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THE CULTURAL VIRUS

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The Crane Child
(An abandoned child adopted by artefacts)

A baby, a boy, called Michel...was born to a disoriented, possibly retarded teenager, the child of a rape. Until he was about two years old, he lived with his mother in a tenement next to a construction site. Every day she stumbled in and around and out of the apartment, lost in her own madness. She was hardly aware of the child, barely knew how to feed or care for him. The neighbours were alarmed at how Michel screamed, but when they went to knock at the door to ask her to quiet him, often she wasn't there. She would go out at all hours, leaving the child alone, unguarded. Then one day, quite suddenly, the crying stopped. The child did not scream, and he did not scream the next night either. For days there was hardly a sound. Police and social workers were called. They found the child lying on his cot by the window. He was alive and remarkably well, considering how severely he appeared to have been neglected. Quietly he played on his squalid cot, stopping every few seconds to look out the window. His play was unlike any they had ever seen. Looking out the window, he would raise his arms, then jerk them to a halt; stand up on his scrawny legs, then fall; bend and rise. He made strange noises, a kind of screeching in his throat. What was he doing? the social workers wondered. What kind of play could this be?

Then they looked out the window, where some cranes were in operation, lifting girders and beams, stretching out wrecker balls on their single arms. The child was watching the crane nearest the window. As it lifted, he lifted; as it bent, he bent; as its gears screeched, its motor whirred, the child screeched between his teeth, whirred with his tongue.

They took him away. He screamed hysterically and could not be quieted, so desolate was he to be divided from his beloved crane. Years later, Michel was an adolescent, living in a special institution for the mentally handicapped. He moved like a crane, made the noises of a crane, and although the doctors showed him many pictures and toys, he only responded to the pictures of cranes, only played with the toy cranes. Only cranes made him happy. He came to be known as "The Crane Child".

And the question Jerene kept coming up against, reading the article, was this: What did it sound like? What did it feel like? The language belonged to Michel alone; it was forever lost to her. How wondrous, how grand those cranes must have seemed to Michel, compared to the small and clumsy creatures who surrounded him. For each, in his own way, she believed, finds what it is he must love, and loves it; the window becomes a mirror; whatever it is that we love, that is who we are.

From the novel *The Lost Language of Cranes* by David Leavitt, 1986. London: Penguin.

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PART I

THE

CULTURAL VIRUS CRITIQUE

1

THE UNTRIED THEORY

Overview

In this chapter a general background to the study is provided, in two basic parts. Firstly, the problem of the "untried theory" is introduced and the basic aims of the study are defined in relation to this problem. These research aims are then related to a number of wider debates, some of which are not necessarily or frequently associated with neo-Darwinian theory.

1.1 An Unusual Abstraction

While the origin of evolutionary theory in anthropology is often traced to Darwin (e.g. Renfrew and Bahn 1991:22; and see White 1959b:106 for an opposing view), it is very important to distinguish the early theories of cultural evolution from Darwinian and neo-Darwinian theories about the physical evolution of humans. Ironically, although Darwin's special contribution to evolution was the elucidation of *how* evolution had occurred, rather than the fact of its occurrence, this innovation did not carry over into the study of cultural evolution. While the notion of the natural selection of random variation was to profoundly influence research in human physical evolution until the present day, no cultural equivalent of this mechanism was championed by the early cultural evolutionists. Rather, like the "red-Lamarckians" of the day (Desmond and Moore 1992:252, 275, 295), it is fair to say that whether inspired by Darwin or not, the early cultural evolutionists "took the idea and ran with it". The explanatory frameworks which they produced over the next century were anything but Darwinism, let alone neo-Darwinism; indeed, they resurrected almost all of the philosophical elements

which characterise the Lamarckian world view (see Chapter 2). The evolutionary legacy which we inherit today in cultural fields of enquiry has, therefore, very little to do with neo-Darwinian principles, and it is crucial that this fact is brought to the attention of the reader right from the very beginning of the study.

It is also important that the reader bear in mind that this study focuses on a deeper or more general level of explanation than that level which theoretical discourse is most commonly directed at. To use Chomskian language, one might say that this study is concerned not with the specific forms that theories of cultural change take, but with the deep structures behind them. However, unlike Chomsky's "rich innate endowment" (Mithen 1989:489), the "deep structures" of evolutionary theory (general concepts like "selection" or "determinism") are obviously not genetically determined.

Selectionism and its alternatives are metatheories, or theories about theories; they are concerned with the syntactical rules which govern the construction of causal explanations of regional sequences - a handful of background assumptions which are generally more implicit than explicit. To put it another way, evolutionary theories (with some exceptions) provide knowledge about the mechanisms operating in history, but not about the course of history itself, which only emerges in the analysis of a particular case (Shennan 1989:340). Whether any given explanation of a regional cultural sequence is judged to be neo-Darwinian or Lamarckian depends not upon the presence of certain diagnostic causes or effects, such as duck's feet and water, or ploughs and food shortage. Rather, it depends on the particular manner in which causes and effects are juxtaposed in an explanation: whether water causes the genesis of duck's feet, or merely selects for them once they appear; and whether food shortage causes the genesis of ploughs, or merely selects for them once they are discovered by happenstance.

When we debate whether to adopt a "neo-Darwinian" position at the expense of a "Lamarckian" one, we are therefore not concerned with the specific constituents of a causal explanation, however universal or particular

that explanation may claim to be. Strictly speaking we should be concerned only with the *manner* in which any such causal factors are *incorporated into* the explanatory scenario. Thus debates about the very use of Selectionism itself (as distinct from debates about the validity of a particular selectionist explanation of a particular structure) are located at an unusually abstract level of discussion. Theoretical discourse, whether in the organic or inorganic sciences, normally focuses on the level of "determining *which* causal forces are at work in a particular system", and not at the more general level of "determining *whether any such forces are selective at all*".

So most of the theoretical discourse that surrounds, say, the explanation of the tiger's stripes, would tend to concern which aspects of the jungle environment (interacting with which aspects of the tiger's phylogenetic heritage) are most responsible for the stripes. It would be already taken for granted that the final cause or causes of the tiger coat pattern, once ascertained, will be selective in nature, not determinative. Similarly, debates about the forces responsible in the evolution of hominids focus at the level of specific hypotheses about which forces are most pertinent in governing the reproductive success of early hominids. Whether a hypothesis concerns the need for the capacity for language, the advantages of upright posture in avoiding heat exposure on the savannah, or the need for social co-operation, it is taken for granted that these factors can only influence hominid gene pools by causing selection of some kind.

Nor is the situation substantially different in the humanities, although it is not identical, as we shall see. Theoretical debates in cultural fields of enquiry commonly take place below the level of abstraction at which the neo-Darwinian/Lamarckian logical dichotomy occurs. A group of archaeologists or anthropologists debating the relative merits of Freudian and Marxist theory are most likely to argue about which list of causes (for example the social and economic versus the individual and psychological) is most correct. It is usually taken for granted that these causes produce their effect by "determining" rather than "selecting for" particular states of mind, and thus the debate takes place within an implicit and largely unquestioned "Lamarckian"

framework. Finally, even theoretical discourse in the inorganic sciences takes it for granted that the causal forces in its desmesne are *not* selective, but rather determinative (or chaotic) in nature. Debates about the causes of earthquakes concern the relative importance of a range of causes, but all of them are assumed to take effect through imparting some direct transformation to a tectonic system, not through causing the differential reproduction of, say, lava at the expense of ash, or *vice versa*.

As in other disciplines, then, many explanations of cultural phenomena are preoccupied with the identification of particular causal factors and local theories, ignoring general theories, in much the same way as many biologists or chemists may ignore the general theories of their domain, in their daily practise. But the similar situations are occurring for somewhat different reasons. If the problems of large scale theory are ignored by a biologist in his or her daily practice, this may often be because they consider these issues to be *already solved*. But when researchers in cultural fields of enquiry do the same, it may often be because they believe this problem to be *unsolvable*.

Whether we assume the "Lamarckism versus neo-Darwinism" issue to be solved or unsolvable, the net result is much the same. Very little anthropological research has been directed toward an evaluation of the intrinsic potential of selective logic itself, and more often than not the whole issue has been side-stepped in order to get to the more palpable problems of particular cultural systems. Like biological and inorganic science, the bulk of anthropological research takes place at a problem oriented level, and theoretical discourse, while moving to a more general level than particular problems, rarely addresses any fundamental universalities. But, unlike biology and physics, cultural fields of enquiry do not have a generally accepted theoretical umbrella which superintends problem-oriented research; there are many theories, but no "universal co-ordinating meta-theory" as it were, no equivalent of theories such as Quantum Mechanics in Molecular Physics, the Theory of Relativity in Astrophysics, or Natural Selection in Biology.

As a consequence, the humanities are now hindered by a peculiar situation; an overwhelming predominance of locally dependent (rather than universal) theoretical structures. The situation might be described as a curious "bottom heaviness" exhibited by the greater part of cultural and psychological theories. Compared to the general theories of other disciplines the general theories of culture are all rather specific. Alternatively, one might say that there is a rich diversity in what is sometimes termed the 'middle range' level of theory in anthropology and archaeology, in the form of a great deal of discourse concerning the causes or lack of causes of various cultural and psychological phenomena. These are the issues dealt with in the bodies of theory associated with words like Psychoanalysis, Phenomenology, Sociobiology, Functionalism, Structuralism, post-Structuralism, Cultural Selectionism, Marxism and so on. So many approaches investigate, in some form or another, the nature of certain causes, and the nature of certain effects, and, once they find a strong correlation, they see the link as something self evident in the correlation. They are thus side stepping the simplest and most general questions of all: what is the nature of these links *between* cause and effect in human behaviour, regardless of which of the particular causes and effects we choose to link? Does a given factor *select* from amongst a range of possibilities generated by the human brain, or does it actually *determine*, or shape directly, the behaviour it causes? Or is there perhaps no link at all? *What is the nature of the link itself?*

Many scholars of cultural fields of enquiry find it difficult to even imagine what is meant by such questions, so accustomed have they become to a dialogue which associates cause and effect without redressing the nature of the causal connection itself. In some cases they may even be theoretically unequipped to envisage a level of theory more general again than the level at which familiar dichotomies such as "Materialism versus Idealism" or "Structuralism versus post-Structuralism" (as evoked by Hodder 1989:70) seem meaningful.

But the issue of the nature of the causal linkages between the described/interpreted state of a given cultural phenomenon and its imputed

cause - in other words, whether these causal linkages are determinative or selective (or whether they are Lamarckian or neo-Darwinian in the strict mathematical or logistic sense of these words) - is indeed more general again than issues of the allocation and redistribution of causal primacy to the various realms of influence represented by different anthropological traditions. To put it another way, it is impossible to place, say, Marxism, Psychoanalysis, or post-Modernism in opposition to either neo-Darwinism or Lamarckism, since this latter dichotomy represents a higher generic category of theory classification. Any particular Marxist causal scenario may be shown to use either Lamarckian or neo-Darwinian causal connections, or a combination of the two, as may, for that matter, any particular Psychoanalytic or post-Modernist causal scenario. Thus it would be perfectly reasonable to ask a Marxist whether he or she intends to impose a neo-Darwinian or Lamarckian logical structure to their causal account; their answer to this question is by no means self evident in Marxism itself. Moreover, they are quite free to choose either one, and indeed they cannot avoid the choice. Any understanding of (or solution to) the "Lamarckism versus neo-Darwinism" problem thus requires some familiarity with an unfamiliar level of discussion. The notion of selectionist logistics itself, as distinct from any particular selectionist scenario, is an unusual abstraction.

1.2 A Latent Orthodoxy

As a consequence, most of the issues which occur at this unusual level of abstraction in cultural fields of enquiry have remained largely unresolved. Yet to a certain extent it may be argued that there is a "default" acceptance of determinism throughout archaeology, throughout anthropology, and even throughout the greater part of psychology; and this can be thought of as some sort of "latent Lamarckian orthodoxy" (Cullen 1986; and see also Chapters 2 and 7 of this study), although it is not always exactly latent. The Spencerian origins of Cultural Evolutionism, for example, are now well known, and Spencer's logic was classically Lamarckian; although this will not be

expanded upon here (again, see chapters 2 and 6 for elaboration). And there is further evidence of the pervasive occurrence of a deterministic orthodoxy in behavioural studies, evidence not directly connected to the selective issues of these chapters. To begin with, Post-modernist or post-processualist manifestos within archaeology have often dismissed all previous views as essentially determinist. Statements pursuing this theme may be found in Engelstad (1991:502), Hodder (1985:1) and Shanks and Tilley (1987b:56). Then again, at least three of the most influential thinkers of the humanities (Freud 1962:57-65; Marx, in Bottomore and Rubel 1963:67; and Skinner 1953:6) all expressed some explicit commitment to determinism. More recently, Edelman (1989:31-32) has summarised non-selectionist views of perception and categorisation in psychology as falling under two main camps: the "realists", who constitute a class of determinists, and the "nominalists" who might be characterised as espousing the opposite view, the simple binary opposite of determinism where categories are not determined by "reality" at all, being simply a matter of habit, or convention, a matter of the arbitrary allocation of names.

Determinism and chaos are really flipsides of the same coin, as each is ultimately defined in terms of the absence of the other, rather like Sartre's assertions of the relationship of the perception of an object with the perception of the absence of that object: the intimate relationship between being and nothingness (Sartre 1957 *passim*). Some evidence for their relationship may be found in the fact that where "human freedom" (randomness in human affairs) is explicitly incorporated into an explanation, it is sometimes quickly "reabsorbed" back into determinism through some notion of "self" determinism. This situation can be elucidated from Christian philosophy (Stevenson 1987:46-48) where free will results in the self determinism of sin, and from Dawkins's account of human freedom from genes (Dawkins 1989:201) where we rebel within the statistical constraints of our genes by making our own decisions; our genes "create" us, but they cannot "control" us; we are "free to determine" our own future. Similarly Jean Paul Sartre's basic concept of human freedom (Sartre 1957:439) makes our existence indeterminate; but we then decide what to make of ourselves, so determining

our future. Even Lorenz, who postulates that most animals are for the most part determined by the laws of Natural Selection (although Natural Selection, by definition, cannot determine anything, and it might be argued that he too conveniently forgets the heritage constraints on mutation) reserves a special freedom for humans and their powers of reason (Lorenz 1963:258). Yet with these powers of reason we then "self determine", taking charge of our future, redirecting our own selective pressures in the right direction. Thus determinism of some kind exists as a background element in many "freedom" philosophies in cultural and psychological studies, although its presence does not, of course, necessarily "spoil" the entire theory, or mean that the theory as a whole is determinist.

But as often as not this notion of the "deterministic" causal structure in cultural fields of enquiry is arrived at by default rather than by reasoned argument; a very large number of archaeologists, anthropologists, and even inorganic scientists are under the impression that the only alternative to determinism is complete randomness, which for some people is no alternative at all. In other words, it is thought that if human behaviour is not determined by "something", then there is no other way to explain any recognizable patterns encountered in the field of enquiry, other than by some appeal to the simple binary opposite of determinism, namely chaos or randomness. For some this is a disturbing thought. If an archaeologist, anthropologist, or psychologist were to admit to themselves any fundamental doubts about determinism, it might leave them with no viable route to explanation of the phenomena they encounter in their field of enquiry. To declaim determinism might be tantamount to forfeiting a claim to expertise.

1.3 The Untried Theory

The problem with these apparently endless oscillations between determinism and randomness is that selectionism is rarely given its full dues as a third possibility (in cultural fields of enquiry). It fact, it is more often

sold short as being one or the other, with those arguing for determinism dismissing selectionism as a theory of chance, and those arguing for "free will" dismissing it as "genetic" determinism. Thus one may sometimes hear a creationist dismissing evolution as a theory of chance; as being no more plausible than the idea that "a thousand monkeys bashing away at a thousand typewriters would eventually produce the complete works of Shakespeare" (to be fair, some unusual selectionist viewpoints - such as may be found in Kimura (1983, 1985) and Monod (1974) - do fit this statement more than others). Then, at the other extreme, we find cases where a selectionist position is dismissed as determinist, as when Dawkins's viewpoint is judged to be such (Dawkins 1989:331) by Lewontin, Rose and Kamin (1984).

A strange conundrum, but it is actually more simple than it seems. Both camps are half right and half wrong. Determinists such as creationists are correct when they say that selectionism is *not* determinism, but they are wrong when they say it *is* chaos. Conversely, the "high priests of the left" (Dawkins 1989) are correct when they say selectionism is *not* about complete freedom, but they are wrong when they say that it *is* about determinism. Selectionism is neither deterministic nor random, nor can it be understood in any but the crudest of terms as something intermediate between the two. Indeed, Wright (1967:117) has argued (and this is expanded upon in Chapter 6) that selectionism is in fact *qualitatively different* in its implications than any of these three alternatives. And Dawkins's selectionist position, so roundly denounced as determinism by Lewontin et al (1984), includes explicit lists of factors (Dawkins 1982:31) which act as constraints on adaptation, preventing Natural Selection from acting as a perfect determinant (see also Foley 1985:225 and Mithen 1989:484).

Finally, selection constitutes a qualitatively different alternative to determinism and randomness in epigenetic contexts also. Psychobiologists such as Edelman (1989:30-40), working within the different context of cellular selection in somatic time (the life time of the individual organism, and therefore in principle concerning purely epigenetic processes) views his "neuronal group selection" model of perception and generalisation as being

unclassifiable in terms of the old "realist versus nominalist" dichotomy. He clearly identifies the definitive mathematical element of his selective theory which precludes its classification under the old categories: its capacity to deal with the Wittgenstein (1953) problem of the "polymorphous set" (Edelman 1989:31).

So selective logic is clearly considered to be qualitatively different to the logic used by other notions of causality, and believed to be so not just by anyone, but by those who really ought to know (see Chapter 6 for expansion). And it appears that this vital element has been lacking in many theories of culture which either call themselves, or else have been thought to be, selective. As a consequence, there is as yet no well established cultural equivalent of the "expanded" synthesis of modern evolutionary biology (as defined by Gould 1983). This is the problem of the "untried theory", as outlined by Blute (1979), and implied by Boyd and Richerson (1985), Dunnell (1980), Freeman (1974), and Rindos (1980) *inter alia*. The problem of the "untried theory" - the lack of an established, genuinely selectionist explanatory framework for cultural process - is the central problem addressed in this study.

1.4 The Three Basic Aims

The three basic research aims all relate to the problem of the "untried theory". Put briefly, they are as follows:

1...To bring together the various evidence which demonstrates the existence of the problem itself. Neither Cultural Evolutionism, nor Cultural Sociobiology may be meaningfully viewed as being examples of the untried theory. Neither of these is a selective account of culture as the first is not selective, while the latter is not strictly "cultural"; i.e. not exclusively concerned with culture itself, but with its effect upon genetic process. And while

a number of recently formulated selectionist models of cultural process do exist, they are still fraught with serious logical inconsistencies; moreover, the approach has not been subjected to a large scale global test aimed at refuting its fundamental premises. (Chapters 2, 3, 4, and 5).

2...To devise a formula for a general or global test of Cultural Selectionism, whereby its fundamental neo-Darwinian premises might be refuted, and to perform a preliminary test in order that *the formula itself may be tested*, along with the hypothesis of a selective cultural process. Global variation in human skulls provides us with a prototype of a neo-Darwinian system, as they display the distinctive neo-Darwinian signature of "phylogenetic heritage constraint". Can this distinctive signature, some sort of "cultural heritage constraint" be found in artefacts and/or conceptual structures? It appears that the basic tenets of Cultural Selectionism, the "untried theory" of cultural evolution, stand up very well to the trial. (Chapters 6, 7, and 8).

3...To devise a new cultural selectionist theory whereby the logical inconsistencies identified in Chapters 4 and 5 might be avoided. Once a systematic critique of existing cultural selectionist models as been submitted (Chapters 2 - 5), there is an obligation to sketch a series of solutions to the problems of the critique. A theory is developed called the Cultural Virus Theory which allows artefacts, ideas, and behaviour patterns to be modelled as neo-Darwinian individuals, without attributing organismic status to them. Moreover, rather than being viewed as "traits" of humans, they are viewed as benevolent or malevolent "parasites". Modern psychobiology, in the form of Edelman (1989), provides us with detailed models of the "DNA" of the cultural virus, and from these models there is the potential to build an evolutionary "memetics" for the artefact (Chapters 9 and 10).

1.5 Some General Discussion of the Three Aims

The first aim is thus to define and describe the basic structure of the untried theory - to define both what a truly selectionist theory of cultural process is, and what it is not. An investigation of some of the theories of cultural process which have been used instead of the "untried theory" is undertaken, with the intention being to illustrate that most of these do not really employ the unique selective logic outlined in section 1.2 and 1.3 above, and expanded upon in Chapters 6, 7 and 8. These "candidates" for the untried theory are explored under the three general headings of Cultural Evolutionism (Chapter 2), Cultural Sociobiology (Chapter 3), and Cultural Selectionism (Chapters 4 and 5). A number of questions guide the enquiry: Which, if any, of the known approaches to culture, most resemble what we imagine the "untried theory" to be, and if so, have they been given a fair trial? Could it be that the theory is not only untried, but also largely uninvented? The conclusion reached is that the recent body of theory known as Cultural Selectionism provides at least a starting point for building an appropriate theory, although a series of fundamental inconsistencies in its constituent models are discussed. This series of flaws is referred to as "the cultural virus critique" - as distinct from the Cultural Virus Theory of Chapter 9 - where the virus concept is used as a heuristic device to elucidate the inconsistencies mentioned above.

It becomes clear that many attempts to solve the problem of the "untried theory" under the banner of selectionism fail to build a model of culture which preserves the peculiar logistics (ie. neither determined nor random) which characterise a truly selective position. As a result, the most complicated task involved in defining and building a selectionist theory of cultural process is the careful identification of the many theories which may appear to be selectionist in their approach to culture, but which really are not. Through demonstrating the non(neo)-Darwinian nature of existing "selectionist" theories of culture it becomes clear that the "untried theory" is still largely untried. Between the vast amount of evolutionary or processual

literature written in the Spencerian or similar traditions, the considerable amount of sociobiological and adaptationist evolutionary literature, and the various theories and approaches to sociocultural anthropology and archaeology which profess not to be interested in any form of processual analysis at all, the absence of truly selectionist perspectives in anthropological theory is quite extensive.

Thus the distinctive neo-Darwinian approach to process remains the untried theory of cultural fields of enquiry. Anthropologists still operate in what is essentially a non(neo)-Darwinian milieu. Sociocultural anthropology has not yet properly absorbed the challenges of nineteenth and early twentieth century Darwinism, let alone the more reaching challenge of the modern evolutionary synthesis, where the logistics of models of adaptation have become even more sophisticated. Such are the conclusions reached by the end of Chapter 3. Yet in Chapters 4 and 5 it is shown that one approach to cultural process provides a least a viable starting point for the building of a selectionist perspective for culture itself, despite the series of logical difficulties elucidated by the "cultural virus critique" of Chapter 5.

In sum, the first five chapters identify the partial lack of and need for the untried theory, and show how the growing body of theory known as Cultural Selectionism comes closest to what we would expect the untried theory to look like. Yet while the cultural virus critique presented in Chapter 5 suggests a whole range of new models which might improve Cultural Selectionism, these possible improvements are not undertaken until the final chapters of the study, where attention is turned toward the third basic aim.

In the meantime attention is focused on the second basic aim, and Chapters 6, 7, and 8 are devoted to devising some sort of testing formula for selective process. While the discussion of Chapter 5 makes it clear that Cultural Selectionism is not a perfect example of the "untried theory", a decision is made to largely ignore these problems, and to test the approach as if it were. The spirit of Cultural Selectionism as a whole may be viewed as encompassing the general metaphysic of the "untried theory", and so it is

treated as such. In other words, an attempt is made to devise a *general* test for the basic premises of Cultural Selectionism, and not to specifically test the Cultural Virus Theory itself; and so the development of the virus account itself is left until after the test. This makes it possible to assess the general potential of Cultural Selectionism before investing any effort in its improvement.

Thus the part of the problem now demanding attention is the relatively "untried" nature of the theory. It now remains to "give it a try"; to subject the selectionist theory to some sort of global test. Yet any conclusive demonstration of the global validity of the general Cultural Selectionist metaphysic would presumably be beyond the bounds of a single study. It was therefore decided to concentrate on devising some method or "formula" for recognising the empirical manifestations of selective process, and then to carry out a trial run of this formula with a view to "testing the testing formula itself", as much as to testing Cultural Selectionism.

There are a number of reasons why comparing and contrasting ethnographic interpretations of different cultural and artefact phenomena is difficult, and they are mentioned briefly below and expanded upon in Chapter 8. Basically these problems all relate to the tendency for anthropologists to develop their own cultural traditions in interpreting the cultural traditions of their field of enquiry, such that different cultural assemblages are researched and interpreted in non-equivalent ways.

Usually, tests for any given general theory have taken the form of analyses of particular cultural sequences in the archaeological record (for example, a general discussion of theory while using the European archaeological record (Clarke 1968) or the origin of agriculture (Rindos 1980, 1984). Even in sociology and social anthropology, the common research profile of "general theory and specific example" is similar, and for a number of good reasons. One basic reason is that many studies in cultural fields of enquiry begin as a descriptive investigation of a particular problem, and only in the process of description emerge as a conveniently illustrative example of certain

general theoretical principles. In such studies the testing of general theory was not the principle focus of the research design in the first place.

Another reason operates on the other end of the spectrum; even in theoretically oriented research the opportunity to amass examples from the relevant field of enquiry is hindered by different factors. One factor would be that an archaeologist or anthropologist who specialises in the analysis of theoretical literature and issues must, by definition, devote most of their time and energy into purely theoretical and literary research, leaving relatively little time for descriptive or interpretative analysis. Secondly, the time left over for the exploration of illustrative examples of theoretical principles is limited once again by the tendency to focus on a small number of examples examined in great detail, rather than on a large number of examples in lesser detail.

For whatever reasons, however, general theories are often tested in the context of very limited data, very specific cases, where the generalities of the framework are not directly or obviously manifest. The theory may be verified by the case in point, but the global application of the theory, implied by its general nature, remains in question due to the infinitely more vast universe of cases not considered. On the other hand, the intensity of focus in these single examples is such that the time and labour involved makes it impossible to cover more than a small number of them. Not that this is necessarily always avoidable or even undesirable, but the situation would suggest that sometimes the single in-depth case study is not the most appropriate context of verification for a general theory of cultural, or, for that matter, genetic process. There is room for the use of a more *synthetic* context of "refutation" for general theory.

The dialogue between discovery and "refutation" at the level of general process theory has thus repeatedly switched back and forth (with some exceptions), going from the sublime to the mundane and ridiculous, and back again. A more appropriate context of potential refutation for general theory is, rather, equally general dimensions of "observation" or "description" in the

field in question. A highly synthetic theory is best tested through exposure to patterns which are observed at an equally synthetic or general level of description, and not through a single exposure to a particular instance.

In view of this problem, an effort is made in this study to test the "untried theory" (and concomitantly, to test its basic alternatives) via different means to the customary one. A methodology was employed which enabled very large numbers of globally distributed cultural assemblages to be compared and contrasted multivariately. A brief discussion of this method, Principal Components Analysis, a variety of multivariate analysis used to analyze tables of data in this study, can be found in the first chapter of the second half (Chapter 6).

In sum, the study can be simply envisaged in two five chapter halves, the first concerned with defining a Darwinian model of cultural process, and the second concerned firstly with testing the theory and then, in the final two chapters, redesigning it. Definition of the theory will involve a classification of the various process related concepts into selectionist and non-selectionist categories. Testing the theory will involve a corresponding classification of general patterns of ethnography and biogeography into selective emergent and non-selective emergent categories. Of special focus will be a contrast between the geographic patterns produced by natural history with those patterns produced by history, to see whether any pattern occurs in one system and not in the other. Obviously, for the neo-Darwinian theory of cultural process to be operative (for the untried theory to work), geographic patterning in genetic and cultural systems must display comparable empirical elements of some sort. The degree to which this is so can be seen through a comparison of the cultural data of Chapter 8 with the cranial data of Chapter 6, and with the null hypotheses of Chapter 7.

Once results of the ethnographic analysis of Chapter 8 appear to support the idea that the Cultural Selectionist metaphysic is basically correct, an attempt is made to redesign its constituent models so that the serious logical problems brought out by the cultural virus critique might be avoided.

This substantially redesigned form of Cultural Selectionism is called the Cultural Virus Theory, and it is shown how its constituent models are indirectly but unambiguously supported by independent research in archaeology, anthropology, evolutionary biology, and psychobiology.

1.6 The Relationship of the Study to General Issues of Archaeological Theory

Although many of the issues associated with the application of selectionist principles to human behaviour are intimately related and often parallel to the issues discussed in debates associated with other theories, these links are not brought out very frequently. A few decades ago, selectionist issues were discussed almost exclusively by selectionists, and as a consequence (what was then) "biological" anthropology (like any other academic community) developed its own terminology. As long as the issues being discussed were essentially principles of sociobiology and evolution in the gene pools of human populations, this was no problem at all. However, as neo-Darwinian anthropology has turned its attention more and more to the problems of learned behaviour and the implications of sociocultural transmission, it has become more of a problem than before. Many neo-Darwinian archaeologists are now preoccupied with precisely the same problems as non-Darwinian archaeologists, but the use of different terminologies conceals the fact. Different terminology now acts as an impediment to what might otherwise be a fruitful cross-fertilization of ideas. It is almost as though neo-Darwinian anthropology (the word anthropology being used here in its widest sense) "independently discovered" culture as it moved away from hardline sociobiology, and as a consequence developed its own terminology for this "new" phenomenon (culture) without making any extensive references to the established terminology of cultural anthropology. If this gap is ever to be bridged, the equivalence of certain "neo-Darwinian" and "non-Darwinian" terms must be recognized, and the common goals of the two approaches clearly outlined.

Given this quasi-isolation between the two academic camps, which are now often inadvertently discussing the same issues using different terminology, it is interesting to compare the prolegomena of "neo-Darwinian archaeology" used so far and throughout the study, with the "cognitive map" (Murray 1987) of "not so Darwinian" archaeological theory. The aim of this section of the chapter is to briefly indicate some possible links between the two cognitive maps.

First and foremost it is important to bear in mind which general level of archaeological theory debates about selection are best classified under. Put simply, it is the most general level of all. This general level of theory was discussed some time ago by David Clarke - one of the authors involved in inciting much of the current theoretical upheaval in archaeology - who summarised the aims of the discipline as follows:

> "1. First, the definition of the fundamental entities that pervade the diverse material, their elements, structures and patterns, and the processes that operate on them, and the effects of the processes on the entities in the dimensions of space and time. A study in statics and dynamics going beyond particular instances.
> 2. Second, the search for repeated similarities or regularities in form, function, association, or developmental sequence amongst the particular entities from every area, period, and environment.
> 3. Third, the development of higher category knowledge or principles that synthesise and correlate the material at hand whilst possessing a high predictive value. The development of increasingly comprehensive and informative general models and hypotheses." (Clarke 1968: 20).

In terms of Clarke's archaeological aims, this study corresponds to the third category of archaeological practice; the development of increasingly comprehensive and informative general models and hypotheses for variation and invariation in the archaeological record. Evolutionary theory represents a subset of the general models used in this area, and the first five chapters mentioned above (especially Chapters 2-5) could be seen, in Clarke's language, to be a discussion of those general models and hypotheses which concern the processes of natural and cultural history, and the similarities and differences between them.

The second section of the study, however, Chapters 6, 7, and 8, relates much more to Clarke's first two aims, as there the general models of the third are related to the descriptions and interpretations of cultural and ecological data produced by the first two. Using the branch of multivariate statistical analysis known as Principle Components Analysis, I shall explore "statics and dynamics going beyond particular instances" in two dimensional graphs of the principal components of large data sets. From the results Clarke's second category can then be realised, in the form of repeated similarities and regularities - i.e. as a series of principal components analyses of variation, in its different forms. The results of these analyses are then considered in light of the general evolutionary theories of the earlier section. While the first five chapters are concerned only with the third of Clarke's archaeological objectives, the remainder of the study constitutes a dialogue between all three.

If selectionism belongs to Clarke's third category of archaeological practise, the development of "higher category knowledge" or of "increasingly comprehensive and informative general models", it would presumably also belong to the "high level theory" category of other cognitive maps of the discipline. But this category is labelled in a variety of ways in different cognitive schemes. In 1988, for example, Schiffer offered a somewhat similar overview of the archaeological discipline to that of Clarke, focusing on a classification of the cognitive tools of research, archaeological theory, rather than on the research objectives themselves:

> "Archaeological theory consists of three great realms, each of which is made up of one or more domains. The three realms are social theory, reconstruction theory, and methodological theory. Social theory...consists of principles for explaining behavioural variability and change. Reconstruction theory permits human behaviour and environmental conditions of the past to be ascertained.... Finally, methodological theory encompasses principles which channel the choice of techniques and methods as well as guide employment of reconstruction theory." (Schiffer 1988:464).

Although the label "social theory" has the connotation of only those high level theories which make specific appeals to social forces, it is clear that Schiffer intends the word to denote explanations of cultural change in

general, so distinguishing them from other kinds of archaeological propositions:

> "Social theories function, in the archaeological research process, to explain variability and change in human behaviour. By behaviour is meant (sic) human activity at any scale or level of abstraction, from the handiwork of a particular artisan to the rise and fall of complex societies." (Schiffer 1988:465).

In Schiffer's terms, therefore, this study might be seen as treatise on social theory, concerned with the various theoretical frameworks employed to explain variability and change in human behaviour. However, I would emphasise that by no means could all such frameworks be considered "social". The word "social" is a convenient label for the category but it must incorporate a more disparate range of theoretical perspectives for archaeology in general than it incorporates for the social theorist. Schiffer would characterise this body of theory concerned with cultural change and evolution as "high" level theory, to be distinguished from "low" and "middle" range theory:

> "Though problematic, the concept of middle-range or middle-level theory is used here. The position adopted is that each hierarchy of archaeological principles contains high-, middle-, and low-level theories. In conformity with the spirit of Merton's formulation, principles within each hierarchy can be related logically. A level of theory (high, middle, low), then, denotes a particular degree of abstraction within one hierarchy of related principles or an analogous degree of abstraction that crosscuts different hierarchies. Thus, we may regard evolutionary theory as high level and the theory of pedestrian tactic survey as middle or low level without implying that the former subsumes the latter." (Schiffer 1988:463).

So the research objectives of this study could be summarised as an initial five chapters of examination and analysis of high level theory, with the next three chapters as an attempt to translate these theories into middle and low level propositions, to arrive at some estimate of their efficacy *vis-a-vis* cultural data, and with the final chapters making certain alterations and additions to one high level theory (Cultural Selectionism).

Having located selectionist issues on the very general cognitive maps of Clarke and Schiffer, it now remains to pinpoint its location within this general category. For this purpose a series of more local cognitive maps of the

general theory category itself will be employed. David Rindos (1989:1), for one example, has stressed that continued successful practice in any field of enquiry depends as much upon advances in high level theoretical perspectives as in advances in data collection. A series of such significant steps have occurred in evolutionary theory in the recent decades, the most important legacy of which, according to Rindos, is that of the diachronic approach to the explanation of human culture:

> "In anthropology, an important conceptual shift occurred during the fifties and sixties when ecological thinking began to influence our descriptions of human culture and cultural change. This shift was initiated in large part by Clark's (1952) economic interpretation of prehistory and refined by Steward's (1955) cultural ecology, approaches entailing a materialistic method linking society to the conditions of life to which all human cultures must adapt. During this same period, cultural evolutionism (White 1959; Sahlins and Service 1960) was enjoying a resurgence, and although it had little to do with a truly Darwinian account of evolution (Campbell 1965, 1976; Dunnell 1980; Blute 1979), it was significant in stressing that any comprehensive explanation of human culture must be diachronic as well as synchronic." (Rindos 1989:1).

In this way Rindos provides another and more local conceptual map on which the objectives of this study may be located, with the two halves of this research plan corresponding to the diachronic and synchronic explanations of culture. The initial section concerns the analysis of various diachronic frameworks for cultural process; while the second section concerns the relationship between diachronic cultural perspectives of different sorts and the synchronic spatial patterning they imply. Rindos's own work (1980, 1984, 1985, 1986a, 1986b, 1989), is of course in the same category of archaeological discourse as this study; exploration of what Clarke would term the various "general synthetic models and hypotheses" and what Schiffer would term "social theory". However, as will become clear later, while much discussion of the applicability of evolutionary theory to the archaeological record has been undertaken, discussion seems to concentrate on evolutionary explanation of univariate or bivariate temporal sequences. Yet this study attempts to address the applicability of evolutionary theory to large scale multivariate patterns in temporal or spatial sequences, rather than limiting the use of models of evolutionary processes to the single dimension of time.

Rindos has also offered a coherent typology of evolutionary philosophy, after Campbell (1965), Freeman (1974), Richerson and Boyd (1978), Blute (1979), and Dunnell (1980), and the terminology of this typology is used in many parts of this study. Significant developments in evolutionary perspectives have occurred since the heyday of the ecological and evolutionist schools, resulting in the emergence of a school of thought which he labels "Cultural Selectionism". This school uses neo-Darwinian theory to arrange and analyze topics in cultural fields of enquiry. Unlike Sociobiology, neo-Darwinian models are not evoked in the context of human genetics, but in the context of cultural processes themselves. Similar categories to those of Rindos are used throughout the study where a three way contrast between Cultural Evolutionism, Cultural Sociobiology, and Cultural Selectionism is used to stress the fact that there are three quite different approaches to culture which are frequently referred to as "neo-Darwinism", not just one. However as the place of this study with respect to selectionist debates is well covered both above and in Chapters 3, 4, and 5, for the time being no further effort need be spent on locating its position on the local "cognitive map" of selectionism (to use the convenient term of Murray (1987) yet again). This section of the chapter is, after all, not about the internal "topography" of our maps of selective issues, but about their relationship of their topography to that of other local cognitive maps in the discipline.

