got it wrong' died out or migrated towards those that 'got it right' and were visibly flourishing as a result. Demographic patterns became attuned to cultural practice, without knowledge of the relative merits of variant practices. But what does this story show? Merely that cultural practices can have serious effects on the numbers and composition of

populations, with or without the direct causes being known by any individuals within the population.

Was this the kind of 'coevolution' that Lumsden and Wilson claimed was going to be vindicated in their work? I think not. In the introduction to Genes, Mind and Culture the authors offer the following definition:

Gene-culture coevolution is correspondingly defined as any change in the epigenetic rules due to shifts in gene frequency, or in culturgen frequencies due to the epigenetic rules, or in both jointly. (L & W, 1981, p.11).

The thesis put forward here is that culture and gene pools can interact with each other in a way which is non-random with respect to the characteristics of particular genotypes within the gene pool. Coevolution is not merely a quantitative change within the gene pool: it is a qualitative change, due to differential selection for particular genotypes. The result is cycles of interrelated cultural and genetic evolution. In the above two examples of dietary customs, there has arguably been an occurrence of cultural evolution by natural selection. That is, particular practices have survived differentially over time due to the effects they have on individual and overall group fitness levels. The fitness potential of any particular individual was related to their inclusion into a particular cultural group: i.e. X survives or dies because they belong to an alkali/non-alkali cooking group. The important point to note is that X's fitness potential here is calculated *irrespective* of their particular genotype. The difference between X's fitness potential, where X belongs to a group of alkali cooks, and Y's fitness potential, where Y belongs to a group of non-alkali cooks is due purely to cultural factors of the group and its practices, and has nothing to do with differences between X and

Y's respective genotypes. The only way that the examples could fit in with the coevolutionary thesis as stated above would be if there also existed genes prescribing the various methods of food preparation and the existence of dietary taboos. In that case, individuals would survive differentially not merely due to their chance inclusion in a particular cultural group, but because the spread or demise of particular cultural practices would be affected by evolutionary changes in the gene pool composition by natural selection for particular genetically heritable characteristics.

It is the latter kind of account which appears to be the true underlying aim of the coevolutionary theory, and if proven would indeed be a significant factor in assessing such examples of human social behaviour. However, even Lumsden and Wilson stop short of suggesting that evolution in the first of the above examples has occurred as a result of selection for 'genes prescribing epigenetic rules favouring alkali cooking methods'. It would appear that some such account is in fact necessary if it is to count as an example of what the authors themselves define as genuine coevolution. In the second example we are again told that the occurrence of the taboos cannot have anything to do with the conscious apprehension of the facts of the matter. But once again, it is difficult to see how the origin of the taboos is to be explained in the way that the coevolutionary theory suggests. It is true that the people presumably lacked knowledge as to a connection between the consumption of the bean and its effects on certain individuals possessing the recessive gene. But this does not mean that they were unable to observe and reflect on instances of illness following consumption of the bean, without knowing the precise details of the causal relation. I would suggest that this is precisely the kind of

situation in which one would expect myths and superstitious taboos to come into existence: when certain phenomena have been observed in regular conjunction with one another, but the true nature of the causal connection is beyond the 'scientific' explanatory powers of the cultural group. To capture and render useful the knowledge from observation, an explanation is formulated within already-existing explanatory schema, provided most commonly by religion, myth, legend or some set of superstitions. If this is not the origin of such taboos, then again one must ask where the authors think the origin truly lies. Are they perhaps suggesting that along with the deleterious reaction to fava beans due to possession of the recessive gene, there has evolved some form of innate psychological aversion also linked to the particular genotype? If so, how would it relate to the existence of the taboo, which is presumably practiced by all members of the group, regardless of individual genetic constitution? (These questions are comparable to those raised by the account of incest avoidance and taboos - see above, pp.235-237). Such problems are never examined by the authors, and one is instead left with a case of observed cultural practice, the origins of which do not appear to require anything like the complexity of the coevolutionary theory being offered. Nor do the examples further the case for the coevolutionary theory in the way that the authors intended.

The fourth and final part of the proof for the coevolutionary theory is the evidence for a link between possession of specific genes or alleles and specific paths of cognitive development. This is the kind of link which in the above examples of dietary practice were shown to be necessary in order to make them genuine examples of coevolution as defined by the authors. The examples offered in support of this aspect of the

theory range from peripheral sensory screening in sight, sound and smell, etc. to operant learning processes. Even the authors admit that research into the cellular basis of cognition processes in humans is "fragmentary", yet they feel confident enough to pronounce that "there is no shortage of models to explain the neuronal basis of consciousness and the mind." (L & W,1981,p.19). This may be so, but such a plethora of models merely reflects the enthusiasm of the researchers and theorists involved: it does not constitute the kind of hard scientific evidence which the authors themselves recognise as necessary to the establishment of their theory, and which they have so far failed to deliver to their readers. (I also doubt that many philosophers would share the apparent opinion of the authors that the relation of consciousness to our physical nature is all but solved!) In short, as with the other aspects of the coevolutionary theory, the authors have little evidence that the precise behaviour-directing structures actually exist in the forms in which they suggest. The bulk of the evidence merely supports the already-stated and uncontroversial thesis that the main forms of sensory perception are limited to certain genetically heritable and species-specific ranges and sensitivity, which is far from the claim that complex forms of social behavioural interaction are under similar kinds of control. I would therefore conclude that Lumsden and Wilson's coevolutionary theory of human social behaviour has not been proven, even by the standards laid down by the authors themselves. Though not for any one particular reason, in general one could say that the failure is due to an oversimplification of important aspects of behaviour and its characterisation; a failure to recognise or entertain equally viable alternative explanations; a lack of sufficient and relevant empirical evidence; and finally a tendency to draw

invalid deductions from certain premises, or to mistake such deductions as the only possible interpretation of the available evidence.

The only areas in which the coevolutionary theory is acceptable is where it is fairly uncontroversial: the authors mistakenly think that the uncontroversial evidence can be extrapolated and used to back up a stronger thesis for which evidence is actually lacking. The weaker thesis which I believe can be supported by existing evidence and by *a priori* theorising from knowledge of evolutionary principles is that the organs and the ranges of sensory perception and peripheral filtering of information they furnish are species-specific and largely unconscious in their operation, being due to morphological structures whose features are genetically heritable. This is in line with comments made in the comparative ethological studies of Lorenz and Tinbergen. Lumsden and Wilson appear to be forwarding a stronger thesis about very specific aspects of behaviour at a much more complex level, but as I have argued, they lack the empirical evidence and the argumentation necessary to establishing this thesis.

If one accepts the above analysis, it would appear that Lumsden and Wilson have failed to prove the worth of their new 'science' of human social behaviour, either on theoretical or empirical grounds. Apart from the doubts I have raised as to the scientific credentials for the coevolutionary theory, one also begins to wonder just what Wilson's conception of human nature and society is, in the light of statements like the following:

The ultimate, evolutionary goals of the mind, toward which minute-by-minute problem solving is directed, reside in the epigenetic rules, and in that sense the core of both humanness and individuality are invested

there rather than in the more purely cognitive and ratiocinating portions of the mind. (L & W,1981,p.348)

And this is following the introductory promises of a proper place in the theory for the nature and effects of human society and thought in all its varieties! Quotes such as the above sound more like statements of genetic determinism, in which consciousness and rationality are relegated to epiphenomenal status, incapable of significant or any behavioural effect. Such an interpretation of the Wilsonian view is made even more plausible when Lumsden and Wilson briefly dispose of the greatest products of human achievement in a similar manner to that suggested above, only a few pages further on in their text:

We should keep in mind that most of the wondrous inventions of science and technology serve in practice as enabling mechanisms to achieve territorial defense, communication of tribal ritual, sexual bonding, and other ancient sociobiological functions. Curiosity, even the artistic impulse itself, might also fill such a role. (L & W,1981,p.360)

This almost grotesque compression of human culture and its meaning into a few biased examples is coupled with unfounded statements of the supposed closed inevitability of present and future development:

Only with difficulty can individual development be deflected from the narrow channels along which the great majority of human beings travel. (L & W, 1981, p.358)

A society that chooses to ignore the implications of the innate genetic rules will still navigate by them and at each moment of decision yield to their dictates by default. (L & W,1981,p.358)

Such statements only go to show that, despite pretensions to a new form of behavioural analysis which gives appropriate weight to cultural factors and the flexibility of human behavioural responses, the authors are still

committed to a model of individuals under the control of genetically acquired behavioural imperatives. Lumsden and Wilson even end their work with a section entitled "The Explanation of History" in which they appear committed to the view that human history is largely determined, and can be explained and even predicted if only one has enough knowledge of the causal laws governing its movement. The problem with such a view is that somewhere along the way, everything recognisably 'human' about history and the cultures which comprise it just disappears, and one is left with a description of a species which it is hard to recognise as one's own. If sociobiology is claiming to be a viable contender for replacing existing human psychology and the social sciences, then it must offer a portrait of human society which captures its richness and depth. In fact, what often emerges in sociobiological texts is little more than a caricature of human individuals and society, in which theories are offered to account for isolated and distorted aspects of behaviour, the features of which are sometimes difficult to recognise as 'human' at all. One might sympathise with the sociobiological contention that there is a greater degree of continuity between human and other species' behaviour than has been previously credited by the existing social sciences, and that this error stands in need of correction. But such sympathy is easily lost when the 'new synthesis' of theories is achieved at the severe cost of accurate representation of the human protagonists.

Chapter Five: Evolutionary Theory and Philosophy of Mind

Is it possible, at certain moments we cannot imagine, a horse can add its sufferings together - the non-stop jerks and jabs that are its daily life - and turn them into grief? What use is grief to a horse? (Equus; Act 1, Scene 1.)

In the previous chapters I have critically examined at some length the attempt by certain theorists to apply the principles of evolutionary theorising to the explanation and description of human social behaviour. I believe that my arguments have shown that, contrary to their claims, they have failed to establish their thesis by way of sufficient reasoning or empirical evidence. Consequently, very little remains of their specific arguments to commend itself to those already involved in the established disciplines of research and description of human behaviour which fall under the umbrella term of 'social sciences'. Whatever the future may hold by way of inter-disciplinary 'synthesis', it will not - in my opinion - take the reductive form of 'cannibalism' boasted by Wilson and other such thinkers in their often polemic style of scientific prophesy (Cf. Wilson, 1975, p.6).

In this final chapter I would like to turn from concentration on the theories of sociobiology, and present some rather more positive thoughts concerning the ways in which I do believe that evolutionary theorising can and ought to have a bearing on human concerns. In particular I wish to provide some justification for the view that the Darwinian explanation of the origin of species has a relevance to several related areas of philosophical thought. Contrary to the claims of some philosophers,

reasoning which takes account of aspects of empirical theory such as biology can and should have far-reaching consequences for traditional philosophical debate. In short, I believe that it would be not only unwise but invalid for philosophers to continue theorising upon certain topics within the analytic tradition as if the synthetic theory of evolution by natural selection had never been propounded.

Leaving aside the specific arguments of sociobiology as forwarded by any particular writer, one could characterise it very roughly by reference to its central tenet, viz: that the behaviour of all species can and should be sufficiently explained within the neo-Darwinian framework of synthetic evolutionary theory. Thus, both the historical origins and method of explanation of at least some aspects of human behaviour are supposedly continuous with those of other species. *Homo Sapiens* may be unique in its natural history and certain species-specific abilities, but the same may be said of every natural species. Within such a theoretical schema, differences are of degree rather than of kind. Such a 'dogma' is the foundation stone of the whole sociobiological edifice of research and theory.

In sharp contrast to such a claim, many philosophers (if not the majority) have argued for various reasons that the origin and explanation of at the very least a large part of human behaviour differs in kind or principle from that of any other species. Its explanation therefore demands and may only be rendered comprehensible within the conceptual domain of a unique explanatory framework. The modern versions of such a view can be traced from enlightenment figures such as Descartes, up to contemporary philosophers such as Donald Davidson, Peter Winch, Antony

Kenny and A I Melden. Though these philosophers have their individual reasons for wishing to draw a sharp division between the description of *Homo Sapiens* and that of the rest of the natural world, the outcome of their theoretical stance is comparable. One could characterise them by saying that they reject the notion of behavioural and corresponding explanatory continuity between *Homo Sapiens* and even the most plausible instances of other species - for example primates such as Chimpanzees or Gorillas. Such examples are regarded as in some way lacking the necessary credentials for admission to the ranks of 'rational agents' or 'conscious beings' or some other office of distinction attainable only by members of our own species. Ultimately, all these philosophers base their distinction on some characterisation of human thought as possessing unique qualities (e.g. intentionality, rationality, meaning, or simply consciousness or self-consciousness), from which it is then argued that the resultant behaviour must also therefore admit of a different order of explanation, however great the temptation to make cross-species comparisons. The mother Chimpanzee nursing its young in the zoo compound is touching because its movements so perfectly *resemble* those of a human mother: to imply that such behaviour is in fact an expression of similar maternal thoughts and feelings is dismissed as unphilosophical and confused anthropomorphism.

The line of argument I wish to pursue in this concluding chapter will attempt to go some way towards resolving some of the issues raised by such different perspectives as I have roughly outlined above. It involves a dangerous passage between the Scylla of sociobiological reduction of all behavioural explanation to the level of cross-species 'types' or 'strategies', and their apparently inadequate and idealised models of one-to-one behavioural interactions, and the Charybdis of human behaviour as

occupying a logically unique and isolated niche in ethological studies and philosophical theorising.

In chapter two of his work Thought and Knowledge (Malcolm,1977), Norman Malcolm analyses the somewhat 'notorious' position of Descartes regarding the comparative mental life of humans and other species. Malcolm's conclusion is that, as regards human thought, Descartes:

explicitly and consciously adopted the position that there is a propositional kernel in every feeling, desire, voluntary act, emotion, and sensation. This is why he could hold that his essential nature consists solely in being a thinking thing. (Malcolm,1977,p.48).

Malcolm goes on to say that, in contrast to the *Res Cogitans* of essential human being, Descartes regarded all other species as being:

devoid of mind, of all consciousness and awareness, of real feeling and sensation, because they do not "apprehend" , "entertain" , "contemplate" ,or, in plain language, think of *propositions*. (ibid,p.49).

This view of Descartes regarding the mental life of other species is one with which Malcolm strongly disagrees. In essence, Malcolm's objection to Descartes' position comes down to the fact that Descartes holds "an absurdly overintellectualized view of the life of man." (ibid,p.49). The undue emphasis which Descartes places on linguistic formulation as necessary to any and every form of thought both distorts the correct presentation of humans, and in so doing condemns all other species to a supposed existence as insentient, unconscious automata. Such views would be disturbing enough if they were merely limited to an era within the history of philosophy, but equivalent views to that of Descartes find forceful expression in the work of contemporary thinkers such as Donald Davidson (Davidson,1975), Wilfrid Sellars (Sellars,1956), Richard Rorty

(Rorty,1965) and Paul Churchland (Churchland,1979). Although based on a different overall philosophical framework, the resultant view of Davidson and others turns out to draw the same conceptual boundaries regarding behavioural explanation and the attribution of mental life as were drawn by Descartes in the seventeenth century. Moreover, Davidson's argument (on which I will focus initially) is in places specifically aimed at the position of thinkers such as Malcolm. I will therefore turn now to an exposition of Davidson's 'neo-Cartesianism' to see if there really are sound philosophical grounds for maintaining such rigid divisions between man and the rest of the natural world.

Davidson's views on this matter find their fullest expression in the article entitled Thought and Talk (Davidson,1975). Davidson begins with the question "What is the connection between thought and language?" and promises to "show how thought depends on speech" (ibid,p.8). The first stage of this argument is to explain how any particular thought can be said to have semantic content. Davidson's first point is that all thought is parasitic on belief, since to think about anything at all is to entertain an indefinite background system of beliefs which relate in various ways to the particular thought.

The system of such beliefs identifies a thought by locating it in a logical and epistemic space....We may say, summarizing the last two paragraphs, that a thought is defined by a system of beliefs, but is itself autonomous with respect to belief. (ibid,p.9).

The next stage involves a somewhat more complex argument whose details are most fully developed in a number of other articles (Cf. Davidson,1973; Davidson,1974a; Davidson,1974b). Briefly, it is argued that the attribution of beliefs to or by a third person depends upon one's ability to

successfully interpret the other's behaviour (linguistic or otherwise), since behaviour is the overt expression of desires, beliefs, etc. This further limits the possibility of interpretation to those whose beliefs, desires, etc. do not differ too radically from one's own, since a radical difference would make the whole process of interpretation impossible.

Davidson concludes:

All this strongly suggests that the attribution of desires and beliefs (and other thoughts) must go hand in hand with the interpretation of speech, that neither the theory of decision nor of interpretation can be successfully developed without the other. (Davidson,1975,p.15).

The reason that 'speech' now appears as the key factor in interpretation and attribution of thought is that:

without speech we cannot make the fine distinctions between thoughts that are essential to the explanations we can sometimes confidently supply. (ibid,p.15).

Semantics is thus not merely parasitic upon belief systems, but upon linguistic systems. In espousing such a theory, Davidson is allying himself with the Wittgensteinian thesis of 'meaning as use', and applying the insight of Wittgenstein's language-game perspective to a theory of radical interpretation. In the absence of possible discourse between the two parties involved (actor and interpreter) the possible "attributions and consequent explanations of actions will be seriously underdetermined." (ibid,p.16).