So far, then, the issues of the study have been related to the most general levels of archaeological theory, and (as is expanded upon throughout the study) the more local theoretical maps of other studies of selective issues. Yet there are also parallels with literary debates about issues not normally associated with debates about neo-Darwinian principles. For instance, it appears that there are certain parallels between the agenda of post-processual archaeology (Hodder 1982, 1985, 1989) and the new, more culturally orientated Darwinian archaeology, despite unremitting attempts to polarise the two lines of thought (Shanks and Tilley 1987a, 1987b). These parallels are a result of a number of thematic similarities which appear to exist in both post-structuralist and selective metaphysics. Put bluntly, the "strange selective

gistics" outlined above and in Chapter 6 appear to constitute some sort of ready made" solution to post-structuralist prolegomena.

Some of these connections, such as a mutual commitment to active ecision making and measures of "free will" have been very forcefully brought ıt (Mithen 1989:487-491). But there are also "deep structure" similarities etween the two philosophical manifestos. The term "post-structuralism" is sed by Hodder to denote the movement from language to text, one of the ısic themes put forward by a group of French writers which includes oucault, Barthes, Derrida and Ricoeur, and ultimately traceable to Saussure's ıalysis of language (Hodder 1989:68). Hodder focuses on that part of the itique of structuralism which emphasises the distance between an abstract nguage and a particular concrete text written in that language. In ructuralism, the structured sets of differences which constitute a language e separated from the actual use of that language via the activity of speech; ıe dichotomy of langue and parole. In post-structuralism, while it is still cognised that abstract concepts or rules are used in social discourse, the cus is on the fact that an action - or text - is intended to have some ngible effect on other people. Because of this, the context of an act is as portant as what it is supposed to signify; without the appropriate context, ıe desired effect will be unattainable. Part of the context is, of course, the atus and/or social identity of the individual who happens to be performing ıe act. Hodder offers the following example:

> "Consider, for example, a member of the royal family planting a tree or a politician cutting a ribbon in order to open a new motorway. We can all go around cutting ribbons and planting trees. These acts, when carried out in a formal public setting, do not simply have meaning in relation to an abstract set of opposed signifiers. They also have meaning in relation to their stage setting. For example, it is important in the formal occasion that the ribbon cutter or tree planter is of high status, that reporters and the public are watching, that the institutions that have paid for the motorway or nature reserve have asked the member of the royal family or the politician to be present and so on. In other words the "writing" of "texts" (such as cutting a ribbon or planting a tree) depends on institutional contexts." (Hodder 1989:69).

The writing of a text "strategically links structure and context". There is, moreover, no right or wrong way to read a text, and due to the varying

subjectivities associated with each individual reader, texts are ambiguous and polysemous. Hodder adds that each signifier merely signifies other signifiers, such that a cultural model of the universe ultimately consists of an ultimately circular series of signifiers of signifiers. An obvious metaphor for this essentially structuralist sentiment would be a dictionary, where each word or signifier may be looked up, only to have it explained in terms of other signifiers, arranged in a certain grammatical order and so constituting a sentence which provides some clue to the meaning of the original signifier. Each of the signifiers in the new sentence may then be found elsewhere in the dictionary, associated with sentences of further signifiers, and so on. But, as we move in the direction of post-structuralism, there is often more than one meaning for each word, and in such cases it is the context "which allows us to fix meanings" (Hodder 1989:69); in other words, it is the context which allows the observer to identify which meaning - of a range of alternative meanings - is the relevant one for interpretation.

Having laid out his interpretation of a post-structuralist perspective, Hodder goes on to relate it to post-processual archaeology:

> "The move from language to text in post-structuralism sidesteps the old dichotomy between, on the one hand, normative, culture-historical, idealist archaeology and, on the other hand, processual, culture-ecological and materialist archaeology, and leads to a post-processual phase in which these dichotomies are broken down. Indeed, it is precisely the relationships between the two sides of these dichotomies that become the aim of the study.
>
> The opposition between adaptational accounts and structuralist accounts which has lead to a recent rearguard reaction on the part of die-hard processual archaeologists (for example, Binford 1982), and which has been followed by the placing of this opposition at the centre of study within post-processual archaeology, can be seen as a trend within this book." (Hodder 1989:70).

Some parallel can be drawn between the two historical summaries of Rindos and Hodder. Rindos contrasts materialistic cultural ecology with the mentalistic, orthogenetic, and progressivist cultural evolutionists; Hodder contrasts processual, ecological materialism with normative culture history. There are also parallels between the solutions offered by Hodder and Rindos; cultural selectionism, as will be shown in greater detail in Chapter 4, is similar to post-structuralism in that it also combines elements from both sides of the

materialist versus idealist dichotomy. Rindos (1985) has offered a typology of selective forces acting within culture [which he terms Cultural Selection 1 (CS1) and Cultural Selection 2 (CS2)], one which recognises the differences between cultural and non-cultural selective constraints, but at the same time this typology provides the basis for an explanatory framework which explicitly relates the two kinds of constraints at another level. Thus, broadly speaking, cultural selectionist perspectives could qualify as post-structuralist conceptions of cultural process in that they are neither exclusively idealist or materialist, ecological or cultural, although they are explicitly processual and in fact more faithfully so than "processualism" itself, in that they focus more on general mechanisms of change than on synchronic notions of adaptation. This fact leads us to a paradox implicit in current terminology, where Post-processual archaeology is in fact more processual than its ever been, particularly in approaches like Cultural Selectionism.

Just as the dichotomies of Rindos and Hodder roughly correspond to each other, they also jointly correspond to the dichotomy implied by the differences between the two paradigms analyzed in Chapters 2 and 3, respectively - Cultural Evolutionism, and Cultural Sociobiology or Ecological Adaptationism. Cultural Selectionism is analyzed in Chapters 4 and 5, and in the process it will become clear to the reader why the latter perspective is far more properly "processualist" than Cultural Adaptationism, and why the approach does not correspond to either of the two more traditional evolutionary frameworks. It is worth reminding the reader, however, even at this early stage, that many examples of Cultural Selectionism do not fully escape all of the premises of the old paradigms, and this persistence of the "organism's eye view" of artefacts and ideas is brought out in the cultural virus critique of Chapter 5.

Building on the points made above, it may be possible to persuade the reader that in spite of the apparently disparate origins of the post-processual and cultural selectionist manifestos, they are in fact making many parallel points. Both approaches are reacting to the same limitations of the status quo, and proposing parallel solutions to that status quo. Both approaches dismiss

historical idealism and ecological materialism, believing that neither paradigm adequately encompasses historical process. Both approaches ultimately seek a synthesis of the symbolic and material, of structure and function; yet they are not seeking some combination of the two, but something qualitatively different from either. Each approach is seeking a view of history which ultimately transcends the symbol/reality distinction itself. Moreover, Hodder and Rindos are not the only archaeologists advocating this course. Fletcher (1977, 1981a, 1981b), Mithen (1989), and Shennan (1989, 1991) have also advocated a future direction for archaeology which avoids the sterile distinction between the "social" and the "material". Throughout this study Cultural Selectionism in general, and the Cultural Virus Theory in particular are explicitly advocated as a potential solution to the avoidance of such unproductive dichotomies.

From the above discussion it can be seen that selectionist issues do have their proper place in archaeological theory. Obviously there are many other archaeological debates and many other cognitive maps of theoretical issues, in addition to those considered so far. But the aim of this section has not been to pass any conclusive comments on the nature of archaeological theory in general; merely to demonstrate that many of the debates we find in selectionist literature bear a genuine relevance to the broader issues of archaeological theory.

1.7 Concluding Remarks

It can be seen that the problem of the "untried theory" relates to a large number of familiar debates and frustrating conundrums, and to a variety of general issues throughout Archaeology, Anthropology, History, Psychology, and, for that matter, throughout the humanities in general. A general overview of these widespread issues has been offered, and it is hoped that this overview will provide the reader with a basic repertoire of terms and phrases which will render some of the more esoteric parts of the study less opaque.

Before moving on to the next chapter, however, I would like to make a few brief remarks about the theoretical pluralism of the research design, and the role that "other" theories will play in addressing the problems of the untried theory. Throughout this study non-Darwinian evolutionary metaphysics (ie. Lamarckian evolutionary philosophies such as Cultural Evolutionism) are discussed and contrasted with neo-Darwinian principles. Moreover three different neo-Darwinian positions (Cultural Sociobiology, Cultural Selectionism, and Cultural Virus Theory) are also considered in the course of the study. It was decided that this strategy would be more balanced and more informative than one which concentrated on one theory only. However similar these four western European philosophies may seem to be - in terms of some hypothetical, and more universal cognitive map of process theory - the different theories are used in the spirit of Paul Feyerabend's advocacy of the employment of incompatible theoretical alternatives (Feyerabend 1975). Each of the above four theories has the potential to be characterised in terms of a distinctive predictive hypothesis about geographic patterning in a cultural system conforming to that theory (see Chapters 6 and 7). In other words the theories are not only mutually incompatible, but potentially refutable. Contrast can be a very effective heuristic device:

> "It emerges that the evidence that might refute a theory can often be unearthed with the help of an incompatible alternative...Also some of the most important properties of a theory are found by contrast and not by analysis. A scientist who wishes to maximise the empirical content of the views he holds and who wants to understand them as clearly as he possibly can must therefore introduce other views: that is he must adopt a pluralistic methodology." (Feyerabend 1975:29).

> "No theory ever agrees with all the facts in its domain, yet it is not always the theory that is to blame. Facts are constituted by older ideologies, and a clash between facts and theories may be proof of progress. It is also a first step in our attempt to find the principles implicit in familiar observational notions." (Feyerabend 1975:55).

However, the reader is reminded that the other theoretical alternatives to a selectionist theory of cultural process are not the focus of this study, although they are given considerable attention. Cultural Evolutionism and Cultural Sociobiology are therefore described and analyzed in the first half of the study only in so far as this process helps in the definition of Cultural

Selectionism, and used in the second half only in so far as they provide heuristic alternatives for the purpose of testing this position. The primary focus of the study remains the development and testing of the "untried theory" alone, not of its comparatively well tried and well tested alternatives.

It is also important to point out that the notion of cultural change being fundamentally constrained by cultural heritage, which is used as an integral part of the general test of Cultural Selectionism in Chapter 8, is not in any sense a new idea. It is explicitly discussed in studies such as that of Durham (1990) and Cavalli-Sforza (1991), and it is, moreover, implicit in a whole range of common sense notions of archaeological practise, such as the diffusionist assumption that similarity suggests common origin. The aim of Chapters 6 - 8 is rather to comprehensively link practical assumptions of cultural heritage constraint to neo-Darwinian notions of process, so that the former may be seen as widespread evidence for the latter. Given wide ranging use of the notion that similarity suggests common origin in archaeology, Chapters 6 -8 aim to point out that the practical ethos of Archaeology and Anthropology is entirely at odds with the covert "Lamarckian Orthodoxy". We need a general theory of evolution which is more suited to the practical consciousness of archaeological analysis.

Now that the general premises of the study have been introduced, the first of the three aims can now be addressed. This is the aim of demonstrating the existence of the problem itself. Over the next five chapters it will be shown that a properly selectionist model of cultural process is not only largely untried, but also at least partly uninvented. How can it be that selectionism is largely untried in culture theory, when approaches such as "Cultural Evolutionism" or "Social Darwinism" are so well established and so well tried? The answer to this question, as already briefly outlined above, is that such approaches are simply not selective, either in origin or in nature, and evidence supporting this conclusion will be presented in the next chapter.

2

CULTURAL EVOLUTIONISM

Overview

There has been a substantial amount of literature written on the Cultural Evolutionists, and so another analysis of their original works, however comprehensive, would be practically redundant. In light of this situation a decision was made to focus not upon the original works, but upon the Cultural Selectionist critique of the approach. Therefore the aim of this chapter is to provide a general outline of the modern discourse about Cultural Evolutionism, and to show that the various arguments of these debates ultimately lead to the conclusion that the approach is not only non-Darwinian, but also a coherent, fully Lamarckian orthodoxy.

2.1 A Case of Mistaken Identity

The story of Cultural Evolutionism (alias Social Darwinism) - of how a completely non-Darwinian evolutionary theory managed to establish a reputation for being precisely what it was not - must surely be one of the great "myths of misconception" in the history of evolutionary theory. However, a single chapter could never do justice to this tale, and no attempt will be made to do so. For the moment, it is enough to say that despite the existence of some very high quality "investigative journalism" which demonstrated this case of mistaken identity beyond reasonable doubt, ignorance of the philosophical identity of Cultural Evolutionism persists.

Indeed, many archaeologists and anthropologists are still under the impression that the "Social Darwinist" and "Cultural Evolutionist" schools of the late nineteenth and twentieth centuries (these include Durkheim 1893, 1895;

Morgan 1877; Sahlins and Service 1960; Spencer 1873; Tylor 1871; White 1949, 1959a, 1959b; and arguably even Marx 1867, Engels 1884 and Steward 1955) are either selectionist or, at least, "selectionesque". But this is simply not the case. Even "Social Darwinism" is now generally accepted as non-Darwinian by those archaeologists and anthropologists who have researched the approach. However, here an attempt will be made to add to this and to show that these schools of thought are much more than just *different to* selectionist thought. They are actually the *logical antitheses of* a selectionist theory of cultural process, in the same way that Lamarckism is the logical antithesis of neo-Darwinism in Biology. Therefore defining a selectionist theory of cultural process must first involve a complete understanding of (and careful avoidance of) the Social Darwinist and Cultural Evolutionist notions of process.

The non-equivalence of the Spencerian and neo-Darwinian evolutionary notions of process has been well articulated by a number of theorists with a range of disciplinary backgrounds, including psychology, archaeology, biology, and social anthropology. Prominent works in this area include Blute (1979), Dunnell (1980), Dunnell and Wenke (1980), Freeman (1974), Leonard and Jones (1987), Peters (1980), Richerson and Boyd (1978), and Rindos (1985), *inter alia*, and they are often referred to throughout this chapter and those which follow. These authors write about the fundamentally non-Darwinian nature of prevailing theories of cultural change, and about the absence of any cultural equivalent of the neo-Darwinian evolutionary synthesis of genetics and Natural Selection.

Since Cultural Evolutionism and neo-Darwinism are so different (although the earliest texts out of which the two traditions evolved are less different - see section 2.2), the many trials and uses to which the former has been submitted in cultural fields of enquiry have no implications for the latter; the failures of Cultural Evolutionism have no bearing on the validity of building a neo-Darwinian model to stand in its place. Spencerism has been tried and found wanting, but neo-Darwinism has remained essentially untried during this process.

At this point it must be made known that the words "Darwinism" and "Darwinian" are not used literally in modern discussions of processual thought, and for this reason I usually use the term "neo-Darwinian" in preference to the more common "Darwinian", in order to differentiate modern Selectionism from Darwin's writings. Modern use of the word "neo-Darwinian" does *not* mean "Darwin's theory" (which as a matter of fact strays in and out of what today would be considered Lamarckian and Darwinian world views), but means rather that at least part of the theory in question bears some structural similarity to Darwin's principle of Natural Selection, with the added philosophical element of some appeal to *random variation*. Consistent definition and identification of a "neo-Darwinian" theory is thus best done on the basis of the presence of the general principle of selection, which can be defined as *the differential reproduction of intrinsically and blindly generated variation* (see Chapter 6 for full description). Theories referred to as neo-Darwinian in this study are simply any theoretical structures in which I have been able to identify the structural component of selection.

Yet, it is important to note that many evolutionary theorists consider the "neo" addition to the word "Darwinian" to be unnecessary, and therefore use the word "Darwinian" whenever they are referring to modern Selectionism. Assertions about the absence of a "Darwinian" theory of cultural process - assertions about the "untried theory" - are therefore referring to the fact that there is no "selectionist" or "neo-Darwinian" theory of cultural process. The assertions are *not* saying that we have not paid enough attention to *what Darwin said about culture*. The Cultural Selectionism account of culture presented in Chapters 4 and 5, and the Cultural Virus Theory of Chapter 9, are very different indeed to Darwin's approach to human society, which is more like the Cultural Sociobiology of Chapter 3 than either of the above perspectives. So the various assertions about the absence of a Darwinian approach to the purely cultural processes of human society - assertions about the untried theory - should be interpreted as a lack of appeal to selective logistics in explanations of cultural change, rather than as a lack of "Darwin-like" approaches to culture. More often than not, modern usuage of the word

"Darwinism" implies a meaning of "neo-Darwinian synthesis", rather than, for example, a literal interpretation of *The Origin of Species.*

The assertions usually take the form of an illustration of how none of the concepts - either those implicit or those explicit in the evolutionary theory of Cultural Anthropology - really employ a neo-Darwinian conceptual framework. Cultural Evolutionism, the general evolutionary framework most well known in disciplines associated with cultural fields of enquiry, is shown to have a different origin and structure to the explanatory paradigm of modern evolutionary biology, in spite of the fact that the two approaches use similar terminology. Blute (1979), for example, writes:

> "We have to be critical of sociobiology's relative neglect of the epigenetic sciences of the individual and, indeed, to reject firmly its implicit contention that there is no order to be found in history, the subject matter of the social sciences, except that imposed upon it by evolving gene pools (Blute 1976). Under the pressure of the challenge, however, a substantial number of social scientists have become acquainted for the first time with the modern neo-Darwinian synthetic theory of evolution and with population genetics and, in doing so, have almost certainly come to realize that sociocultural evolutionism has never been an evolutionary theory in the way in which that theory has been employed in the life sciences." (Blute 1979:46).

Freeman (1974) has also expressed this view in responding to assertions made by Marvin Harris in *The Rise of Anthropological Theory* (1968). If one accepts that neo-Darwinian theory has not been given a proper trial in the context of cultural process, then logically claims to the contrary must depend upon the mistaken allocation of a neo-Darwinian label to a non-Darwinian discourse. Harris claimed that Spencer and Darwin constituted an evolutionary synthesis, thereby implying that the two approaches share the same conceptual structure, and by inference, the same conceptual flaws. Freeman drew out the pronounced differences between the approaches and thereby refuted the claims of Harris. Like Blute, Freeman concentrates on a suite of pre-Darwinian evolutionary principles present in Spencerian thought:

> "The lumping together of the evolutionary theories of Spencer and Darwin is, in the light of the evidence, unwarranted, for the theories of Darwin and Spencer were unrelated in their origins, markedly disparate in their logical structures, and differed decisively in the degree to which they depended on the supposed

> mechanism of Lamarckian inheritance and recognized "progress" as "inevitable". (Freeman 1974:213).

Given the differential influence of each approach in cultural and biological fields of enquiry, we can conclude from Freeman's position that Darwin has not been as influential in cultural fields of enquiry as some anthropologists may believe. In the concluding section of this chapter I will attempt to push the "non-synthesis" argument concerning Darwin and Spencer even further by showing that Spencer and Lamarck, on the other hand, may be considered a synthesis. Spencerian and related theoretical influences in cultural fields of enquiry have covertly established a Lamarckian orthodoxy in anthropology which can be contrasted with the neo-Darwinian orthodoxy of the biological sciences. For the time being however, attention will be returned to theoretical analyses which have emphasised the pre-Darwinian structure of Spencerism without necessarily emphasizing any possible synthesis with Lamarck. Given the profound similarity between Spencer and Lamarck, demonstrated in the last section of this chapter, it is curious that Harris (if he had to propose a synthesis at all) did not propose *this* synthesis instead.

It is worth noting that anthropologists cannot be held completely responsible for their mistaken belief that all evolutionary theory (including the one they are most familiar with) is neo-Darwinian. Biology itself has contributed to the tendency of non-biologists to attribute the very idea of evolution to Darwin. It is not unknown for biologists to refer to the modern neo-Darwinian synthesis as "The Theory of Evolution", implying quite inaccurately that there is only one. It is not difficult to imagine that such usage sustains "common sense" notions which equate Darwin with evolutionary theory in general, sustaining the mistaken belief that there is only one evolutionary philosophy, and that this philosophy is neo-Darwinian.

Having drawn similar conclusions about the structure of sociocultural evolutionism to Freeman, Blute goes on to say that the use of Spencerian evolutionism as a framework within anthropology does not negate the value of all empirical research done to date in the social sciences. A general theory of cultural process based on selectionist principles may well turn out to be

highly compatible with the data amassed by researchers in cultural fields of enquiry. In other words, Blute is not of the opinion that the descriptive information that has been accumulated in anthropology and archaeology is seriously biased toward the Spencerian evolutionist framework. Sociocultural evolutionism is non-Darwinian in theory rather than practise (for Blute), and while there is, therefore, no general theory of cultural evolution that is structurally equivalent to the neo-Darwinian synthesis, this does not necessarily mean that anthropological descriptions compiled under the auspices of the Spencerian paradigm are invalid.

These remarks of Blute's foreshadow an argument which is indirectly employed in Chapter 8, where general patterns of cultural diversity are drawn out and submitted as definitive signatures of selective process. Blute implies that it is quite possible that the observations and interpretations amassed in the last century under the auspices of what I would call the "Lamarckian Orthodoxy" may well exhibit patterns which conflict with that orthodoxy. In Chapter 8 an attempt is made to show that a definite pattern of historical continuity (alias heritage constraint, or origin and influence dependent similarity), is indeed ubiquitous in cultural data regardless of the philosophical paradigm under which it was compiled.

Having contrasted the structure of evolutionary theory in the two fields of enquiry, Blute goes on to contrast their respective origins, firmly labelling the origin of sociocultural evolutionism as "pre-Darwinian", via White, Spencer, and Tylor, with the following words:

> "Spencer was a pre-Darwinian evolutionist who borrowed his concept of differentiation from Von Baer, the great German embryologist, and applied it to organic phylogeny as well as to the sociocultural world...He believed that phylogenetic change occurred via the same process as did embryological development. Coherence, cooperation, integration, etc., are its outcome according to this view." (Blute 1979:49).

Using this historical and structural distinction, we have on the one hand a pre-Darwinian evolutionary paradigm, based principally on "generative" or "deterministic" notions of causality as will be shown later, which dates back through Spencer and Tylor to Lamarck and Von Baer; and on the other

hand a neo-Darwinian paradigm, based principally on notions of selective causality, which dates back through population genetics to Darwin.

It is important to recognise that while most current analyses of evolutionary theory characterise modern biology as neo-Darwinian, and Cultural Evolutionism as Spencerian, even modern biologists who do not classify themselves as "neo-Darwinian" are nonetheless "selectionist" as far as this general distinction is concerned. Modern approaches which criticise "hardened" or deterministic adaptationism, including Depew and Weber (1988), Gould (1983), Ho and Saunders (1982), Kimura (1985), Monod (1974), Platnick (1979), and Wright (1967), are in no sense advocating a departure from "selective process" in general, but against the excessively strict application of the principle of Natural Selection in particular. Such critiques argue for the inclusion of greater indeterminism within selective process, not for its expulsion from evolutionary biology altogether.

So, if anything, the more recent ideas of modern evolutionary biology are evolving *further away from Lamarckism again* (i.e. they are not crossing back to the determinism of the pre-Darwinian world view). As a consequence, their presence in recent western thought does not constitute any threat to the integrity of generic terms such as neo-Darwinism or Selectionism, terms by which even the most recent innovations within the modern synthesis are adequately encompassed. In light of this the general word "Selectionism" is used throughout the study to indicate any framework which makes at least some reference to a properly interpreted notion of differential reproduction of blindly generated variation, regardless of whether the selective agencies concerned are viewed as random, or more systematic.

Returning to the problem of the pre-Darwinian nature of Spencerian philosophy, it can now be pointed out that the problem of the "untried theory of cultural process" is linked to the profound influence of pre-Darwinian world views in cultural fields of enquiry. In any case, the pervasive absence of neo-Darwinian approaches to cultural process is largely synonymous with the pervasive presence of Spencerian philosophy, since the two approaches

are mutually exclusive. As already suggested in passing, a number of other theorists besides Blute have been aware of this gap in anthropological thinking. However, the gap is almost always described as either "the absence of the cultural equivalent of the neo-Darwinian synthesis", the absence of "the scientific theory of evolution", or as the absence of "Darwinism", and not described as the absence of a "selectionist" perspective in particular, although these phrases would all indicate similar opinions. Below, a number of different statements, each expressing a thematically similar position, are brought together.

As some of these assertions which support Blute are presented below, it is worth reminding the reader that since these statements were made, various attempts have been made to remedy the situation. The movement is collectively referred to by Rindos (1985) as Cultural Selectionism, a set of ideas which date back to Campbell (1965), and perhaps earlier, with some notable recent examples including Richerson and Boyd (1978), Cavalli-Sforza and Feldman (1981), Boyd and Richerson (1985), Lumsden and Wilson (1981), Leonard and Jones (1987), and Rindos (1984, 1985, 1986b, and 1989). Others include Dunnell (1978, 1980), Dawkins (1976, 1982), Clarke (1968), Clegg (1978), and Fletcher (1977, 1988) (see Chapter 4 for a more complete list). Such attempts to begin building a cultural equivalent of the neo-Darwinian evolutionary synthesis are dealt with in Chapter 4, while sociobiological attempts to fill the gap are examined in Chapter 3. However, the Cultural Selectionists are of interest here not because of their own ideas, but for their opinions on the nature of Cultural Evolutionism.

Thus the following are a series of assertions from different Cultural Selectionists, each of which supports the conclusions of Blute quoted above. These statements concern the absence of a properly selectionist perspective for cultural process, while some of them also support Blute's dismissal of sociobiology as a viable candidate for such a theory.

Freeman (1974), for example, as implied already above, contrasts both the structure and origin of the evolutionary theories of sociocultural

anthropology and biology. The paper focuses on an in-depth contrast of Spencer and Darwin. As Rindos (1985:66) later reiterates, both were influenced by Malthus, but in very different ways, with Spencer's evolutionary framework remaining fundamentally Lamarckian (and therefore pre-Darwinian), while Darwin moved decisively in the direction of the modern evolutionary framework which now bears his name. Freeman quotes one of Darwin's great anti-Lamarckian disclaimers, also quoted recently by Desmond and Moore (1992:315):

> "...Darwin's theory made a decisive break with certain of the evolutionary concepts of Lamarck - a point which for this present paper is of prime importance. Of this break Darwin was well aware. In January, 1844, for example, when working on the second "sketch" of his "species theory," he wrote (F. Darwin 1888a:23) to J.D.Hooker: "Heaven forfend me from Lamarck (sic) nonsense of a "tendency to progression", "adaptations from the slow willing of animals",&c.! "" (Freeman 1974:213).

Similarly Dunnell (1980), discussing evolutionary theory in archaeology, writes:

> "if evolution is taken to mean as it does in the sciences,it has yet to be systematically applied in either sociocultural anthropology or archaeology. The approach represented by cultural evolution is a social philosophy directly derived from the tradition of Herbert Spencer and the early anthropologists and is unrelated to Darwinian principles." (Dunnell 1980:37).

Richerson and Boyd (1978), discussing evolutionary theory in sociocultural anthropology, make similar assertions to Blute, Dunnell, and Freeman:

> "although there is an enormous literature in anthropology and sociology which examines cultural evolution, very little of it is properly within the Darwinian tradition. Oddly enough this comment applies to Social Darwinism and related ideas as well as explanations conceived in opposition to the Social Darwinist." (Richerson and Boyd 1978:130).

Rindos (1985), with reference to Dunnell (1980), Blute (1979), and Richerson and Boyd (1978), writes:

> "Use of the word "evolution" in the context of social and cultural change is no guarantee that we are speaking of anything resembling Darwinism. Hence, we are beginning to realise that Social Darwinism was not Darwinian, Cultural Evolutionism has yet to speak of evolution, and Sociobiology confounds society with biology." (Rindos 1985:66).

For a final example, consider these words from Leonard and Jones (1987):

> "Our criticisms of Cultural Evolution are few, but we feel that they are of consequence. Attributed to Spencer, and built upon primarily by Sahlins, Service, and White (e.g., Sahlins 1960; Sahlins and Service 1960; Service 1962, 1971; and White 1949), among others, the model of Cultural Evolution is demonstrably inappropriate as a scientific archaeological paradigm for several reasons....A model based upon the general selectionist tenets of scientific evolution is more appropriate." (Leonard and Jones 1987:201).

Many other anthropologists and archaeologists have reached similar conclusions about the fundamentally non-Darwinian nature of Cultural Evolutionism, including Betzig (1986, 1988), Daly (1982), Dunbar (1988), Durham (1978, 1982), Cavalli-Sforza and Feldman (1981), Lumsden and Wilson (1981), Pulliam and Dunford (1980), and Rosenberg (1990). Some of the above have also stressed at different times that Sociobiology is not the framework required to fill this gap. As briefly indicated above, in Chapter 3 an attempt shall be made to demonstrate why this is so, and show that sociobiology and adaptationism fit quite neatly within the "Lamarckian" evolutionary framework (to be described more fully in the final sections of this chapter, Sections 2.3 and 2.4, and in Chapter 7), and are therefore not the decisive break they were proclaimed to be. For the time being, however, the reader need only note the opinions of the above authors concerning Cultural Evolutionism.

The function of this section has been to show that it is well established - amongst those who have made special studies into this body of literature - that Cultural Evolutionism is not selectionist either in origin or in nature. It can therefore be dismissed as a candidate for the "untried theory". Before moving on, however, it is interesting to examine the minority who oppose this generally held belief, and to consider the merits of their position rather than dismissing it out of hand. In the next section it is shown how a small number of scholars of evolutionary theory have remained adamant that Spencerism and neo-Darwinism *are* related, and an attempt is made to come to some understanding of their position.

2.2 Why Spencerian Philosophy has been Mistaken for Darwinism

Not all anthropologists agree that Cultural Evolutionism is a Lamarckian or even, for that matter, a non-Darwinian evolutionary framework. Harris (1968), as Freeman (1974), and Dunnell (1980:41), have pointed out, has asserted that the evolutionary theories of Spencer and Darwin constitute, to some degree, an "evolutionary synthesis". In reply to Freeman's rejection of this position, Harris firmly insisted that this was indeed the case:

> "We know from the autobiographies, notes, and letters of Darwin, Spencer, and Wallace that they independently achieved basically similar interpretations of the natural processes responsible for bio-evolution under the direct stimulation of Malthus's Essay on Population. That essay in turn can only be understood in the context of the 18th and 19th century sociopolitical debate concerning the perfectibility of human nature and the progress of human institutions...That the struggle for existence is not one jot less integral to Darwin's evolutionism than it is to Spencer's or Wallace's must be admitted by all who are familiar with the full title of *On the Origin of Species and the Preservation of Favoured Races* and its conclusions." (Harris 1974:225).

However, this apparent discrepancy between the views of Freeman and Harris is not as fundamental as it might first appear. Each is in fact using a subtly different definition of "Darwinism" for the contrast with Spencerism. This strange state of affairs can be summed up by the apparently paradoxical axiom "Darwin's ideas were not all neo-Darwinian" - Darwin was not a typical neo-Darwinian in the modern sense of the word. It is also linked to the fact that Darwin's account of the evolution of humans involved no explicit separation of cultural and genetic evolutionary processes. The two were inextricable aspects of the general fabric of human variation. By this I mean that it is very difficult to maintain the modern "genes versus culture" distinction in the many accounts of human diversity he provides in *The Descent of Man and Selection in Relation to Sex*. What would be called genetic or cultural differences in modern anthropology jointly constituted a general biocultural fabric of human diversity for Darwin, as it apparently did for most, if not all his contemporaries. Furthermore, his explanation for that biocultural diversity involves not only the ideas for which he became famous, but also ideas which today would be classified as "Lamarckian" or "Spencerian". Both Lamarckian Progressionism and Lamarckian inheritance of

characteristics acquired through changes of habit (two basic principles of the Lamarckian world view described in the next section of this chapter) appear to occur in Darwin's writings, although absolute progression is often vehemently denounced also. Consider, for example, his discussion of the effects of the use and disuse of human organs:

> "It is well known that use strengthens the muscles of the individual, and complete disuse, or the destruction of the proper nerve, weakens them. When the eye is destroyed the optic nerve often becomes atrophied,...bones increase not only in thickness, but in length, from carrying a greater weight. Different occupations habitually followed lead to changed proportions in various parts of the body. Thus it was clearly ascertained by the United States Commission that the legs of sailors employed in the late war were longer by 0.217 of an inch than those of soldiers, though sailors were on average shorter men; whilst their arms were shorter by 1.09 of an inch, and therefore out of proportion shorter in relation to their lesser height....Whether the several foregoing modifications would become hereditary, if the same habits of life were followed during many generations, it is not known, but is probable." (Darwin 1871:116-117).

While Darwin does not exactly commit himself to an "acquired characteristics" position in this paragraph or the many others similar to it, the publication of such statements in modern evolutionary biology would be both controversial and rare. Progressionism, or orthogenesis, the Lamarckian principle involving an intrinsic life force which propels organisms to become more and more complex as time passes, also seems to be implicit in Darwin's writings from time to time. Although, as we shall see, Lamarck's commitment to this principle was far greater; and although Darwin both dismissed progressionism ("heaven forfend me from Lamarck nonsense of a "tendency to progression"" Darwin, C. in Darwin, F. 1888:23, see Freeman 1974 above), and ultimately sowed the seeds of its demise with his principle of Natural Selection, consider the following words:

> "It will suffice for our purpose to refer to the arrested brain-development of microcephalous idiots, as described in Vogt's great memoir. Their skulls are smaller, and the convolutions of the brain are less complex than in normal men. The frontal sinus, or the projection over the eyebrows, is largely developed, and the jaws are prognathous to an "effrayant" degree; so that these idiots somewhat resemble the lower types of mankind. Their intelligence and most of their mental faculties are extremely feeble." (Darwin 1871:121).

Darwin also at times refers to a "series" of animals, arranged as a "chain of being" from simple to complex, from "lower" to "higher", as though our contemporaries actually represent archetypal ancestors, rather than simply being products of a different historical trajectory, as they are seen in modern evolutionary biology. Such concepts would be considered anathema in modern "neo-Darwinian" discourse, and, ironically, other words from Darwin might even be used to silence anyone who said so. Yet these progressionist concepts are reflected in his persistent use of words such as "high" and "low" and "series" in a way which cannot be simply dismissed as metaphorical. He writes:

> "Various other anomalies in man,...have been advanced by different authors as cases of reversion; but these seem not a little doubtful, for we have to descend extremely low in the mammalian series before we find such structures normally present." (Darwin 1871:125).

That Darwin included (in a very "Social Darwinist" and thus "non-Darwinian" way) so-called "primitive" people or "savages" in this "chain of being", this series of contemporary ancestors is suggested by the following assertion, made several pages later:

> "The belief that there exists in man some close relation between the size of the brain and the development of the intellectual faculties is supported by the comparison of the skulls of savage and civilised races, of ancient and modern people, and by the analogy of the whole vertebrate series." (Darwin 1871:145).

Consider also Darwin's introductory and concluding remarks of the "Manner of Development" chapter:

> "We have now seen that man is variable in body and mind; and that the variations are induced, either directly or indirectly, by the same general causes, and obey the same general laws, as with lower animals." (Darwin 1871:135).

Darwin later concludes:

> "As all animals tend to multiply beyond their means of subsistence, so it must have been with the progenitors of man; and this will inevitably have led to a struggle for existence and to natural selection. This latter process will have been greatly aided by the inherited effects of the increased use of parts; these two processes incessantly reacting on each other." (Darwin 1871:155).

Obviously, none of the above passages taken from Darwin involve an explicit commitment to Lamarck or a Lamarckian world view, but some readers

may be surprised by the similarity of certain phrases of Darwin's and the Spencerian and Lamarckian world view examined in the previous and next sections (the empirical implications of which are then examined in Chapter 7). Others, such as Desmond and Moore (1992:230,242) have documented Darwin's exploration of Lamarckian notions very thoroughly. Certainly, the above quoted statements would seem to have a more Lamarckian flavour than equivalent statements in modern neo-Darwinian thought.

It is not surprising that Darwin's writings seem quite different to those of modern Selectionism as he was entirely without recourse to half of the modern synthesis; he had no knowledge of the basic replicators of non-cultural evolution - genes - and no knowledge of the mechanism which produces variation in those replicators - mutation. The net effect of a "geneless" nineteenth century biology was a blurring of the lines between "Lamarckism" and "Darwinism" (although the two approaches are still quite separable right from the beginning with painstaking scholarly research such as that displayed in Freeman's paper). One of the by-products of this blurring effect is a great deal of semantic confusion in discussions of nineteenth century evolutionary biology. Different conclusions about the fundamental nature of the theories of individual biologists working during this period can emerge as a result of purely semantic differences in the conceptual apparatus of modern analysts.