For some philosophers, (e.g. Armstrong,1973) the underdetermination of attributable beliefs does not in itself pose a serious problem, and is in fact only to be expected in cases where we are concerned with the

behaviour of other species. After all, both Armstrong and Davidson insist that the success of radical interpretation depends upon the degree to which we can attribute thoughts similar to our own (i.e. some degree of isomorphism between the beliefs of the actor and the interpreter is necessary to any process of interpretation). Why, asks Armstrong, should we even expect our concepts to 'map' directly onto those of, say, a dog? The inevitable impossibility of precision in belief attribution is for Armstrong no serious philosophical problem at all:

In saying that the dog believes that his master is at the door we are, or should be, attributing to the dog a belief whose exact content we do not know but which can be obtained by substituting *salva veritate* in the proposition 'that his master is at the door'. (Armstrong, 1973, p.26 - cited in S. Stich, 1979).

The problem for Armstrong lies not in the question of whether other animals can be attributed thoughts and beliefs in general, but in our lack of sufficient knowledge which makes our interpretation of other species' behaviour always more underdetermined than that of our own. However, one can still use existing terminology from one's own linguistic repertoire to approximate to the supposed thoughts of the species under interpretation, as in the above quote. Unlike Davidson, Armstrong asserts that some sort of behavioural interpretation can go forward somewhat ahead of a full animal psychology, whereas for Davidson this is not an option. Where Armstrong thinks that our knowledge of the psychology of other species is imperfect, Davidson thinks that it can never even get off the ground.

The opposing views of Davidson and Armstrong are examined by Stephen Stich in his article Do Animals Have Beliefs? (Stich, 1979) with a view to deciding whether the indeterminacy of specific belief attribution

is as serious as Davidson believes it to be. Rather disappointingly, however, after a careful analysis of both viewpoints, Stich concludes:

So the question we are now asking amounts to asking how central to our concept of belief is the having of specifiable content. Is a belief-like state which lacks a specifiable content simply a somewhat peculiar belief, or is it, in virtue of lacking content, no belief at all? The answer I would urge is that there is no answer. (Stich,1979,p.27).

The reason for such diplomatic but unhelpful caution is Stich's view that there are no clearly necessary or contingent features intrinsic to the definition of the concept of 'belief'. The argument between Davidson and Armstrong therefore becomes, in Stich's view, one of *post facto* definition. Armstrong regards 'hazy' beliefs as inevitable at this stage of our knowledge of other species; Stich thinks it merely raises the question of definition; Davidson thinks it grounds for denying mental states altogether for other species, since "many alternative systems of attribution, many alternative explanations, will be equally justified by the available data." (Davidson,1975,p.16). The final stage of Davidson's argument is compressed into the last few paragraphs of his article, and centres once more on the role of belief.

Davidson argues that membership of a speech community and involvement in the interpretation of others' speech is necessary to having the *concept* of belief. It is the *concept* of belief which is then claimed to be necessary for any kind of thought:

Can a creature have a belief if it does not have the concept of belief? It seems to me it cannot, and for this reason. Someone cannot have a belief unless he understands the possibility of being mistaken, and this requires grasping the contrast between truth and error - true belief and false belief. But this contrast, I have argued, can emerge only in the context of interpretation, which alone forces us to

the idea of an objective, public truth. (Davidson,1975, p.22).

Up until this point Davidson has argued that all thought is parasitic upon a background structure of beliefs. The ascription of thoughts by a third person to a non-linguistic creature would always be 'hazy' or underdetermined because sophisticated communication is necessary to pin down particular beliefs and thoughts with a sufficient degree of accuracy. At this point Davidson and Armstrong could still be reconciled. However, Davidson goes further, arguing that a notion of truth/falsity comes from membership of a linguistic community and publicly shared standards of truth, from which one gains the general *concept* of belief.

At this point it becomes clear that Davidson's model of thought is essentially linguistic: i.e. all beliefs are uttered or silent statements of a proposition to oneself or others. To have a belief, for Davidson, is to entertain a statement of the form 'I believe X' where 'X' is filled in by some propositional clause describing a supposed state of affairs in the world. Clearly, in such a case the individual must have language in order to entertain such propositions, and they must possess the concept of true/false belief. It therefore becomes finally clear that Davidson's whole theory of thought is bound to show a necessary link to language, since - although never explicitly stated - his model of thought is propositional. Though I agree with his reasoning to the conclusion that propositional thought necessitates the individual's grasp of the concept of true/false belief, and that this in turn comes from being an active interpreter and member of a speech community, I would challenge the implicit premise (unsupported by further argument) that all thought is necessarily propositional in form whether uttered publicly or not.

The above analysis of Davidson justifies my characterisation of him as 'neo-Cartesian' as regards his view of our relation to other species and what we can say about their mental lives. Malcolm's analysis of Descartes cited above also concludes that the implicit model of thought is propositional, and therefore necessarily exclusive of non-linguistic creatures. Malcolm argues that one should distinguish between "thinking P" and "having the thought 'that P' " or "believing P" and "having the belief 'that P' ". Malcolm argues that whereas the second version in each case is necessarily propositional, the first is not. He claims that writers such as Descartes and Davidson overlook this distinction and urges that "We need to avoid identifying thoughts with their linguistic expression" (Malcolm,1977,p.55). He concludes:

It is the prejudice of philosophers that only propositional thoughts belong to consciousness which stands in the way of our perceiving the continuity of consciousness between human and animal life. (ibid,p.57).

Though I am in complete agreement with Malcolm's conclusion, I am not altogether convinced of the strength of the argument which he presents as a counter to the Descartes/Davidson position. I believe that further support for the position which Malcolm, Armstrong and I myself wish to defend can be gained by bringing in considerations from evolutionary theory. What I need to show is that there are behaviourally significant modes of thought which are non-propositional in form. This idea is, in my opinion, not merely possible, but a necessary postulate of evolutionary theory. If this can be shown, then Davidson's neo-Cartesian position about the necessary connection of thought and talk will be shown to be a special case applying to a subset of 'thought' in general. The way will then be opened to extend the concept of mind and resultant forms of behavioural

explanation to species other than our own, irrespective of their capacity for linguistic expression.

The publication in 1859 of Darwin's Origin of Species was as much a milestone in philosophical thought as it was in the history of the natural sciences. The reason is that for the first time there existed a theory capable of placing human history in an all-embracing framework whose terms were within the range of natural science. Hence, at least potentially, the need for non-naturalistic metaphysical explanations of the origin and continuing course of human history were made redundant: the promise was that, given time and research, vast areas of knowledge previously confined to the realms of myth and religious explanation could be brought within the scope of the natural sciences. Not since the emergence of mathematically-based science in the Enlightenment had there been such a prospect for the furtherance of knowledge.

Philosophy, however, deals not so much with contending theories from an empirical perspective (since this is the job of the scientist proper) but with the credentials and *logical* compatibility of contending theories aspiring to the title of 'knowledge'. Since evolutionary theory is logically compatible with any number of other theories within philosophy, its impact has been perhaps less than it deserves. Despite this, several philosophers have made an attempt to put forward and develop a case for the significance of evolutionary theory to philosophical topics centring on the philosophy of mind and related areas (Cf. Hooker,1987; Crook,1980; Seitelberger,1984; Kaspar,1984; Campbell,1982).

The modern evolutionary approach to the philosophy of mind typically employs the vocabulary and descriptive technique of information-processing theory. Obviously drawing on theories of computer and artificial intelligence, this approach is employed since its terms are 'neutral' as regards the kind of thing which might be a candidate for possessing a mind. That is, it is essentially a functionalist approach, allowing for the possibility that minds could be instantiated in any kind of 'hardware' system capable of sustaining the requisite functions. The first important philosophical consequence of such an approach is therefore the rejection of all substance dualist (Cartesian) theories of mind, along with any other forms of theory which postulate non-naturalistic elements or properties of elements in their descriptions. Though I must accept the logical constraint that Cartesian dualism as a theory of mind is logically immune to any form of empirical argument, I would contend that the expanding neurosciences and ethology show increasing evidence for the thesis that mental phenomena are in some way identifiable with or explicable in terms of changes in the organism's brain states and CNS. Any philosophical attack on modern dualist theories of mind (e.g. the dualism put forward by Eccles and Popper, 1977) must therefore be made in terms of plausibility, since as far as I can see there can be no form of a *a priori* knockdown argument against the substance dualist. The naturalist position does, however, coincide with experimental data on the effects of drugs and lesions on certain areas of the brain, and the relation of these changes of physical state to the subject's subjective psychological experience. The dualist may reply to such empirical evidence with some form of interactionist theory, but this merely complicates their case further without removing the central questions concerning causal relations between physical and supposedly non-physical substance. The naturalist stance has

parsimony on its side, and an affinity to the well-established body of theory and experimentation in all existing areas of natural science. In short I believe that the onus is on the dualist to produce some reason why, now that there exists a naturalist theory for the existence of mind, one should not accept it. As one of the strongest recent advocates of the naturalist approach to the philosophy of mind has put it:

In the absence of solid evidence for the separate existence of the mind, the appeal of substance dualism fades. (Churchland,1986,p.321).

(For further discussion of the claims and counter-claims of substance dualists and physicalists Cf: Churchland,P.S.,1986,especially pp.317-335). I will therefore offer no further argument at this stage than that already given above, and state that as a rough definition of the evolutionary approach to mind, it is taken to be the subjective experience of as-yet imperfectly understood processes occurring in the physical organism, due to the evolved structures and capacities of the brain and CNS.

The theory of evolution by natural selection states that all species of life on this planet have evolved from historically earlier forms. Whatever characteristics an extant species displays have come about as a result of a process of genetic inheritance and change across generations, whereby historically later forms have evolved from their ancestors. If one defines 'behaviour' in the general terms of the interaction of organisms with their environment, then evolutionary theory tells us that the behavioural capacities of all organisms owe their form to the capacities of phylogenetically earlier species, from which they have evolved. Although it is notoriously misleading to suggest that the facts of evolution point to any 'line' or 'direction' of development (since this suggests that

natural history has a teleological structure) one may nevertheless attempt some generalisations concerning the course of evolution so far. In his work The Evolution of Human Consciousness J H Crook discusses the theories of the biologist L T Hobhouse (Cf. Hobhouse,1915), who like many late nineteenth century thinkers was inspired by the promise of Darwinian theory:

In posing the question as to whether there is some constant direction from lower to higher states of biological organization, Hobhouse concluded that the one consistent change of this type lies in the growth of mind. (Crook,1980,P.17).

Though I would be wary of any attempt to characterise the general evolutionary process as displaying any particular line of development across time for the reason noted above, I do find the line of thought suggested by Hobhouse and Crook sympathetic to my own argument here.

Mind...is known by its functions. The function which modern philosophy seized upon as expressing the vital essence of Mind was that of bringing things together so that they have a bearing upon one another. Where there is Mind there is order and system, correlation and proportion, a harmonising of forces and an interconnection of parts." (Hobhouse, cited in Crook,1980,p.18).

Whether or not one takes exception to the teleological tenor of Hobhouse's thought, it is interesting to note how at the turn of the century mind was already being identified with and explained in terms of an organism's capacity for what would nowadays be termed 'information-processing'. Modern theorists have reached similar conclusions to that of Hobhouse - for example Robert Kaspar: "the characteristic of the process of evolution is a permanent increase in *order*" (Kaspar,1984,p.51). The same line again is taken up by Franz Seitelberger:

The general function of nervous systems, therefore, is the processing and integration of information for the formulation of instructions which meet the needs of the situation and of the organism with regard to adaptive behaviour and survival in the environment.In the course of evolution these characteristics became more and more clearly expressed. (Seitelberger,1984,p.125).

Having begun with such a general characterisation of the origin of mind, one can go on to distinguish between different species according to their capacities for entering into different kinds of informational exchange via behavioural interaction with their environment. Some organisms, though well adapted to their environmental niche in terms of their behavioural repertoire, show no capacity for reacting to novel informational inputs. Their behavioural response is fixed by the information coded for in their DNA and their nervous system is incapable of modifying future behaviour in response to past experience. In short, such organisms show no capacity for learning. Their behaviour is explicable within a model of genetically heritable responses to a limited range of environmental stimuli:

Such organisms have evolved their complex response repertoires as the means whereby they can relate to stimuli impinging from outside, either through direct tactile contact or at a short distance, by vibration or chemo-sensory means. Behaviour here is a genetically programmed capacity for an adaptive response to the environment. (Crook,1980,p.21).

However, other organisms evolve locomotive capacities, with the result that "one end is the front and the other the back" (ibid,p.21). Such an elementary feature of morphology has drastic consequences for the resultant development of the organism's behaviour-regulating systems.

The consequence of having a front end is that stimuli tend to make their impact there first. It is thus that the 'head' as bearer of an impressive array of receptor organs (sight, sound, odour) has evolved. And

to integrate this bombardment of sense stimulation and to organize responses to them, the ganglion of this segment (often plus several additional ones) becomes greatly enlarged to form the brain. In vertebrates the brain is the much expanded front end of the spinal cord. (ibid,p.22).

Thus, as the organism evolves novel behavioural capacities for interaction with the environment, there is a corresponding 'demand' or selective pressure for the evolution of more complex information-processing capacities to organise and integrate the informational novelties with further appropriate behavioural response. Once the autocatalytic nature of such a process is grasped, it becomes clear that to speak of 'behaviour' and 'morphology' as if they were in fact distinct and separable features of an organism is a theoretical falsification of the true complexity of evolutionary development. (It is the tendency to create such isolated theoretical schemes for the description of organisms which spurs much of the work of Piaget, who insists on the importance of grasping the necessary relation between behaviour and morphology, and taking a theoretical stance which links behavioural change and morphological evolutionary development. See, for example Piaget,1979 & 1980)

The point of this brief excursion into evolutionary theory is that it soon becomes clear that non-linguistic species of organism may nevertheless have sophisticated ways of organising the information provided by their physical senses. This information is then integrated with other sources, such as hereditary information coded for in their genes and memory traces held in their nervous system. All of this is further integrated with ongoing behavioural interaction with the present environment. Traditionally the term 'concept' is used within analytic philosophy to denote an individual's grasp and use of certain linguistic

units or ideas whose formulation and expression is necessarily achieved through the subject's use of language. However, I see no reason why it should be so restricted. I believe that it may also be reasonably used to denote any example of relatively complex information processing carried out by an organism in integrative behavioural response to the environment. Such a non-linguistic and essentially functionalist definition of 'concept' is employed by C A Hooker in the following passage:

To possess a concept is to possess a requisite structure in the mind-brain that can act in the appropriate ways as a factor in the information processing. For to possess a concept is, roughly, to be able to classify in certain ways, relate classifications in certain ways, reason in certain ways...and on the present view these processes are transformations in the total information flow, they are to be construed as information processings. (Hooker,1975,p.2).

In an attempt to place the phenomenon of human consciousness within an evolutionary perspective, J H Crook lists those aspects of consciousness which he regards as being typical of an 'advanced' organism. They are: an increased capacity to store past events in memory; awareness through introspection; re-representation of the past for comparison with the present; discriminatory planning and foresight; and finally a series of different levels of information 'screening' so that the flood of novel information at any one time does not swamp the system's receptors (Crook,1980,p.24): Such processes clearly increase the 'adaptive potential' of the organism for novel and appropriate responses to immediate and possible future environmental exigencies. Crook goes on in the same chapter to offer a view of 'mind' and non-linguistic concepts which is in line with those already quoted above from Hobhouse and Hooker, and which I am suggesting show a greater awareness of developments in the empirical

sciences, rather than relying on traditional usage as is perhaps the case within the analytic tradition of philosophy.

The interpretation of 'mind' as information processing with a self-monitoring facility called consciousness goes a long way towards resolving the problem of body-mind interaction which Popper and Eccles (1978) have recently revived. (Crook,1980,p.28).

Consciousness is not always a patterning in words. While the inner metaphors of sensory experience are, as Jaynes (1976) affirms, verbal, consciousness is clearly not confined to linguistic themes. (ibid,p.30).

Hooker also recognises the relevance of his approach to contemporary issues in the philosophy of mind, and the effect of limiting one's definition of 'thought' in precisely the way in which my analysis of Davidson has brought out:

Only when all 'thought' in the general sense of the term, is viewed as covert talking in a language - a view apparently widely (if often tacitly) held today - does prelinguistic intuition take on a mysterious, ad hoc character. (Hooker,1975,p.3).

Further to these arguments about the definitional scope of such terms as 'mind' and 'concept', and in support of my thesis that at least some forms of thought are non- or pre-linguistic, I would offer the following two related arguments. These both rely on deductive reasoning from general evolutionary theory, and on empirical evidence which backs up the deductive reasoning. In my opinion they raise insoluble problems for Davidson's view of language and thought as inseparable, and point to the necessity for the incorporation of certain aspects of empirical knowledge into areas of philosophical speculation.