The differences between Freeman and Harris constitute a case in point. While Harris is comparing Darwin's evolutionism with Spencer's evolutionism, and finding a synthesis, Freeman is contrasting neo-Darwinism with Spencerism, or to be more precise, contrasting the neo-Darwinism in Darwin's evolutionism with the Spencerism in Spencer's evolutionism, and finding an antithesis. The evolutionary writings of Darwin and Spencer, which Harris compares, are far more alike than the neo-Darwinian and Spencerian evolutionary frameworks, and therefore Harris's assertion does not refute the fundamental differences between these two perspectives. Darwin's evolutionism, perhaps due to the historical influences of his social context which Harris mentions, contained far more pre-Darwinian philosophy than the modern

evolutionary synthesis which traces its origin and identity to his work. Thus Harris needs only to articulate the common pre-Darwinian elements of Darwin and Spencer to support his assertion of similarity. Freeman, and indeed most commentators, contrast the "neo-Darwinian element of Darwin", namely Natural Selection, with the pre-Darwinian elements of Spencer, to emphasise their dissimilarity. Thus, while there may sometimes be a case to be made for common elements between Darwin and Spencer, the case for corresponding commonalities between Spencer and neo-Darwinian evolutionary biology would be much weaker.

While Harris is probably aware of the above distinction between Darwin's evolutionism (i.e. literally Darwin-ism) and modern neo-Darwinian evolutionary theory, and limits his assertion of equivalence to the actual writings of Darwin and Spencer, others are not so precise.

Yoffee (1979, 1980, and 1985; and see also Ross 1987:37-38 for an identical misrepresentation of neo-Darwinian theory), for example, has made a number of statements which imply a belief in the equivalence of Cultural Evolutionism and modern selectionist thought, statements which Peters (1980), and Dunnell and Wenke (1980) have drawn attention to before. As Peters points out, Yoffee equates concepts of adaptation in the neo-Darwinian synthesis with a definition of adaptation from Rappaport, a definition which sees all adaptation as precipitated by environmental change. Ironically, this does not conform to the neo-Darwinian evolutionary paradigm at all. Yoffee (1979) summarises Rappaport's position as follows "Rappaport...conceives of adaptation as a systemic cybernetic process which is capable of systemic self-transformation only in the context of directional environmental change (Rappaport 1971:23)". In doing so, Yoffee does not misrepresent Rappaport; his mistake is to confuse Rappaport's cultural ecology with modern evolutionary biology.

Yoffee therefore goes on to grossly misrepresent the entire discipline of modern biology, as he is evidently not aware that in neo-Darwinian theory variation is constantly generated independently of environmental change,

whether that change is directional or not. Mutation is a ubiquitous and on going process, a result of the fact that no replication process is perfect. Even a potentially perfect replicator would be subject to the availability of the raw materials used for replication, and may be forced to substitute a new available element for an original element now unavailable. And in most instances, replication can be expected to be even more unreliable than this. Mutation is influenced by the environment in a direct manner, but is not directed by those environmental effects. The direct environmental effects on organisms only affect the likelihood and magnitude of mutation; they do not direct the course of those mutations, and therefore usually do not produce adaptive results.

The main problem which the concept of mutation presents for Yoffee's equation of Rappoport with Darwin, however, is that these small scale environmental effects on copying fidelity continue to take place regardless of the large scale environmental effects of natural selection. Even if the environment is very stable indeed, errors in copying fidelity still occur and as a consequence mutations continue to appear. As the current structural status quo in any population is never perfectly adapted (see Foley 1985, discussed in Chapter 6), it is always possible that a new mutation may be adaptive instead of lethal or neutral, and as a consequence replace the old structure, without any environmental change at all. Thus "systemic self-transformation" can most certainly occur in the absence of directional environmental change in neo-Darwinian process, and Yoffee's equation of neo-Darwinian theory with any system where such cannot occur is misrepresentation on a grand scale.

Thus it can be seen that the minority of analysts (ie. Harris, Yoffee, and Ross) who do not distinguish neo-Darwinian theory from other process theories usually do so through misrepresenting selective process, rather than through misrepresenting Cultural Evolutionism.

The few exceptions not withstanding, it is clear that Cultural Evolutionism and modern Selectionism are fundamentally different evolutionary

theories. For those readers who may still be in doubt, I point out that even the Cultural Evolutionists themselves were emphatically aware of this fact. Dunnell (1980) established this point very convincingly; White, Sahlins and Service often admitted the links between themselves and Tylor and Morgan, while Carneiro, *inter alia*, connects this tradition to Spencer (Dunnell 1980:41). Consider this often quoted paragraph from White, which Dunnell uses to illustrate his point:

> "It would be most gratifying to be able to report, in a paper commemorating the publication of The Origin of Species, that cultural anthropologists had borrowed the concept of evolution from Darwin and that they had employed this concept to establish and enrich their science. Unfortunately, we are unable to make such a report. On the contrary, we must point out that the theory of evolution was introduced into cultural anthropology independently of Darwin and, indeed, of biology in general." (White 1959b:106).

Thus there can be little doubt about generally held views on the structure and origin of evolutionary theory in sociocultural anthropology. The consensus of both the cultural evolutionists themselves, and their cultural selectionist critics, is that this philosophical tradition is non-Darwinian and fundamentally different in explanatory structure to the evolutionary frameworks of modern biology.

In the next section of the chapter, an attempt shall be made to build on the characterisation of Cultural Evolutionism as non-Darwinian, which has so far been accepted upon the basis of the conclusions of other Cultural Selectionists. The idea was to make the general outline of these conclusions the subject itself, to illustrate just how generally accepted the belief in the non-Darwinian nature of Cultural Evolutionism is.

An attempt will now be made to show that three of the most important structural elements of Cultural Evolutionism are not only *absent* in late twentieth century neo-Darwinism, but *present* in early nineteenth century Lamarckism. This make it possible to embellish the critique of Cultural Evolutionism, and in such a way that the critique itself can be more fully described without simply rehashing the conclusions of others.

The reader is also reminded of the fact that there is further discussion of aspects of Cultural Evolutionism, at the level of "empirical implications" in Chapter 7, in addition to the discussion below.

2.3 Cultural Evolutionism as a Lamarckian Orthodoxy

So far, then, all that has really been established is that a large number of neo-Darwinian anthropologists and archaeologists have completely discredited the belief that there is any structural equivalence between Cultural Evolutionism and neo-Darwinian theory. For the main part, this is all that need be established for the purpose of the overall research design of the study. Cultural Evolutionism has been shown not to be the "untried theory", and now other candidates for the theory will need to be considered.

However, a final illustration of how the Lamarckian paradigm persists in Cultural Evolutionism will, I hope, make the selectionist critique of this position stronger still. Moreover, it is interesting to note the existence of such profound structural continuities in evolutionary theory, as it is easy to over-estimate the degree to which "new" philosophies manage to escape the old.

Let us accept the conclusions of the above authors concerning the essentially non-Darwinian nature of evolutionary philosophy in cultural fields of enquiry, and concur with them in rejecting Harris's (1968) notion that the ideas of Darwin and Spencer constitute an evolutionary synthesis. However, non-Darwinian evolutionary thought does have sufficient consistency, I believe, to be considered an evolutionary synthesis, it is just that the synthesis is better characterised as a Lamarckian one. In other words Harris is half right; there is a logical synthesis between Cultural Evolutionism and Biology, but it is with pre-Darwinian biological philosophy rather than with theory of the neo-Darwinian variety. The most obvious biological equivalent of Spencerism is not neo-Darwinism but its binary opposite, Lamarckism. And not only Spencerism, but a range of other related positions in cultural fields of

enquiry can also be shown to be more compatible with a Lamarckian conception of historical process than with a selectionist one. There is, however, enough space to give this only the briefest attention here, and the argument is in any case only employed for one purpose: to impress upon the reader the *degree* to which Cultural Evolutionism is non-Darwinian. It is not just different to neo-Darwinism; it is the theoretical opposite of neo-Darwinism.

In order to present this argument, it is also important to impress upon the reader that the Lamarckian world view encompasses a great deal more than just "Lamarckian inheritance" (of the environmental determinist or psychological instructionist sort). While cultural perspectives often include some notion of an environmentally directed or a "rational" and "objective" dimension to cultural invention, where such inventions may be passed on culturally from parent to child in apparent accordance with Lamarckian inheritance, there are other dimensions to Lamarckian Zoological Philosophy. Cultural equivalents of these additional elements of Lamarckian philosophy can also be shown to exist in cultural fields of enquiry, and in this way the full extent of Lamarckian influence can be traced. The aim is to convince the reader of an interesting pattern: an evolutionary concept which is demonstrably non-Darwinian is very often also demonstrably Lamarckian. In other words, the range of evolutionary principles available in the humanities appears to be much more restricted than what might at first appear to be the case.

Basically, then, the aim of this section is to show that Cultural Evolutionism is even more non-Darwinian than most people think, by virtue of being almost comprehensively (rather than just occasionally) Lamarckian. As the approach has been already very well researched, similar tactics to the previous sections of this chapter will be employed. Established, general accepted assertions about the conceptual structure of Cultural Evolutionism will be briefly paraphrased, and then an attempt will be made to show that these assertions are also true of the Lamarckian world view. In other words, as in the above sections, Cultural Evolutionism itself will be given only the briefest attention. Modern accounts of the nature of Cultural Evolutionism will

be taken as established, as the role of this section is simply to link *what Darwinian Anthropology already knows* about the approach to the Lamarckian world view.

One element of Cultural Evolutionism has already been linked to Lamarck, and that is the principle of environmental determinism known as "The Inheritance of Acquired Characteristics", a principle which Darwin disparagingly referred to as "adaptation from the slow willing of animals". In modern evolutionary discourse, this is the principle generally being referred to when the word "Lamarckism" is used.

Freeman (1974 *passim*), and Rindos (1985), among others, have documented Spencer's explicit faith in the principle of Acquired Characteristics, while Boyd and Richerson (1978), Blute (1979), Dunnell (1980), and Leonard and Jones (1987), as documented in greater detail in Section 2.1, have all, in turn, linked the Cultural Evolutionists to Spencer. Dunnell has captured the non-Darwinian nature of the Cultural Evolutionist concept of adaptation in the following words:

> "In cultural evolution, the role of natural selection is largely usurped by the notion of adaptation...Selection is clearly and unambiguously linked to the materialistic metaphysic. Adaptation, on the other hand, has no such limitation. As a functional concept, it is certainly compatible with a selectionist view of change, but it is no less compatible with a transformational view of change and vitalistic causation." (Dunnell 1980:49).

Even Yoffee (1979:10, and 1985 see Section 2.2) has extensively documented the "environmentally driven" nature of approaches to cultural evolution, although he mistakenly believes that this is also true of modern evolutionary biology.

As this principle is well known to be Lamarckian in origin, the need to "prove" its connection with Lamarck (through reiterating this principle in *his* words) is not so urgent. It is the links between other aspects of Cultural Evolutionism (Progressionism and the Typological Approach) and Lamarck which are less well known, so more attention will be directed toward "proving" these other links, as will soon be seen below. For the sake of those such as Yoffee

(1979, 1985) and Ross (1987) however, who have mistakenly attributed environmental determinism to neo-Darwinism and not Lamarck, I offer a brief archetypal summary of Lamarck's variety of adaptationism, and of modern cultural equivalents of the position.

Virtually any quote from Lamarck will confirm that it is his adaptationism, and not the Natural Selectionist sort, which implicitly invokes environmental instruction. The environment instructs, not through physically shaping the bodies of animals, but through physically determining their "needs". In struggling to meet these needs, the animals transform their own bodies through the effects of use and disuse. Lamarck expresses this concept in the following words:

> "...the environment affects the shape and organisation of animals, that is to say that when the environment becomes very different, it produces in the course of time corresponding modifications in the shape and organisation of animals.
>
> It is true that if this statement were to be taken literally, I would be convicted of an error: for, whatever the environment may do, it does not exert any modification whatever in the shape and organisation of animals.
>
> But great alterations in the environment of animals lead to great alterations in their needs, and these alterations necessarily lead to others in their activities." (Lamarck 1809:107).

The powers of necessity are not just capable of enlarging or diminishing animal organs, they are also capable of generating new organs which never existed before, or of erasing obsolete organs altogether. In the latter case, it is the absence of necessity for an organ, not the necessity for its absence, which brings about its disappearance:

> "...that new needs which establish a necessity for some part really bring about the existence of that part, as a result of efforts; and subsequently its continued use gradually strengthens, develops and finally greatly enlarges it.
>
> ...when new needs have altogether destroyed the utility of some part, the total disuse of that part has resulted in its gradually ceasing to share in the development of other parts of the animal; it shrinks and wastes little by little, and ultimately, when there has been total disuse for a long period, the part in question ends by disappearing.
>
> ...and these acquired modifications are preserved by reproduction among the individuals in question."
>
> (Lamarck 1809:108).

There are many other examples of similar reasoning throughout Lamarck's *Zoological Philosophy* (for example, Lamarck 1809:118-119; 1809:123; and 1809:170). Moreover, a little later in *Zoological Philosophy* he is more explicit about how new organs come into being, despite the fact that, put crudely, a non-existent muscle cannot be flexed. How does an animal struggle to fulfil needs with an organ which does not exist? Lamarck states that new organs are spontaneously generated by the animal's inner feelings of need:

> "Every new need, necessitating new activities for its satisfaction, requires the animal, either to make more frequent use of some of its parts which it previously used less, and thus greatly to develop and enlarge them; or else to make use of entirely new parts, to which needs have imperceptibly given birth by efforts of its inner feeling."
>
> (Lamarck 1809:112).

Cultural equivalents of these ideas also hinge on the concept of the generative powers of "necessity". It is almost as though sheer desperation is enough to guarantee the survival of any human individual. Although many readers may be familiar with some of the modern cultural versions of Lamarckian adaptationism, it is important to point out that it occurs both in a specific form, and in a more general form. At its most general, the principle is embodied by any notion that the environment actually informs or instructs, and thereby directs, the course of inventions. This environmental instruction occurs *during the actual moment of invention itself.* Then, in the more specific form of the approach, these environmentally driven or instructed inventions are then asserted to be inheritable as "acquired characteristics". It may be illuminating to add that both the general and specific assertions are interconnected, in that the "acquired characteristic" inheritance of "random" inventions is implicitly excluded from the agenda. It is also important to point out that both the general and the specific forms of Lamarckian environmentalism are aspects of the process which is termed "specific" evolution by the Cultural Evolutionists (Watson and Watson 1969:14). The cultural evolutionist concept known as "General" evolution concerns a different aspect of cultural process, that of increasing complexity, and this concept may also be found in Lamarck, as will be shown below, where it is labelled "Progressionism" or "Orthogenesis". For the moment, however, discussion will

be focused around Lamarckian adaptation (i.e. "specific" evolution in Cultural Evolutionist terms), in *both its general and specific forms.*

A cultural equivalent of the Lamarckian approach to adaptation can be illustrated with a simple example. Let us imagine that a group of Neanderthals with a classically Mousterian artefact technology are faced with a survival problem, lets say, a series of attacks from packs of wolves. Let us also say that due to certain aspects of the environment (which must be taken purely on faith for the purposes of the argument), the only effective defence against wolves is some stone age equivalent of the modern dog-whistle, which produces a powerful blast of high pitched noise beyond the upper range of the human ear. A Lamarckian environmental determinist would essentially have to argue that wherever a human group was faced with this particular sort of wolf problem, the dog whistle would appear in the artefact assemblages of the group. In other words, a mind which has never been taught how to make metal, leave alone whistles, leave alone how to produce a sound from them, and even leave alone being shown how this may affect wolves, is somehow capable of conceiving of the dog whistle; and, at that, solely through contemplation of the problem of wolf attacks. This is because, in a Lamarckian system, necessity is truly the "mother of invention". It is the stimulus of the problem itself which brings about the very genesis of human awareness of how to make and use the dog whistle. This is why the equivalent philosophical position in psychology is known as *instructionism* (Edelman 1989:31), as features of the environment are seen to actually shape the part of the mind which will ultimately be used to "represent" them, ie. "instruct" the nervous system in an almost didactic way. Lamarckian notions of perception thus imply a "voice in the burning bush" telling the brain what should be perceived (Edelman 1989:32).

In contrast, a selectionist account of perception sees the environment as selecting from within a range of shapes which the mind generates in and of itself, in readiness for a varied environment. Hence Edelman's use of the immune system analogy (Edelman 1989:17), where pathogens do not inform B-

cells as to the appropriate antibodies to manufacture. Rather, a huge variety already exists in the B-cell population, and those B-cells with antibodies which happen to "match" a particular pathogen then undergo accelerated reproduction.

The facts of Spencer's commitment to environmentally driven adaptation (Freeman 1974, *inter alia*), then, as already implied above, needs no reiteration; nor does the fact of Spencer's influence on the Cultural Evolutionists, and their subsequent use of the concept (see above also). But it is worth noting that the influence of "environmentally directed" models of adaptation, where the environment actually guides the invention process, instructing the evolving entity in such a way as enable it to generate the "best solution", has spread far beyond Cultural Evolutionism, both in the general form of the notion of environmentally directed invention, and in the specific form of the inheritance of inventions acquired through environmental instruction. Bargatsky (1984:399), Binford (1981:202), Kirch (1980:147), and Rindos (1984:3) have made remarks about the occurrence of the concept in archaeology. Even committed "Darwinists" sometimes reserve a certain amount of "Lamarckianess" for cultural process. So, for example, while Cultural Selectionism (see also Chapter 5) must by definition view cultural change as a Darwinian process in some sense, statements to the effect that cultural transmission involves "Lamarckian" inheritance sometimes occur, as they do in the work of other committed neo-Darwinians. Examples (both implicit or explicit) would include Alexander (1979:74); Boyd and Richerson (1982:329; and 1985:8); Daly (1982:402); Gould (1980:71); Koerper and Stickel (1980:463); Leonard and Jones (1987:214); Rindos (1985:69-70; and 1989:6); and Spuhler (1984:13).

I would argue, however, that Lamarckian "acquisition" in culture is an illusion created by the unwarranted assumption that humans are the "individuals" of cultural evolution, and that cultural phenomena are merely the "traits" of those individuals (see also Chapter 5, where this argued from a specifically viral critique of the cultural selectionist perspective). Yet if we make the culturally transmitted behaviour pattern or artefact, as much an "individual" of selection as the human individual by which it is acquired, the process of "acquisition of new traits" becomes "colonisation by other individuals" or "colonisation by new parasites". The result of this process is not a transformed individual, but a transformed ecological assemblage. Human cultural inheritance can thus be considered as a kind of colonisation. So the illusion of Lamarckian inheritance results from the artificial adherence to the notion of the human individual as a single inviolable Darwinian individual which acquires "traits", rather than as a susceptible host which "acquires" - and thus is really colonised by, or develops an ecological relationship with - other less complex individuals (again, see Chapters 5 and 9 for related arguments).

In fact, any assertion of functional equivalence between human invention and genetic mutation may usually be considered to be an implicit reference to a Lamarckian model of the origin of cultural variation, as invention is rarely cast as a purely stochastic process in such contexts. The conscious, directed process of human invention is seen as playing the role of "creating" new ideas, in a far more instructed manner than the way in which mutation does so. When the Cultural Virus Theory is introduced in Chapter 9, it is shown how there is no need to deny the existence of highly intentional innovative processes in human affairs, nor even any need to dismiss them as present but insignificant in evolutionary terms (as in Rindos 1985). It is possible that

these processes may be highly intentional but *selective* events, which are actually occurring *after the fact* of other random creative processes which have simply escaped detection (Chapter 9 also). But unless claims of the equivalence of invention and mutation (for cultural and genetic evolution respectively) are qualified with an explicit assertion about the fundamentally random nature of cultural invention (or qualified in the manner just deferred to Chapter 9), they are usually implicit assertions of Lamarckian adaptationism.

2.4 The Widespread Nature of Cultural Lamarckism

It can therefore be seen that at least one of Lamarck's philosophical concepts is present in twentieth century approaches to culture. What is less well documented is that the other two well recognised (and interlinked) non-Darwinian elements of Cultural Evolutionism, "Progressionism" or "Orthogenesis", and the "Typological Approach", are also pervasively present in Lamarckian Zoology, and the function of this section is essentially to demonstrate this fact.

Firstly, however, as was done with Lamarckian Adaptationism, I would like to briefly reiterate established opinion on the presence of these philosophical principles in Cultural Evolutionism, before moving on to outline their essential qualities, and to show how similar concepts are employed in Lamarckian Zoology.

The presence of both Progressionism and the Typological Approach in Cultural Evolutionism has been well documented by Dunnell (1980:37-49), Fagan

(1980:25-31), Freeman (1974:215), Leonard and Jones (1987:201), Rindos (1985:66-68), and implicitly by Yoffee (1979 *passim*; and 1985:41). Some more recent actual uses of structurally similar concepts include Cohen (1981:202-206), Lourandos (1983:81), and Watson and Watson (Watson and Watson 1969:145-148). It is well established, then, that Cultural Evolutionists often appeal, either explicitly or implicitly, to some law of absolute progress, propelling cultural systems toward ever increasing complexity and/or perfection, and that they also use a taxonomic system based on such progression (ie. the Typological Approach). It is also fairly well established that any concept of *inevitable* progress is anathema to selective explanation, as will be discussed in greater detail in Chapter 6.

"Progressionism" (see also Chapter 7) is a term used in the evolutionary literature to describe notions of change which view either organisms or cultural phenomena as possessing some intrinsic force which drives them toward greater and greater complexity and corresponding perfection. One of the simplest ways to identify progressionist reasoning in explanations of cultural change is to find any use of the word "devolution" (e.g. Cohen 1981:206). This word is implicitly Lamarckian, as neo-Darwinian evolution has no intrinsic direction which "devolution" can turn it away from in the first place. Relatively simple organisms such as bacteria have not remained simple for billions of years due to lack of evolution, or due to some cataclysmic "devolutionary" event. Actually, the opposite is true; their evolution is *even more rapid* than complex organisms such as the chordates, so much so that the capacity of bacterial populations to adapt with almost lightning speed to antibiotics is a modern legend (Sonea 1988).

It is just that neo-Darwinian evolution, unlike Progressionism, does not necessarily involve increasing complexity, and may, as in the case of bacteria, involve an endless number of mutations and selective substitutions within a genome of fixed size. It is important to note, however, that this rule of "no necessary increasing complexity" in Selectionism does not apply so reliably at the ecological level. If, for example, we take the whole planet earth as an ecological assemblage, it is generally true that "life on earth" *as a whole* has proceeded toward greater and greater complexity. Dawkins, for example, has used his concept of the "meme" not to develop directions for a general theory of culture (as the Cultural Virus position is used for in Chapters 5 and 9), but to illustrate the general tendency of neo-Darwinian systems toward *ecological* complexity:

> "The word meme seems to be turning out to be a good meme. It is now quite widely used and in 1988 it joined the official list of words being considered for future editions of Oxford English Dictionaries. This makes me the more anxious to repeat that my designs on human culture were modest almost to vanishing point. My true ambitions - and they are admittedly large - lead in another direction entirely. I want to claim almost limitless power for slightly inaccurate self-replicating entities, once they arise anywhere in the universe. This is because they tend to become the basis for Darwinian selection which, given enough generations, cumulatively builds systems of great complexity...Whether the milieu of human culture really does have what it takes to get a form of Darwinism going, I am not sure. But in any case that question is subsidiary to my concern...My purpose was to cut the gene down to size, rather than to sculpt a grand theory of culture."
>
> (Dawkins 1989:322).

Of course, even ecological complexity is subject to "backward" turns, in the form of ecological collapse as a result of natural disasters of various kinds. Yet, in a selective framework, such mass extinctions are not considered to represent "devolution", but evolution, which, if anything, becomes *more* intense at these times. So, in an authentically neo-Darwinian perspective, all "devolutions" are just cases of evolution. We can therefore conclude that the mere use of the term "devolution" implicitly betrays progressionist (and thus by definition non-Darwinian) reasoning.

The Typological Approach (again, see also Chapter 7) is a natural corollary of Progressionism, as it is simply a classification scheme based on the gradient of increasing complexity allegedly produced by the latter (see Leonard and Jones 1987 *passim* for a succinct discussion). The familiar Cultural Evolutionist sequence of "Band, Tribe, Chiefdom, State", or related sequences such as "Band, Tribe, Chiefdom, Capitalist State, Communist State", all rest upon some notion of necessary sequence, of progress toward some ideal. Progress along the sequence can be hastened or slowed, brought to a halt, inverted, or even reversed; but the sequence itself remains. Moreover, its *fundamental linearity* also remains, and this is why even the "multilinear" concept of Steward (1955) is still quasi-typological, and quasi-progressionist. In fact, even Lamarck allowed a small number of collateral branches in his linear series (see Elliot's introduction to *Zoological Philosophy*, Lamarck 1809:XXVI).

In contrast, a truly neo-Darwinian metaphysic is an infinitely (literally) radiating branching and sub-branching hierarchy based purely on genealogical descent and local (not absolute) progression (see above and also Chapter 6 for related points of discussion). It is therefore (which some Postmodernists may be surprised to learn) the ultimate relativism. Selection may be a law, but unlike Progressionism in Cultural Evolutionism or Marxism, it is not a law about the course of history itself. It is a law of divergence, rather than convergence, despite the instances of quasi-convergence which do occur within selectionist systematics. If Selectionism must be considered a law, it is a law about the very indeterminacy of the course of history, and in this sense its a law which immediately deconstructs (at the level of specific sequences) its own lawfulness; in the same way that the banner of relativism in Postmodernism or Critical Theory ultimately deconstructs itself into just another voice amongst the multitude (Renfrew and Bahn 1991:430).

As already noted at the beginning of this section, the presence of Progressionism and Typological classification schemes in Cultural Evolutionism has been well documented by Blute (1979) and Dunnell (1980) *inter alia*. However, few archaeologists have pointed out that Progressionism is as much

an integral part of Lamarckian Biology as it was part of Cultural Evolutionism. In other words, Progressionism was not something invented by nineteenth century anthropologists; rather, it was a basic constituent of pre-Darwinian philosophy in general. The importance of this observation is to show that there is nothing particularly "cultural", leave alone selective, about the idea of necessary progress.

It is also interesting to note that Lamarckian Progressionism is not unlike Lamarckian Adaptationism, in that both processes are driven by the "feminine goddess" of "Nature", and that in both cases the basic mechanism by which "she" takes effect is through affecting the needs of organisms, which arise from increasing complexity elsewhere in the body. Progression in one area leads to more, as the energy of the movement of fluids gradually "accelerates", thereby providing a series of constraints which complement acquired adaptations:

> "if nature had not been able to endow the organising activity with the faculty of gradually increasing the complexity of organisation by accelerating the energy of movement of fluids and hence that of organic movement, and if she had not preserved by reproduction all the progress made in complexity of organisation and all acquired improvements, she would assuredly never have produced that infinitely varied multitude of animals and plants which differ so greatly from one another both in their organisation and in their faculties."
>
> (Lamarck 1809:130).

Lamarck later explores a number of particular instances of this orthogenetic "accelerating movement of fluids", in his discussions of infusorians and polyps (1809:269) and even mammals (1809:166). Nearer the beginning of *Zoological Philosophy* he introduces the concept with reference to the nervous system (1809:49).

Progressionism is a universal force, a "transgenerational developmental ontogeny", an in built complexity clock which ticks away from the moment each new organismic lineage is brought into being by spontaneous generation. Moreover, just as in Cultural Evolutionism and Marxism [indeed, for Spencer evolution and the ontogeny of the embryo were one and the same thing (Desmond and Moore 1992:393)] it is not just a course toward greater

complexity, but also toward perfection, culminating in the class mammalia (Lamarck 1809:167). The effects of this clock may be obscured by environmentally generated effects, and thus arrested, reversed or slowed, but the series itself may not be transcended, as we may only travel up or down along the linear dimension, or else remain motionless. Moving outside the series, or "leap-frogging" over a particular stage, is not allowed.

> "the increasing complexity of the organisation of animals from the most imperfect to the most perfect exhibits only irregular gradation, in the course of which there occur numerous anomalies or deviations with a variety in which no order is apparent.
>
> Now on seeking the reason of the strange irregularity in the increasing complexity of animal organisation, if we consider the influence that is exerted by the infinitely varied environments of all parts of the world on the general shape, structure and even organisation of these animals, all will then be clearly explained." (Lamarck 1809:107).

Elsewhere, Lamarck makes it even more abundantly clear that such "degradations" (akin to the "devolution" of Cultural Progressionism), produced by the environment, actually exemplify rather than confound the spirit of Progression (see Lamarck 1809:69 and also 1809:126). All in all, the appearance of complex forms is ineluctable from the moment the simplest forms appear:

> "That, since all living bodies are productions of nature, she must herself have organised the simplest of such bodies, endowed them directly with life, and with the faculties peculiar to living bodies.
>
> That by means of these direct generations formed at the beginning of both of the animal and vegetable scales, nature has ultimately conferred existence on all other living bodies in turn."
>
> (Lamarck 1809:236).

In this way the orthogenetic forces of Progressionism produce a natural series of animals and plants, the "Great Chain of Being" which bears a striking resemblance to the Cultural Evolutionist Typological scheme which is now such a familiar part of the Cultural Selectionist critique of that approach:

> "I have divided into six distinct stages the various modes of organisation recognised throughout the animal scale.
>
> Of these six stages of organisation, the first four comprise the invertebrate animals, and consequently the first ten classes of the animal kingdom according to the new order that we are going to follow; the last two stages comprises all the vertebrate animals and consequently the four (or five) last classes of the animals.
>
> By this method it will be easier to study and follow the procedure of nature in the production of animals that she has

> brought into existence; to recognise throughout the animal scale the progress made in complexity of organisation and every where to verify both the accuracy of the classification and the propriety of the rank assigned by examining such characters and facts of organisation as are known." (Lamarck 1809:130-131).

So incorruptible is the series that Lamarck must argue that stages of different complexity must be necessarily of different age:

> "it is not true that Species are as old as Nature, and have all been in existence for an equal period...(rather) it is true that they have been formed one after another...they have only relative constancy, and are only invariable temporarily."
> (Lamarck 1809:35).

The two Lamarckian forces of Need and Orthogenesis thus work in concert to produce a rigid series of organisms, divided into six basic stages according to mode of organisation, with the most imperfect at the beginning and the most perfect at the end. The first five constitute the living "contemporary ancestors" of the last, just as they do in the Cultural Evolutionist series of Band, Tribe, Chiefdom, State. Moreover, each principle is dependent upon the other. Without the capacity to respond to the environment, the overly rigid products of orthogenesis would be robotic archetypes unable to survive any environmental change, while without orthogenetic progression the products of the environment would be so varied and "degenerate" that no coherent pattern of diversity could ever be elucidated by the biologist.

It is no coincidence, then, that Lamarckian Adaptationism, Progressionism, and Typological systems of classification tend to occur together, whether we are speaking of nineteenth and twentieth century anthropology, or eighteenth and nineteenth century biology. These concepts are integral parts of the same evolutionary framework. As a consequence, the three non-Darwinian elements of Cultural Evolutionism, which have been so well documented by the Cultural Selectionist critique, should not be viewed as evidence that Cultural Evolutionism is some sort of unique anthropological invention, or as some special theory which was custom made for cultural fields of enquiry. They are not "cultural anthropology's answer to the tyranny of selectionism". Rather, they represent the persistence of an even older naturalistic "tyranny", an even older biology, in explanations of cultural

change. It is fair to say, then, that much of non-Darwinian archaeology is dominated by a Lamarckian orthodoxy of the kind that Selectionism originally faced in nineteenth century biology, and there is no reason to suppose it is any more appropriate for use in cultural fields of enquiry, than it was in fields of enquiry centred upon nonhuman animals and plants.

2.5 Concluding Remarks

It can thus be seen that while the Cultural Evolutionists can legitimately boast of being independent of Darwin, White's claim (1959b:106) of independence from *biology as a whole* may be somewhat misleading. This second and more exaggerated assertion may well reflect the romantic notion that Cultural Evolutionism is some sort of unique invention, specially designed for the "unique process" of cultural change. But I have tried to argue that this is not the case, as the three main explanatory elements of Cultural Evolutionism are profoundly similar to the three main elements of Lamarckian Zoology. Ironically, then, Cultural Evolutionism is just as "biological" as popular belief would have it to be; it just that it is "biological" in a way that is too ancient for many modern anthropologists to recognise. It is biological, but not neo-Darwinian. To use the approach's own Spencerian rhetoric against itself, one can argue that Cultural Evolutionism is simply the "primitive contemporary ancestor" of modern evolutionary biology, "pushed out into the adjacent field of culture", where it has somehow managed to survive. It is simply Lamarckian Zoology transposed into a Lamarckian Anthropology.

The paradox of the Cultural Evolutionist position can be illustrated with a metaphor of warfare. Imagine that during the nineteenth century there are two tribes, the "Lamarckians" and the "Darwinians", and that these two tribes are fighting for possession of a small continent, a tract of land which we will call, for want of a better term, "Biology". The "Darwinians" are more successful, and despite some sustained guerilla warfare from the "Lamarckians", ultimately prevail. The "Lamarckians", all but dispossessed,

retreat into the highlands of "Biology", a vast mountain range known as "Culture", and by the turn of the century they manage to re-establish the old regime in their new mountain territory. After several decades of peace and prosperity, however, the "Darwinians" march to the borders of "Culture", and begin to invade, moving into the mountains of "Culture" along valleys of "Genetic Constraint".

In order for the "Lamarckians" to incite the people of "Culture" to resist the Darwinian invasion, the Lamarckians must avoid overemphasizing their biological origins, as a battle cry of "save the old biology from the new" would presumably arouse very little sympathy amongst the natives of "Culture". So the "Lamarckian" overlords of "Culture" now need to forge a new identity, one which is completely polarised from the Darwinian invaders. The "Lamarckians" therefore become the "Cultural Evolutionists", champions of cultural fields of enquiry, defending us from the evils of "Biologists" and "Darwinians".

The loser is the vast hill-kingdom of "Culture", which, in defending its borders from the "Darwinians", is not resisting biology at all; "Culture" is really only defending the old and obsolete Lamarckian biology from the new.

The above story is obviously only a metaphor, but it intended to reflect an overall impression of the true identity of Cultural Evolutionism. Nonetheless, for the purposes of this study, the value of the metaphor is to emphasize the non-Darwinian nature of the approach. Whether we choose to recognise the fundamentally *Lamarckian* nature of Cultural Evolutionism or not, the main point of the chapter is that this approach can certainly be dismissed as a candidate for the untried theory. It is a well tried and well established theory of cultural change, but it cannot be called neo-Darwinian, or even thematically selectionist. The typological or essentialist system of classification, the unqualified assumption of increasing complexity or orthogenesis, and the environmentally driven or instructionist model of adaptation, are all anathema in modern Selectionism. If the untried theory is to be found, attention must be turned elsewhere. So, in the next chapter, another body of evolutionary

principles - culturally oriented Sociobiology - is considered as a second candidate for the untried theory.

3

CULTURAL SOCIOBIOLOGY

Overview

In the last chapter it was shown why Cultural Evolutionism has not solved the problem of the untried theory. Here it is shown why cultural Sociobiology can also be dismissed as a potential solution. Although the sociobiological perspective provides an adequate framework for the explanation of human genetic process, it does not do the same for cultural process. The unwarranted assumption that the survival of a particular behaviour pattern is simply self evident in the survival of its host leads cultural Sociobiology to violate the fundamental Darwinian principle of adaptive selfishness. In doing this cultural Sociobiology actually sustains, rather than removes, the Lamarckian orthodoxy of Cultural Evolutionism, since a cultural phenomenon ordered by the imperative of organismic reproduction is thereby "ordered from without".

3.1 Selfishness and Cultural Sociobiology

While Cultural Evolutionism cannot claim to be neo-Darwinian in either structure or origin, cultural Sociobiology can legitimately claim to be neo-Darwinian at least in the latter. Moreover, while few Cultural Evolutionists have ever identified closely with Darwin, as shown in the previous chapter with reference to Dunnell (1980:41), Sociobiology has always had a self proclaimed neo-Darwinian identity. Cultural Sociobiology, however, falls far short of achieving a neo-Darwinian theory for cultural evolution itself, basically through its refusal to take full account of the differences between genetically and culturally transmitted behaviour. It will be argued throughout this chapter that an implicit Lamarckian explanatory structure for cultural adaptation is used by Sociobiology in concert with an otherwise selectionist

approach to human genetics. In other words, Sociobiology's perfectly selectionist approach to human survival and reproduction collapses into Lamarckism in cultural contexts where the survival and reproduction of socially transmitted phenomena should properly be the focus of attention. In this way Sociobiology unintentionally maintains the "Lamarckian orthodoxy" of Cultural Evolutionism. Once genealogical independence from genetic inheritance is granted to cultural phenomena, any direct causal influence of genes over cultural evolution must assume the qualities of Lamarckism, since culture is then effectively being "ordered from without" (see Rindos 1986b for a parallel assertion).

A fundamental difference between culturally and genetically transmitted behaviour is that while any genetic trait must be transmitted downwards in the genealogy of a population, ie. from parent to offspring, a cultural "trait" can be transmitted in a much wider array of relationships and directions, including the diagnostic pattern of "horizontal" transmission (Cavalli-Sforza 1991:78, Cavalli-Sforza and Feldman 1981). Such transmission includes that from offspring to parent or grandparent, between sibs, from parent's sib to offspring, between cousins, or even between unrelated members of the population (in so doing, it thereby "deconstructs" its own "traithood", as is discussed in Chapters 5 and 9). Furthermore, even parent to offspring cultural transmissions are not "synchronised" with genetic reproduction. These differences would suggest some more than *ad hoc* amendments to the explanatory frameworks used to explain social behaviour in acultural societies such as eusocial insects (Wilson 1971) or mammals such as mole rats (J. Jarvis and R. Brett, in Dawkins 1989:314). The difference essentially grants a separate and potentially different genealogy to the behavioural "traits" and genetic traits, respectively, of a cultural organism; while, on the other hand, the behaviour and genotype of the acultural organism have a synonymous genealogy. This basic point will be developed throughout the chapter and shown to conflict with a basic qualification of selectionist theory made by Darwin himself. By doing this, Sociobiology is thus obliged to take up a Lamarckian explanation for cultural adaptation, and consequently uses essentially the same approach to cultural process as sociocultural evolutionism;

a conception of evolution as either internally or externally directed. Later in this study, in Chapters 6, 7, and 8, these two approaches become the pre-Darwinian or Lamarckian null-hypotheses for the multivariate analysis of patterns of cultural variation.