The first of these arguments is the 'ontogenetic argument', and points to the facts of individual development in human infants. Observation of

pre-linguistic behaviour, coupled to reasoning about how and when language is acquired seem to lead to the conclusion that an infant's mental development and the rudiments of conceptual thought precede acquisition of language. This evidence, apparently in contradiction to the thesis of Davidson and others highlighted above, is examined by Gerard O'Brien (O'Brien,1987). O'Brien is attacking the philosophical position he labels 'Psychological Nominalism' which he defines as: "the claim that all perceptual awareness is preceded by the acquisition of, and mediated through, language." (O'Brien,1987,p.51). This view is chiefly attributed to Wilfrid Sellars, Richard Rorty and Paul Churchland, though it is also the position of Davidson which I have analysed above. O'Brien makes this clear in the following passage:

The picture that Sellars, Rorty and Churchland conjure up, then, in embracing the thesis of psychological nominalism is one in which the developing child prior to language acquisition is conscious of nothing but a complete 'blur'. As the child acquires its first few words it begins to become dimly aware of some ambient structure. This awareness increases proportionally with the development of language....In sum then, according to the thesis of psychological nominalism, perceptual awareness and language develop in a parallel fashion: the ontogenesis of the former crucially depends on the acquisition of the latter. (ibid,pp.54/55).

O'Brien then proceeds to cite empirical evidence which contradicts the thesis of psychological nominalism. This is based on experiments in self-recognition, in which infants were presented with mirrors, having first had a visible mark surreptitiously placed on some part of their face. The point of the experiment was to see if the infants would be able to use the mirror as a mirror; to examine themselves and the mark on their face. The results of the experiment showed that infants displayed some degree of self-directed behaviour by using the mirror appropriately during the first

year of life, and prominently so by the age of eighteen months. The use of the first person pronoun does not normally occur, however, until around twenty four months. O'Brien concludes:

Consequently, perceptual development in infancy is not solely mediated by the acquisition of language: to reiterate, children are *pre-linguistically aware*. As a result, therefore, we must posit the existence of conceptual structures underlying the perceptual awareness of young children which are not acquired in a linguistic fashion. Psychological nominalism as originally propounded by Wilfrid Sellars appears untenable. (ibid,p.59).

Further to this specific example of empirical data, I would urge the thesis of pre-linguistic conceptual development from theoretical considerations about ontogenetic development. A human infant's behaviour may in the earliest stages be confined to a range of reflexive actions geared towards a narrow group of goals (e.g. food, warmth, maternal comforting, avoidance of pain, etc.). But this early repertoire soon develops and adjusts to specific features of the environment, these features becoming incorporated into the infant's development through exploration and learning. Thus, if 'conceptual thought' is posited on the evidence of behaviour which displays an ability to integrate and manipulate one's perceptual data in certain logical, essentially beneficial or advantageous ways, then I would urge that there is clear evidence of such mental states in the behaviour of the pre-linguistic infant. Not only this, but I would argue that it is difficult to see how an infant *could ever begin* to become linguistically adept unless it already possessed certain pre-linguistic mental structures. If natural language is a system of formalised symbolic expression and representation of one's thoughts, it would seem necessarily true that some rudiments of thought must exist prior to the process of language learning by any individual. If infantile 'thought' prior to language acquisition were

just the 'booming, buzzing blur' in which the psychological nominalist would have us believe, then it would appear impossible for such creatures to ever get off the ground as far as language acquisition is concerned. Even before one begins to consider the theoretical implications of a Gricean account of the necessary extra-linguistic features of being involved in linguistic communication, it seems that such confused creatures as pre-linguistic infants supposedly are could never begin to grasp the significance and structure of language at all.

On Davidson's account it would appear that one must already be a language user in order to recognise any language as such (since to be pre-linguistic is to be 'thoughtless' and therefore incapable of any form of recognition or systematic assimilation of information). Such an argument would also seem to apply to possible amelioration of existing language users: if conceptual capacity is necessarily limited to linguistic ability and vice versa, an account of intellectual development would appear to be at the least problematic. The thesis that language and thought are two sides of the same process is surely brought into question by the occurrence of urges to express feelings or ideas for which adequate linguistic formulations may be lacking, suggesting that conceptual development and mental life in general may at times outstrip certain specific areas of vocabulary and linguistic competence. (The strict identification of thought and talk makes the notion of searching for 'le mot juste' incomprehensible.) To sum up, consideration of human ontogenesis points to the conclusion that perceptual awareness and even elementary forms of *conceptual* development are neither continuous nor identifiable with linguistic development. To insist on a theory of mind which limits the use of the term 'mind' to purely linguistic abilities therefore appears

to be a false and unnecessarily narrow theoretical approach, and one which is in obvious contradiction to deducible and observable facts about individual development.

The second argument against the view of Davidson and the other psychological nominalists is based not on considerations of individual development, but on considerations of phylogenetic (i.e. species-ancestral) development. Once again coming from evolutionary theory, it could be introduced under the slogan 'Natura non facit saltus' ('Nature doesn't take leaps'). Simply, the theory asserts a principle of psychological continuity across evolutionary time and evolving species. The principle is stated in an article by G Matthews as follows:

The Principle of Psychological Continuity is the principle that psychological acts, states and functions in lower animals *model* those in higher animals....For any given psychological state, act or function, ψ , if a given animal belongs to some species other than the lowest one and that animal is capable of ψ , then there is an animal of some lower species such that the lower animal is capable of some psychological state, act or function, ψ' , and ψ' is a model of ψ . (Matthews, 1978, p437).

Matthews cites Aristotle as an intellectual forerunner for such a concept of the relations of species and their capacities, and once again the arch-enemy of such views and the father of modern opposition to the principle of psychological continuity is Descartes:

Yet modern thought offers a very important threat to the Principle of Psychological Continuity. It is Descartes' concept of mind....according to Descartes, despite the apparent continuity between human beings and lower animals, human beings are conscious whereas non-human animals are mere machines, automata. (ibid, p.441).

In my opinion, the thesis which Matthews is putting forward here is the inevitable philosophical outcome of the acceptance of the naturalistic account of the origin of species offered by Darwinian evolutionary theory. If man is a product of evolution, then the capacities he possesses are the result of a gradual process of change stretching back through his phylogenetic ancestry. Present-day capacities - morphological, mental, and behavioural - are inseparably tied by a history of development from earlier forms. Such a thesis is taken for granted in the case of morphology, but often denied in the case of the behavioural or mental capacities of man. Such a denial, however, is an implicit denial of the general application of evolutionary theory, and without further argument, constitutes an arbitrary rejection in one area of the implications of a theory whose credentials go unquestioned in other areas. The 'commitment' to acceptance of evolutionary theory and its implications in all areas is recognised by Hooker, who also espouses the principle of psychological continuity in the article already referred to above:

The leading motif here is the commitment to theorising humans as a natural species, to seeing humans first as part of nature. An important part of this commitment is therefore to theorising cognition as a natural capacity, one 'grading back' into the more generalised abilities of other species. (Hooker,1987,p.8).

judgement, risk and optimising all 'grade back' to more elementary operations. And present capacities can be understood as outcomes of selection because belief-forming processes are directly related to, and tested by their outcome. (There is no inherent linguistic bias, since there is no a priori commitment to the forms which cognitive states take or to the channel characteristics of epistemic sources). (ibid,p.22).

It would seem from the considerations I have laid out above that the phylogenetic argument from evolutionary theory also provides grounds for

rejecting the thesis of psychological nominalism. Whatever the capacities that linguistic man has, they did not appear simultaneously *ex nihilo*. Such a view was available to Descartes, since his theory of mind was grounded in an ultimately theological doctrine about possession of a soul. Once one accepts the post-Darwinian story of man as one species among many, and the latest product of a continuous chain of gradual evolutionary development, this carries further philosophical commitments of relevance to philosophy of mind. Unless there had been some prior development of pre-linguistic perceptual awareness and rudimentary conceptual abilities geared to the coordination of behaviour to pertinent environmental features, our species-ancestral line would simply have become extinct long before our own appearance on the planet.

This argument applies not only to our own species' ancestry, but also to the other extant species. Except for those species that inhabit a very stable and isolated environmental niche, some degree of active, updated coordination of behaviour and integration of information is required if the species is simply going to survive. Simpler organisms may survive by evolving only a small repertoire of innate behavioural responses well-tuned by constant and consistent adaptation to environmental pressures. The price paid for this method of survival is the risk of sudden and complete extinction due to rapid environmental change (such as that brought about by man's intervention in otherwise undisturbed ecological systems), and consequently only a few species can perhaps afford the long-term risks of such a somnambulant existence. For the rest - and this certainly includes all mammals - the ability to assimilate and respond to information concerning the environment in complex and advantageous ways is the price demanded by their informationally-rich and varied habitat.

The biological advantage of being able to 'mentalise' or 'represent' the features of the external world in some way is recognised by evolutionary theorists:

Glancing at the animal kingdom as a whole it is apparent that biologically simple organisms regulate their transactions with the environment primarily through a genetic endowment of relatively inflexible action patterns. The addition of learning skills and the intelligent construction of knowledge through exploration gradually becomes a predominant feature of the transactional style....With increased capacity for learning and intellectual functioning the *concept* replaces the *percept* as the controller of schematic behaviour. (Crook,1980,pp.32/33).

Having argued against the psychological nominalist thesis, I feel bound to end this discussion on a note of caution which will perhaps redress the balance to some extent. To say that the *a priori* denial of a mental life to human infants or other species is an untenable position is not, as some appear to think, to open the flood-gates and allow anything at all to be ascribed to dumb creatures. In chapter two of Brainstorms (Dennett,1978) Daniel Dennett attempts to offer a functionalist account of mind, in which the possession of a 'print-out facility' (i.e. the ability to communicate in some language) also appears to be a prerequisite for awareness of any kind. At least, Dennett argues, without some means of two-way linguistic communication we cannot even begin the process of inferring and ascribing mental life to others. However, in the course of explaining his own position Dennett makes what I believe to be an unfounded criticism of Thomas Nagel's seminal paper "What is it like to be a bat?" (Nagel,1979). In this paper Nagel forwards the argument that there are no *a priori* reasons for denying subjective states of awareness to creatures such as bats, though - in view of the very different perceptual apparatus they have been furnished by evolution - we as humans

may not be able at all to imagine what kind of mental life a bat has. Beyond the mere ascription of 'consciousness' the specific phenomenal features of a bat's mental life must remain alien to us. As far as Nagel is concerned, however, this merely reflects the limitation of our own powers of imagination, and does not constitute any grounds for withholding our ascription of a mental life to such species. (Cf. Nagel, 1979, especially p.168.) Nagel's basic intuition is that it is like being *something* to be a bat, though not to be a brick or a hamburger. Nagel's argument thus aligns him with others such as Matthews who espouse some thesis of psychological continuity between species. Dennett, however, takes exception to this ascription of mental life to creatures which lack the all-important factor of linguistic communication:

Nonhuman, nonverbal creatures have no print-out faculties, or at best very rudimentary and unexpressive print-out faculties, yet some philosophers - notably Nagel - insist that full-blown, phenomenological consciousness is as much their blessing as ours. (Dennett, 1978, p.152).

Since Dennett is attempting to give a functionalist account of essentially human consciousness, which he describes as "full-blown, introspective, inner-world, phenomenological consciousness" (ibid, p.149) it would appear that Dennett is accusing Nagel of a patently false position. He appears to be accusing Nagel of attributing consciousness to a bat of a kind comparable to our own, since Dennett describes both human and a bat's supposed consciousness as 'full-blown, phenomenological consciousness'. However, Nagel is at pains to point out that whatever consciousness a bat may have, it is surely of such a different order that - beyond the mere ascription of its existence - we cannot even begin to conceive of it. Dennett's 'Village Verificationist' stance as regards the ascription of

consciousness to others seems to lead him mistakenly into interpreting the ascription of consciousness to others as an all-or-nothing affair. If I am correct in portraying Nagel's position as coinciding with the 'evolutionary perspective' outlined above, then consciousness is being posited as a matter of degree or richness as regards different species. If 'full-blown' is some measure of human consciousness, then that of a bat is far from such a state, and to use the same terms to describe the two sets of phenomena is misleading of Dennett.

Although Nagel may not be guilty of the crime of which Dennett apparently accuses him, there are others whose innocence would be harder to prove. In his article 'Self-Awareness in Primates' (Gallup, 1979) G G Gallup also presents empirical evidence in support of the thesis that there is a degree of psychological continuity between humans and other species. Using the same experiment as described above in the case of human infant development, Gallup discovered that Chimpanzees are the only other known species able to use a mirror as a mirror. However, such a discovery merely points to the fact (as in the human case) that there is behavioural evidence for some degree of perceptual awareness and conceptual thought in the creature. Thus, such data would seem to suggest that - in some respects at least - the inner states of an adult Chimpanzee are comparable in terms of conceptual ability to those of an eighteen month old human infant. If Gallup were to end his article with such a conclusion, I would find no fault with it. But he then goes on to extrapolate from the evidence of self-recognition capacities to speculate about far more complex concepts such as 'the self' and 'identity'. This line of reasoning leads him to state that:

In principle, once you can conceive of yourself, you can begin to think about yourself. Once you can

become the object of your own attention, you can begin to contemplate your own existence. (Gallup,1979,p.421).

Such reasoning is, in my view, already fallacious, since I can see no necessary logical connection between the ability to recognise one's own image and the possession of such abstract concepts as 'existence'. The falseness of Gallup's reasoning becomes more obvious in the next lines:

If you can contemplate your own existence, then it is a fairly simple and maybe even logical next step to begin contemplating your nonexistence.... In principle it ought to be possible to educate the chimpanzee about its inevitable demise, but at least one psychologist has already expressed hesitation about doing so. (ibid,p.421).

Though I applaud the moral conscience of such researchers, I am not so impressed by their reasoning about the logical implications of the behavioural evidence they have gathered. To argue merely from a creature's ability to apparently conceptualise about certain visual stimuli and the significance of an object before it, to the worry that it is potentially capable of entertaining concepts such as personal existence, life and death, is in my view to commit the fallacy which Dennett falsely attributes to Nagel. It is to conflate evidence for certain specific forms of conscious awareness with those elements typically known from human consciousness. In short, it is to ignore once again the evolutionary perspective regarding the likely comparative mental lives of different species. Consciousness is not an all-or-nothing affair, but will differ from one species to another in terms of richness and specific features. Behavioural evidence of some aspects of consciousness as it is known to ourselves does not imply actual or potential possession of *all* aspects of consciousness (as it is known to us), however tempting it may be to speculate on such an inference. To return to an earlier example: to be

touched by the maternal behaviour of an adult Chimpanzee with her baby is to recognise and ascribe sufficiently *similar* feelings at work. It is not, however, to attribute to the Chimpanzee the 'full-blown, phenomenal experience' of a human mother, which would be simplistic anthropomorphism as opposed to adopting a truly evolutionary perspective. It may be that in many cases, as Nagel suggests, we cannot even begin to imagine the real nature of a species' psychological experience. But, as Nagel and others have been at pains to point out, this is in itself no reason to erect an *a priori* barrier between our own species and others, as if any question of behavioural inference and the ascription of some form of inner mental life were in every case invalid or nonsensical. Such a prejudicial stance is exemplified by the neo-Cartesian views I have discussed above, and which I have tried to expose as untenable in the light of empirical evidence and deductive reasoning from evolutionary theory.

One of the main philosophical claims which I have argued is implied by acceptance of the Darwinian theory of evolution is that substance or Cartesian dualist theories of mind are ruled out as redundant and implausible (though logically possible) metaphysical views. There is another form of dualism, however, whose defenders find no fault with the evolutionary claim that mental processes are in some sense identical with physical processes instantiated in an evolved neurophysiological structure. Such theorists, whilst accepting a monistic materialist metaphysics, nevertheless deny the physicalist thesis that all explanatory terms and descriptions of phenomena including those of first person consciousness can ultimately be captured within the third person stance and causal relations vocabulary of the natural sciences model. The phenomena which they deny being in principle or practice amenable to the physicalist

programme of reduction are characteristics of the mental such as intentionality (Cf. Searle,1983; Malcolm,1982) and qualia (Cf. Nagel,1979; Jackson,1982).

In response to the worries raised by the 'ineffable' nature of such aspects of first person phenomenal experience, the most promising attempt to construct a plausible materialist account of the mind is functionalism. One of the staunchest defenders of the functionalist programme is Daniel Dennett, whose work has already been cited above. In this same text, Dennett offers a definition of the functionalist standpoint vis-a-vis the mental, and how the project of functionalist translation of existing mentalistic terms is to be carried out:

The *content* (in this sense) of a particular vehicle of information, a particular information-bearing event or state, is and must be a function of its function in the system....The content of a psychological state or event is a function of its function, and its function is - in the end, must be - a function of the *structure* of the state or event and the systems of which it is a part. (Dennett,1978,p.163).

Though such a model satisfies the materialist stance of a naturalist evolutionary theory of mind, it does raise further problems. The most damaging of these, in my view, is that the functionalist account of mind may serve as the basis for the claims of epiphenomenalism. Since functionalism insists that psychological phenomena are identical with or instantiated in physical systems, it may be argued that such systems function in the way they do because of the sufficient causal connections and succession of causally antecedent system states. Such a view would render the whole consideration of consciousness in the human or any other species a redundant question, since it is argued that what is necessary to understand and describe behaviour is not any knowledge of first person

phenomenal experience (since this is causally inoperative) but rather a complete science of the physical properties of the neurological 'hardware' possessed by the organism.