Other writers, many of whom have already been referenced in the previous chapter, have also identified conceptual weaknesses in cultural Sociobiology. Most of the analyses which detract from Sociobiology do not, however, question the authenticity of its reputed links with the neo-Darwinian synthesis of modern evolutionary biology. Yet some characteristics of cultural Sociobiology which have already been well documented are incommensurate with properly interpreted Selective principles. Blute (1979), Dunnell (1980), Durham (1990:193), Lewontin, Rose and Kamin (1984), Peters (1980), Rindos (1986b), Ross (1987) and Yoffee (1979, 1980, 1985), have given significant attention to the shortcomings of Sociobiology when it comes to cultural contexts. Similar studies which discuss basically sociobiological principles in cultural "adaptationism" and "processual" archaeology include Bray (1973), Hole (1973), Alland (1975), Kirch (1980), and Foley (1985). Bargatsky (1984) has provided a particularly comprehensive analysis. Yet none of the above critiques have, to my knowledge, featured the argument submitted below; that Sociobiology violates its own self proclaimed neo-Darwinian principle of adaptive selfishness in failing to take full account of cultural transmission.

The initial stage of the argument will be take the form of an ethnographic case study, Betzig's *Despotism and Differential Reproduction.* Discussion will then be transferred to the general issues raised by the case study, and reference will be made to a number of other cultural adaptationist and sociobiological approaches.

3.2 Despotism and Differential Reproduction

In the early eighties, Betzig undertook a comprehensive ethnographic and historical study of social hierarchy in pre-industrial societies. In 1986, this work was published in a book under the above mentioned title. Using a regional sampling procedure, Betzig covers societies from all areas of the world, with most of them dating to the ethnohistorical period. The incidence of "despotic" leaders and the corresponding incidence of high degrees of "reproductive success" form the focus of the world wide comparisons. "Despotism" is defined as "the exercised right of heads of societies to murder their subjects arbitrarily and with impunity" (Betzig 1986:2), and is used by Betzig as evidence for extreme social asymmetry amongst the rights of the individuals of a given society. "Differential reproduction" is measured both in terms of historical reports of numbers of wives or consorts, or numbers of actual children. What Betzig seeks to illustrate, is that where an individual finds himself (and most are male) in a position where his social rights are considerably greater than those of his fellows, he will attempt to translate that greater power into greater reproductive success via the acquisition of wives and consorts. In other words, human behaviour is viewed to be exclusively shaped by criteria of reproductive success. This typically adaptationist explanatory structure will be examined in greater detail later in this section; at this early point in the discussion Betzig's data and methodology will be outlined.

Vast numbers of ethnographic accounts make this piece of research interesting and informative to read. In general, Betzig's prediction that the incidence of despotism and differential reproduction will be highly correlated finds ample support in her sample societies. For example, in societies of the Natchez of the lower Mississippi valley in North America, there seems to be just such a correlation. Betzig quotes and paraphrases J.R.Swanton's *Indian Tribes of the Lower Mississippi Valley* in the following excerpt:

> "In the lower Mississippi valley, the word despotism is almost always used in the early accounts of the government of the Natchez chief. According to Dupratz, he was "absolute master of the goods and life of his subjects," disposing of them at his pleasure; "his will is his reason" (quoted in Swanton 1911:106).

> According to Charlevoix, the relationship between a chief and his subjects could be characterised almost as slavery; as soon as anyone had the misfortune to displease either the Sun or the woman most closely related to him, they were ordered to be killed by the guards. "Go and rid me of that dog" they would say, and be immediately obeyed (in Swanton, 1911:101)." (Betzig 1986:50).

Whenever the chief said anything the subjects had to "howl" nine times; they also had to howl when approaching or leaving the presence of the chief, when visiting his cabin, when formally saluting the chief, and after every question the chief asked (Betzig 1986:51). In accordance with Betzig's prediction, such despotic social inequality does translate into unequal access to women and differential numbers of offspring among the Natchez. The chiefs not only had the above rights to impinge upon their subjects lives, but also huge houses with many wives and beds, and they had only to notify a woman's relatives that they desired her and she automatically became one of their wives (Betzig 1986:74).

Another of Betzig's sample societies is the Mbau of Fiji. Here leaders also have both absolute rights over the fate of their subjects and large numbers of wives. Betzig draws on three sources, D. Toganivalu (1912), M.D. Wallis (1851), and T. Williams (1884). The absolute rights which chiefs held over their subjects included the following:

> "(There was) the case of the chief who requested a hoe from a subject and, on being refused, took his wife instead; the case of the Mbau chief who, on finding his villagers had cut him fewer reeds than he wanted, had the village burned; and the case of the mere petty thief who, discovering that a man had accidentally injured one of his ducks, supposing it wild, demanded the fingers of four individuals in compensation...In general, "Offences, in Fijian estimation, are light or grave according to the rank of the offender. Murder by a chief is less heinous than a petty larceny committed by a man of low rank." (Betzig 1986:54).

Chiefs had the right to take anything from their subjects. When arbitrating over cases concerning theft, they would at times release the guilty party providing a portion of the stolen goods could be extracted in payment. As evidence for the fact that leaders with these absolute rights also had differential access to women, Betzig offers the single example of the chief Tanoa who spoke of the advantages of polygyny with great emphasis, and was said to have had 100 wives (Betzig 1986:75).

A more dramatic confirmation of Betzig's predicted correlation was found amongst the Azande of Sudan. Her sources include Brock (1918), Casati (1891), Czekanowski (1924), Lagae (1926), and Baxter and Butt (1953). Death was the punishment for the least indiscretion sustained against the ruler, revoked only if the crime was perpetrated by a member of the upper classes. Common offenses such as disloyalty, use of magic against chiefs, committing adultery with the wives of the chief, or treason, usually resulted in instant strangulation at the chief's hands (Betzig 1986:51 after Baxter and Butt 1953). Mutilation was the only alternative to the death sentence, and in adultery cases the offenders were emasculated, and their hands, ears, and lips were cut off. Often the relatives of newly deceased offenders were required to pay indemnity to the chief in the form of several women. Leaders who possessed these rights invariably used their power to maintain disproportionately large entourages of women. Numbers of wives were dependent on the degree of power held by the man concerned. In Azande society, every man with a position of authority had several wives, even though the vast majority of adult men were either bachelors or monogamously married. According to one of Betzig's sources, Zande chiefs had between thirty and one hundred wives, and the king had more than five hundred women. Young women were monopolised at an early age, and female children were spoken for well before puberty (Betzig 1986:74).

Many other societies are offered by Betzig as examples of her "Darwinian" prediction. Khmer kings in southeast Asia mutilated and killed those found guilty of crimes, while maintaining entourages of up to five thousand women. Leaders in Inca Peru possessed the right to "murder arbitrarily, and the more powerful position a man occupied the more women he was allowed. Many of the chiefs also went to great lengths to prevent other men from gaining access to their women, often by isolating a household of women from the rest of the community. Fourteen societies were included in the sample with more than half of the leaders having more than one hundred wives, and all of them having exclusive access to large numbers of women.

Since Betzig's basic method of assessing the reproductive success of these despotic leaders involved counting women rather than children, the next step in her argument was to demonstrate that these large numbers of women were in fact being maintained, at least indirectly, for the purposes or intention of reproductive success. Were the women in the sample societies being valued for their reproductive ability, or maintained for some other social purpose? Betzig argues that the reason is indeed the former. In support of this assertion she appeals to other cross-cultural studies. For instance, ten cross-cultural studies have arrived at the conclusion that childless couples are more likely to divorce, a statistic from which Betzig concludes that at least some marriages are undertaken partly for the purpose of reproduction. Other studies show that in some societies the payments associated with matrimony are not transferred until the marriage has been "validated" by the birth of the first child. In such societies brideprice payments are frequently delivered only after the birth of children; and where it is paid before the birth of a child, bridewealth compensation may be demanded in the event of a childless marriage; moreover divorce is often the result of adultery. Betzig finds several other variables affecting marriage breakdown which can be logically related to the ability of women to produce children in her ethnographic sample, concluding that an important motivating factor involved in marriage, and by inference, the multiple marriages of despots, is indeed the desire for children.

Thus, Betzig finds ample confirmation in the ethnographic literature for her predictive hypothesis; yet the sociobiological explanatory structure which lead her to this apparently correct prediction is flawed. I will now focus on the flaws in her explanatory structure in order to demonstrate how her explanation is in fact (arguably) Lamarckian, and how, therefore. the explanation would be untenable within a properly interpreted neo-Darwinian framework.

3.3 Do Despots Reproduce Despotism When They Differentially Reproduce?

One of the basic flaws in Betzig's explanatory structure is that it does not discriminate between individually learned behaviour and culturally transmitted behaviour. The question is never asked whether these two kinds of learned behaviour might be completely equivalent. The assumption of equivalence is implicit throughout the study, but particularly apparent in the chapter where the results of the ethnographic sample are freely compared with a range of animals which do not have the human capacity for culture, including seals, mice, chickens, wasps, and deer (Betzig 1986:87). I am not by any means suggesting that interspecies comparisons of dominance behaviour would not be very illuminating for this kind of human behaviour, but simply that a discussion of the cultural component to human behaviour should be an integral part of such comparisons. While each of the animal studies cited by Betzig also describe correlations between dominant social position and reproductive success, no mention is made of the fact that wasps, say, have virtually no capacity for cultural transmission. This dismissal of the significance of cultural transmission is typical of many sociobiological frameworks, and in this case, despotic wasps and despotic humans are explained via an appeal to an utterly homologous explanatory framework. Implications of this denial of cultural process will be drawn out later in this section.

But what leads the author to ignore such an obvious process as cultural transmission in the first place? Betzig readily grants the existence of learned behaviour, as suggested above, and correctly cites certain conclusions of psychological research, which maintain that genetically reproduced structures in the brain mediate the learning process, thereby constraining it (Betzig 1986:9). There is no doubt that genetics can influence behaviour. This does not, of course, mean that genetics determines behaviour; but Betzig accepts this. Betzig counts genetics mechanisms of behavioural *constraint* as sufficient support for a sociobiological approach to human culture.

Having established some link between genes and behaviour, and thereby the basic heritability of behaviour, the next step taken is the articulation of the Darwinian approach. Betzig uses the phrase "heritable traits which promote reproduction will spread" as her basic definition. The reader might care to note at this point the conspicuous absence of any reference to constraints on adaptation (e.g. Foley 1985 after Dawkins 1982), in this Darwinian phrase which Betzig relies upon. The phrase is a simple summary of natural selection or adaptation at the organismic or phenotypic level of biological populations. In no part of the phrase is there any reference to genes or the genotype, mutation as the source of the variation in heritable traits which must exist in the first place, to random recombination, or to random selective processes such as genetic drift. Nonetheless, the phrase is, at least, an adequate summary of the process of selection.

Thus the two basic premises upon which the sociobiological argument is subsequently built are not unsound in and of themselves. Behaviour is influenced indirectly by genes, and heritable traits which promote reproduction will spread. However, Betzig's avoidance of any distinction between cultural and acultural learned behaviour interferes with the manipulation of these two assumptions.

In manipulating the first premise concerning learned behaviour, the author fails to recognise that the human behaviours which she seeks to explain with the Darwinian framework are not merely learned, but *learned in a very peculiar way*. Learned not only in the dialogue between the individual and the physical world, but also in dialogue with a social environment from which certain concepts may be "learned" as *fait accompli*. For this reason human despotic behaviour cannot be explained purely as an outcome of individual experience and learning interacting with genetically inherited psychological biases. Human individuals have other sources from which to learn, in addition to individual trial and error; obviously the cultural state of other individuals in the society provides the learning individual with the results of the learning experiences of others. If human behaviour was primarily acultural, as behaviour may well be amongst some of the animal

groups to which Betzig compares human societies, a given behaviour pattern could well be explained in terms of a combination of individual learning experience and the genetically constrained capacity to learn from that experience. The typical sociobiological framework would be appropriate in such contexts, with the prevalence of certain behaviour types in a given society explained in terms of the natural selection of those genotypes which constrained the individuals learning in that direction.

Culturally learned behaviour patterns however are not so amenable to the classic framework, since the structure of such behaviour is not simply the outcome of complex interaction between learning capacity and learning process. An individual with the capacity for culture can have whole behaviour patterns transmitted to it, and that behaviour pattern is thereby reproduced within the society. The frequency of such behaviour patterns in a particular community is thus an outcome of individual learning and the reproductive success of genetic capacities, and the reproductive success of that behaviour pattern itself. In the despotism case study, the lack of distinction between different kinds of learned behaviour therefore neglects the fact that despotic behaviour patterns are not perennially re-emergent simply through the reproductive success of genetic capacity and individual learning experience, they also require maintenance through cultural transmission.

Distinctions within the "learned behaviour" category are commonly omitted in sociobiological frameworks, in association with the treatment of culture as a dimension of phenotypic plasticity, the order of which is explainable only through an appeal to the order that can be imposed by the genotypic basis to that plasticity. In the case of the behaviour of a human in a position of power, ignoring cultural transmission leaves only one conceptual function for selectionist theory; explaining the reproductive success of the genetic capacity for culture through natural selection of those organisms which possess it. Once a culturally transmitted behaviour pattern is considered to be explainable only via an appeal to phenotypic plasticity, it can be explained in the same manner as any other organismic trait.

organism formed exclusively for the good of another organism, and that such could not have been formed through natural selection. Therefore, the explanation of cultural structures purely in terms of the good of the genetically emergent structures of organisms presumably implies that some processes *other* than selection are operating in culture itself. Correspondingly, the sociobiological explanatory framework, through its avoidance of discussion of cultural selection, implies that the processes of transformation by means of which the cultural system serves as "man's extrasomatic adaptation" are something other than selective.

To put this somewhat convoluted argument another way, a sociobiologist who intends to continue breaking this Darwinian principle, and to see cultural entities as formed exclusively for the good of human individuals, must find a mechanism other than natural selection for this process, or else deny the validity of Darwin's assertion that "such could not have been produced by natural selection". The latter alternative is not a real option, as it would require taking up the basically untenable position that reproductive success in one system could automatically propagate structures in another.

Thus, ironically, a sociobiologist must take the first option, and espouse a Lamarckian notion of cultural process (if that process is to be directed exclusively by human reproductive criteria) since neo-Darwinian process by its very nature cannot be exclusively directed by anything, let alone by criteria of reproduction in a separate system of inheritance. The cultural system cannot serve human adaptation without being a system of Lamarckian adaptation. Evolutionary processes which are externally directed are basically "Lamarckian" processes, and as a consequence some sociobiologists simultaneously advocate a Lamarckian approach to culture, and a neo-Darwinian approach to the effects of that Lamarckian system on human genetic evolution. Consequently, assertions of the selective or neo-Darwinian nature of human evolution, and the fundamentally Lamarckian or deterministic nature of cultural evolution, often occur in the literature as part of the same theoretical position.

Thus three assumptions are bound together in Betzig's explanation of Despotism:

(A) A lack of consideration of the implications of cultural transmission for distinguishing socially learned behaviour from individually learned behaviour; leading to

(B) The general classification of culturally transmitted behaviour along with individually learned behaviour, under the general umbrella of phenotypic plasticity; leading in turn to

(C) The ultimate classification of cultural behaviour as a typical class of organismic traits, and seen (as are all traits in adaptationism) purely as vehicles of organismic reproductive success.

3.4 A Wider Perspective on Cultural Sociobiology

So far attention has been focused on a single case study within Sociobiology. But Betzig is by no means alone in her use of these assumptions. They are apparent in the work of many sociobiological expositions in addition to the case study. Flinn and Alexander (1982), for example, expressed the third of these concepts when they wrote:

> "(we argue that) cultural traits are, in general, vehicles of genic survival, and that the heritability of cultural traits depends on the judgements...of individuals with regard to their effects on the individual's inclusive fitness." (Flinn and Alexander 1982:397).

Crook and Crook (1988) reveal their acceptance of these interlinked assumptions in the following quoted paragraph. The key phrases are italicised for more specific discussion below. They write:

> "Irons (1979), following the pioneering lead of Haldane (1956), remarked that the most reasonable hypothesis is that behavioural differences shown by contrasting human groups *"are environmentally induced variations in the expression of basically similar genotypes"* and that the ability to vary behaviour in response to environmental differences is itself the prime human adaptation to a social world. (there is) Evidence that the social expressions of human phenotypes are often strategic in the sense that they *maintain the inclusive fitness of individuals in ways*

formally comparable to those without culture..." (Crook and Crook 1988:97, my italics).

In the first italicised phrase, Crook and Crook use a definition of culture which does not distinguish culture from individually learned behaviour in acultural organisms, or even from physical phenotypic alterations by the inorganic environment such as a sun tanned skin. As mentioned above in the case study, cultural behaviour is thus classified under the general category of phenotypic plasticity, and the possibility that cultural transmission might make cultural phenotypic plasticity any different to other forms of plasticity is thereby ignored. In the second italicised phrase, cultural plasticity, as part of the phenotype, and like any other plasticity, assumes its typical role; the maintenance of inclusive fitness.

Daly (1982) also employs the above series of sociobiological premises in his approach to culture. Throughout his paper in which the critique is made, a sociobiological viewpoint is presented in which cultural "traits" are considered to be typical expressions of phenotypic plasticity (1982:405). All such traits are then viewed to be exclusively shaped by adaptation, as if cultural and genetic "traits" jointly constitute a coherent organism with a single genealogy:

> "...all evolved traits of all organisms serve a single goal: the replication of the focal individual's genes (the promotion of inclusive fitness). Fitness-promotion entails both (1) accrual of those resources convertible to fitness - being an effective reproductive competitor - and (2) the conversion itself - being an effective dispenser of benefit to kin, especially but not exclusively offspring, and hence an effective "nepotist"...This theory of organismic, including human, nature can provide a heuristic metatheory for many areas of social science." (Daly 1982:406).

Other studies in which these interlinked assumptions are either implicit or explicit include Alexander (1979, 1988); Betzig et al (1988); Dunbar (1988); Chagnon (1982): Dickemann (1981); Foley (1985, 1987), Irons (1988), and Wilson (1971, 1975). Where culturally learned behaviour is discussed at all, it is treated no differently to aculturally learned behaviour, which is in turn approached as a kind of phenotypic plasticity, the expressions of which are

seen to be part of the organism. They are then explained, like any other organismic trait complex, via an appeal to genetic adaptation.

3.5 Problems with the Three Assumptions

Thus it can be seen that these three interlinked sociobiological assumptions are by no means limited to the Despotism case study. And, at first glance, the application of the classic Darwinian phrase "heritable traits which promote reproduction will spread" to the reproductive success of Despotism, or for that matter, any other cultural reproduced structure, seems fairly straight forward. Despotism (a suite of heritable human traits) will undergo differential reproductive success if it conveys reproductive success to their bearers. Despotic behaviour should be common in those societies where individuals increase their reproductive success by adopting it.

But introduction of the acultural/cultural distinction in learned behaviour poses some problems for this apparently simple application of Darwinian concepts to human behaviour. The fact of cultural transmission threatens Sociobiology by threatening the use of the phrase "heritable traits which promote reproduction will spread" in the explanatory framework. In a behavioural system where behaviour can be culturally transmitted the phrase becomes too general; a picture is given of a trait which possesses heritability and the capacity to promote reproduction, but it is not clear whether the trait is affecting its own reproduction, or the reproduction of some associated entity. The reverse is also true of the phrase; it is not clear whether the trait's heritability applies to itself or to the genealogy of the associated entities whose reproduction it is affecting. Consequently the phrase "heritable traits which promote reproduction will spread" requires additional qualification for the set of limiting conditions beyond which it does not apply. Such a qualification can be made simply by inserting the words "their own" to produce the more specific phrase: "heritable traits which promote *their own* reproduction will spread". The reproduction must be that of the heritable

traits, not of some other traits or entities. Heritable traits cannot spread through affecting the reproduction of some other organism or trait, unless in doing so they also enhance their own chances of reproduction.

Treating despotism as acultural learned behaviour, and therefore as simply the outcome of learning capacities and individual experience, leads Betzig to ignore the process through which despotism itself may be reproduced independently of the reproduction of a despot - cultural transmission. Cultural transmission, in turn, makes it possible for despotism to "escape" the lineage of a despot in a way no ordinary human trait can. To use a very simplistic metaphor, the sins of the fathers may not necessarily be visited upon the sons. This circumstance makes it possible for despotism to exploit the despot, or vice versa, such that patterns of adaptive selfishness become highly complicated and problematic. Darwin considered the problem of exploitation important enough to comment on it in the *Origin of Species*:

> "Natural selection cannot possibly produce any modification in any one species exclusively for the good of another species; though throughout nature one species incessantly takes advantage of, and profits by, the structure of another...If it could be proved that any part of the structure of any one species had been formed for the exclusive good of another species, it would annihilate my theory, for such could not have been produced by natural selection. Although many statements may be found in works on natural history to this effect, I cannot find even one which seems to me to be of any weight." (Darwin, C. 1859:229).

Darwin expounded this rule on a number of occasions (e.g. Darwin 1859:67, 163). Wallace is equally clear about the principle, for example when explaining how the apparent "altruism" of edible fruit production in plants is really a strategy for seed dispersal:

> "In the restriction of bright colour to those edible fruits the eating of which is beneficial to the plant, we see the undoubted result of natural selection; and this is the more evident when we find that the colour never appears till the fruit is ripe - that is, till the seeds within it are fully matured and in the best state for germination. Some brilliantly coloured fruits are poisonous, as in our bitter-sweet (Solanum dulcamara), cuckoo-pint (Arum) and the West Indian machineel. Many of these are, no doubt, eaten by animals to whom they are harmless; and it has been suggested that even if some are poisoned by them the plant is benefited, since it not only gets dispersed, but finds, in the decaying body of its victim, a rich manure heap."

(Wallace 1889:307).

Any reader who remains in doubt about whether adaptive selfishness is an essential philosophical element in any Darwinian or neo-Darwinian explanation, even in complex displays of altruistic behaviour, might consider any of the basic arguments of Dawkins with respect to this issue (Dawkins 1989 *passim*).

An entity cannot undergo differential reproduction purely on the grounds of promoting the reproduction of some other entity, unless conditions are thereby made more favourable for its own reproduction.

Unless the existence of cultural transmission is denied, we must recognise that human behaviour is reproduced by non-genetic means, and thus reproduced independently of human reproduction. Given that the reproduction of despotism and the reproduction of humans are two separate processes, an explanation of the reproductive success of despotism which relies purely on the observed effect of despotism on human reproduction must necessarily be violating Darwin's tenet. Betzig's scenario requires natural selection to produce a modification in one entity (a culturally reproduced behaviour pattern) exclusively for the good of another (a genetically reproduced animal), with no explanation of how despotism might indirectly propagate itself in the process.

The error is connected to the attribution of false equivalence to both forms of learned behaviour, because if despotism was simply individually learned behaviour and not culturally transmittable, there would be no danger of breaking the above Darwinian premise. In such a society human reproduction would be the only reproductive process, and despotism could

only become prevalent through the differential reproduction of human genes or gene complexes which predisposed human individuals toward such behaviour.

It must be stressed that this Darwinian principle does not deny the existence of animal or organism partnerships where only one party benefits; the phrase is not in conflict with commensal or parasitic ecological relationships. As Darwin states above "throughout nature one species incessantly takes advantage of, and profits by, the structure of another". The important difference is that the structure or structures from which one party derives benefit must be already formed in the host party, for some other reason or via some other process. In other words, for the host population, commensal relationships may be formed by preadaptation but not by adaptation, while adaptation would be the basic process by means of which the parasitic population would establish the relationship. The structure from which the parasitic population benefits, therefore, was not formed via natural selection exclusively for the good of another species, but for the good of the population in which the structure exists (ignoring, for the moment, alternative scenarios implied by genetic drift or epigenetic processes in the expanded synthesis).

In its approach to *cultural reproduced* phenomena, Sociobiology breaks this basic Darwinian principle, thereby rendering its claim to Darwinian identity untenable. More specifically Betzig has used an invalid and non-Darwinian interpretation of the phrase "heritable traits which promote reproduction will spread". She is explaining the selective viability of one entity (Despotism) in terms of its effect on the reproductive success of another (a human individual). A behaviour pattern is seen to be shaped and

propagated exclusively for the reproductive advantage of the animal which exhibits that behaviour, and not seen to be spreading according to its likelihood of being culturally reproduced. Cultural transmission is not denied, but it is nonetheless ignored; consequently cultural "traits" cannot be distinguished from genetically or environmentally emergent organismic states, and cannot therefore be given different treatment. Men and women are viewed by Betzig as not more than the "products of natural selection", implying a belief that the only order to be found in history or cultural diversity is that which is imposed by patterns in the human genome (see also Rindos 1986b):

> "Lord Acton once pointed out that "absolute power corrupts absolutely" (1948:364), surprising hardly anyone. The perhaps naive question asked here is, why? The answer investigated follows from Darwin. If men and women are products of natural selection, then the evolved end of their existence should be essentially, very simply, the production of children, grandchildren, and other nondescendent kinsmen." (Betzig 1986:2).

A little later, Betzig explains how this leads to extreme forms of behaviour in those with absolute power:

> "The important result, as Darwin pointed out, must be that winners leave the legacy of whatever heritable traits helped in that competition to their children. Among Darwin's (1859) own examples: high giraffes should have evolved long necks (p. 161); orchids should have evolved their spout-shaped lower lips in order to get pollinated (p. 142); cuckoos should have evolved to lay eggs in other birds' nests (p. 190).
>
> And men and women should have evolved to seek out positions of strength as a means to reproduction. Power, prestige, and privileged access to resources should be sought, not as ends in themselves, but as prerequisites to procreation." (Betzig 1986:2).

As suggested near the beginning of the chapter, cultural transmission allows a fundamental decoupling of the genealogies of cultural traits and acultural traits, with the obvious implication being that the reproductive success of a trait could be traced down the wrong genealogy. Humans, even in the most strict and orthodox adaptationist accounts, are not simply the products of natural selection, but also the products of cultural selection, two separate selective processes acting in concert. Betzig has simply dismissed cultural transmission as another body of environmental "noise" interacting

with a range of genetically emergent propensities, when in fact culture is another assemblage of systematically reproduced entities with their own genealogy, one that is quite distinct from the genealogy of the human genome. The ordered patterns which Betzig has encountered in human affairs are not simply the order imposed by the phylogenetic constraints of the human gene pool, but also the order imposed by the phylogenetic or historical constraints implicit in cultural traditions.

Thus sociobiology's "environmental" interferences need to be subclassified into cultural and noncultural categories, corresponding at a more specific level to the distinction between cultural and acultural learned behaviour mentioned above. Lack of awareness of the importance of these dichotomies in human behaviour, while seemingly a denial of common sense in most anthropological circles, is more understandable in an overly literal interpretation of Darwin such as Betzig's. Traditionally, Darwinian theory is considered applicable to organisms and parts of organisms, and its applicability to culture (unamended) demands the classification of cultural entities as one or the other; i.e. either whole organisms, or parts of organisms. A view which maintains human behaviour as simply the product of learning and experience, and therefore as the outcome of those parts of the organism associated with learning, can simply classify cultural entities as parts of organisms, and focus all discussion of evolutionary process on the gene pools associated with the organism. However, given that cultural transmission adds a new replicative process to evolution in human behaviour, such misplaced focus leads to the discussion of the structure of one class of entities purely in terms of the reproductive success of another class; anathema in a selective framework.

3.6 The Collapse into Lamarckism

In the quote from Darwin presented above a "complete annihilation" of his theory was predicted in the event of the finding of a structure in one

organism formed exclusively for the good of another organism, and that such could not have been formed through natural selection. Therefore, the explanation of cultural structures purely in terms of the good of the genetically emergent structures of organisms presumably implies that some processes *other* than selection are operating in culture itself. Correspondingly, the sociobiological explanatory framework, through its avoidance of discussion of cultural selection, implies that the processes of transformation by means of which the cultural system serves as "man's extrasomatic adaptation" are something other than selective.

To put this somewhat convoluted argument another way, a sociobiologist who intends to continue breaking this Darwinian principle, and to see cultural entities as formed exclusively for the good of human individuals, must find a mechanism other than natural selection for this process, or else deny the validity of Darwin's assertion that "such could not have been produced by natural selection". The latter alternative is not a real option, as it would require taking up the basically untenable position that reproductive success in one system could automatically propagate structures in another.

Thus, ironically, a sociobiologist must take the first option, and espouse a Lamarckian notion of cultural process (if that process is to be directed exclusively by human reproductive criteria) since neo-Darwinian process by its very nature cannot be exclusively directed by anything, let alone by criteria of reproduction in a separate system of inheritance. The cultural system cannot serve human adaptation without being a system of Lamarckian adaptation. Evolutionary processes which are externally directed are basically "Lamarckian" processes, and as a consequence some sociobiologists simultaneously advocate a Lamarckian approach to culture, and a neo-Darwinian approach to the effects of that Lamarckian system on human genetic evolution. Consequently, assertions of the selective or neo-Darwinian nature of human evolution, and the fundamentally Lamarckian or deterministic nature of cultural evolution, often occur in the literature as part of the same theoretical position.

This brings us to the fundamentally Lamarckian nature of the sociobiological conception of cultural change and cultural adaptation. While Sociobiology may legitimately see itself as attempting to introduce Darwinism into human affairs, the introduction was, apparently, never intended for human *cultural* affairs. The approach seeks to apply Darwinian principles to anthropology exclusively in the context of human genetic evolution, and the explanation of other parts of human society in terms of this process. This double standard for genetic and cultural process is widespread in many applications of biological theory to sociocultural anthropology, and in many speculative comparisons of biological and cultural evolution. The juxtaposition of Darwinian genetics with Lamarckian cultural systems occurs not only in cultural Sociobiology, as will be seen below, but also occurs when "regular" biologists briefly compare cultural process with the one which they know best. Spuhler, for example, has stated:

> "Clearly, the mode of inheritance for cultural behaviour is fundamentally different: by transmission of gametes in biology and by transmission of learned symbols in cultural behaviour. Biological inheritance is non-Lamarckian, cultural inheritance is Lamarckian, in the sense of the inheritance of acquired characteristics." (Spuhler, J. N. 1984:13).

Similarly, Steven Jay Gould has remarked:

> "Cultural evolution has progressed at rates that Darwinian evolution cannot begin to approach. Darwinian evolution continues in Homo sapiens but at rates so slow that it no longer has much impact on our history. Human cultural evolution, in contrast to our biological history, is Lamarckian in character. What we learn in one generation we transmit directly to the next by teaching and writing. Acquired characteristics are inherited in culture and technology. Lamarckian evolution is rapid and accumulative. It explains the cardinal difference between our past, purely biological mode of change, and our current, maddening acceleration toward something new and liberating - or toward the abyss." (Gould, S.J. 1980:71).

The reader might consider the above quotes to be mere speculation, but parallel assertions occur in the words of those who specialise in applying Darwinian theory to human affairs. Richard Alexander, for example, advocates a selective framework for human genetic evolution, while reserving a Lamarckian element in the process of human invention, as the generator of

variation in culture (mutation and invention are equivalent in function for Alexander, but not isomorphic in nature):

> "Immediately, differences are apparent between the processes of change in genetic and cultural evolution. Perhaps the most profoundly, causes of mutation and selection in cultural evolution are not independent. Most of the sources of cultural "mutations" are at least potentially related to the reasons for their survival or failure. A new kind of plow or computer is built for its expected usefulness: it is adopted if it works."
> (Alexander, R.D. 1979:74).

Another unambiguous example of the juxtaposition of the notion of the applicability of biological theory to cultural entities, with the notion that biological theory does not provide an analogy for cultural process itself, is that of Daly (1982:402): "Innovation (in culture) is not random in its effect like genetic mutation, but can be Lamarckian". This assertion is in the midst of a paper which takes up the view that biological theory is nonetheless applicable to culture due to the fact "that all evolved traits of all organisms serve a single goal: the replication of the focal individual's genes".

Boyd and Richerson are also careful not to exclude the possibility for Lamarckian evolution in the midst of the application of Darwinian principles to human society (although their theoretical approach has been classified as cultural selectionist for the purposes of the study, the category of theory given attention in Chapters 4 and 5). Reiterating similar earlier statements (1982:329), they write:

> "The cultural information acquired by an individual may be affected by the events of his or her life, and, if so, the changes will be transmitted to an individual's cultural offspring. This property of cultural transmission makes for a kind of "Lamarckian" evolution, in the sense that acquired variation is inherited." (Boyd and Richerson 1985:8).

Obviously not all of the above writers could be considered to be sociobiologists; my assertion is that in juxtaposing a Darwinian genetics with a Lamarckian cultural system they are *expressing an essentially sociobiological*

sentiment. The point is that Darwinian and Lamarckian processes are commonly seen as acting in concert in human affairs, moreover it would seem that the two positions are implicitly linked (ie. that a Lamarckian model of culture is logically emergent in any selectionist model of human evolution which ignores selective processes occurring outside the human genome). In such a framework, the order in culture can only be seen to be ordered from without. The explanatory framework implied must, almost by definition, conform to the typical external direction model of causality upon which (environmental) Lamarckism is based.

3.7 Concluding Remarks

The idea that Sociobiology as a whole has an essentially Lamarckian approach to cultural process may seem counter-intuitive to some readers. But it is certainly the case that cultural process cannot serve the adaptive imperatives of an independent inheritance system without somehow being "freed" from the Darwinian constraints of its own inheritance patterns. One cannot argue that systems of cultural inheritance exist only to promote organismic survival without also asserting that cultural process is basically non-Darwinian in its intrinsic logistics. To put it another way, in order to be able to exist for the sole purpose of serving the Darwinian requirements of other phenomena, a phenomenon must first be liberated from the Darwinian imperative of serving itself.

In any case, proving the Lamarckian nature of cultural Sociobiology beyond all doubt would presumably require a more comprehensive analysis of the sociobiological literature than that attempted in this chapter. Yet this extreme position has been employed only in so far as to show that an allegiance to neo-Darwinian theory is no guarantee of a selectionist theory of cultural process (although it is for genetic process), and indeed it appears that for many it implies just the opposite. Certainly, many modern Darwinians explicitly reject any notion of the wholesale applicability of their theories to

cultural fields of enquiry, and, as has been shown, some explicitly advocate Lamarckism for these fields of enquiry.

What has been demonstrated is that the tendency of sociobiologists to explain cultural adaptation in terms of human adaptation violates one of the most important principles of neo-Darwinian theory, that of "adaptive selfishness". Despite the fact that artefacts, ideas, and socially acquired behaviour patterns undoubtably display genealogical independence, an explanatory framework is used in which such cultural phenomena are exploited comprehensively by human individuals. Cultural structures are formed exclusively for the benefit of humans, and their fitness is thus measured not in terms of their own likelihood of cultural reproduction, but in terms of human reproduction. Culture is literally viewed as "man's (and woman's) means of extrasomatic adaptation".

Had Sociobiology succeeded in fulfilling this agenda (in finding even one example of a culturally reproduced structure which existed solely for the purpose of facillitating human reproduction, without also facillitating its own reproduction) they would have simultaneously refuted their own approach, and any possibility of applying selective principles to cultural phenomena. They would have inadvertently pulled off an ironic oedipal conquest, and "killed off" the great father of their own approach. In other words, Sociobiology's self proclaimed agenda (for culture) is a program of self annihilation (in cultural contexts).

Sociobiology provides a remarkable selective framework for disentangling the convoluted interaction of different human reproductive strategies within a society, and their ultimate effect on human genetic process. But the approach does not represent a neo-Darwinian approach to cultural process itself, as this is precluded by their focus upon the survival and reproduction of individual organisms, and their relative disregard for the implications of social transmission and genealogical independence. By virtue of this curious double bind, we must therefore reject cultural Sociobiology as a candidate for the "untried theory", and once again turn our attention elsewhere. Could it

be that a third evolutionary approach, Cultural Selectionism, *does* provide a selectionist framework for culture, and could it therefore qualify to fill the gap of the "untried theory"? This is the basic question addressed in the next two chapters.

4

CULTURAL SELECTIONISM

Overview

In the preceding chapters I have argued that an implicit Lamarckian orthodoxy pervades explanations of cultural process. However, it appears that this orthodoxy is now under threat, due to the rapid development of a new perspective known as Cultural Selectionism. This chapter provides a general overview of Cultural Selectionism and a concise definition of the essential differences between this perspective and other approaches with which it has the potential to be confused; approaches such as the cultural Sociobiology of the last chapter, and Diffusionism.

4.1 A Brief Definition of Cultural Selectionism

"Cultural Selectionism" may be defined as a neo-Darwinian approach to human behaviour which focuses on the "survival and reproduction of cultures", in direct contrast with the sociobiological focus on the "survival and reproduction of organisms". In other words, a true cultural selectionist is at least as interested in tracing the effects of a particular culture upon its own reproduction, as in tracing the effect that a culture might have upon the reproduction of the organisms which maintain it. Cultural variation is seen to be a product not of genetic selection, but of an intrinsic or purely cultural selective process. It is this basic element - the extension of selective principles into purely non-genetic contexts - which identifies the cultural selectionist, and differentiates his or her position from the sociobiologist.