Some functionalists (e.g. Dennett,1982) claim that from the functionalist point of view, a human and a chess-playing computer may be regarded as equivalent, in the sense that although the specific computations involved in playing may be different for the two, and instantiated in very different kinds of 'hardware', the overall functions in terms of relations of informational input and output are equivalent. (i.e. No matter how they go about it, both humans and computers play recognisable games of chess.) Thus, not only is the (for some) all-important distinction between conscious and non-conscious entities blurred or rendered irrelevant, but it can be further argued that the conscious states of the human are causally (and therefore explanatorily) superfluous. Just as the computer performs its functions through its possession of appropriate causal relations and events occurring in its system, allowing it to make the kind of functional relations of informational in- and output which constitute following the rules and tactics of a chess game, so it can be argued that in the human case it is the physically causal relations of states and events in the individual's neural system which enable the individual to play chess, and that behavioural explanation should refer only to such third person observable phenomena. (Functionalism is thus aligned with Behaviourism as far as its implications for behavioural explanation are concerned.) Any attendant phenomenal experiences of the subject, such as conscious planning, deliberation, frustration, excitement, inspiration, etc. which constitute the subjective experience of playing the game are just the epiphenomenal froth

on top of the brain's alpha waves. It would therefore appear that the attempt to capture the properties of human mental experience in terms of a 'neutral' vocabulary of materialist descriptive terms results in explaining away mental phenomena. The redundancy of the mental under a functionalist explanation thus leads to the position of epiphenomenalism, and an apparent evolutionary puzzle, which is recognised in an article by J Belloff:

That consciousness should have arisen at all becomes all the more puzzling when we consider how far adaptive behaviour can go without the benefit of sentience.... And, since consciousness confers no biological advantages, one may speculate that, given a slightly different twist at some earlier point of the phylogenetic sequence, evolution could just as well have culminated in a race of wholly insentient automata! (Belloff,1965,p.48).

The topic I wish to focus on finally is whether Belloff's puzzle about the evolution of consciousness and his imagined alternative natural history are really plausible philosophical positions in the light of evolutionary theory. Although I can see no knockdown argument against the epiphenomenalist position (i.e. I accept its *logical* possibility, as in the case of Cartesian dualism) I believe that the onus is once again on the epiphenomenalist to prove their thesis or disprove mine, since I will argue that there is more evidence in support of my own position, and that the epiphenomenalist story goes against a very basic intuition about the nature of our own behaviour and its explanation. I will therefore side with the 'anti-reductionist lobby' in their intuitions about explaining at least some instances of behaviour by reference to phenomenal experiences of the subject, rather than by reference to third person observable changes of state in the individual's brain and CNS.

The central argument of the epiphenomenalist position is sometimes implicit and sometimes explicitly stated. In his article 'Epiphenomenal Qualia' Frank Jackson states it in the following way:

No amount of physical information about another *logically entails* that he or she is conscious or feels anything at all. Consequently there is a possible world with organisms exactly like us in every physical respect (and remember that includes functional states, physical history, et al.) but which differ from us profoundly in that they have no conscious mental life at all. (Jackson,1982,p.130).

Thus we are told that there could be organisms capable of passing the most rigorous form of Turing Test, which would nevertheless lack the experience of any kind of inner mental life whatsoever. From this it is an obvious deduction that, in our own case, mental properties *may* therefore be redundant as far as causation or explanation of behaviour is concerned. However, it seems to me that the epiphenomenalist wins the argument a little too easily here - in fact I believe that the position outlined in the above quote from Jackson begs the very question at issue.

In a discussion of this question, Terence Horgan (Horgan,1987) also points out that the epiphenomenalist position rests on a piece of dubious deductive reasoning. Horgan concentrates on arguments about the supposed possibility of individuals who have 'inverted spectrum qualia' or who lack the qualia of visual perception altogether. (A version of this argument can be found in Block and Fodor,1972). Horgan's argument is to draw a distinction between 'imaginability' and 'metaphysical possibility'. Just because we can imagine something as logically possible does not mean that we are therefore bound to accepting it as metaphysically possible in some actual world. The criterion of imaginability is too weak for this, since it only guarantees logical possibility - i.e. X is imaginable if it does not

imply any form of logical contradiction. Horgan concludes the relevant section of his paper thus:

So the upshot is this. If one has an argument for the metaphysical impossibility of the above-described inverted spectrum and absent-qualia scenarios, then one will be able to handle the seeming imaginability of these scenarios in either of two ways. Either one can cleave to a stringent notion of imaginability which suffices for metaphysical possibility, and then deny that the scenarios are really imaginable; or else one can adopt a more liberal notion of imaginability, and then claim that these scenarios are metaphysically impossible despite being imaginable. (Horgan,1987,p.500).

The fallacy of the epiphenomenalist's argument is that although metaphysical possibility necessarily implies logical possibility, logical possibility does not necessarily imply metaphysical possibility. Just because the inverted or absent qualia scenarios are 'imaginable' does not mean that they are in fact possible in any world. Thus, though I may agree with the *logical* possibility of Belloff's imagined alternative natural history, this does not carry the further stipulation that I am bound to accept it as *actually* (i.e. metaphysically) possible. The point about Belloff's and other epiphenomenalist arguments is that they beg the question by stating that there is a possible world of insentient automata whose behaviour is identical to our own, and that it therefore follows that consciousness is causally inoperative and not essential to explaining behaviour. However, it is only logical possibility which is required as a criterion for imaginability, and not metaphysical possibility, which is the kind assumed in the various epiphenomenalist arguments about creatures which lack qualia or consciousness of any form.

Thus, one can follow Horgan's line of argument, and either adopt a liberal or a stringent notion of imaginability, neither of which will

commit one to accepting the actual possibility of the imagined alternative worlds. But this is to leave out a further side of the argument: what reason have I for doubting whether the imagined worlds are, after all, metaphysically possible? The initial response to this question must be one's intuitions. Again, Horgan expresses the natural response to the thesis of epiphenomenalism:

Denying the causal efficacy of all our qualia is, I submit, just too much. Epiphenomenalism concerning qualia should be an utter last resort, to be embraced only if all viable alternatives prove to be even more paradoxical and untenable than epiphenomenalism itself. So unless and until that point is reached, I think it is reasonable to adopt the assumption that qualia normally are causally efficacious. (ibid,p.504).

Beyond this intuitive response to the thesis of epiphenomenalism, I would offer a further argument from evolutionary epistemology. As already stated above, one can distinguish between different organisms in terms of their relative abilities to assimilate information from their environment, and to integrate this into future behaviour patterns (i.e. in terms of the creatures' ability to learn during its lifetime). The obvious aspect of specifically human thought which figures in discussions of behaviour is the ability to make abstractions from particular experiences, and to relate these via the use of symbols, to form further chains of reasoning. Thus, individual percepts take on a new and wider significance once they have been incorporated into a symbolic concept, through which they can be further rationalised. The importance of this aspect of human thought is discussed by Crook:

symbolization of events in the perceptual continuum provides the units for a pictorial logic from which the abstraction of grammar eventually emerged as a concomitant of sound production in the development of verbal communication.....An animal capable of symbolization can carry away from a situation an inner trace that stands for the response it may make

when it next encounters the situation. A central process comes to function as a substitute for actual sensory cues. (Crook,1980,p.34).

The importance of the ability to literally 're - present' the external world to oneself through remembered perceptual experience, plus the linking of such experiences with anticipated future events via their symbolic manipulation is recognised by other evolutionary theorists, who also make specific reference to the effect that such an ability may have on human behaviour:

object-like constructs are possible as images which are moved in imaginative action in order to once again be modularly distributed and revolved like concrete sensory data...This also seems to be the exit through which human beings leave the domain of the accidentally determined evolutionary happening, and try to find their way as the 'first freedman of nature'. (Seitelberger,1984,p.143).

It seems to me that this is generally the reason why man, who is able to detach himself from ratiomorphic thinking, is the only being who can *theoretically* think and need not remain in the grip of the phenomenon itself. He can reach a level of abstraction and comprehend connections of a causal nature which cannot be deduced from direct perception. (Kaspar,1984,p.61).

The central point of such passages is that the individual organism is able to 'carry off' some form of record of past events, abstracted from their particular time and place of occurrence, and thus to combine them with future events and, in the case of humans at least, with imagined possible future scenarios of the individual's own making. (This is captured in the German verb 'to imagine' which is *sich vorstellen*: literally 'to place before oneself'.) Thus, the organism which represents the world to itself in some form of consciousness gains a certain degree of autonomy from the stimulus-response mode of behavioural interaction to which non-conscious beings are tied. The important factor then is the ability to represent the

external world in some form of phenomenal consciousness, in which events are registered by some mode of subjective experience of phenomenal properties. The epiphenomenalist may argue at this point that, since I side with some version of a mind/brain identity thesis, it may not be the *phenomenal* properties of the inner representational states themselves which are of significance, but the *physical states* within the organism's neural system to which such phenomenal states correspond. Once again, the phenomenal properties themselves may be causally and explanatorily redundant.

In reply to this, I would return to an earlier point about what it is to have a certain thought. Davidson and others have described thoughts as being 'located in epistemic space' by their logico-semantic relations to the totality of other thoughts with which they share some semantic aspect, or beliefs on which they depend for their meaning. A thought is also (according to the evolutionary account) a state or event in the physical structure of the organism's neuronal system, and it must therefore also be 'located in *functional* space' by its actual and potential causal relations to the rest of the organism's physical system. The epiphenomenalist's position comes down to the argument that the location of a thought in functional space, by its causal relations to the rest of the physical system, is prior to its location within a logico-semantic network of representational phenomena within consciousness. In other words, the structure of the mind is dependent upon and wholly explicable in terms of the structure of the brain. This position is, in my view, invalid for the following two reasons.

Firstly, it ignores the strength and meaning of the mind/brain identity theory offered by the naturalist account of mind. The epiphenomenalist suggests that one can investigate and describe the physical system of the brain without at the same time having explained something about the mind, and vice versa. But this would only be true if the two were contingently related, rather than two aspects of the same thing. This is just what the epiphenomenalist in fact presupposes - which is why they hold that one could have two identical physical systems which nevertheless differed radically in terms of their subjective properties of qualia (see above quote, p.281). If, as I am arguing, the brain and the mind are merely two different aspects of the same thing, then the events and properties which fall under one side of the description cannot be said to be causally, explanatorily or in any way whatsoever 'prior' to the events and properties of the system seen from its other aspect. To use a common analogy, it is not the electrostatic discharge to earth of atmospheric particles which causes the bolt of lightning; rather it is the bolt of lightning. There is no question of relations of priority between the discharge and the lightning, since their relation is one of identity, and priority would require them to be distinct events. In the same way, certain kinds of physical system don't just happen to have or cause certain kinds of mental phenomena; rather they are certain kinds of mental phenomena. Once again, it becomes clear that the thesis of epiphenomenalism relies on the assumption that the physical and the mental are 'detachable' from each other, and one can therefore have the one without the other, with no effect on the properties of either. This, I have argued, is paraded as the conclusion of epiphenomenalism, whereas it is in fact its unsupported first premise, and one which is certainly not in line with common intuition or capable of standing unsupported by further argument.

The second argument against the epiphenomenalist comes from the existence of qualia and the properties of thought which allow them to take on logico-semantic relations in our minds. The whole idea of 'representation' which, it was argued above, is essential to explaining the behavioural capacities of some species, presupposes a mental life in which the world is 'internalised' by the organism via some form of mental process. It follows from the mind/brain identity theory that for some kinds of physical states in an organism there are two sets of corresponding properties. There are the physical properties of the state viewed as part of the organism's nervous system, and there are the phenomenal properties of the state as it appears to the organism in its mind. For a thought to have the epistemic relations that it has, it must be physically possible in the sense that the system must be capable of instantiating the sufficient causal relations which correspond to the epistemic relations experienced in thought. But *that* it becomes so related in thought is a function of its phenomenal properties, and the logico-semantic relations which these properties suggest or reveal. This is not to deny that physical properties of the organism's nervous system can have an effect on the mental capacity, and put limitations on the representational ability (e.g. through the effect of lesions, drugs or other forms of physico-chemical imbalance, or of metabolic processes such as ageing, etc.). What I am asserting here, however, which the epiphenomenalist denies, is that the kinds of epistemic relations which occur in the mind and which are referred to as constituting a logico-semantic as opposed to a causal system would not and could not occur if there were no qualia, or if the mental properties of certain brain states were different in the way envisaged in inverted qualia and similar imaginary cases. (This is not to deny that non-conscious systems can be built which are functionally

equivalent if not identical to conscious systems, as in the example of the chess-playing computer. However, the nature of the causally functional properties of such a system are determined by a human designer, and it can therefore be said to mechanically instantiate a 'derived representational capacity' which, in my opinion, is still initially dependent upon the designer's mental capacities. The same would hold for existing forms of so-called 'artificial intelligence' which function because their causal connections have been carefully designed via hard- and software architecture to emulate the informational responses of a conscious being.) In such cases, there are no 'logico-semantic' relations within the machine. What is present are causal relations within the hard/software which are functionally equivalent to epistemic relations experienced by the designers of chess machines. To summarise, what I am asserting is that in some (but not necessarily all) cases, our behaviour and that of some other species would *not* be the same if we lacked or had a different set of phenomenal properties as a result of our interaction with the physical world. To state an obvious case, it is not that we react to a stimulus in a certain way *and* it is painful, but *because* it is painful. The possession of a mind confers the ability to manipulate internal representations of reality, removing us from the exigencies of the immediate sensory world, and thus opening the way for behaviour which is planned, experienced and purposeful. To put it crudely, we no longer have to bump into things to find out or learn new facts about them.

Having said this, I do not wish to imply that linguistic ability has no resultant effect both on cognition patterns and behaviour. The appearance of language obviously furthers the symbolic and conceptual capacities which I have stressed above as being so essential to

'intelligent' forms of behaviour. The individual becomes linked to a common source of knowledge, so that potentially every member of the linguistic community can tap into the knowledge resources of the group as a whole. At this stage, the concept mediated through linguistic symbols takes over from first hand experience as the most important factor in guiding and shaping the individual's behavioural development, and the existence of a linguistic system becomes the central factor in determining the logico-semantic significance which further personal experiences are going to take on during the individual's life. After all, how much of our behaviour do we owe to the shaping force of first hand experience, and how much of it is due to culturally-derived knowledge of specific facts, or customs and social rituals which govern our behaviour and provide us with social codes and an identity? The process of informational exchange within this linguistic community is Lamarckian and not Darwinian, since new forms can be acquired and passed on during an individual's lifetime, can be passed on to non-genetically related individuals, and can become extinct for generations, only to be rediscovered later and incorporated into the life of the community once more. None of these forms of informational exchange are possible within the range of phenomena governed by Darwinian theory. The storage sites for information become progressively externalised from particular individuals, in the sense that the record of experience and thought may be held in some semi-permanent form by the production of cultural artifacts, whereas the storage site for information within Darwinian evolution is limited to an individual, or at the most a genetically related line of individuals.

It is because of the above views concerning the importance of conceptual thought in shaping the behaviour of individuals, that I reject

the sociobiological thesis that the social behaviour of human beings can be sufficiently explained within the methodological and theoretical framework of Darwinian theory and the biological sciences. To attempt to derive a full explanation of human behaviour from a knowledge of the sum characteristics of individual genetic endowments must be futile, since no individual can be fully explained except in terms of the social group to which they belong and from which they have derived the conceptual scheme which governs their interpretation of the world and the judged appropriateness of certain responses. This in turn demands an appreciation not of the sum biological characteristics of the group, but of its social history, and the ascendance and decline of the major concepts and leading symbols throughout its history. This is not to deny that there will be certain 'biological' constraints upon any social group, since some forms of behaviour will have a detrimental effect and can therefore be described as 'biologically disadvantageous'. What I do reject is the sociobiological assertion that *all* forms of behaviour are rigorously governed by their outcome in terms of maximising biological fitness potential of individuals or groups, and that such a theoretical stance can therefore be adopted as sufficient for analysing behaviour in general. Moreover, the processes by which information is acquired by individuals via linguistic communication and culture is not Darwinian, and differs in every major respect from the genetic process of information acquisition. It cannot therefore be made analogous to or brought within the theoretical scope of the biological sciences. Contrary to the assertions of the sociobiological texts, the key to understanding human behaviour lies not in any appreciation of relative genetic advantages, but in the appreciation of social history and the effects of social environment on individual psychological development. Our conduct in the world is not regulated by our genes and their derived

physical structures, but by our possession of certain concepts, and their effects on our interpretation of the world presented to us in phenomenal consciousness. It is not what our ancient cave-dwelling ancestors saw and experienced which counts, but what we ourselves now believe, and the processes of belief acquisition, social change, and cultural dissemination of information differ from those processes by which DNA is selected and replicated. The gap between biology and the social sciences can no doubt be usefully narrowed by a willingness to participate in interdisciplinary exchanges of information; but it can never be closed entirely by a grand theoretical synthesis. The positivist dream of constructing a seamless fabric of scientific theory, on which the tapestry of human knowledge could be embroidered, remains as elusive as ever.

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