So not all applications of neo-Darwinian theory to human affairs are dependent upon sociobiological premises of genetic determinism, or even, for

that matter, genetic constraint. Human genetic process is not the only process in human affairs amenable to selectionist explanatory frameworks. Such process models also have immense heuristic value in the interpretation and explanation of the purely cultural processes normally associated with the word "History". As a result, there is no need to appeal to human genetics in order to apply neo-Darwinism to human affairs.

It was Sociobiology's failure to realise this tenet which caused them to view cultural process as a servant of human genetic process - "man's extrasomatic adaptation" - and thereby break the fundamental neo-Darwinian principle of reproductive selfishness (the reader is reminded here that the neo-Darwinian use of the word "selfish" denotes "that which increases the likelihood of self-reproduction" or reproduction of other "selves" such as kin, not the more personified "conscious self-aggrandizement"). History was reduced to natural history rather than explained via an appeal to the intrinsic processes of history itself. Cultural Selectionism, a different approach developed in reaction to Sociobiology, attempts to escape this problem by viewing historical process as another selective process in human affairs, operating separately from and in addition to genetic process.

However, it is worth noting that not all anthropologists and archaeologists examined in this chapter identify consciously with the label "Cultural Selectionism". Some, in fact, such as Leonard and Jones (1987:211) even prefer not to use the term "Cultural Selectionism" at all. Yet there is a definite similarity in the explanatory frameworks dealt with in this chapter. My use of the term basically encompasses any statements to the effect that selective concepts can be applied to extra-genetic and particularly cultural processes. The essential element in the work which I have classed as "cultural selectionist" is an interest in transposing principles of the neo-Darwinian synthesis specifically into the context of cultural transmission and evolution. Unlike cultural Sociobiology, the theoretical approaches of this chapter are not trying to explain cultural variation and invariation in terms of neo-Darwinian models of human genes, individuals, and genomes, but in terms of cultural equivalents of these entities. The identifying characteristic of Cultural

Selectionism is thus an attempt to explain cultural change with models which allow invention and cultural transmission to operate separately from each other, but nonetheless to explicitly incorporate both processes into a selectionist explanatory framework.

When we expand the brief definition of Cultural Selectionism above, the distinctive philosophical identity of the approach becomes more and more apparent. The cultural selectionist can be distinguished from the sociobiologist by the fact that he or she sees human evolution as the result of the interaction of two logistically independent neo-Darwinian processes (one in human culture, and one in human genetics), rather than one general (and ultimately genetic) neo-Darwinian process. This is why they may be reliably identified by the notion that neo-Darwinian process can operate in substrates other than DNA.

Leonard and Jones (1987) express the position by coining the phrase "replicative success", which allows them to avoid the genetic connotations they feel are inevitably associated with the phrase "reproductive success":

> "Replicative success is a pragmatic construct. It implies neither mode of trait transmission, as reproductive success does, nor that some degree of selective advantage is conferred as a result of the transmission of a trait. Success is gauged as changes in the representation of traits through time." (Leonard and Jones 1987:214).

Many other cultural selectionists make parallel assertions, all with the goal of communicating the fact the Darwinian processual framework is not tied to any particular field of enquiry, providing reproduction of some sort occurs in that field of enquiry. Rindos puts the position as follows:

> "...that the mode of transmission of cultural characteristics ("the inheritance of acquired traits") is irrelevant to the applicability of a selectionist model for evolutionary change...Darwinian selectionism requires undirected, heritable variation...The symbolic aspects of culture are not in any sense analogous to the genotype but rather functionally equivalent; because the capacity for symbolling is not directly adaptive, it provides the undirected variation required for a theory of selection-mediated cultural evolution." (Rindos 1985:65).

Boyd and Richerson put it like this:

> "there is no necessary logical connection between Darwin's idea of evolution by natural selection and the particular features of genetic inheritance. There are two necessary conditions in order for natural selection to operate: (1) There must be heritable variation in the phenotype. (2) Phenotypic variation must be associated with variation in individual survival and reproduction. (Boyd and Richerson 1985:173).

Most recently, Durham has presented this background assumption of Cultural Selectionism very clearly:

> "The word "evolution" has meant many things to many people (e.g. see reviews in 27, 28, 42, 44, 129, 155, 188, 205). Swayed by Darwin's simple yet simple rendering, "descent with modification" (63), I consider evolution to have occurred in any set X whose elements (X1,X2,X3,etc) are all related by descent - that is, by hereditary derivation from a common ancestor. Using this definition, together with a suitable conception of heredity, one can argue that many things evolve: for example, designs (191), technologies (57), texts (34,156), languages (90,91,197), knowledge (35,37; but see debates in 97,163), ideas and theories (111, 158, 201), whole disciplines (80, 202), even Darwinism itself." (Durham 1990:188).

Bateson, for final example, has also succinctly articulated the general applicability of models of selection to both genetic and learning processes, and by implication from the latter, cultural process. Bateson prefers to use the word "stochastic" to describe the dialogue between random variation from the status quo ante, and differential persistence, which occurs in selective processes:

> "...both genetic change and the process called learning (including the somatic changes induced by habit and environment) are stochastic processes. In each case there is, I believe, a stream of events that is random in certain aspects and in each case there is a nonrandom selective process which causes certain of the random components to "survive" longer than others. Without the random, there can be no new thing." (Bateson, G. 1980:160).

There are other similar assertions (see Braun 1990:63; Dunnell 1980:53; Chen, Cavalli-Sforza and Feldman 1982:366). Each position expresses that the fundamental premise upon which Cultural Selectionism applies neo-Darwinian theory to culture is one of analogy. This is in stark contrast to Sociobiology (as pointed out throughout the last chapter) which is based on the explanation of human behaviour in terms of genetic process itself. To my knowledge, this fundamental distinction is generally accepted by all anthropologists who have

carried out research into neo-Darwinian theory, and the distinction is neatly articulated by Blute (1979:57-58), Dunnell (1980:52), Durham (1990:193) and Rindos (1986b:315).

However, while in the last chapter the Sociobiology versus Cultural Selectionism distinction was used to emphasize that there were certain differences between the two perspectives which lead Sociobiology to violate the neo-Darwinian rule of adaptive selfishness, I have used the distinction now to characterise the distinctive theoretical identity of Cultural Selectionism, and of the structural differences of this body of theory to Sociobiology.

Thus the most basic premise of Cultural Selectionism is an extension of neo-Darwinian theory into non-genetic contexts, particularly that of cultural change. Sociobiology, on the other hand, maintains an ultimately organismic context for its neo-Darwinian principles - other processes are not seen to be independent neo-Darwinian processes in their own right, but rather as non-Darwinian influences on the underlying neo-Darwinian processes which take place in organismic populations.

4.2 A Note on Regional Differentiation Within Cultural Selectionism

Although it has not been customary to make use of regional distinctions in the discussion of archaeological and anthropological theory, some use of them will be made in this part of the study. This is a *selectionist* view of the origin of Cultural Selectionism; it therefore takes the view that independent inventions rarely duplicate each other, and that degrees of regional isolation produce corresponding degrees of regional differentiation (in proportion to the degree to which that isolation impedes the movement of ideas from one population to the other). As a result of these sorts of processes, American Cultural Selectionism (e.g. Boyd and Richerson 1985) is different from those positions which originated in British academia (e.g. Clarke 1968). The American form arose principally as a critique within American sociobiology which

gradually came to terms with human behaviour and cultural transmission as it developed. Its "human evolution" heritage is most evident in the fact that although cultures are one focus of attention, individual organisms and populations thereof are still the basic units of analysis.

Conversely, it is unlikely to be mere coincidence that those cultural selectionists who emphasize the artefact or assemblage, and not the organism, as the unit of analysis, are either British or were educated in Australia by British academics (e.g. Fletcher 1977, Clegg 1978a, Cullen 1990). The more artefact/cultural entity orientated forms of Cultural Selectionism, and its Australian derivatives, arose principally within Cantabrigean Archaeology (or within a discourse dominated by Cambridge educated archaeologists), and initially has more kinship with positions like that of Clarke (1968) than it does with the American position.

As will become clear, the different versions of Cultural Selectionism which exist bear the signatures of their different heritage, independent origins, and subsequent regional and behavioural isolation; in fact, just the sort of differences one would expect if cultural process were basically selective. Although there is a substantial flow of ideas between the different populations of cultural selectionist concepts, the flow between is not as great as the flow within, and so it is more easy for an idea to become fixed in *either* the American or British concept population, than it is for it to become fixed in *both*, as they are essentially allopatric (ie. geographically separated). The same situation now exists in microcosm for cultural differentiation to occur between the English and Australian cultural selectionist concept populations, although the Australian offshoot of the Cantabrigean tradition is more recent. The isolation involved in these regional separations is not total, of course; it is rather a problem of the fact that the difficulty of communication increases with distance, given the increasing cost of mail, telephone, and travel services. These difficulties have the ultimate effect of decreasing the amount of interaction which occurs between academic communities which are a long way apart.

It is worth noting that there is a fourth "cultural selectionist" position, also with a quite independent heritage, and that is the "meme" concept of Dawkins (1976, 1982, 1989). Its origin is essentially British, and more particularly Oxonian, ethology. This position will not be analyzed in this or the next chapter (although other, explicitly genetic Dawkins material will be), which consists of, for the main part, a critique of the American position from the point of view of the Cultural Virus position. Memetic approaches are given more attention in Chapters 9 and 10, and are only briefly listed here as part of a general overview of Cultural Selectionism. As Maschner and Mithen have remarked (1993), the number of new Darwinian approaches to culture is bewildering. In recent decades it would seem that they have undergone an "adaptive radiation" all of their own.

4.3 An General Overview of Cultural Selectionist Literature

One of the earliest steps toward a cultural selectionist perspective, and one which was given some attention by David Rindos (1984), was that of Campbell (1965). Other more recent examples are numerous, although they by no means represent the majority of Cultural Anthropology (they have not, as yet, overturned the Lamarckian orthodoxy). Rindos, in a later article (1986b), lists six works which profess a cultural selectionist position - Boyd and Richerson (1985a), Cavalli-Sforza and Feldman (1981), Dawkins (1982), Durham (1978), along with Rindos (1984 and 1985). Others can also be classified in this theoretical category to varying extents, including Ball (1984); Bateson (1958, 1973, 1980); Blute (1979); Braun (1990); Chen, Cavalli-Sforza and Feldman (1982); Clegg (1978a, 1979, 1985); Cullen (1986, 1990, 1991, 1992a, 1992b, 1993a, 1993b); Corning (1987); Csanyi (1989); Delius (1989, 1991); Dunnell (1978, 1980); Fletcher (1977, 1988, 1989, 1992); Freeman (1983); Heylighen (1992a, 1992b); Leonard and Jones (1987); Lumsden and Wilson (1981); Mithen (1989); Neff (1992); O'Brien and Holland (1990, 1992), Pulliam (1982, 1987); and Sperber (1985).

There also a number of other early "cultural selectionist" theoretical statements which are referenced in introductory literature reviews of other cultural selectionists, but often in only one such review, and their existence is not therefore as generally known as it might be. One might conclude from this that the full heritage of the cultural selectionist position has yet to be fully articulated. Dunnell's 1980 list, (1980:52), for example, includes a variety of quite early works which are not extensively referenced in modern "cultural selectionist discourse", including Blum (1963); Bray (1973); Durham (1976); Gerard, Kluckhohn and Rapoport (1956); and Lenski (1970). There are also additional publications by the authors Rindos mentioned in his early analyses, such as his own (Rindos 1986a and 1989); Dawkins (1976); Durham (1982, 1990, 1991); Richerson and Boyd (1978, 1985); and Boyd and Richerson (1982). Moreover, some anthropologists have used one of the above formulations as the basic theoretical framework for research in the field - Flannery, Reynolds, and Marcus, for instance, in a study of Llama herders in the Andes of central Peru (1989:205-211), employed Boyd and Richerson's dual inheritance theory as a basic explanatory framework.

Shennan (1989, 1991) has also given the work of Boyd and Richerson, and to a lesser extent Cavalli-Sforza and Feldman, considerable attention and analysis, thereby contributing to the growing body of cultural selectionist literature. He has also brought the work of two other cultural selectionists, Elster (1983, 1985), and Harre (1979), to the attention of archaeologists. While Elster has discussed the applicability of cultural selectionist principles to technical change, Harre has demonstrated their relevance to social theory (Shennan 1991:342). Moreover, Shennan identifies a number of points of consensus between the work of these theorists and other, less explicitly selectionist concepts, such as those of Giddens (1979, 1984), and the ideas of Bourdieu (1977) which were used by Hodder (1986) (Shennan 1991:333). By demonstrating the selectionist concern with a variety of established problems within social theory and Postprocessualist archaeology, Shennan has not only added to our understanding of the wide spread nature of the neo-Darwinian resurgence, but also the widespread nature of problem areas which selectionism is uniquely equipped to resolve. It is also worth noting that the

"cognitive-processual" position advocated by Renfrew and Bahn (1991:431), while lacking any explicit identification with the neo-Darwinian tradition, is at least broadly consistent with the modern cultural selectionist position.

In addition to the above, there are also certain earlier works, such as those of Bartlett (1923), Barnett (1953), and Clarke (1968) which treat invention and cultural transmission separately, without an explicit identification with a neo-Darwinian or selectionist tradition. Dawkins (1989:190) has classified two additional early works (Cullen 1972, and Popper 1974) as being thematically cultural selectionist. Going back even further than Bartlett's work of the 1920s, one can find an explicitly selectionist model of human perception (there is no explicit reference to culture) in the work of the psychologist William James (1896). In fact, one can go on adding to this list indefinitely, listing theoretical perspectives which are gradually more and more distant from the distinctive selectionist position on cultural process. Darwinian influence has been quantitative rather than qualitative in the sense that its influence gradually increases as one approaches the present. The rise of Darwinism into cultural fields of enquiry has not been an all or nothing process.

It is worth cautioning the reader that many additional works which might seem to be examples of Cultural Selectionism (due to their advocacy of a "neo-Darwinian approach to culture") are often, in fact, part of the adaptationist sociobiological tradition dealt with in the last chapter. While some of the above approaches do indeed achieve a neo-Darwinian or selectionist perspective for culture, not all have made the decisive step beyond simple human adaptationism to a cultural equivalent of the expanded evolutionary synthesis of modern biology. Often a mosaic of both cultural selectionist and sociobiological concepts and categories are used, and many approaches reserve some role for Lamarckian (and thereby non-Darwinian) processes in History. They have all fallen under my broad definition, however, due to the *presence* of selectionist principles, rather than due the *absence* of anything non-Darwinian.

In addition to the actual attempts to formulate a selectionist model of cultural process, there are other kinds of evidence which suggest a growing interest in selectionism within Anthropology (meant in the widest sense of the term). For example, a number of anthropologists have commented very favourably on some of the non-sociobiological selectionist formulations mentioned above, although not submitting (cultural) selectionist perspectives of their own. These may also be considered selectionist in orientation and are evidence of the widening support for the approach. Nickerson (1985), for example, has this to say about Rindos:

> "In sum, I applaud Rindos for his efforts at applying a Darwinian model to cultural evolution, one in which cultural types and progress are not essential ingredients. His paper, I believe, has significant potential for contributing to our understanding of how cultures change/evolve as well as bringing us closer to the meaning that evolution has in other scientific disciplines." (Nickerson 1985:83).

Gullick (1985) also views Cultural Selectionism in a very favourable light, in the process of commenting on the Rindos (1985) paper:

> "I welcome Rindos's paper, with its defence of evolutionism in present-day anthropology. I am always glad when someone defends my position, even if it is from a very different angle! We are, however, both attempting to point out the better aspects of evolutionism and reject its 19th-century aberrations and are both considering the biological model rather than the Marxist and Spencerian interpretations of it." (Gullick 1985:81).

Others amongst Rindos's commentators who have expressed some form of optimism for a selectionist approach to culture include Carneiro (1985); Cooper (1985); Dunnell (1985) (who is already listed above); and Ellen (1985).

Lastly, it might also be pointed out that there are general advocates of a Darwinian approach to human affairs who do not specifically formulate the theory in a cultural selectionist framework. To a certain extent, all of the sociobiological approaches of the last chapter would be members of this category, but there are also a variety of approaches to culture which are difficult to classify as either sociobiological or cultural selectionist, yet which are explicitly optimistic about selectionist explanatory frameworks. These hint that there may be a further area of sympathy about selectionist perspectives, made up of literature which is not so specifically cultural selectionist as those

immediately above, nor so specifically sociobiological as those approaches of the last chapter. Binford, for example, is confident about the application of Darwinism to human systems:

> "It is my opinion that we need to give much more serious thought to Darwinian arguments, where the driving forces of change lie in the interaction between the environment and the adaptive system being considered. Given such a view, the system of adaptation may enjoy relatively stable periods of varying duration, representing times when it is able to cope successfully with the perturbations of the environment. Selection for change occurs when the system is unable to continue previously successful tactics in the face of changed conditions in its environment. The source of such changes may be accumulated effects of the system's history, but such effects are build-ups of changes in ecological relationships rather than the continuous operation either of some inner vital principles or of unrelenting external pressures."
>
> (Binford, L.R. 1983:203).

Similarly, Mithen (1989:483) expresses a general optimism over the use of evolutionary theory in Archaeology, although its use is seen to ensure that the "biological constitution" of humanity is not ignored. Thus Mithen, like Binford, is not advocating an explicitly cultural selectionist position. Rather, he argues that the use of evolutionary theory ensures that the adaptive significance of the human cultural capacity is recognised. However, Mithen is careful to dismiss any implication of sociobiological genetic determinism; it is the adaptive significance of the *capacity* for culture which must not be dismissed:

> "By recognizing that the capacity for culture is a product of natural selection one is accepting that it conferred some reproductive advantage on its bearers to have evolved. That is, thought and action were channelled by natural selection in directions that were adaptive in the evolutionary environment of *Homo sapiens sapiens* (Durham 1979:43). As a result we have today, and have had through prehistory, psychological propensities to think and act in certain ways rather than others. Such propensities concern "sensory-perceptual functioning, responsiveness to stimuli, motor patterns, motivational and cognitive processes and constraints on learning" (Hinde 1987:67). In certain circumstances these may become manifest as conscious desires to survive and reproduce (Durham 1976:98)."
>
> (Mithen 1989:484).

While the less explicitly cultural selectionist advocates of the use of selectionist theory (see Handwerker 1989 for another example) in Archaeology and Anthropology have not been added to the list of cultural selectionists in this chapter, it is interesting to note that, in modern theoretical discourse, such theorists often carefully avoid the sociobiological position. In other words, even where human genetic process is the focus of attention (as it must often and legitimately be), biologically oriented anthropologists often carefully limit their statements to the origin and maintenance of the human capacity for culture. In doing so, Sociobiology is carefully avoided, and a cultural selectionist approach to cultural specifics is, by implication, left open as a possibility.

Overall, then, despite the problem of the "untried theory", selectionist perspectives for cultural process are certainly present in anthropological research, and a certain amount of optimism for their development has been expressed. Some of these perspectives have already established a separate identity from Sociobiology, and they are growing in number and sophistication. Selectionist explanatory frameworks for cultural process are not, however, as generally accepted nor as clearly understood within cultural fields of enquiry, as their equivalents in modern evolutionary Biology.

So it can be seen that the potential of selectionism for purely cultural processes is at last being explored, with growing sophistication and enthusiasm. In the next chapter a variety of such approaches, loosely referred to as "Cultural Selectionism" by Rindos (1985:65), are given attention. However it will become clear that certain areas of this body of literature betray its sociobiological heritage. Cultural Selectionism employs cultural redefinitions of all the major neo-Darwinian concepts, but each redefined concept corresponds too closely to the biological original. The units of cultural change are still defined in relation to aspects of human societies which are more pertinent to human survival than cultural survival. Basic neo-Darwinian categories, such as "individual", "phenotype", "population", and "society", have not been redefined in relation to units of cultural reproduction, but have simply become

those cultural entities which most closely correspond to units of organismic reproduction.

But a human individual's culture is not necessarily an individual cultural phenomenon; a human population's culture is not necessarily a population of cultural phenomena; a human society's culture is not necessarily a "society" of cultural phenomena; and the enculturated components of a human phenotype do not necessarily constitute the "phenotype" of a single cultural phenomenon. Yet this is precisely how each of these concepts has been redefined in the American cultural selectionist framework. However, as stated above, a more in depth analysis of the "organismic" bias of this cultural selectionist sub-tradition will not be undertaken until the next chapter. The role of this chapter is merely to provide a general outline of the full extent of the growing cultural selectionist movement, and to distinguish its theoretical identity from its nearest philosophical relatives. The approach has so far been contrasted with sociobiology; it remains to contrast its basic premises with those of Diffusionism.

4.4 Distinguishing Selectionism from Diffusionism

There is a certain superficial resemblance between neo-Darwinian selectionist perspectives and the diffusionist theories of the first half of the twentieth century, since both schools treat invention (mutation, innovation), and spatial or temporal expansion via adoption (differential reproduction, selection) as separate, independently operating processes in culture. It is this similarity between diffusion and Selectionism (if properly worked out in selectionist terms, diffusion can simply be a special case of cultural adaptation) which has lead Shennan (1989) to suggest that current renewed interest in the social processes associated with diffusion and the renewed interest in selectionist models of historical process are centred upon similar aspects of cultural fields of enquiry (Shennan 1989:331). Shennan suggests that the renewed interest in the process of emulation can be, and for that

matter would benefit if it were, incorporated into a broader framework of cultural transmission and evolution, using Boyd and Richerson's framework as an example.

Both Diffusionism and Selectionism use two stage models of transformation (two basic kinds of processual event, involving first an invention or mutation, followed by propagation), in contrast with the one stage models which prevailed in pre-Darwinian philosophy. The "double barrelled" complexity of selectionist perspectives is one of their distinguishing elements:

> "Without a method for the non-directional generation of variation and, indeed, some evidence that observable variation is in fact undirected, Darwinian selectionism is far less elegant and parsimonious than Lamarckian evolutionism. Why should we theorize that evolution is a two stage process when a simple one stage model for the generation of directed variation will produce the same results, at least in so far as adaptation and directional change are concerned?" (Rindos, D. 1986b:70).

A similar discussion of the two stage complexity of selective process may be found in the work of the geneticist Wright (1967:117). The irony of this situation is that the similarity makes Diffusionism more like Selectionism than the Cultural Evolutionism of Chapter 2. Because of this, and very ironically, not only were anthropological translations of selective process *mis*translations (in the form of Social Darwinism and Cultural Evolutionism), but Diffusionism, the approach developed by anthropologists as a critique of Cultural Evolutionism, (Hugill and Dickson 1989:xii) can be considered a far more similar (although still pre-Darwinian in its basic conformation) explanatory framework. To be fair, however, the second misconception springs out of the first. Once the strange phenomenon of a "Social Darwinism" which corresponds more to Lamarckism than neo-Darwinism has emerged, it is understandable that a reaction to that position might deliberately *or even inadvertently* lead back toward Selectionism, while simultaneously believing itself to be "inventing" a completely new position.

Like Cultural Selectionism, Diffusionism developed as part of a reaction to pre-Darwinian Cultural Evolutionism. But important differences between Cultural Selectionism and Diffusionism make it nonsensical to view the latter

as an early variety of the former, purely on the basis of its two stage structure. However, Shennan (1989:331) is correct when he states that Cultural Selectionism must ultimately present some account of the process of diffusion given that the process itself is clearly a special case of cultural transmission. I only wish to remind the reader that such an account (Shennan uses the term "neo-diffusionist) would look quite different from early twentieth century Diffusionism.

It is not my intention to provide more than a brief summary of the "two stage" similarity of the approaches. However, it is important to distinguish the apparently similar language and expression of (early twentieth century) diffusion and Cultural Selectionism, and thereby dismiss the superficial similarities in terminology. If this is not done, it is very easy for a reader to attribute (consciously or otherwise), the well known conceptual problems of Diffusionism to those aspects of cultural selectionist language which bear a false resemblance to the Kulturkries school. My objective for the moment, therefore, before examining some cultural selectionist perspectives in detail, will be to briefly emphasise the differences between the two schools in their areas of greatest apparent similarity. This preliminary contrast is also necessary because many of the familiar terms of the neo-Darwinian synthesis are used in diffusionist explanations of cultural patterning in non-equivalent contexts, and these terms have been discredited within Cultural Anthropology and Archaeology in the process. This is a parallel situation to that suggested in Chapter 2, where other familiar terms from neo-Darwinian perspectives had been misappropriated in the language of Cultural Evolutionism and "Social Darwinism", and similarly discredited. While Cultural Evolutionism misappropriates concepts such as adaptation, progression, survival and species, diffusionist perspectives misappropriate concepts such as traits, homology or actual historical connection, and analogy or functional convergence.

Yet the most basic difference is a general one; while Diffusionism was essentially a descriptive school of thought (in spite of its explanatory aspirations), Cultural Selectionism is genuinely explanatory in that it seeks to

model mechanisms and processes of cultural evolution rather than the specific course of history. Other differences can be identified in the analysis of the diffusionist school provided by Hugill and Dickson's *The Transfer and Transformation of Ideas and Material Culture* (1989).

A more specific difference between Diffusionism and Selectionism involves the source of invention and new variation in the two theories. As mentioned above both have separate mechanisms for the origin of a new variant and its subsequent persistence. In both approaches there are one or very few places of origin for an innovation or mutation, from which it may or may not subsequently spread. However, while each original invention in a (expanded synthesis) cultural selectionist framework is a random event which can occur in any community in the world (although not with equal likelihood), and anywhere in that community, in Diffusionism all original inventions occur in one of a small number of "original cultures" or Urkulteren. While random inventions can occur anywhere, the location of inventions in the diffusionist paradigm all occurred in a small number of "superior" cultures, and then "diffused" down the cultural gradient to "less enlightened" cultures, in a series of concentric circles:

> "(1) the Urkulteren were few in number, (2) the historical configurations of human culture in the world resulted from the diffusion of single traits together with the migration of peoples bearing whole complexes of traits from these Urkulteren centres, and (3) diffusion and migration tended to radiate outward from the centres in roughly concentric circles or kreise and could thus be traced back to them by means of the careful description and comparison of culture traits." (Hugill and Dickson 1989:xii).

This situation implies some special propensity for invention in a tiny sample of the world's cultures, a position which would be untenable in an cultural selectionist perspective, which relies on the random and unpredictable generation of new varieties in the inventive process. Cultural equivalents of the concept of mutation would therefore necessarily take up the position that invention was a ubiquitous process occurring in all cultures, while at the same time arguing that very few inventions were absolutely homologous or identical, due to the tendency of random events not to duplicate each other. Cultural selectionist notions of invention and the origin of variation will be covered in

greater detail below; at this point the reader need only keep in mind the nature of the fundamental difference outlined above.

Another major element distinguishing Diffusionism from Cultural Selectionism is the simplistic use of structural similarity as evidence for common origin. An expanded selectionist perspective would rely on a similar notion, but in conjunction with awareness of certain limitations. The heritage constraint of common origin (see Chapter 6) is an important aspect of neo-Darwinian process, but it is not the sole cause of similarity. Hugill and Dickson unambiguously point out the irrefutability of the diffusionist linkage between form and origin as follows:

> "Despite the systematic nature of their method, the Urkulteren "reconstructed" by members of this school were marred by their excessive reliance on formal similarity between culture traits as proof of historical connection. This lead them to the fallacious premise that distance or distributional discontinuity between occurrences of a trait or trait complex were essentially irrelevant to the reconstructions of the historical patterns of diffusion." (Hugill and Dickson 1989:xii).

Mutation in the expanded selectionist perspective is not "perfectly" random in the sense of equally likely in all directions, but historically constrained (Foley 1985:225, after Dawkins 1982; and Rindos 1985:70 after Gould 1980. See also Fletcher's (1977) discussion of coherent and incoherent change). The range of mutations produced by a population of marsupials in Australia, for example (see Chapter 6, section 6.2 for a brief discussion of this point), would be very different to those produced by a population of antelopes in Africa, despite the fact that populations of both can be found in very similar "Savannah" environments. This is because the status quo is different in each case, and as mutations are derivatives of the status quo they generally bear some fundamental resemblance to it. Thus we can find very different dominant herbivores - kangaroos versus gazelles - in broadly similar environments on different continents. Mutation is, however, random in the sense that it is impossible to predict which of a vast array of possible mutations will appear at any point in time from a given historically or phylogenetically constrained state. Such constraints impose a certain amount of concurrence between form and origin via the mechanism of "phylogenetic inertia".

4.5 An Answer to Daly's Charge of Non-Equivalence

Martin Daly (1982) has published one of the strongest pieces of "devil's advocacy" against Cultural Selectionism, and it is interesting to observe that cultural selectionists have, on the whole, ignored rather than addressed the elegantly structured arguments he has advanced. Some readers will, perhaps, dismiss Daly as a naive sociobiologist; yet Daly has in fact pinpointed the weakest premise of Cultural Selectionism, and he has thereby brought to our attention the important desiderata for future development of the approach.

Daly has argued that the conceptual approach used by Boyd and Richerson (1982); Chen, Cavalli-Sforza and Feldman (1982); Dawkins (1976); Lumsden and Wilson (1981); and Pulliam (1982), is based on a weak and problematic analogy between cultural information and population genetics. These two mediums of transmission have certain fundamental differences which lead Daly to the conclusion that the analogy is without practical or heuristic value. If the analogy between the two mediums is valid, and by inference the cultural selectionist perspective also, then it should be possible to offer a coherent refutation for each of Daly's caveats. There are three basic arguments in the Daly point of view, all focusing on basic differences between cultural and genetic transmission. He adds these differences to two others laid out by Boyd and Richerson, Lamarckian innovation and biased transmission, which he also accepts (see Chapter 5). However, due to the general space constraints of this study, I intend concentrate on what I consider to be the central point of his challenge, the assertion that there are no discrete particles of cultural inheritance. The argument is based on a false conception of both neo-Darwinian genetics and the logistics of cultural transmission. Put simply, Daly both over-estimates the amount of discrete inheritance in genetics, and under-estimates the amount of discrete inheritance in cultural transmission.

The basis of Daly's challenge is that there are no segregating particles underlying any given cultural entity. As Daly writes:

> "Memes and culturgens are clearly explicit analogs of the gene; Cavalli-Sforza and Feldman (1981) appear sometimes to follow a similar model (e.g., pp. 70-71). Yet, surely it is more appropriate that the traits in the classes of models correspond; they really seem to refer to the same attributes. Durham recognizes this in referring to genetic and cultural "instructions" as codeterminants of traits. What, then, is to be the gene's analog? What transmission "particle" can we envisage, distinct from the traits, with "polygenic" (polymemic?) effects on a variety of traits? The model of Mendelian genetics remained successful because there really was such a particle. The cultural model has none." (Daly 1982: 402).

While Daly makes a convincing case for the lack of segregating particles in culture, to suppose that this poses any threat for analogies between cultural and genetic evolution is based on a specious premise. Daly's mistake is not that culture is not perfectly segregated, but that genetics is. While it is generally accepted that the Mendelian concept of segregation was indeed important in the discovery of genes, cases of pure segregation are now recognised to be quite rare in most species. Daly is in fact indicating that he upholds the now obsolete "classical" model of genetics (Rindos 1986b:324, after Lewontin 1974), where segregation is the rule, and not the modern, more systemic "balance" genetics, where the form in which a gene is expressed is dependent upon a range of contingencies in both the environment of the gene and the genotype of which it is part (for a very lucid example, see also the Dawkins metaphor of individual rowers rowing in different teams 1989;38-9; 84-6; 271-2).

Under the "classical" model of genetics (Rindos 1986b:324), which Rindos has shown to underly the Darwinian explanatory frameworks of structuralism and sociobiology, along with cultural selectionists such as Cavalli-Sforza and Feldman (1981) and Boyd and Richerson (1985), the genetic background for the human capacity for culture is particulate, with individuals being genetically predisposed to realising different cultural states through the possession of different combinations of the particulate capacities. As Rindos writes:

> "(in the work of these authors)...cultural performance is the result of the fixation of specific genes for specific traits. Here,

> language acquisition is governed by a process that not only accommodates learning but also dictates the deep structure common to all human languages. General cultural differentiation over time and space must therefore be the result of point substitutions in genes governing traits such as those controlling the ability to learn language." (Rindos 1986b:324, my parentheses).

In this approach, general subcategories of "culture" are seen to be genetic dispositions toward certain themes, such as temperament, altruism, intelligence, and sexual orientation. The human cultural phenotype is then made up of various "traits", which while requiring enculturation and learning to be expressed, nonetheless correspond to actual genotypic configurations which convey that particular potential. This one gene, one trait model of genotype/phenotype correspondence is now viewed as overly simplistic; genetics is no longer seen as offering discrete "particles of inheritance" for which, as Daly asserts, cultural equivalents cannot be found. Depew and Weber (1988), in their assessment of the consequences of nonequilibrium Chaos theory for the Darwinian tradition in biology, remark:

> "Further, structural and molecular genetic studies have shown in recent years that the genome is not just a string of independently assorting genes. Genes have a complex internal structure. They are, for example, often arranged in "multigene families" that give rise to a variety of non-Mendelian phenomena (Campbell 1982, 1985). For instance, genes can exhibit "molecular drive" (Dover 1982), ramifying copies of themselves throughout the genome in defiance of Mendel's Laws, resulting in the spread and persistence of traits for which there has been no selection." (Depew and Weber 1988: 320).

Rindos has advanced a more sophisticated model of the human capacity for culture to deal with these kinds of genetic phenomena, thus providing both a translation of new biological theory into the particular area of human genetics, and a theoretical prototype (ie. nonparticulate inheritance) by means of which Daly's initial argument against Cultural Selectionism becomes superfluous. Rindos refers to this alternative to "classical" genetics as the "balance" model, and the model has been used for many kinds of complex phenotypic phenomena besides the human capacity for culture. The most notable characteristic of these models is their avoidance of notions of particulate inheritance.

In the "balance" model, culture is viewed not as a trait, but as a range of behaviours (Rindos 1986b:324). The "classical" model implies the existence of an optimal genotype which corresponds to the human capacity for culture, a "particular set of alleles at specific genetic loci". In contrast, under certain circumstances, the "balance" model would view any notion of the optimal genotype as misguided. Immense numbers of alleles exist for each relevant locus, with each combination conveying both the general capacity for culture, and a certain amount of individual variance in the nature of that capacity. But the variation persists in the population, since there is no way for the environment to select for one particular combination and "fix" it in the population at the expense of the others. The full range of behaviours represented by the human cultural capacity is thus selected indiscriminately, due to the general advantages conveyed by the vast set of genes responsible for the capacity.

Not only has discrete inheritance been attacked along this front, but also from the point of the definition of individual genes themselves. Dawkins (1989:33) has explicitly rejected perfect segregation from his definition of the gene, remarking that the word "gene" should be defined as a kind of "fading out" definition, like the words "big" or "old". Dawkins also points out that even a cistron (piece of DNA which codes for one protein) is occasionally divisible, and any two genes on the same chromosome are not completely independent of each other (see Chapter 5, Section 5.2, for the full treatment of these remarks).

Lastly, Ho and Saunders (1982) have asserted the obsolete nature of the "classical" Mendelian model of particulate inheritance, in line with the two papers quoted above. Discussing their "epigenetic" approach to organismic evolution, they criticise the simplistic Mendelian notions of orthodox, neo-Darwinian adaptationism. They are evidently not aware that "selfish-gene" theory, (again see Chapter 5, Section 5.2) explicitly includes organisms, and explicitly deconstructs discrete inheritance; Ho and Saunders prefer to deconstruct Mendelian inheritance in their own words:

> "There are no organisms, only genes which mutate and compete in a changing environment. The "selfish-gene" concept - widely criticised and condemned, sometimes even by neo-Darwinians themselves - is nonetheless the inevitable, logical conclusion to neo-Darwinism...(But) An organism is not just a collection of accidental mutations, nor is its development an incidental and inconsequential unfolding of a preformed genetic programme. On the contrary, it can be understood only in terms of systemic principles and relationships which transcend the properties of individual genes. Moreover, organisms develop and evolve because they are open to the environment. The environment enters into development not as mere disturbances to be overcome, but as necessary formative influences." (Ho and Saunders 1982:343).

Thus Daly's objection becomes a red herring; the requirement of particulate inheritance would be necessary to build a cultural equivalent of Mendelian Darwinism, but not to a Cultural Selectionism based on the expanded neo-Darwinian synthesis of modern evolutionary biology. Cultural analogies with genetic inheritance are under no obligation to demonstrate discrete particles of cultural evolution, since the gene concept involves no necessary "perfect segregation" in the first place.

4.6 Concluding Remarks

It can be seen that in the last two decades there has been a gradual renewal of interest in the use of selectionist principles in archaeology and anthropology. This is part of a wider trend within the humanities as a whole (as well documented by Degler 1990; and Durham 1991). Concurrently, there has been a steady growth in the use of Postprocessualist principles (Hodder 1982, 1985, 1989; and Shanks and Tilley 1987a, 1987b), which is also part of a wider Postmodernist and Poststructuralist trend, such that Postmodernism and Selectionism are now, perhaps, two of the most rapidly growing bodies of theory in cultural fields of enquiry. A general overview of the neo-Darwinian resurgence was provided in this chapter, and it was shown how a number of different subtraditions have already begun to develop within the perspective, such that it can no longer be viewed as a coherent movement in any but the broadest terms.

The principal role of the chapter was to emphasize the distinctive theoretical identity of Cultural Selectionism. It was shown that despite certain superficial similarities in terminology, Cultural Selectionism was fundamentally different from both the Sociobiology of the last chapter, and from Diffusionism.

It is also apparent that while Cultural Selectionism is by no means about to overturn the "Lamarckian Orthodoxy" of Cultural Evolutionist and Sociobiological approaches to culture, it is already a substantial body of literature, and one which is growing rapidly. The greater part of the literature can be dated within the last decade. It is conceivable that cultural selectionist perspectives could begin to provide a serious threat to the "Lamarckian Orthodoxy", and for that matter, provide substantial competition to (and/or consensus with) the Postmodernist movement, within the next decade.

However, it may well be that this hypothetical Cultural Selectionism of the future may be substantially different to that of the eighties and early nineties. There are a number of serious problems with current selectionist notions of cultural process, and it is possible that some of these problems will need resolution before the perspective can continue to expand. In the next chapter, these problems are brought out in the cultural virus critique, where the virus position is used as an heuristic alternative to the more established "organismic or "phenotypic" form of Cultural Selectionism. A more comprehensive exposition of the Cultural Virus position is not undertaken until Chapters 9 and 10.

5

THE CULTURAL VIRUS CRITIQUE

Overview

Generally speaking, then, Cultural Selectionism has the potential to solve the problem of the "untried theory", and to fill the gap which Cultural Evolutionism and cultural Sociobiology could not. Yet there are some major conceptual problems which arise from the common but unwarranted assumption that cultural phenomena must be modelled as "traits" of the human phenotype, a series of problems referred to in this chapter as the Cultural Virus Critique. It is stressed that Cultural Virus principles are used here only for the purpose of the critique, as theoretical statements concerning Cultural Virus models and foundation arguments are not offered until Chapters 9 and 10.

5.1 A New Mode of Inheritance

Amongst the general body of literature outlined in the last chapter, the most prominent sub-tradition is one which developed in America. Well known examples of this sub-tradition include Boyd and Richerson (1982, 1985); Braun (1990); Cavalli-Sforza (1991); Cavalli-Sforza and Feldman (1981); Chen, Cavalli-Sforza and Feldman (1982); Dunnell (1980); Durham (1982, 1990, 1991); Leonard and Jones (1987); O'Brian and Holland (1992) and Rindos (1985, 1989). Lee's perspective (1991) lies near this group, although her theoretical statements are centred more exclusively upon organismic reproduction than the others. In any case, in grouping these perspectives together I am not trying to simplistically "gloss over" any of the differences which may exist within the tradition. I do not intend to enter into any such disputes, as this will distract attention away from certain similarities which bind them together, and which I now wish to focus upon. These similarities may be summarised under the

label of the "Inclusive Phenotype" or "cultural traits" approach. The bias toward organismic reproduction does not involve an implicit exclusion of the process of cultural reproduction (as it often does in sociobiology); rather, it subtly pervades what are (ostensibly) models of "purely cultural" processes.

But before delving into these similarities, it is important to bear in mind that all of the above approaches satisfy the basic definition of Cultural Selectionism which has just been established. This definition has been summarised as an extension of neo-Darwinian theory into purely cultural contexts. Sociobiology, on the other hand, maintains an ultimately organismic context for its neo-Darwinian principles - other processes are not seen to be independent selective processes in their own right, but rather to be secondary influences on the primary selective processes which take place in organismic gene pools. Thus, although this chapter is a critique of the American or "Phenotypic" position, in no sense is it an attack on their claim to a cultural selectionist identity. Rather the critique raises questions about how best to fulfil what are essentially shared cultural selectionist objectives, and how best to transcend the sociobiological heritage I believe is reflected in the organismic reproduction bias of the "Phenotypic" position.

Even such fundamental word categories as the words "evolution" and "biology" reflect the organismic reproduction bias in Cultural Selectionism. Lee's recent paper (1991), for example, defines both of these terms in this way. The words "biology" and "evolution" are not defined in any genetic determinist or sociobiological sense; however cultural phenomena can only "evolve", and are only "biological", to the extent to which they affect organismic fitness. They are not considered biological phenomena in their own right. Thus Lee defines the word "biological" in the following words:

> "The term "biological" can be limited to the genetical basis. Since, as we know, genes code for proteins and not behaviour, any genetic link with the evolution of human behaviour would be difficult to substantiate. But a more productive line of reasoning would be to define as "biological" those changes in the genetics, morphology and behaviour that increase the likelihood of an organism surviving and reproducing. Behaviour is thus not placed as some outlier or separate process, but rather integrated into the biology of the organism as a whole." (Lee 1991:208).

For Lee, then, behaviour is biological not because of what it is, but because of what it affects. Behaviour is included in the discipline of biology because of its effect on organismic reproduction, rather than due to its effect upon its own reproduction along social conduits, or due to its material reality as a class of actions generated by learned neuronal structures. It is almost as though Lee believes that behaviour itself does not really exist.

Not only are cultural phenomena only biological to the extent that they affect organismic reproduction in Lee's view, they can only evolve to this extent also. Thus Lee defines the word "evolve" in a similar manner to the way in which she defined "biology" above:

> "We can ask if behaviour itself "evolves"? If, by evolve, we mean that behaviour which increases the relative success of an organism will itself increase in frequency over time, then the answer is yes. If we mean is it under single gene (or even gene complex) control, then the answer is probably no in terms of complex behaviour. However, behaviour can be shown to play a role in genetic evolution (Boyd & Richerson 1985, Bateson 1988) despite the misgivings of some anthropologists (Ingold 1990). Thus behaviour itself provides the conditions for propagation of adaptive traits, as in mechanisms of female choice, mate selection or learning paradigms (see Borgerhoff Mulder 1988, Alexander 1974, 1989)." (Lee, 1991:209).

So while cultural selectionists clearly retreat from the sociobiological premise that genes, however indirectly, ultimately cause behaviour, they do not always retreat from the reverse position. Behaviour can very definitely affect genetic fitness (which, of course, it must). However Lee implies that in doing so, it directly affects its own likelihood of survival - "behaviour which increases the relative success of an organism will itself increase in frequency over time". Also implied is the idea that behaviour which does not impart a positive effect to organismic survival or reproduction cannot evolve. Thus any such learned neuronal structures (which underly the behaviour patterns), while being in no sense under genetic control, neither exist nor evolve in the biological demesne, according to Lee's view (see Ingold 1991:220 for a parallel conclusion).

Such "genetic survival focused" definitions of the words "biology" and "evolution", are dramatic indicators of the persistence of "the organism's point

of view" in American Cultural Selectionism. Below, however, we shall see that an organismic bias subtly pervades a variety of specific cultural selectionist models as well.

This "Phenotypic" or "Organismic" bias is also reflected in the common use of the phrase "mode of inheritance". While Cultural Selectionism's distinguishing banner typically involves some allegiance to the shifting of neo-Darwinian theory to a new context, that of culture, it is a shift to no more than a *new mode of inheritance*, not a shift to a new class of phenomena (ie. as in socially transmitted neuronal structures in Chapters 9 and 10). Humans remain the basic "organisms" or "units of selection" in the framework (with rare exceptions). Culturally transmitted phenomena are still treated as "traits" of organisms, and together they make up the "cultural phenotype" which differs from other traits and other parts of the phenotype only by "mode" of inheritance. Culturally transmitted traits and genetically reproduced traits are thus treated as though they share a coherent, uniform genealogy, jointly comprising an organism in just the same way that genetic traits normally do.

Thus, as will be explored in greater detail below, the only "cultural individuals" are those bodies of culture associated with particular human individuals; the only "cultural phenotypes" are the bodies of culture associated with single human phenotypes; and the only cultural "populations" are the bodies of culture associated with human populations. These bodies of culture are not the *logistic equivalents* of the old organismic categories; they are merely the bodies of culture which happen to *physically correspond* to the old "biounits". Cultural phenomena remain as a class of *human "traits"*, in spite of the fact that - by virtue of cultural transmission - they now have genealogical independence from the old units.

Consequently, although cultural phenomena and human behaviour are not seen as under any sort of genetic control, discussion still involves the effect of behaviour on organismic survival and reproduction; the psychological retention and social transmission of particular ideas and behaviour patterns occurs, but is not the focus of attention. Animal fitness, not symbol or

artefact fitness, retains a fundamental relevance to the interpretation of cultural change in the American tradition. Presumably, the symbolic fitness of the artefact or manufacturing concept is seen to be integral to its effect upon the fitness of its host, and not substantially independent from that effect.

So, in addition to such basic word categories as "biology", "evolution", and "inheritance", the focus upon organismic reproduction is reflected in cultural selectionist definitions of a wide range of familiar neo-Darwinian categories. As will be shown below, each of these categories is defined in such a way as to allow for extra-genetic social transmission of cultural phenomena to occur, but genetic fitness, or organismic reproduction, usually remains an important factor of consideration. In the Cultural Virus position, as will be explained in greater detail in Chapters 9 and 10, genetic fitness is all but removed as a factor in cultural change, as "fitness" is seen to be purely a factor of comprehension, retention, and appeal; in short, any factor at all which affects the "survival" and "reproduction" of particular neuronal structures. All the familiar neo-Darwinian categories (organism/individual, environment, phenotype, fitness, etc.) are all redefined in "purely cultural" and "purely psychological" terms. The "individual organisms" or "units of selection" in Cultural Virus Theory are thus "viral" phenomena, which can be best described as socially transmittable learned neuronal structures.

The goal of the critique, then, is to demonstrate that there are inherent mistranslations of neo-Darwinian principles in cultural selectionist concepts of the cultural "individual", and the cultural "population", and ultimately all the other important neo-Darwinian concepts to which these concepts are integrally related, including notions of (cultural) "mutation", "variation", "phenotype", "reproduction", "environment", and, of course, "adaptation". Attention will be focused, however, on the key concept of the cultural "individual", and its relationship to the concept which is normally used in modern evolutionary Biology.

5.2 The Unamended Individual

"The individual organism" is often said to be the cornerstone of neo-Darwinian theory, and so it is perhaps the most logical point at which to begin in the analysis of any variety of neo-Darwinian theory. Even major critiques of the idea of the "individual" as the "unit of selection", such as that of Dawkins (1976, 1982, 1989), can be thought of as redefining the concept of the "individual" at a different level (i.e. the level of the gene or "piece of chromosome", and its totality of phenotypic effects), rather than rejecting it altogether. In any case, American Cultural Selectionism takes the more orthodox (at least, until the last few decades) position (after Darwin 1859; and Mayr 1976, 1982), employing the "individual organism" as its basic unit of selection. They support their use of individual organisms, however, purely with reference to modern evolutionary biology, that is, without discussing the relevance of units of cultural transmission to the analysis of cultural process. The Phenotypic perspective thus employs the very same unit of cultural analysis as that which is used for organismic/genetic analysis (as used, for example, by Betzig 1986:7-9), even though its relevance to the new context is immediately made suspect by the fact that culturally transmitted phenomena exhibit genealogical independence. However, elucidating the implications of this oversight will require some additional background discussion.

Integral to the concept of the "individual" being the "unit of selection" is the idea of the "population" of individuals, which "evolves". Individuals are selected, and, as a direct result, populations evolve. Speaking very broadly, it can also be said that the concept of a population of genes is, structurally speaking, not too different from a population of organisms. In each case the population evolves as a result of the differential reproductive success of its components, and each model is equally neo-Darwinian and equally selective. It is just a question of the level at which one chooses to see "indivisible units" being selected, and assemblages of these units evolving. So even while the word "individual" bespeaks an implicit definition of "individual organism", and thereby a particular level of biological systems, in another sense any

Darwinian theory must, by definition, consider "individuals" the units of selection. In other words, to claim the gene as the unit of selection is simply to claim the gene as one's individual, as one cannot claim something to be the unit of selection without claiming it to be "indivisible" in some sense of the word.

Even when the concept of the "individual unit of selection" is considered to be most applicable at the level of the gene, the size and indivisibility of these units can vary according to a variety of contingencies, ranging all the way from a single cistron (a piece of DNA which codes for a single protein), to the entire genome of an asexually reproducing organism. Thus Dawkins (1989), for a series of examples, defines a "gene" as "a unit which survives through a large number of successive individual bodies" (Dawkins 1989:25); as "any portion of chromosomal material that potentially lasts for enough generations to serve as a unit of natural selection" (Dawkins 1989:28); as "not just one single physical bit of DNA...it is all replicas of a particular bit of DNA, distributed throughout the world" (Dawkins 1989:87); and as "in the case of an asexual stick-insect, the entire genome (the set of all its genes) is a replicator" (Dawkins 1989:273). When no crossing over occurs between a population's chromosomes "a whole chromosome would have to be regarded as one "gene" " (Dawkins 1989:43). At times, the gene is even defined by Dawkins as a virus - "we might just as well regard ourselves as colonies of viruses! Some of them cooperate symbiotically, and travel from body to body in sperms and eggs. These are conventional "genes". Others live parasitically, and travel by whatever means they can" (Dawkins 1989:182).

Dawkins's most elegant definition synthesizes all of the above:

> "I am using the word gene to mean a generic unit that is small enough to last for a large number of generations and to be distributed around in the form of many copies. This is not a rigid all-or-nothing definition, but a kind of fading out definition, like the definition of "big" or "old". The more likely a length of chromosome is to be split by crossing-over, or altered by mutations of various kinds, the less it qualifies to be called a gene in the sense in which I am using the term. A cistron presumably qualifies, but so also do larger units...
>
> To be strict, this book should be called not The Selfish Cistron nor The Selfish Chromosome, but The slightly selfish big

> bit of chromosome and the even more selfish little bit of chromosome..." (Dawkins 1989:33).

Dawkins is even careful to exclude Mendelian "discrete inheritance" from his units of selection:

> "It was the great achievement of Gregor Mendel to show that hereditary units can be treated in practice as indivisible and independent particles. Nowadays we know that this is a little too simple. Even a cistron is occasionally divisible and any two genes on the same chromosome are not wholly independent. What I have done is to define a gene as a unit which, to a high degree, approaches the ideal of indivisible particulateness. A gene is not indivisible, but seldom divided." (Dawkins 1989:33-34).

Despite well constructed arguments to the effect that all units larger than the gene are "like clouds in the sky or dust-storms in the desert...temporary aggregations or federations" (Dawkins 1989:34), there is no doubt that Dawkins places great importance on the individual as a unit of selection, precisely because of, rather than in spite of, his understanding of the significance of genes. An "organisms worth" of genes is a very significant higher category for the Dawkins model, since the genes in an organism "leave the body in the same vehicle" , namely the sex cells, or whole cells in the case of mitosis. Thus Dawkins extensively discusses the importance of the individual with reference to parasites and hosts (1989:253-254); to wolves and wolf packs (1989:255-266); and using the extended metaphor of teams of oarsmen preparing for the Oxford and Cambridge boat race (1989:38-39; 84-86; 271-272). Far from being unimportant to Dawkins' selfish gene theory, the individual is a crucial unit of selection, since unlike higher ecological categories, it exhibits a degree of reproductive synchronicity. The gene is the basic "replicator", but the organism is the basic "vehicle". Nonetheless, the gene is considered the basic unit of selection in the equation.

As already stated, and in contrast to Dawkins, American Cultural Selectionism focuses on the "individual organism" or "phenotype" as the unit of selection, regardless of whether they are discussing genetic or cultural evolution. This question is entirely independent of the "levels of selection" debate in modern biology, as it is not a question of which level one views selection to be occurring, but a question of whether any unit, once chosen, is ever likely to be a unit of another process, in another class of

genealogically independent phenomena, *even at the same level.* In the American position neo-Darwinian theory is transferred from genetic process to cultural process, without substituting a new definition of the "individuals" involved. Human individuals remain the basic units which are selected, and populations of human individuals the basic entities which evolve, even when the phenomena in question are entirely culturally transmitted. Thus Rindos writes:

> "Again following Boyd and Richerson (1985:33), we may define culture (note, not necessarily human culture) as "information capable of affecting individuals' phenotypes which they acquire from other non-specifics by teaching or imitation." This definition implies that cultural transmission involves populations rather than individuals. Of course, it is the individual that learns culturally transmitted information, and individuals are selected in any evolutionary process, but it is the population that evolves (Ghiselin 1974, 1987; Hull 1976). Cultural traits, like genetic traits, evolve solely at the populational level. Since the culturally transmitted information that modifies phenotypes is heritable, the information possessed by the individuals in a population is capable of evolutionary change over time." (Rindos 1989:7).

In other publications, Rindos makes it equally clear that he believes the individual phenotype is just as useful as an analytical category for cultural process as it has always been in genetic process (consider Rindos 1986b:331; Rindos 1985:71-72). Boyd and Richerson also make it clear that they consider human individuals to be as much the unit of cultural selection as they are of natural selection. While a number of their publications make unambiguous statements to this effect (Boyd and Richerson 1982, 1985; Richerson and Boyd 1978), they also state the position succinctly in their 1985 comment on Rindos' paper:

> "Natural Selection operates whenever (1) heritable variation exists and (2) the variation affects the life chances of individuals." (Richerson and Boyd 1985:83).

The individuals discussed in these contexts are invariably individual "organisms", or the total cultural phenotypes thereof. For other examples of the explicit use of the individual's phenotype, consider Dunnell's brief discussion of Masai bridewealth payments (1980:61), and also the following words from Leonard and Jones:

> "Beyond structural homologies, specific concepts drawn from the biological paradigm are seen as more broadly applicable in an inclusive evolutionary model. Such a concept is the phenotype.

> Mayr (1970:442) defines phenotype as "the totality of characteristics of an individual (its appearance) as a result of the interaction between genotype and environment." This definition subsumes both physical and behavioural components of the individual and, in keeping with the spirit of the concept, necessarily includes cultural acquisitions as part of the human phenotype. It follows that material culture - artefacts of any scale - is an expression of human behavioural variability and thus should be regarded as one class of cultural traits and hence a component of the human phenotype." (Leonard and Jones 1987:213).

Thus the basic premise or concept sustaining the Inclusive Phenotype approach in the American cultural selectionist tradition begins with its conservative definition of the "human individual" as the unit of cultural selection, in spite of the fact that (in contrast to its role in genetic process) the human phenotype is not a unit of cultural transmission or reproduction. Yet as far as any comparison with the cultural virus approach is concerned, the question is not whether individuals are the units of selection, as any unit of selection must display some degree of indivisibility. Rather, the question is whether the *cultural* components of individual organisms, as implicitly defined by Rindos above, are also the indivisible units of *cultural* selection (quite apart from issues of whether they are even the units of selection in genetic process). The human cultural phenotype is even more "divisible" than the non-cultural phenotype, which is only divided into halves during meiosis in sexual reproduction. These halves may then be further divided when crossing over occurs between different chromosomes. But a typical human psyche, on the other hand, is made up of countless thousands of separately transmittable ideas, concepts, and techniques. Any idea which can be taught or imitated independently must surely qualify as a "cultural individual" far more rigorously than an entire human psyche (which in turn must correspond more closely to a cultural equivalent of a "local faunal assemblage").

Of course, human individuals are the units which learn, but in doing so they are actually playing a completely different role to the one they play in genetic process. Human individuals are undoubtably important in both processes, but while they are the *units of selection* in genetic process (forgetting, for the moment, the problems raised by the Dawkins critique), they are the *units which select* in cultural process. Humans are thus the

selectees in one process and selectors in the other, playing utterly nonequivalent roles in each. Yet in American Cultural Selectionism, human phenotypes ironically remain the basic unit of selection. Thus the approach uses a completely different unit of analysis to the Virus position of Chapters 9 and 10 (see also Cullen 1990, 1991, 1992a, 1993a, 1993b), where the cultural equivalent of the "individual organism" is the socially reproduced learned neuronal structure (which is too simple and passive to be called an "organism" and is called instead a viral phenomenon), and its phenotype any actions or artefacts generated therefrom.

5.3 Traits of the Phenotype or Viral Phenomena?

The use of the human individual as the unit of cultural selection in American approach is partly a result of this unwarranted belief in the lack of a viable cultural alternative. The possibility of treating artefacts or ideas as individual "organisms" in their own right is often discussed as shall be seen below. However, in their haste to assert the obvious - that artefacts are not organisms - the more modest possibility that they may be some sort of simple viral phenomenon is forgotten. American cultural selectionists are under the impression that artefacts and learned behaviour patterns cannot be granted the status of neo-Darwinian individuals unless we attribute all the qualities of self-reproducing organisms to them. They are not aware that viral status alone is quite sufficient. Leonard and Jones discuss the issue (with reference to Brew) in the following words:

> "...(quoting Brew):"We still are faced with the fact that, with the exception of skeletal material, the objects and concepts of archaeology are not living organisms or parts of living organisms. Consequently, their development is not properly represented by a classificatory technique based on the genetic relationships of living organisms." But just the same, we ask if the skeletal material Brew refers to, or even the parts of living organisms, reproduces in a manner identical to that of an individual. It does not." (Leonard and Jones 1987:214).

They are left with no alternative but to espouse the fallacy of cultural phenomena being utterly equivalent to other parts of the phenotype, as to

deny any organic connection altogether (ie. to reject *both* the "artefacts as organisms" and the "parts of the extended phenotype" premises) would also preclude the applicability of Selectionism. Rather than choosing to deconstruct their position in this way, they ignore the fact that while cultural phenomena such as artefacts and ideas are genealogically independent, all skeletal material is reproduced as a single blueprint during meiosis or mitosis. Moreover, Leonard and Jones are not the only ones to give the "artefacts as organisms" idea little credence. Foley (1987) implies a similar opinion when he speculates as to why physical and behavioural anthropology are rarely integrated:

> "Few people - and Glynn Isaac was a notable exception - have attempted to integrate the two. The reasons for this are no doubt complex...Certainly dissatisfaction with earlier archaeologists' attempts to treat stone tools as evolving and even reproducing organisms in their own right has made many contemporary workers sceptical of the value of an evolutionary approach." (Foley 1987:380).

Once the possibility of treating artefacts as individual organisms is dismissed, and the "artefact's eye view" of adaptation is perceived as a blind alley, cultural phenomena have to be forced back into the human phenotype, or be excluded from the neo-Darwinian scheme. Since "they know" that artefacts are not organisms in and of themselves, those who advocate the Phenotypic position have to make them parts of others. How else could they be inserted into a discourse which mentions the word "organism" on every page? Yet the "genealogical independence" or "reproductive asynchronicity" of culturally and genetically transmitted phenomena is undeniable; surely no one would seriously argue that cultural transmission cannot occur between genealogically unrelated organisms (between human individuals from different families, even between organisms from different genera, such as between *Pan troglodytes* and *Homo sapiens* (Linden 1974)). Yet reproductive "synchronicity" is perhaps the only tenable way to define a unit of selection (Dawkins 1989:254-256). How can something that is not a unit of reproduction possibly be a unit of differential reproduction (selection)? The "artefacts as traits or organisms" issue is thus a serious dilemma for the American cultural selectionist tradition.

Cultural selectionists have often, therefore, agonized over the "complexity" of cultural inheritance, trying to show how it is really just another dimension of organismic inheritance. Rindos has discussed this issue with reference to an oft-quoted passage from Levi-Strauss (1963):

> "...(quoting Levi-Strauss) "what makes (evolutionary genetics) valid for the historian is the fact that a horse indeed begets a horse and that, in the course of a sufficient number of generations, *Equus caballus* is the true descendant of *Hipparion*. The historical validity of the naturalist's reconstruction is granted...by the biological fact of reproduction. An ax, on the contrary, does not generate another ax. There will always be a basic difference between the two (tools)...because each of them is the product of a system of representation." (Levi-Strauss 1963:4). Yet neither does an axe generate itself. It is made by a human under the instruction of other humans by means of a culturally transmitted symbolic system. This system, with its products, is passed from generation to generation and from person to person." (Rindos 1985:72).

Predictably, the genealogical conflicts of genetic and cultural inheritance are not mentioned. The peculiarities of cultural inheritance are dismissed as being merely more complicated than other traits, rather than qualitatively different due to their unique quality of genealogical independence. Thus Dunnell has written, when commenting on Rindos's words:

> "...in noting, as others have done before, that axes don't make axes he seems to be contrasting cultural and genetic evolution. Yet just as clearly as axes don't make axes, neither do fingers make fingers. To get another finger, one has to go through the genome and then translation into a new phenotypic finger. Getting an axe from another axe does not seem so terribly different. More generally, I am led by these kinds of noncontrasts to suspect Rindos's CS2 (selection of cultural phenomena by cultural phenomena - my addition, see discussion below) selection is not nearly as contrastive as his expository form suggests. What differs between cultural and genetic systems is that the selection Rindos identifies as CS2 takes place in the poorly understood and observationally obscure translation from genotype to phenotype in the genetic case. These same processes are much more readily observed in the cultural transmission case because the process of phenotypic development is more obvious." (Dunnell 1985:79).

Dunnell implies that the passage from axe to axe can actually be traced via the human genome - in a complicated manner, but ultimately in a manner no different to that of fingers, and this is where he is just plain wrong. Fingers do go through the genome, but axes don't; knowledge of axe making, like any other parasite, can pass directly between unrelated people,

presumably from master to apprentice in the blacksmith's forgery, through the media of language, demonstration, and imitation. No cellular division or DNA-based replication takes place, and so it is indeed "terribly different" to the reproduction of genuine "traits" such as fingers. But Dunnell manages to assert equivalence just the same. A similar kind of confusion occurs when O'Brien and Holland (1992:48), discussing whether human invention is Lamarckian or not, use the idea of "a person who sits down to make a stone tool" as an example of an intentional invention. The example implies that people reinvent stone tools every time they make them, which is entirely fallacious. The process of sitting down to make a stone tool is obviously intentional, but the process is in no sense analogous to invention. From the point of view of the cultural virus critique one would have to argue that such processes are cultural analogues to the production of phenotype from genotype, such as the production of protein molecules from DNA. In other words, sitting down to make a stone tool is perfectly intentional but the process is not an evolutionary one.

Those who remain doubtful about the link between the trait approach and the rejection of genealogical independence (via assertions of equivalence), might consider an additional statement from Leonard and Jones (1987). They clearly state that they consider cultural phenomena to be typical "organismic traits", as though their genealogical independence does not make them unique:

> "Like other kinds of traits, artefacts need not, in fact cannot, assume the logical status of individuals with reproductive capabilities. Instead, as a quality of the phenotype, artefacts are the equivalents of physical and behavioural traits. None is capable of reproduction, but each incorporates useful phenomenological classes which are the measurable units of change." (Leonard and Jones 1987:215).

O'Brien and Holland (1992:48) express a similar position when they argue "if the trait is inheritable or transmissible *by any means*, then all else being equal the trait will be represented differentially in the next generation". With sentiments such as these, American Cultural Selectionism departs utterly from the "ideas and artefacts as (simple, viral) organisms" of the Cultural Virus position. Rather, the cultural phenomena must assume the status of traits of the human phenotype. They cannot be granted the status of "individuals" in

their own right, cultural selectionists argue, since they lack the capacity to self reproduce.

What fails to be noted is that, like cultural phenomena, microbial viruses also lack this capacity - being unable to reproduce themselves within their own cellular structures (which are at best only simple envelopes), they must instead inject their genetic material into another more complex cell. Yet they are still treated as "individuals" and units of selection, and classified on the basis of their phylogenetic relationships, although this is fraught with difficulty (Fenner, McAuslan, Mims, Sambrook and White 1974:1-33; Kuby 1992:459; Postgate 1989:40-44; implied by Dawkins' comparison of genes and viral entities 1989:182; and despite the recombinative effects of exchange of virions in bacteria, e.g. Sonea 1988:38-56). So as far as the neo-Darwinian individuality of ideas and artefacts is concerned, neither inanimacy nor reproductive dependence are grounds for disqualification. Artefacts can be considered individuals (but only as "quasi-organisms" or "viral phenomena"), without attributing anything more to them than the genealogical independence they undoubtably display.

In sum, the prevalent but mistaken belief in the American position - that cultural phenomena cannot be granted any "individuality" in neo-Darwinian terms - leaves Cultural Selectionism with two theoretical choices; either to abandon the distinctive neo-Darwinian discourse which centres upon "individuals" (and thereby deconstruct their own position), or to compromise and use human individuals (the cultural aspects thereof) as their basic unit of cultural selection. This, in turn, arises from the fact that there would appear to be only one class of organisms involved in human affairs, namely humans, and these then appear to be the only candidates for "individuals", and "phenotypes".

Yet even a brief analysis of cultural transmission would reveal that the structures of any given human psyche have a multiplicity of origins, and a kaleidoscope of different genealogies. And an "individual" constituted from phenomena which exhibit a multiplicity of genealogies is by definition not an

organism at all. Once the capacity for culture emerges in a population, its "individuals" are no longer individuals, but ecological assemblages, each consisting of one "organism" and thousands of viral neuronal structures. The relationship between any given human individual and any given idea can be understood as a *cultural* equivalent of Dawkins's *genetic* contrast of "host/parasite" combinations and "single organisms" (Dawkins 1989:255; Cullen 1993a; and see Chapter 9 for expansion). This avenue is not pursued in the American position, and as a consequence cultural phenomena are viewed as aspects of the human individual, and as a class of human traits.

5.4 Human-megaliths and Squirrel-oaks

As already stated, this chapter is not an exposition of Cultural Virus Theory, but a critique of its primary theoretical alternative, the Inclusive Phenotype perspective. Elsewhere (Cullen 1990, 1991, 1992, 1993a, and 1993b) a variety of aspects of the theory itself are discussed. However, I would now like to present a brief summary of the Virus theory, and a brief illustration of its analytical advantages over an Inclusive Phenotype approach, with reference to one of the most frequently used "stamping grounds" of new theories - the explanation of megaliths. Others have used the European megaliths as archaeological archetypes for the comparison of different theoretical perspectives (Renfrew and Bahn 1991:428-9; Hodder 1992:45-80). However, it will quickly become clear that neither the Inclusive Phenotype nor the Virus varieties of Cultural Selectionism compete directly with recent accounts of such phenomena. Rather, they compete with each other, and other stated or unstated general theories of culture, such as Sociobiology and Lamarckism, at a more general level of culture theory, as the fundamental explanatory frameworks within which relatively specific explanatory packages such as Post-Processualism or Cognitive-Processualism may choose, or choose not, to align themselves. This point is embodied in the fact that, unlike most well known theories which concern cultural or psychological phenomena, neo-Darwinian or Lamarckian theories are not concerned with *which* factors are

involved in causing a particular *status quo*, but with the more general problem of the *very means by which* any and all such factors take effect. For this reason, the megaliths are used only as hypothetical archetypes, and the issues discussed are entirely different to those normally presented in megalith discourse (again, see Renfrew and Bahn 1991:428).

The aim is to show that the widely held Inclusive Phenotype view, which, as shown in previous sections, conflates culturally and genetically reproduced phenomena into a single human phenotype, is analytically awkward; but that this awkwardness escapes our notice due to the complicated nature of cultual inheritance. Yet as soon as we transfer the essential problems of the Inclusive Phenotype into a purely biological context, the awkwardness becomes readily apparent. For the purpose of simplicity and familiarity, this point will be pursued with reference to two of the most ubiquitous organisms of Northern Hemisphere universities, squirrels and oaks. Yet the example would apply equally well to other mammal/tree relationships, such as that between koalas and gum trees in Australia.

So the demonstration will involve the manipulation of four basic kinds of organism/artefact; humans, megaliths, squirrels, and oaks. The "case studies" are purely hypothetical, although based on reality. The general objective is to show that the inclusion of megaliths in the human phenotype is every bit as confusing and counterproductive as the hypothetical inclusion of oaks in the squirrel phenotype. Yet so entrenched is this notion that cultural selectionists and sociobiologists deal with this confusion unwittingly, and indeed daily, in their attempts to explain human behaviour in neo-Darwinian terms.

First, a short description of the squirrel/oak relationship. Let us imagine that there are two neighbouring forests, one composed of white oaks (*Quercus alba*), and one composed of red oaks (*Quercus rubra*). Let us imagine, again for simplicity, that the red oak forest is inhabited by red squirrels (*Sciurus vulgaris*), and the white oak forest is inhabited by grey squirrels (*Sciurus carolinensis*). Let us imagine that both squirrels eat the

acorns of their respective oaks, and also engage in the practise of storing for the winter by burying some of the acorns they find, many of them beyond the borders of the shaded area below the tree. Some acorns are forgotten, and are left behind to germinate in the spring, conveniently buried in favourable positions for successful growth. On the other hand, the squirrels rely on the oaks as places in which to shelter from predators, places in which to build their nests and reproduce, and as a ready supply of nutritious food. Thus in each forest, a particular species of squirrel and a particular species of oak have a close co-evolutionary relationship.

Second, a short description of the human/megalith relationship. Let us imagine that there are two neighbouring groups of people, one of which build Neolithic chambered tombs, and one of which bury their dead using a form of ritualised cremation. Obviously, tombs and cremation rituals are reproduced via human activity, and so it is self evident they are every bit as dependent upon humans for their successful reproduction, as the oaks are dependent upon squirrels. On the other hand, the tombs have great significance in their respective cultural contexts. On the one hand they might well provide a range of functional benefits, such as legitimating land ownership of the society as a whole, or legitimating the special rights of a social elite; the kinds of factors which might be emphasised in a processual or neo-Marxist explanation (Renfrew and Bahn 1991:429). The megaliths might also sustain appeal due to their symbolic significance within the local cultural traditions of Western Europe, due to "objectifying and giving meaning to social strategies" in the way that Hodder has remarked (1992:75).

But as far as the competing neo-Darwinian accounts of culture are concerned, the question is not which of these factors are most important, but whether the megaliths can be reasonably included in the human phenotype. A Cultural Virus model of the megalith building tradition would say that it cannot. A virus account would consider the megaliths to be phenotypes in and of themselves, extrasomatic expressions of a lineage of socially transmitted learned neuronal structures (similar to a Dawkins "meme complex" 1989:197-199), which have "actual physical existence" in the nervous systems of people

(Dawkins 1989:323, Delius 1989:46; 1991:83; Edelman 1989 and 1992) and which display genealogical independence from the human phenotypes in which they reside. These neuronal structures are a set of inter-related ideas which jointly convey to their bearers the capacity to build megaliths. In other words, they constitute a body of knowledge of the various techniques of megalith masonry which must be socially transmitted in order to survive. Whether there ever actually were specialised artisans analogous to masons, or whether megalith masonry was distributed more indiscriminately amongst the community, will probably never be known. The real problem for the Inclusive Phenotype position is the genealogical independence of the tombs; which means that megalith masonry does not need to increase the reproductive success of masons in order to reproduce itself. It could even survive if the masons were a celibate caste with zero genetic fitness. If the demand for the building of new megaliths is sufficient, megalith masonry could continue to pass to new apprentices from genetically successful non-mason families for an indefinite period of time. Moreover, the demand for the megaliths is governed not by actual benefit, but perceived benefit; the social context of the megaliths may well lead to a situation where their value for controlling reproductive and productive resources are vastly overvalued. But megalith cultural fitness can only be defined in relation to what is perceived, and not in relation to what is.

A neo-Darwinian account would have to reject any notion of the megaliths being generated in a Lamarckian manner through a rational response to the needs of human societies, or those of powerful factions within the society, as some processual explanations would seem to imply. The first architectural blueprint which could truly be called a megalith would have had to occur as a result of a spontaneous divergence from a pre-existing body of masonic concepts, most of which would presumably remain unchanged. A good candidate for the immediate cultural ancestor of the megalith could well be the central European long house, given the similarities discussed by Hodder and others (Hodder 1992:50). Once invented, the megaliths could then prove to be symbolically adapted to the perceived needs of local and neighbouring communities and then undergo differential cultural reproduction at the expense

of other burial practises for these reasons. This would not be a symmetrical and unstructured process of random diffusion, but a highly structured displacement of one burial practise by another, due to the greater symbolic fitness of the megalith form under the local conditions of increasingly restricted resources.

So both the human/megalith and the squirrel/oak relationship have been briefly introduced. There are many ways in which an Inclusive Phenotype model could contrasted with a Virus one through manipulating these categories. Let us suppose that a PhD student in a Biology department decided to devote his or her thesis to the study of the ecology of squirrels and oaks and their symbiotic relationships, and that the researcher discovered a remarkable fact; that only acorns which had been briefly nibbled or handled by squirrels were capable of germination. Imagine that the researcher is so excited by the close evolutionary relationship, that he or she proposes a new model for interpreting the evolution of squirrels and oaks. The whole idea of separating squirrels and oaks is seen to be misguided; the researcher believes that an exciting new example of an "evolutionary merger" has been found, and from now on insists on speaking of a single organism, the "squirreloak", whose phenotype includes both the mobile squirrels and the oaks amongst which they live. No longer are squirrels adapting to oaks (amongst other forest conditions), and oaks to squirrels; the researcher feels that it is more accurate to think of squirreloaks adapting in unison to the rest of the forest.

It is intuitively easy to see that it would be unlikely that the PhD would be favourably received. But why? There are many issues, and a specialist in evolutionary Biology could no doubt elucidate them more effectively. But I think the key issue is the fact that different parts of the squirreloak display genealogical independence. It is obvious that an oak could never be considered to be a true squirrel organ since the two structures, despite their symbiotic dependence, are genealogically independent of each other. Squirrels and oaks reproduce independently of each other, and indeed exist independently of each other. One may find squirrels without oaks and oaks without squirrels. But even in forests well populated with both squirrels

and oaks, there is no guarantee that the offspring of a given pair of squirrels will build their drey in the oak trees that their parents helped to germinate. The grey squirrels in the white oak forest may well, indeed, become so successful as to displace the red squirrels in the neighbouring red oak forest, in which case none of the red oaks would be inhabited by the offspring of the squirrels which buried the relevant acorns.

Dawkins (1982, 1989:240–242) has analyzed the essential difference between parasites inhabiting the body and other structures we interpret as organs, and identified the essential difference between the two. The key characteristic of a parasitic structure is that it is not passed on in the germ cells of its host, but is passed on by other means. Such circumstances do not guarantee that the parasite will exploit its host, but they make exploitation possible, as the parasite can escape its host and go on to infect new unrelated hosts. Conversely, the hosts offspring may be infected by other parasites, unrelated to those which inhabit the host itself. What is adaptive for a parasite may not be adaptive for its host, and *vice versa.*

Organs and traits, on the other hand, are structures which are reproduced together; the information which allows their synthesis travels between generations in the germ cells. Since they are reproduced together, such structures cannot escape the lineages in which they currently reside; an organ or trait which inhibits the reproductive success of the body in which it resides also inhibits its own survival. What is adaptive for a trait is adaptive for its host, and *vice versa.* Parasites which are transmitted from generation to generation in the germ cells of their hosts, on the basis of this argument (Dawkins 1989:243), are virtually indistinguishable from traits, organs, or organelles, and indeed they may become just that. Such an argument has been advanced, for example, for the origin of cellular organelles such as mitochondria (Margulis 1981). In fact, the definition of the organism as being a group of structures which are collectively reproduced would appear to be not only a necessary condition, but a sufficient one. It is very hard to define an organism as anything other than a symbiotic assemblage of

phenomena, colonies of viruses which reproduce via the same means and at the same time (Dawkins 1989:182).

We are now equipped with convenient set of concepts with which to continue to elucidate the problems of viewing cultural phenomena as a class of human traits or organs. True individuals are structural combinations which reproduce together, host/parasite or host/guest structural combinations are those which consist of parts which reproduce at different times and via different means. An Inclusive Phenotype view of culture would classify megaliths as part of the human phenotype, and therefore view Neolithic prehistory in terms of individual humanmegaliths with combined phenotypes of bone and stone, jointly adapting to prevailing ecological and social conditions. The cultural transmission of megalith masonry may not always be to the genetic advantage of the people involved, but when this occurs megaliths are viewed as maladaptive traits, rather than as adaptive parasites (see next section for further discussion of this). This is as misleading as viewing an oak which inhibits squirrel reproduction, but promotes its own reproduction, as a maladaptive squirreloak trait, rather than as an adapted oak. The illusion of maladaptedness is purely an artefact of the insistence of assessing fitness at the level of the inclusive phenotype of the squirreloak, rather than at the level of individual oaks and squirrels. The humanmegalith, like the squirreloak, is a false individual which consists of separately reproduced parts, and what is adaptive for one part may not be adaptive for the other. For instance, let us suppose that a local chieftain commissions a young mason to come and supervise the building of a megalith, and that the leader is not only willing, but anxious to apprentice her youngest son to the tradesman, so that the art of megalith building might be taught to him. It is not a foregone conclusion that she would be equally happy to consent to the marriage of the same tradesman to one of her daughters. As far as the phenotype of the humanmegalith is concerned, such circumstances are both a cultural windfall, and a genetic catastrophe.

But false individuals do not only cause problems for the assessment of fitness and the manipulation of concepts of selection and adaptation. More

insidious problems emerge when attempts are made to define *populations* of false individuals. The populations of megaliths and humans do not coincide; they are not sympatric. While the gene pools of the megalith builders may well merge with peoples who build central European long houses, and indeed with those of western Asia, the Megaliths themselves are confined almost exclusively to western Europe (Hodder 1992:50). Moreover, the socially transmission of megalith masonry could well have involved a degree of specialisation in knowledge acquisition, a class of "megalith masons", and so many members of a community might have little or no knowledge of megalith manufacture. The question is whether individuals who do not possess any of the neuronal structures for megalith masonry should be included in the megalith population at all, just because they share a common gene pool with the masonry bearers. In a Cultural Virus approach the megalith population consists of no more, and no less, than the megaliths themselves, and their attendant neuronal structures of megalith masonry. On the other hand, the Inclusive Phenotype approach, which implies a humanmegalith phenotype, becomes awkward when humans are found without megaliths, or when megaliths are found with completely different cultures or people. Are such individuals to be classified as megalithless humanmegaliths? This would be as taxonomically awkward as a Biology which classified oaks, squirrels, and squirrel/oak combinations as three different species of squirreloaks: squirrelless-squirreloaks, oakless-squirreloaks, and standard squirreloaks. A squirrel could alter its taxonomy simply by jumping to another sort of tree. Similarly, when a single family member became apprenticed to a mason, so becoming a true humanmegalith instead of a humanmegalith with the trait of "megalith absence", the structural transformation involved would require a large taxonomic re-evaluation in the lifetime of a single genetic individual.

The only way to avoid such conceptual nightmares is to restrict taxonomy to individuals which are seldom divided; in other words to allow single phenotypes to include only those elements which are reproduced together. In a Virus approach the humanmegalith is thus divided into two classes of individuals, along the lines of greatest genealogical discreteness; the Homo sapiens population, and the megalith population, with the latter

consisting of a "meme pool" of viral neuronal structures in the minds of "megalith masons", with their behavioural and artefactal phenotypes embodied in the megaliths themselves. Moreover, it may often be the case that only certain elements of the old Inclusive Phenotype (the artefacts, rather than the skeletons) survive in sufficient numbers for a reasonable reconstruction of the population to occur. Given that the lost skeletons could well have possessed a completely different genealogy anyway, what obstacle does their loss present to the analysis of the European megaliths as the remnants of purely cultural population of architectural and design concepts? Here one need only argue that the Virus approach is the route of convenience.

Although there is insufficient space to discuss another issue in any detail, it is important to note in passing that the existence of "people" (that is, human individuals) is in no way denied by the Viral premise that cultural phenomena may be considered individuals in their own right. The fitness of viral cultural individuals can only be assessed or understood in relation to an analysis of human individuals. No epidemiology of a parasite could afford to ignore the structures of its host. The fitness of a cultural phenomenon is dependent upon the symbolic "micro-ecology" of the minds of different individuals. Why a young person would decide to join the hypothetical "masons" of the Neolithic and so perpetuate the knowledge of how to build megaliths would obviously involve a whole range of factors peculiar to that individual and the various social influences and ideas that they had already been exposed to. While many of the more personal aspects of the individual's mind would be archaeologically unrecoverable, it is conceivable that a range of cultural reproduced phenomena, such as the "aesthetics" of megaliths and the social "charisma" of the "masons", and many of the territorial, dominance related and structural aspects submitted in the megalith discourse so far (Renfrew and Bahn 1991; Hodder 1992), could well be expected to be shared by many individuals of a given community. So while Ingold (1990) has advanced a range of arguments which claim that neo-Darwinian theory disregards and largely deconstructs "the organism" and "the individual", human individuals are as crucial (in understanding the fitness of any given

viral phenomenon), as indeed they are in any variety of neo-Darwinian theory. But a discussion of this issue would be another chapter in itself.

The issues raised in this section could have been discussed in far greater detail than space has allowed, but the aim of this simplified and hypothetical example was not to solve any problem of the evolution of megaliths or oaks, merely to demonstrate that the inclusion of more than one organism under a single phenotype can make neo-Darwinian explanation extremely complicated. The simplest route is to avoid chimaera-like phenotypes such as humanmegaliths and squirreloaks, and try to explain the evolution of one lineage of reproducible information at a time, with *reference to* other lineages, but not as *part of* those other lineages. Finally, and particularly in the case of the archaeological record, it may well be the case that only one population of phenotypes, that of the artefacts, has survived to any substantial degree.

5.5 Three Familiar Conundrums

For those readers still in some doubt as to whether the American form of Cultural Selectionism uses the human phenotype as one of its basic units of cultural selection, it is worth considering how the organismic bias pervades other well used cultural selectionist categories. The "organism's eye view" which pervades the "Inclusive Phenotype" definitions of the "cultural individual" and the "cultural phenotype" has produced other conceptual problems in the approach. Most prominent among these are the idea that cultural evolution is in some sense Lamarckian (or involves "acquired characteristics") (e.g. Boyd and Richerson 1985); the concept of "maladaptive cultural traits" (including its relationship to Rindos' CS1/CS2 distinction) (e.g. Rindos 1985, 1986b); and finally the use of whole "cultures" as an additional unit of analysis to human cultural phenotypes (Durham 1990:188-192). Each of these concepts is either directly or indirectly related to the use of the "organismic" or "human phenotype" premise, and, consequently, each is

anathema in a Cultural Virus framework. This is because when learned neuronal structures become the units of cultural selection, and their motor products become the "individual phenotypes" under analysis, all of the above concepts become largely redundant.

The connection between the idea that cultural evolution involves a process of "acquired characteristics" (and thus a process which is Lamarckian in one modern specific use of the term, rather than in the broader sense of the "Lamarckian orthodoxy" articulated in Chapters 2 and 3), and the Inclusive Phenotype perspective, will be articulated first. While Cultural Selectionism must by definition view cultural change as a neo-Darwinian process in some sense, statements to the effect that cultural change is a process of "acquired characteristics" frequently occur. Thus Boyd and Richerson write:

> "Cultural transmission differs from genetic transmission because it includes the inheritance of acquired variation. Errors in social learning, other random environment effects on behaviour, and systematic, nonrandom variations acquired by learning may be transmitted." (Boyd and Richerson 1985:283).

Other examples of this position include those of Alexander (1979:74); other statements from Boyd and Richerson (1982:329; and 1985:8, especially their notion of "guided variation"); Daly (1982:402); Gould (1980:71); Harre (1979, see Shennan 1991:343); Koerper (1980:463); Leonard and Jones (1987:214); Rindos (1985:69-70; and 1989:6); and Spuhler (1984:13). The brief point to be made is simply that cultural change can only be seen to be a process of Lamarckian "acquisition" if the human individual remains the basic "individual" in question, and if cultural phenomena are considered to be "traits" of those individuals, rather than being individuals in their own right. Once the culturally transmitted neuronal structure and its extended phenotype is considered as much an "individual" of selection as the human individual by which it is acquired, the "acquisition of new traits" process becomes one of "colonisation by other individuals" or "colonisation by new parasites". The result of this process is not a single transformed individual, but a new ecological relationship between two or more individuals. Human cultural inheritance can thus be considered as a special case of the common process

of colonisation, rather than an example of Lamarckian inheritance. So the illusion of Lamarckian inheritance is an artefact of the "organism's eye view" of the Inclusive Phenotype approach, created by the treatment of the human individual as a single inviolable neo-Darwinian individual which acquires "traits", rather than as a susceptible host which "acquires" - and thus is really colonised by, or develops an ecological relationship with - culturally reproduced parasites.

The reader is reminded that there is also the closely related but subtly different issue of whether human innovation itself is ever "Lamarckian" in the first place (see Chapter 2, section 2.3 for an enlarged discussion; and see also Cullen 1993a), as this issue has been ignored above in order to focus upon the "Lamarckian inheritance" issue. Even if we choose to refer to enculturation as a process of "the inheritance of acquired characteristics", through adopting the phenotypic (and not the viral) perspective, we have only established the existence of the "inheritance" half of the Lamarckian equation. There is still the problem of whether the innovative process by means of which they are first "acquired" is ever a process of genuine environmental instruction. To put this hypothetical situation in other words, it is quite possible that cultural transmission really only *enables* the inheritance of variation which is actually "acquired" in a perfectly Darwinian way. This is as much a problem for psychologists as for archaeologists or anthropologists.

As a complete exploration of this issue would be beyond the bounds of this chapter, it is fortunate that psychologists such as Edelman (1989) have already explored the issue in some detail (briefly referred to in Chapter 1, sections 1.2 and 1.3, and to be discussed more fully in Chapter 9). For some time in psychology, the major theoretical alternative to a "realist" or "environmental instructionist" model of perception (which in terms of cultural process is essentially Lamarckian invention/environmental rationalism) was the "nominalist" position, where the symbol was seen as completely random or arbitrary in its relationship to reality (something very much akin to Structuralism). Edelman has shown that Neuronal Darwinism provides a unique alternative to these two major positions, showing how "reality" can be

effectively modelled by the brain *without any process of actual environmental instruction ever taking place.* He provides a convincing account of how the nervous system provides an intrinsically generated and predetermined range of symbols (the primary repertoire), from which particular symbols are selected by sensory experience for their "match" or fit to sensory patterns. This primary repertoire is so vast and diverse, and so "degenerate" (Edelman's word for the fact that more than one symbol may be selected by a sensory experience, and that more than one sensory pattern may excite a match with the same symbol), that its "neuronal space" (my term for the analogue of Dawkins's "genetic space") is practically infinite from a predictive point of view. In other words, the "secondary repertoire" (Edelman's term for the subset of the primary repertoire realised through learning and memory) cannot be predicted (except in the most general of senses) via contemplation of the primary repertoire. The implication of this, which is explored in Chapter 9, is that cultural transmission becomes a basic means of establishing continuity in the secondary repertoire, within the otherwise colossal space defined by the primary repertoire.

The relevance of Edelman's Neuronal Group Selection theory to the matter at hand here, however, is merely that if his theory is correct, then there is no analogue of Lamarckian environmental direction to be found *at any level in the human mind.* The environment can only, at best, provide an heuristic stimulus for a process of self discovery whereby we realise the most appropriate elements we can find in our vast primary repertoire. Moreover, the *repeated* elucidation of concepts of great complexity from the primary repertoire may require the mediation of other minds in which the potential for the concept is already realised - in other words, cultural transmission. If Edelman is correct, notions of Lamarckian instruction will become theoretically redundant, if not completely untenable.

The interesting question which would then arise would go something like this: What kind of neo-Darwinian process is human invention, then, if it can so easily give the illusion of Lamarckian environmental instruction? If Lamarckism *per se* does not occur, what is the specific nature of the pseudo-

Lamarckisms that do occur in culture? Finding an answer will involve a careful deconstruction of the cultural processes which have been submitted as candidates for Lamarckisms or quasi-Lamarckisms (such as Carneiro's appeals to scientific discoveries above, Boyd and Richerson's "guided variation", etc.), to show that they are really not Lamarckian at all.

Although an in depth elaboration of this issue is not necessary to this part of the study, I would like to briefly speculate about a possible solution to the problem of "pseudo-Lamarckism". So far, most attempts to "neo-Darwinise" human invention have involved analogies between mutation and errors in cultural transmission; hence Boyd and Richerson's interest in "guided variation", a process which does not involve transmission error, but which results from individual trial and error of an existing cultural pattern (Shennan 1991:338). But perhaps the key to identifying the underlying neo-Darwinian logistics of human creativity and intentionality may actually lie with models of *selection* as much as with models of mutation. Firstly, it must be recognised there is no reason why errors in memory or retention may not be considered as much copying or reproductive errors as are those of cultural transmission, as individual learning itself involves a process analogous to differential reproduction or selection, within the brain (Edelman 1989 *passim*). Secondly, it may be the case that conscious attempts to solve a problem - conscious attempts to innovate - do not actually generate solutions, but actually represent a deliberate intensification of selection in small populations of ideas in our memories, in which retention errors are constantly occurring (Cullen 1993a). Thus, when a mind is under stress, it may be able to run a kind of artificial "punctuated equilibrium" scenario (a psychological equivalent of the model of Eldredge and Gould (1972)), speeding up the evolutionary process, and "fixing" new versions of old ideas very rapidly in a small concept population, but never actually directing the course of the "mutations" themselves. The error rate may even be deliberately but temporarily increased in the hope of hazarding upon unforseen conceptual solutions. This series of random culturally constrained innovations and rapid fixations in our minds might occur so quickly that it may be a *fait accompli* even before it emerges as a coherent solution in the conscious train of thought of the inventor, and,

just as in the genetic process of palaeontological record, it may appear very suddenly in the archaeological or historical record. In this way, the key to incorporating "Pseudo-Lamarckian" inventions into a neo-Darwinian framework may be to view such events as a complex series of small random innovations and concentrated selections or retentions occurring in a short space of time, as cases of psychological "punctuated equilibria". Such a route may well prove more successful in explaining "Pseudo-Lamarckian" inventions than current attempts to dismiss such events as single, random "macro-mutations", rather like cultural equivalents of Goldsmidt's "hopeful monsters". To summarise this speculative point, it may be that special models of intentional *selection* (rather than special "Lamarckian" models of intentional *mutation*), are what is actually required to model human intentionality and individual agency in neo-Darwinian terms.

In any case, the main point to note from above is that notions of the so called "Lamarckian" nature of some cultural processes are directly linked to the false assumption that human behaviour patterns must be classified as part of the organismic phenotype. Nor is this the only corollary of a the Phenotypic perspective. For example, the idea that cultural phenomena can somehow be "maladaptive", while nonetheless spreading successfully over an indefinite period of time, is also an artefact of the use of the "human individual" as an analytical unit of cultural change. Richerson and Boyd provide a good example of this "maladaptive" concept, when, with reference to Cavalli-Sforza and Feldman (1981), they write:

> "Our models assume that selection and learning favour the same traits. This is not necessarily true. Sometimes, the rules that govern learning and other forces on cultural evolution derived from individual choice may act in the opposite direction from natural selection. For example, certain bad habits, smoking, drinking, and the like, are easy to learn but tend to result in elevated mortality (Cavalli-Sforza and Feldman 1981:342-346). To the extent that they are imitated, systematic variations that are negative with respect to fitness must cause selection against them by definition." (Richerson and Boyd 1985:83).

Other examples of the idea that cultural phenomena can somehow "maladapt" (i.e. be selected in spite of reducing fitness) include statements from Dunnell (1980:63); Fletcher (1977:46); Leonard and Jones (1987:213); and

Shennan (1991:198). The idea is also implied by the CS1/CS2 distinction made by Rindos (1985:72-74; and 1986b:321). The implication of the notion of cultural "maladaptation" is that there is more than one way of assessing the fitness of a cultural phenomenon, once in relation to its effect on its own replication, and once in relation to its effect on the fitness of the human individual it occupies. Yet it is established neo-Darwinian orthodoxy (as also evoked in the critique of cultural Sociobiology in Chapter 3) that selection can only favour structures which have a positive effect on their own reproduction, in other words, that only the former of these two criteria need ever be applied [e.g. Darwin's various statements to the effect that Natural Selection cannot produce any modification whatsoever in one species solely for the good of another (1872:67,163,219); Wallace's discussion of how plants benefit from bearing edible fruit (1889:306-309); and throughout Dawkins (1989)]. Thus any notion that cultural phenomena can somehow become "maladaptive" through "maladapting" over a period of time implies that their "fitness" is not always being assessed in relation to the reproductive success of the cultural phenomenon itself. This is neo-Darwinian nonsense, and more specifically implies the cultural equivalent of truly genetic altruism (in other words, genuine memetically-altruistic altruism toward human genes displayed by cultural phenomena). In Cultural Selectionism, and particularly the Phenotypic position, the very use of the term "maladaptive" often implies a subtext of "maladaptive *traits*". In the absence of explicit accounts of how such "traits" are maladaptive in relation to their own reproduction, the term itself is an artefact of the Phenotypic position. It would be so much simpler to assess the Darwinian fitness of artefact function and reproduction purely in terms of artefact futures.

In a Cultural Virus framework, no cultural phenomenon which positively affects its own likelihood of social transmission could ever be referred to as "maladaptive" - even if it is universally lethal to its host - unless it somehow impedes its own social transmission. Its lethality cannot affect its fitness unless in doing so it somehow affects its likelihood of social transmission (ie. unless its negative effects on people indirectly affect its own psychological niche, as it were). The fitness of a viral neuronal structure is exclusively

defined in terms of its likelihood of cultural transmission, and its effect on the fitness of its hosts has no greater relevance than does the affect of any parasite on the fitness of its host. It is not very meaningful to argue that a parasite which damages its host's reproductive opportunities, while enhancing its own, is a "maladapted trait"; it remains, by virtue of its genealogical independence from the host, an "adapted individual" living inside another. In other words, a smaller, adapted whole, rather than a maladapted part. Once alcoholism, for example, is defined as a parasitic viral phenomenon, and a neo-Darwinian "individual" in its own right, it cannot possibly be "fit to spread" and "maladaptive" at the same time, without violating Darwin's original principles of selfishness referenced in the above paragraph. Once we abandon the use of the human individual's cultural phenotype as a unit of cultural selection, it is no more meaningful to call alcoholism a "maladaptive" human trait, than it is to call a tick (a parasitic arachnid) which causes paralysis in humans a "maladaptive trait" (for example by mistaking it for a mutant wart or hair follicle). The absurdity of externally defined "maladaptedness" becomes most obvious when it is phrased in terms of predator/prey relationships, as in, say, a neo-Darwinian explanation of Lions which defined all successful predations on Zebras as "maladaptive" Lion behaviour, due to the negative effect it had on Zebra reproduction.

Fletcher (1977 *passim*) provides an interesting intermediate position, where he carefully allows separate lines of descent and adaptive criteria for artefacts (material behaviour) and humans, yet persists with a vestigial (and perhaps purely heuristic) use of the "maladaptation" concept with reference to "active" human behaviour. In this position artefacts which are harmful but serve their own replication are not termed "maladaptive", yet they may still force individuals from the communities which maintain them to behave in a way which *is* "maladaptive". In any case, the main point to note is that the "culturally adaptive/genetically maladaptive" paradox, like the notion of cultural "acquired characteristics" above, is largely an artefact of the "organism's eye view" or Inclusive Phenotype view of adaptation in cultural phenomena.

The last concept which reflects the American cultural selectionist focus on the human individual is the use of a second important analytical category, namely the use the whole "culture" as the basic unit in which cultural evolution occurs (as distinct from the use of the human individual's cultural phenotype as the unit of cultural selection). In other words, the "culture" operates as the nearest cultural equivalent to a neo-Darwinian population. Durham provides a quintessential example of this, in a section of his 1990 overview of Cultural Selectionism entitled "What Evolutionary Culture Theory Is":

> "Evolutionary culture theory (ECT) is a collection of arguments seeking to explain the "descent with modification" of human cultures. It is, in a word, a theory of cultural phylogeny. It seeks answers to questions like the following: What are the predominant mechanisms of cultural change? How does a new culture get started? How and why does an existing culture assimilate or die out? What is the particular historical branching pattern or "cladogram" of human cultures? And why have there emerged so many different cultures in the course of the human career?" (Durham 1990:190).

Similar statements, where the "culture" is the default level at which cultural phenomena are discussed, include those from Clarke (1968:164-165); Childe (1951); Kroeber (1948:260); and Rindos (1986b:321; 1989:7-9). Since this "whole culture" concept, and the related but distinct "society as an organism" concept, have been integral parts of non-Darwinian (and therefore, through the arguments of Chapter 2, Lamarckian) philosophies, such as those of Spencer (Rindos 1985:66-69) and of Durkheim (Betzig 1986:67-69), its almost monolithic persistence in Cultural Selectionism would seem to be somewhat counterintuitive. It is all the more unexpected when one is reminded of the fact that the validity of any given category of analysis in neo-Darwinian theory, for the reasons outlined throughout this chapter, essentially depends upon its having some relationship to units of reproduction or transmission. Yet few would argue that the "culture" is a unit of transmission, it being a unit far too large to be comprehended and reproduced all at once.

I believe, however, that the *persistence* of the "whole cultural system" concept in Cultural Selectionism (although this is by no means the only framework which employs it), is due to the fact that the use of the

"Phenotypic" or "organismic" perspective provides indirect support for its retention. In the beginning of this section, a quote from Rindos (1989:7) was used as an example of the use of the individual as a unit of selection. Rindos went on to say however, with reference to Ghiselin (1974, 1987) and Hull (1976), that it was the population which evolves. Logically, then, an approach which uses human individuals as the units of selection will view human populations as the entities which evolve. Correspondingly, any approach to culture which uses the human cultural phenotype as its unit of selection would understandly use populations of those human cultural phenotypes as their basic evolving entity. A human "culture", then, seems to be used in Cultural Selectionism as the nearest cultural correspondent of an organismic population. If a "culture" is a population of human cultural phenotypes, one would expect that selection can occur within it, and this is indeed the case. Rindos expresses this idea when discussing his distinction between "demic cultural selection" - where the human group associated with a culture undergo differential reproductive success in relation to other groups, due to genetic advantages conveyed to them by their cultural tradition; and "symbolic cultural selection" - where ideas are selected within a culture on the grounds of "enculturated rules of meaningfulness". Thus the second process produces selection within a "culture":

> "Another reason for clearly demarcating cultural and natural selection is that a second kind of selection occurs within the cultural system. This process will be intimately related to any demographic effects arising from variation in cultural behaviour. Cultural selection of the second kind is more concerned with processes governing the generation, interaction, and selection of traits within a given culture. This "internal" or "symbolic" selection has certain formal similarities to the concept of "transmission biases" as used by Cavalli-Sforza and Feldman (1981) or Boyd and Richerson (1985). It differs in that the variant forms that a symbolic system generates will be so numerous and, initially at least, so highly randomised in terms of the selective pressures existing within the society that selection within the system (perceived according to enculturated rules of meaningfulness by the members of that culture) will be more important than the specific nature of a transmission bias in determining the direction cultural evolution will take." (Rindos 1986b:321).

At least at the superficial level tapped by such brief attention, it appears that there might be some connection between the use of the "whole

culture" concept, and the idea that human behaviour must be considered to be part of the human phenotype. A culture usually corresponds to some kind of human population; logically, then, any framework which employs the human individual "cultural phenotype" as its unit of selection, would use the "cultural population" as its evolving entity. These "cultural populations" are presumably the "cultures" which form the unit of analysis. The "culture" is the cultural entity which most closely corresponds to a human population, and is therefore, an indirect corollary of the Inclusive Phenotype perspective.

But once the human individual is rejected as the basic unit of cultural selection, and the viral neuronal structure put in its place, a very different "cultural population" is implied. The cultural population is defined on a much smaller scale, which may even, at times, exist in the head of a single individual. Groups of ideas, defined on the basis of similarity, become the most appropriate level at which to define the cultural "population". In other words, a whole "culture" then consists of hundreds of different populations of different "species" (used very loosely) of viral neuronal structures (or ideas), each of then distributed throughout some subset of the human individuals of a community (see Chapter 9 for a series of related discussions). Once it is recognised that a human cultural phenotype is not a unit of cultural reproduction, we can also say that a human "culture" is not a cultural "population", but a vast ecological assemblage of many different kinds of cultural phenomena, many of which are best classified into a series of sympatric populations distributed to varying extents within the larger "culture". At the other end, we can bring the population concept "down", and remark that at times a single mind might contain a whole "population" of ideas, consisting of the awareness of, say, a dozen different definitions of the word "dialectic", in a community where no other individuals had any awareness of that concept.

Each of the three above concepts, then - "maladaption", cultural "Lamarkcism", and the use of the "culture" as the main unit of cultural evolution, can be considered to be a corollary of the use of the "cultural phenotype" of the individual as an analytical unit of cultural selection. Once

one redefines the "cultural individual" as a viral neuronal structure (ie. grant individuality to what the Phenotypic position would call cultural "traits") the discourse which surrounds notions of maladaptation and Lamarckism becomes largely unnecessary. Likelihood of cultural transmission becomes the only criterion of "fitness" for cultural and psychological phenomena, and "acquired characteristic" scenarios become simple cases of endemic or epidemic colonisation. And, finally, the use of the "culture" as the basic unit of cultural evolution (in contrast to the use of a great number of small but sympatric populations of similar concepts, behaviour patterns, or manufacturing techniques) may also be seen to be a logical corollary of the "Phenotypic" or "Organismic" perspective. Moreover, by virtue of its dependence upon the notion that the human "individual" is the basic unit of cultural transmission, the concept of a "culture" would appear to be an awkward and unfaithful "cultural equivalent" of the neo-Darwinian population.

5.6 Concluding Remarks

In conclusion it can be seen that the established cultural selectionist traditions place great emphasis on the "organism's eye view" or Inclusive Phenotype approach to cultural change. Cultural change is certainly allowed far greater independence than it ever was in Sociobiology, and is never reduced to a point where it merely functions as "man's extrasomatic means of adaptation". Yet ultimately the effect of cultural behaviour on organismic survival remains disproportionably significant in the process, and cultural phenomena, despite their unique genealogical independence, are still viewed as "traits" of human individuals. The cultural equivalents of various neo-Darwinian genetic process concepts used by the approach reflect this primary focus in a variety of ways which were outlined in this chapter.

The basic goal has been to show that there is the potential for two quite different cultural selectionist positions, the "Phenotypic" and "Virus" positions, and that the differences cannot be ignored by anyone hoping to use - or to come to some understanding of - modern neo-Darwinian culture theory.

Most importantly, the Cultural Virus Critique of Cultural Selectionism highlights the fact that neo-Darwinian anthropologists and archaeologists may be using wholly inappropriate "cultural equivalents" of familiar neo-Darwinian concepts; a whole series of ill considered and awkward "biocultural chimeras" which bear no logistic relationship to the real units of cultural transmission (however variable these bits and pieces may be). Neo-Darwinism does have great potential for the explanation of the evolution of culturally transmitted phenomena, but before this can be realised a threshold must be crossed. We must recognise that any structure which exhibits genealogical independence from the human phenotype cannot properly be considered a "trait" of that phenotype; it is instead a "parasite". The quality of genealogical independence is the only consistent means of distinguishing traits and parasites in the first place. Because of this, culturally transmitted phenomena cannot be simply tacked on to the established neo-Darwinian discourse where "individuals" are human phenotypes, and "populations" are merely constellations thereof. We must develop new and purely "psychocultural" models of "individuals" and "populations", ones more appropriate to the kaleidoscope of socially transmitted phenomena which constitute cultural fields of enquiry. Once the perimeters of the cultural "individual" and the "cultural population" are redefined, it follows that the concepts of "phenotype", "environment", "reproduction" and "adaptation" must all be redefined as well.

If we jointly summarise the last three chapters, it can be seen that the nature of the neo-Darwinian resurgence can be more clearly understood through the identification of two instances of "philosophical divergence" which have occurred in its relatively short history. On the one hand we have the major distinction between Sociobiology and Cultural Selectionism (Chapter 3), which is also discussed elsewhere (Blute 1979, Cullen 1990, 1991, Rindos 1986b, *inter alia*), and which was not given attention in this chapter other than in the definition of Cultural Selectionism in the first part of section 5.3. On the other hand we have the secondary divergence which exists *within* Cultural Selectionism, that between the "Phenotypic" or "Organismic" position which predominates in American Cultural Selectionism (and which, for that matter, persists on the Sociobiological side of the previous distinction), and the

Cultural Virus position. It is this secondary divergence, between virus and phenotype, which has been the focus of this chapter, which has taken the form of a critique of the "Phenotypic" position from the point of view of the Cultural Virus position.

As stated briefly near the beginning of the chapter, the Cultural Virus Critique has not questioned the fundamentally selectionist philosophical identity of Cultural Selectionism, as was done with Cultural Evolutionism and cultural Sociobiology. Moreover, it was stated at the beginning of the chapter that, whatever its problems, Cultural Selectionism has the potential to fill the theoretical gap left by the "untried theory". The theoretical changes which I believe are necessary for Cultural Selectionism to fulfil this potential, are not, however, undertaken until Chapter 9. In the next three chapters, Chapters 6, 7, and 8, attention is turned to the objective of subjecting *Cultural Selectionism as a whole* to a general test. In other words, the goal of the next few chapters is to demonstrate the need for a cultural selectionist framework *of some sort*; the potential that exists for a move from a Phenotypic to a Viral perspective, *within* Cultural Selectionism, is not focused upon until the final chapters of this study.

PART II

SELECTIVE PROCESS, GENEALOGY AND CULTURAL PHENOMENA

6

PHYLOGENETIC CONSTRAINT AS A NEO-DARWINIAN SIGNATURE

Overview

The second part of the project begins with an attempt to devise a general test for Cultural Selectionism. In this chapter, the first part of the test, an attempt is made to find a pattern in biological data which exemplifies selective process, so that, in succeeding chapters, we may know what to look for in culture. Firstly, a number of theoretical assertions about the nature of selective concepts are discussed. Secondly, their processual implications are explored, using a series of schematic representations or "visual motifs" of evolutionary concepts. Finally, patterns of variation in a body of biological data (human cranial material) are shown to display a distinctive signature which is consistent with the predictions of selective process. This pattern is termed "heritage constraint".

6.1 A Unique Process

As already implied above, this chapter does not so much seek to test Cultural Selectionism itself, but rather to develop a testing procedure for the presence of a selective process in any system. This guideline will then be used as a "litmus test" for selective process (the presence or absence of heritage constraint) in order to test for the presence of selective process in patterns of cultural data in Chapter 8. The immediate goal is therefore to establish the Darwinian "litmus test", and then to give it a trial run on a system known to conform to neo-Darwinian principles, in the hope that it will

yield a *positive* result. After this the "litmus test" is given a trial run on two known "non-Darwinian" systems, in the hope that it will consistently yield a *negative* result.

Developing the "litmus test", however, must first involve a careful consideration of the fundamental qualities of selection. In 1967, Wright summed up the nature of selective process in the following words:

> "Darwinian process of continued interplay of a random and selective process is not intermediate between pure chance and pure determination, but in its consequences qualitatively different from either." (Wright 1967:117).

The implications of these words are profound. It would appear that *neither* of the two "default" patterns of human understanding - complete randomness or complete determination - are appropriate models for a neo-Darwinian system. However, for better or for worse, like many other more lengthy contrasts of evolutionary theory (such as the several dozen papers and books discussed in the first five chapters of this study) the above contrast is in a relatively abstract form.

In other words, while many readers will be familiar with basic terms of evolutionary theory, such as "selective", "determined", and "pure chance", the empirical patterns implied by these terms are not self-evident. The role of the next two chapters is to translate these theoretical statements into three empirically distinguishable spatial patterns. In this way a clear picture can be established of what patterns of diversity ought to be found in cultural systems, if cultural process is truly selective, and conversely, what other patterns ought to be found if it is not.

It is clear that Wright is employing a fundamental classification of evolutionary processes, one of pure chance, and one of pure determination; but neo-Darwinian process is defined only as neither of these. Yet if neo-Darwinian process is so unique, one might expect that this uniqueness would be manifest in its products. It ought to be possible to find some pattern which physically embodies Wright's comment, a pattern which is "neither determined, nor random, nor yet intermediate between the two", a pattern

which can be used as a recognisable signature of neo-Darwinian process wherever it operates. In this chapter an attempt will be made to define a pattern for this "qualitatively different" process in the context of large matrices of data from biological systems in which selective process is *known* to occur (for a similar procedure, see also Hendy and Penny 1986).

Any one of the many organisms well distributed throughout the human world could supply a suitable data set for this objective. The gene pools and morphologies associated with any common domesticate, for example, would all be examples of systems with a similar geographic distribution to culture, systems in which selective process is known to occur. If cultural process is selective, there should be a strong thematic similarity between the biogeography of faunal assemblages and the geography of cultural assemblages. However, the bone assemblages with the most convenient and global distribution, invariably present where cultural phenomena are present, and invariably absent where they are absent, are humans, and for this reason human osteological remains were chosen as the "known" neo-Darwinian system. W.W. Howells' world wide sample of human cranial measurements will be analyzed in the later sections of this chapter, with the aid of R.V.S. Wright's multivariate analysis software. A Principal Components Analysis is undertaken of the 2524 crania, in which the computer performs a rapid and thorough comparison of each cranium with each of the others. What will become clear is that human cranial variation is governed as much by phylogeny as by environment. To control the possibility of the pattern merely being an artefact of principal components analysis, an artificially generated data set of random numbers is subjected to the same transformations, in the next chapter. This pattern is then contrasted with the patterns to be found in data from a "purely determined" system. Then, in Chapter 8, patterns in cultural data will be compared with the newly defined "neo-Darwinian" pattern, to provide some indication of whether cultural evolution is a random, Lamarckian, or neo-Darwinian process.

It is important to note that the cranial analysis of this chapter is not intended as any sort of analysis of human diversity, such as those of Durham

(1990, 1991) or of Cavalli-Sforza et al (1988) or Cavalli-Sforza (1990). The data is employed merely to yield an example of biogeography in a neo-Darwinian system, in order to establish a thematic hypothesis for the testing of Cultural Selectionism in general, later in the study, and in the context of cultural data.

Showing that a particular distinctive pattern exists in both faunal and cultural assemblages is only part of the challenge, however. What has to be demonstrated first is that this pattern is an integral and inevitable product of the selective process. For this reason the next section is concerned with establishing the nature of selective process - analyzing the structure of selectionist theory - so that its links with the selective products can be convincing demonstrated. Selectionist theory and biogeography must be shown to be intimately interrelated, not just to coexist.

The next two sections are devoted to this task. Heritage constrained biogeography is shown to be integral to selectionist theory both structurally and historically. Firstly, in section 6.2, heritage constraint or "descent with modification" is shown to be an unavoidable implication of the structure of selectionist theory itself, along with its implication (in cultual contexts) of complete cultural specificity and relativism. Although most Darwinian anthropologists would be well aware of this fact, many non-Darwinian anthropologists are not. Postprocessual archaeology, for example, has made cultural relativism and historical or contextual specificity its *raison d'etre* (e.g. Hodder 1989), while at the same time condemning "naturalism" (Shanks and Tilley 1987a) as though it were the enemy of any sort of cultural relativism. In selective process, phylogenetic constraint is a direct result of differential reproduction, as differential reproduction constantly produces a population with more and more recent (and fewer and fewer) common ancestors. Secondly, in section 6.3, it is argued that not only is "descent with modification" a logical ontological extension of selectionism, but selectionism is in turn a logical theoretical extension of the ontology of descent. The "descent with modification" heritage pattern elucidated by Darwin in the voyage of the *Beagle* is far more than just an incidental empirical accompaniment to his theoretical inventions. In fact, it can be argued that the *Beagle* observations

were crucial in the development of selectionism in biology, as anthropologists such as Freeman (1974) have established, and as will soon be seen in sections 6.2 and 6.3. Moreover, the notion of descent was used by Darwin not only to promote selective reasoning, but also to differentiate his evolutionism from Lamarckian evolutionism (Desmond and Moore 1992:312, 315). This would suggest that cultural heritage could be the key to overturning the implicit Lamarckian orthodoxy in cultural fields of enquiry; and the issue is discussed in Section 6.3.

After the link between heritage and selectionism has been established, the final section of the chapter (6.4) articulates the particular form the heritage pattern tends to take in a global sample of human crania. Human crania are used as a convenient example, but any global sample of faunal remains would have been just as effective. Thus, in this final section, the general principle of "descent with modification" is translated into a specific "selectionist hypothesis" or "neo-Darwinian signature" for a global sample of cultural assemblages. If cultural change is in fact a selective process, its "biogeography" should be similar to the biogeography of global samples of faunal assemblages. The patterns of biogeography exhibited by such faunal assemblages should therefore stand as the basic "cultural selectionist" empirical hypothesis for cultural assemblages.

6.2 The Nature of Selective Process

Even without reference to biogeography it can be shown that heritage constraint is in fact an inescapable part of neo-Darwinian theory itself (for example Dawkins 1982:30; Foley 1985:225 as below; Mithen 1989:485). While adaptation may be considered in some sense as a process involving a causal relationship between the external surroundings of a replicator and its chances of replicative success, this causal relationship is in no sense one of determination. Foley (1985) has summarised the constraints operating on natural selection, constraints which prevent the environment from "determining" organismic structure. Foley's summary was in turn derived from

The Extended Phenotype (Dawkins 1982). Foley calls these constraints "factors inhibiting perfection of adaptation or optimality" and outlines them in the following words:

> "(1) There may be a time lag between environmental conditions and the effect of natural selection on a population. As a result of this time lag, although there is a tendency toward optimality, perfect adaptation would not actually be achieved.
> (2) Selection can only operate on existing material or behaviour, which may constrain adaptive fit in two fundamental ways. First, selection can only operate in relation to the genetic variation present, and this will set independent constraints on the optimality of an adaptation. And secondly, the history of an organism - its phylogenetic inertia - will set limits on the modifying power of natural selection, and thus the attainment of perfect adaptation.
> (3) As stated above, organisms tend toward optimality, but their component parts do not. Organisms are under a range of selective agencies that affect their reproductive success. Many of these conflict with each other, preventing the achievement of some absolute perfection in nature.
> (4) Some environments are inherently unpredictable. As optimality often involves fine tuning to the environment, in some circumstances this will be an impossible goal." (Foley 1985:225).

Foley uses this summary to show how models of optimality by no means predict the occurrence of perfect adaptation, or perfect correlation between environmental conditions and the structure of individual organisms. It is worth noting that all the above constraints are even implicit in the most orthodox or "hardened" neo-Darwinian synthesis, where evolutionary process is seen simply to proceed via the natural selection of random mutations only; additional constraints come into play with the main additional principle of the "expanded" neo-Darwinian synthesis (Gould 1983), genetic drift or random selection.

Foley's considerations are important for the use of the principle of adaptation, because they mean that optimality principles are about a tendency toward optimality, rather than its achievement. However, the principles also provide a clue to the nature of environmental correlation in a evolving system which conforms to the neo-Darwinian synthesis, and a means for distinguishing these correlations from the kinds which occur in a non-selectionist or Lamarckian system. These differences should prove important for the

translation of neo-Darwinian principles into an hypothesis about spatial or temporal patterning.

But as already suggested, the goal of this section is to show that heritage constraint is not just demonstrable in biogeography but that it is also an integral part of the concept of Natural Selection. By contrast, as will be laid out in the next chapter as an alternative hypothesis, Lamarckian adaptation actually directs or engenders appropriate responses in organismic structure and therefore cannot be constrained by the factors which limit adaptation in a selectionist system. For the moment, however, the important thing is to demonstrate why it is that heritage is a logical corollary of neo-Darwinian adaptation. Statements from evolutionary biologists such as Foley and Dawkins above clearly show that phylogenetic constraint is known to be part of modern selectionist thought. In order to demonstrate why I will use not theoretical statements, but a series of heuristic diagrams or visual motifs.

To do so the reader must first be introduced to a simple visual language. Consider a diagram of a population of seven asexually reproducing single celled organisms, in which each row of objects represents a single generation **(FIGURE 6.01)**. Each lineage is represented by a column, with each shape along that column one generation of that lineage. Thus the total diagram represents a population of seven organisms viewed through eight generations, with the arrows showing the direction of time, heredity, and reproduction. In this simple idealised population each organism reproduces asexually, at the same time, and at the same age. For the moment, allow each organism to leave behind one and only one surviving offspring (in other words, allow inheritance to be perfectly symmetrical). These regularities make the diagram much easier to "read", and the visual language easier to understand.

If an attempt is made to bring the visual motifs closer to reality, they become more difficult to follow. For example, in some populations the generations of each lineage are not synchronous; each organism reproduces at a different time **(FIGURE 6.02)**. This can be represented by sliding the lineages up or down in relation to each other. Similarly **(FIGURE 6.03)**

organisms often reproduce at a range of ages, and the space between each generation can be shortened or lengthened according to the age at which the last generation produced its one surviving descendant. It is even possible to idealistically represent patterns of heredity in a sexually reproducing population **(FIGURE 6.04)**, where each surviving offspring receives genetic material from two lineages instead of just one (once again an entirely symmetrical pattern of heredity is assumed for convenience).

For the sake of simplicity the idealised asexual population will be used for the purpose of illustrating the essentially descent based nature of neo-Darwinian theory. In the next chapter, the same visual system will be used to illustrate the nature of the Lamarckian view of a changing population. It is important to remember that nothing whatsoever of the environments of these idealised populations is represented in the visual motifs. The diagrams of the next two chapters merely contrast the internal logistics of neo-Darwinian and Lamarckian processes, although external factors will be referred to in the discussion of some of the diagrams.

Consider the most basic neo-Darwinian process, that of adaptation, in the context of this idealised population **(FIGURE 6.05)**. Selection cannot occur in a population unless variation is produced through mutation. Although mutation is in fact more common, let us concentrate on a single mutation for the moment. The mutation is represented symbolically by a change from white to black, and the location of the mutation represented by the circled point between two generations (always on the left for simplicity, although it could occur in any lineage). Unlike adaptive transformation in Lamarckian evolution, small scale changes occur as intergenerational events, and not as a continuous process of change during the lifetime of each individual. Let us assume that some change has occurred in the environment at some time in the past, a change which now confers a reproductive advantage to darker mutations. It is worth noting that in neo-Darwinian adaptation the "perfect" mutation (the fully black form) would most likely never occur, due to the heritage constraints which already exist in the population **(FIGURE 6.06)**. A series of milder mutations would be the more likely scenario.

However we choose to represent the process, no change at all will occur until the relevant mutation occurs. Then the population will transform, not as a result of all lineages transforming together, but as a result of the differential reproductive success of the mutant lineage, within the population. The final generation is now derived exclusively from the mutant lineage, and the phylogenetic heritage of the population has narrowed. Thus we can say that adaptation represents a process by means of which a population adapts, due to a reduction in its phylogenetic heritage, and a corresponding increase in its heritage constraint.

Alternatively, the process of drift can be represented in this idealised population and its eight generations. Drift is a gentler form of "random" selection where a small amount of differential reproduction occurs in each generation for logistic reasons unrelated to the morphology of individual organisms. But once again, until some variation exists in the population no drift can occur, **(FIGURE 6.07)**, as the forms randomly eliminated in small proportions (represented on the right) are indistinguishable from the forms which are randomly selected in small proportions (represented on the left).

However, if we represent a continuous series of mutations on the left **(FIGURE 6.08)**, as a recurring event which continually injects new variation into the population, and these mutations are all allowed to be essentially neutral, drift becomes a possibility. At first variation simply expands as the more common forms are more likely to be eliminated (although not as uniformly as implied in this visual motif). Once the population becomes "saturated" with variation **(FIGURE 6.09)**, the morphology of the population undergoes a stochastic wander, which can only be crudely represented in this stylised visual language.

Note however that once again each time a form is eliminated due to the random selective effects of drift, heritage is lost from the population. Every form ever produced is invariably a derivative (via mutation) of the status quo ante. New forms which bear no relationship to the population are never generated by the environment, and thus no matter how much the population

changes due to either or both of the selective effects, adaptation or drift, the environment can only ever enhance the heritage constraints of the population.

This subtractive aspect of selective process is most dramatic in an asexual population (except where bacterial virion exchange occurs, see Sonea (1988)), where a new trait cannot spread to other lineages before they are eliminated from the population **(FIGURE 6.10)**. If we return to a situation where a particular mutation (represented by the black form) is extremely advantageous, this lineage could entirely replace the other lineages. But the trait represented by the black shading is not the only aspect undergoing differential reproduction; the aspects represented by the oval shape are also reproductively successful, simply by virtue of association. All the lineages with other shapes that existed in the population before the shaded mutation are eventually eliminated, not because oval is better, but purely because unlike the oval shaped lineage they were not associated with the new trait. Thus the heritage of the population becomes restricted to the heritage of a single lineage, due to the effect of one mutation.

As already briefly suggested above, patterns of heredity are more complex (at the organismic level) in a sexually reproducing population **(Refer back to FIGURE 6.04)**. Adaptation still consists of a constriction or narrowing of the common heritage of the population in question, but recombination allows new genes/qualities to spread from the lineage in which they originated, to other lineages **(FIGURE 6.11)**. Here the shaded mutation occurs in the fifth lineage, but spreads throughout the population in six generations. This recombinatory potential of sexual reproduction reduces the possibility of elimination or reproductive success purely on the grounds of association **(Refer back to FIGURE 6.10)**. A sexually reproducing population with the very same starting point **(FIGURE 6.12)** can "share" a mutation once it occurs, and much less of the existing variability - and its associated heritage - is eliminated as a result of the reproductive success of the new mutation. The mutation appears in the first lineage, and reassociates with all the structures of the other lineages before they are eliminated.

In all of these permutations of selective processes it is clear that not one of them ever "escapes" the heritage of the population concerned. Heritage constraint is a logical extension of selectionist theory itself; the link between selectionist theory and heritage constrained biogeography is therefore not just an *ad hoc* appropriation, but an integral relationship.

There is one more auxiliary reason for this, and it concerns the concept of mutation, since this is the only source of the variation upon which the purely subtractive process of selection acts. Mutation is not purely "random" in the mathematical sense of being equally likely in all directions.

For example, the mutations "thrown up" by a population of marsupials in Australia do not constitute a random sample of mammalian diversity, as mutation is not perfectly random (Rindos 1985:70). There is no evidence that any of the placental mammals of Australia were derived by mutation from the "marsupial majority". They are all apparently recent arrivals from overseas, such as the Dingo, a *Canis* species which arrived during Holocene times. We can therefore conclude that in the many millions of years the marsupial population of Australia and New Guinea (more precisely Sahul, or "sub-Wallacian Meganesia") very few indigenous mutations producing a placental mammal occurred. There was a great deal of evolution, and a plethora of complex and efficient adaptations, but all the later forms clearly exhibit evidence of being derived from marsupial ancestors. The huge variety of marsupial "inventions", the "unique derived traits" of Sahulian faunal assemblages, exemplify marsupial heritage as much as the more conservative forms. Far from beginning to approach European faunal assemblages by the time of European contact, Australian fauna probably contained an even larger number of unique adaptations than in earlier geological periods.

There is insufficient space to illustrate this assertion of uniqueness with descriptions of say, the unique aspects of koalas, macropods, wombats, and dasyurids, or to detail why marsupial wolves are not wolves, and marsupial cats are not cats. The point is merely that the complete absence in Australia of the vast majority of common placentals graphically illustrates the fact that

mutation does not randomly sample the universe of all mammalian possibilities. Mutation has been unable to transcend the heritage constraints of the Antipodean continent, unable to transcend the original "founder effect" of the first marsupial populations.

Thus heritage constraint can be considered to be a logical empirical consequence of an ongoing selective process. In the next section the historical importance of heritage constraint is explored, in terms of the role it played in Darwin's innovative thesis. An attempt is made to add to the arguments already made for the integral relationship between "selectionist theory" and "heritage constrained facts". Not only can the empirical phenomenon of heritage be inferred from the selectionist theory, as shown in this section - it also works in the reverse. Selectionist theory can be elucidated, or at least inspired, by the empirical phenomenon of pervasive heritage constraint itself. This second process was arguably a factor in Darwin's arrival at the principle of Natural Selection, and his decisive rejection of prevailing Lamarckian orthodoxy.

6.3 Darwin's Voyage As A Global Elucidation of Heritage Constraint

Derek Freeman has stressed that the biogeographical and palaeontological data compiled by Darwin during the voyage of the *Beagle* were crucial in leading him not only to the fact that evolution 'of an unspecified nature had occurred, but to the principle of natural selection itself. Freeman quotes the following passage from Darwin's autobiography in order to convincingly demonstrate this point:

> "During the voyage of the *Beagle* I had been deeply impressed by discovering in the Pampean formation great fossil animals covered with armour like that on existing armadillos; secondly, by the manner in which closely allied animals replace one another in proceeding southwards over the Continent; and thirdly, by the South American character of most of the productions of the Galapagos archipelago, and more especially by the manner in which they differ slightly on each island of the group; none of these islands appearing to be very ancient in a geological sense. It was evident that such facts as these, as well as many others, could be explained on the supposition that species gradually

Figure 6.01: *Heuristic Diagram of an Idealised Population of Seven Individuals Viewed Through Eight Generations.*

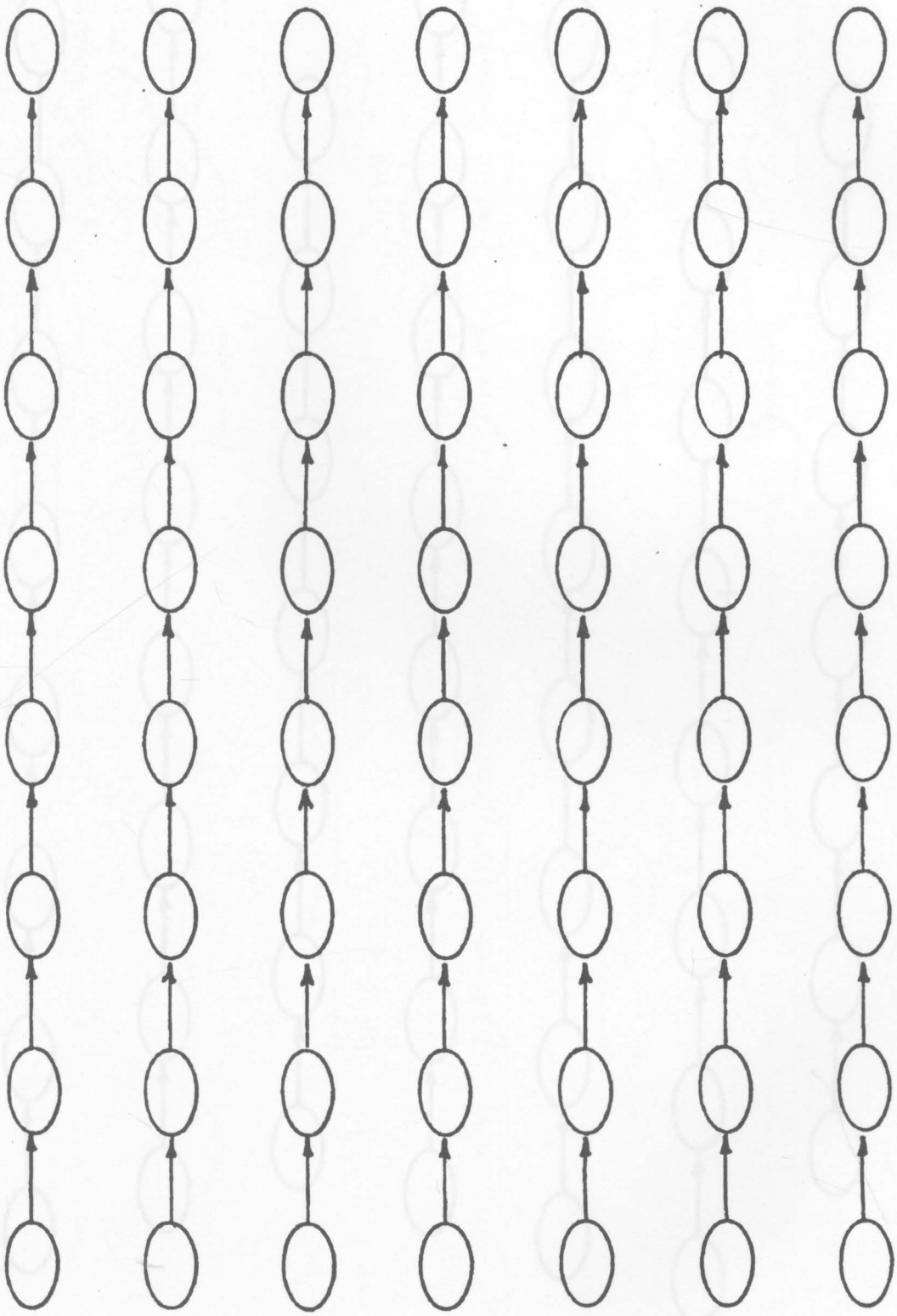


Figure 6.02: Heuristic Diagram of an Ideal Population of Seven Individuals Viewed Through Eight Generations with Desynchronised Reproduction

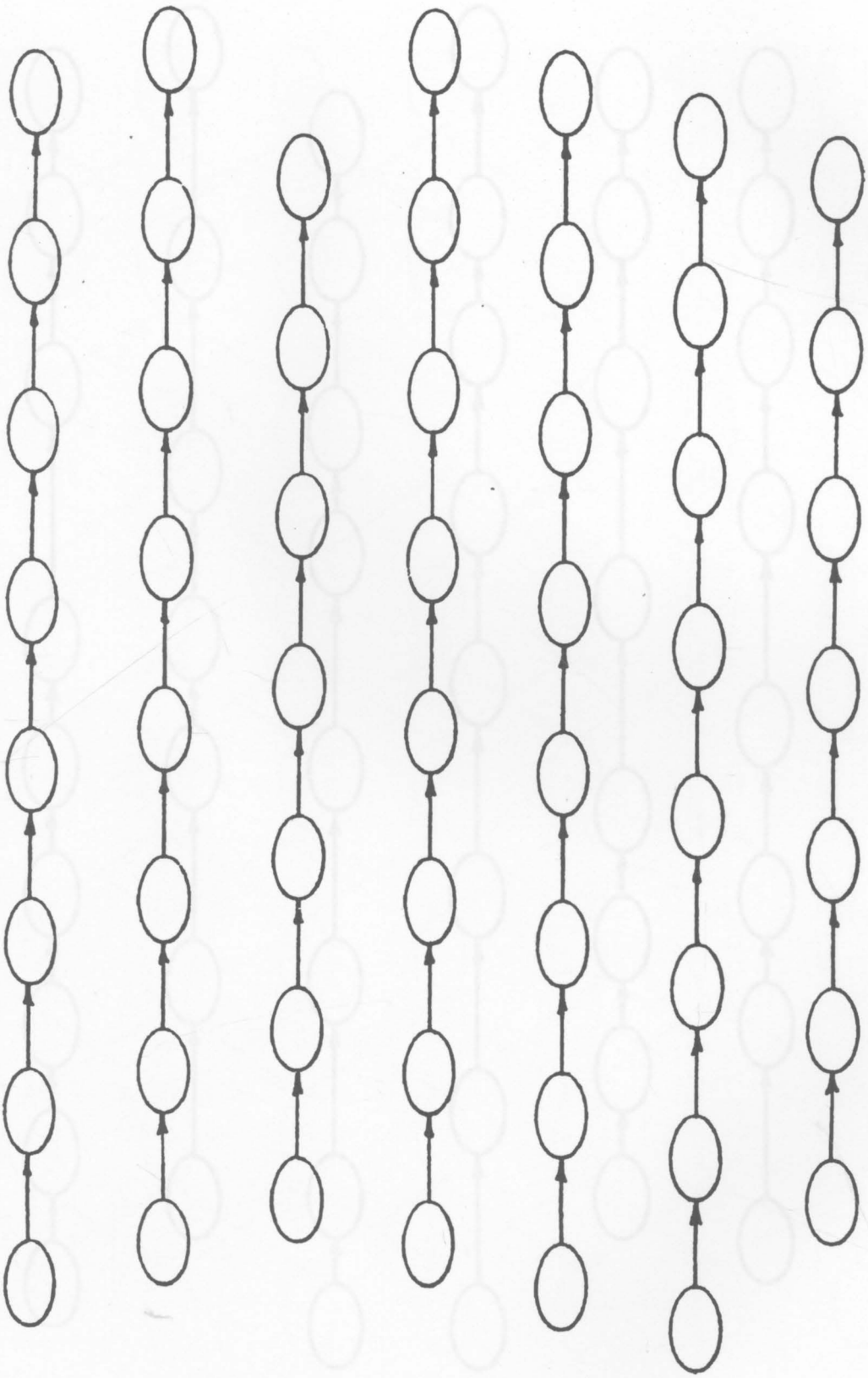


Figure 6.03: Heuristic Diagram of an Ideal Population of Seven Individuals Viewed Through Eight Generations with Desynchronised Time and Age of Reproduction

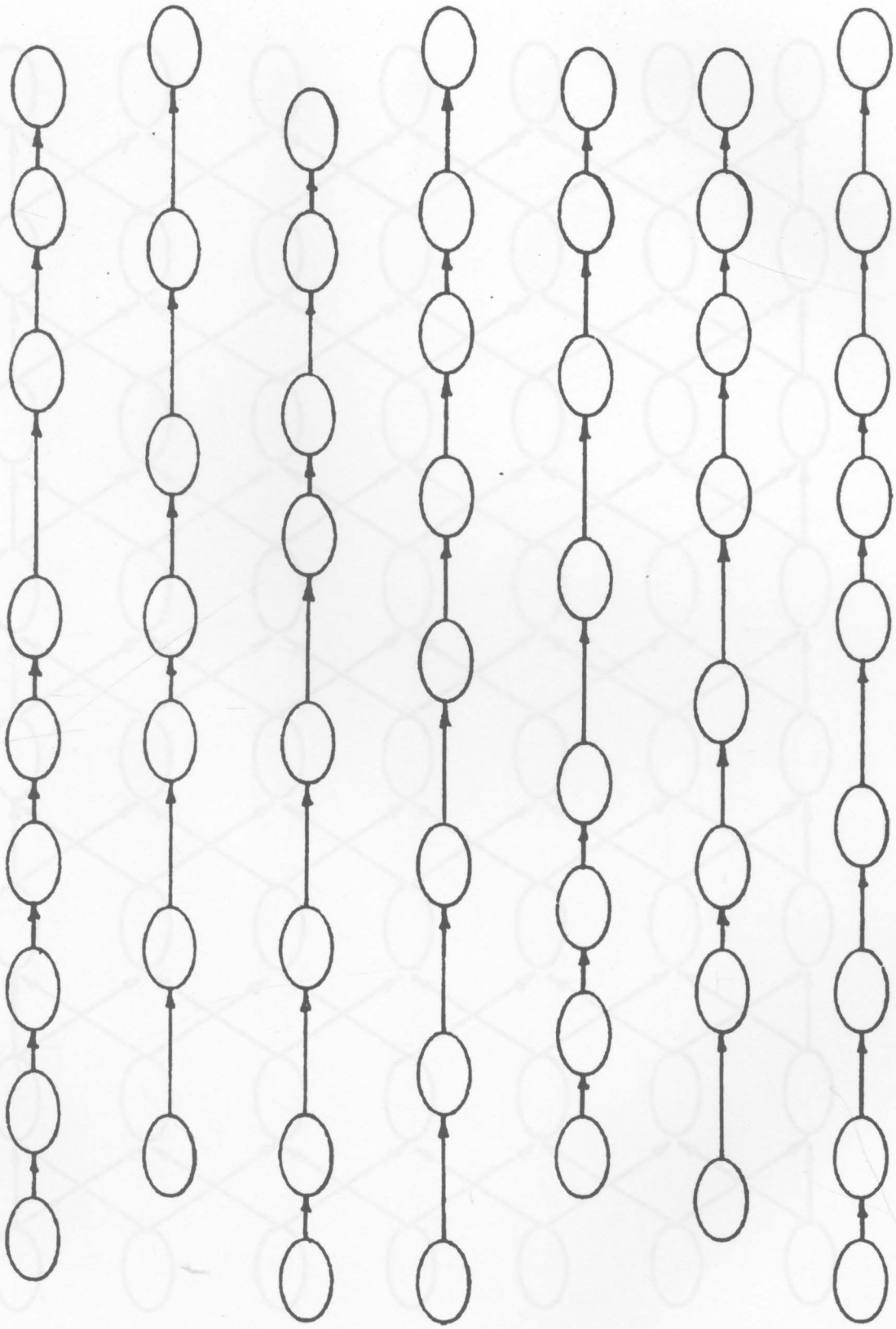


Figure 6.04: Heuristic Diagram of an Ideal Population of Seven Individuals Viewed Through Eight Generations with Inheritance Pattern Representing Sexual Reproduction.

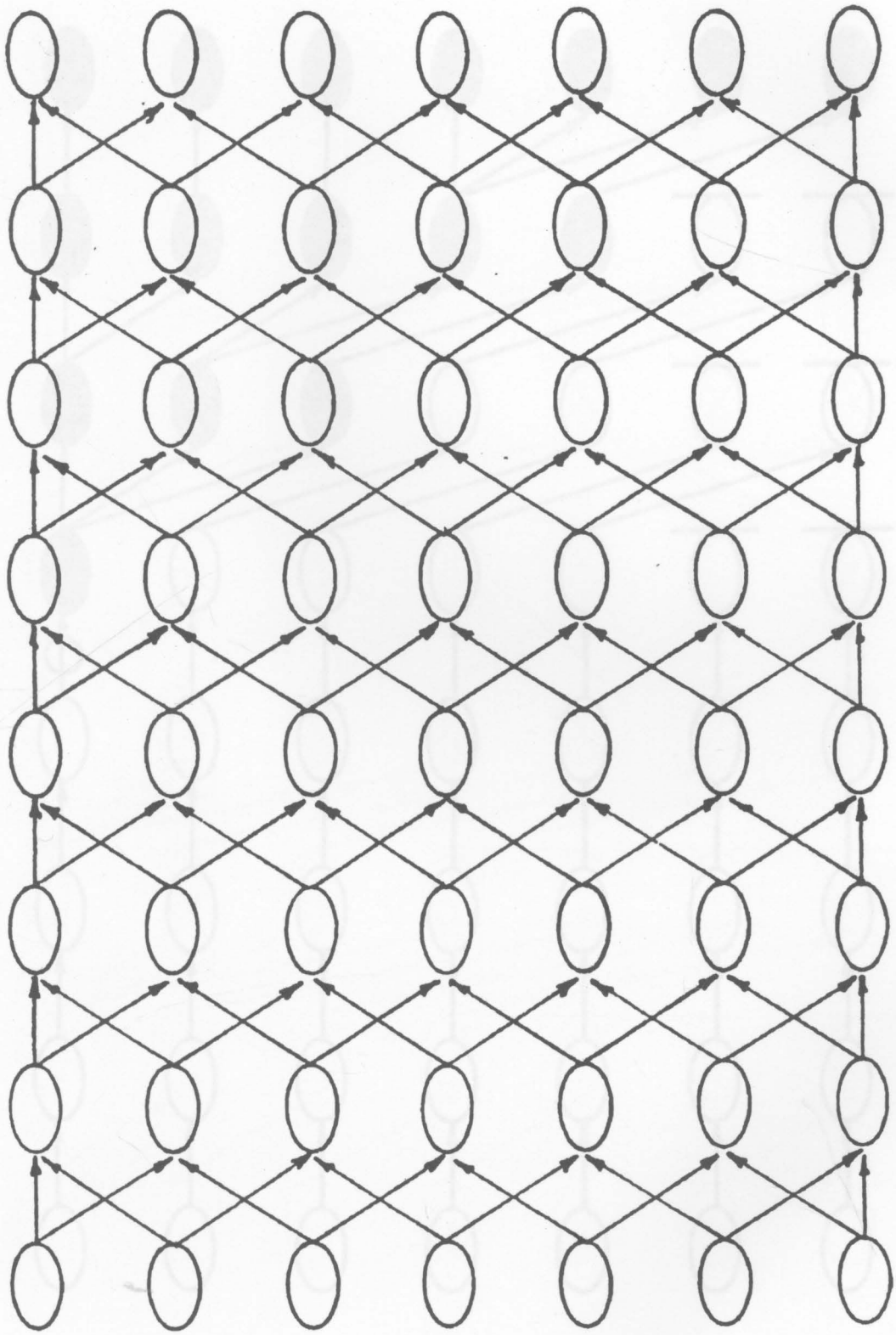


Figure 6.05: Heuristic Diagram of Darwinian Adaptation in an Ideal Population of Seven Individuals Viewed Through Eight Generations.

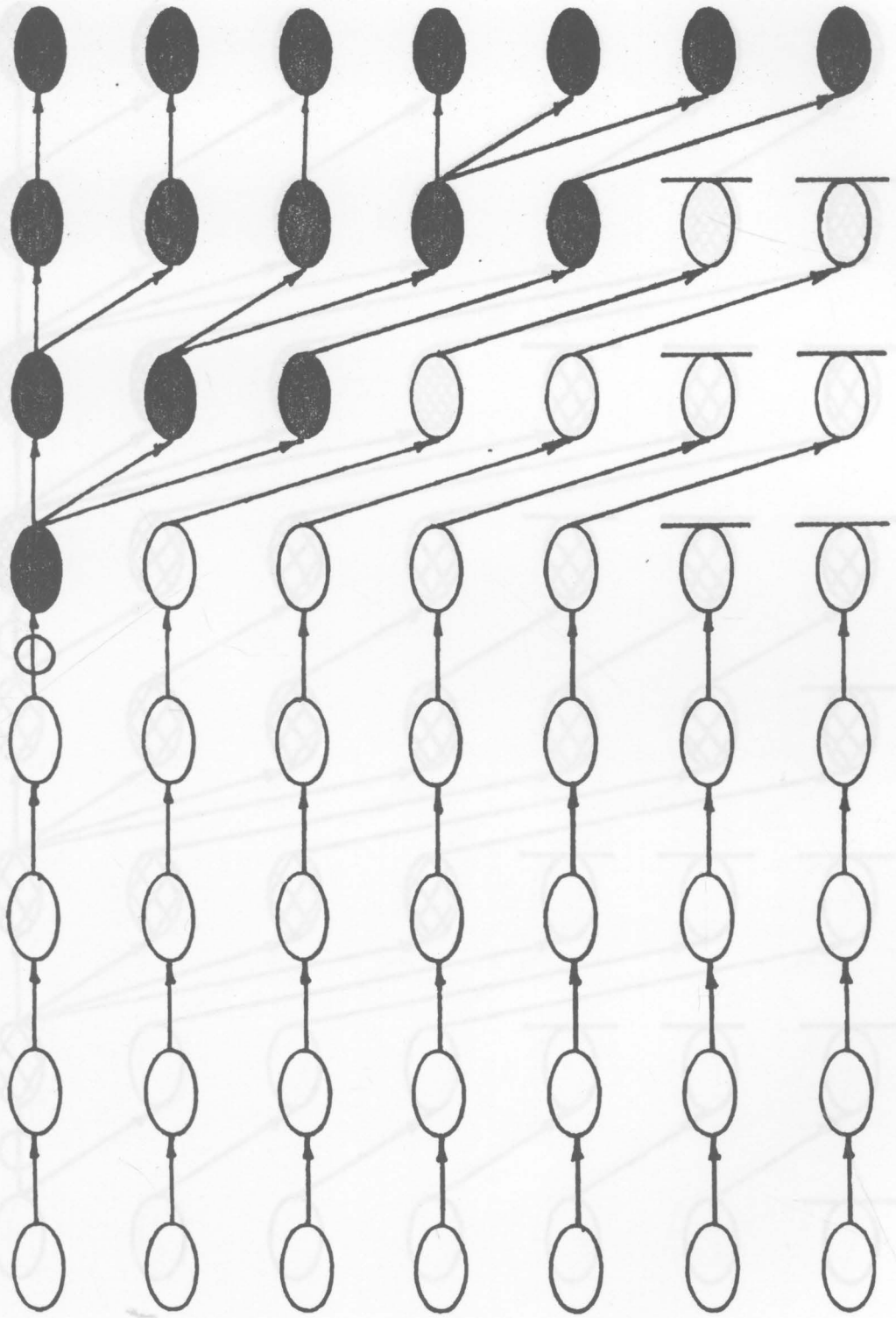


Figure 6.06: Refined Heuristic Diagram of Darwinian Adaptation in an Ideal Population of Seven Individuals Viewed Through Eight Generations.

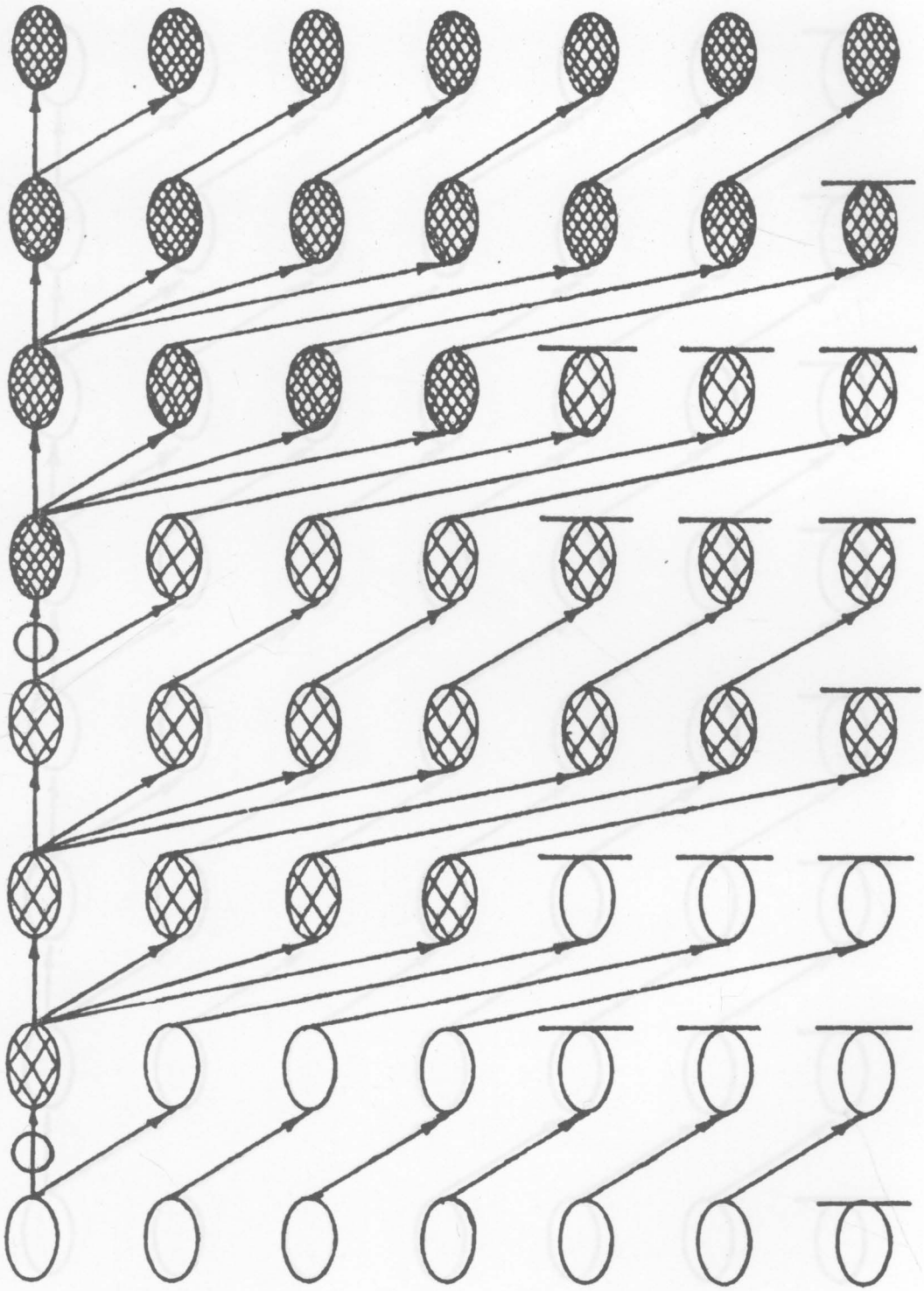


Figure 6.07: Heuristic Diagram of Random Selection in the Absence of Variation, in an Ideal Population of Seven Individuals Viewed Through Eight Generations.

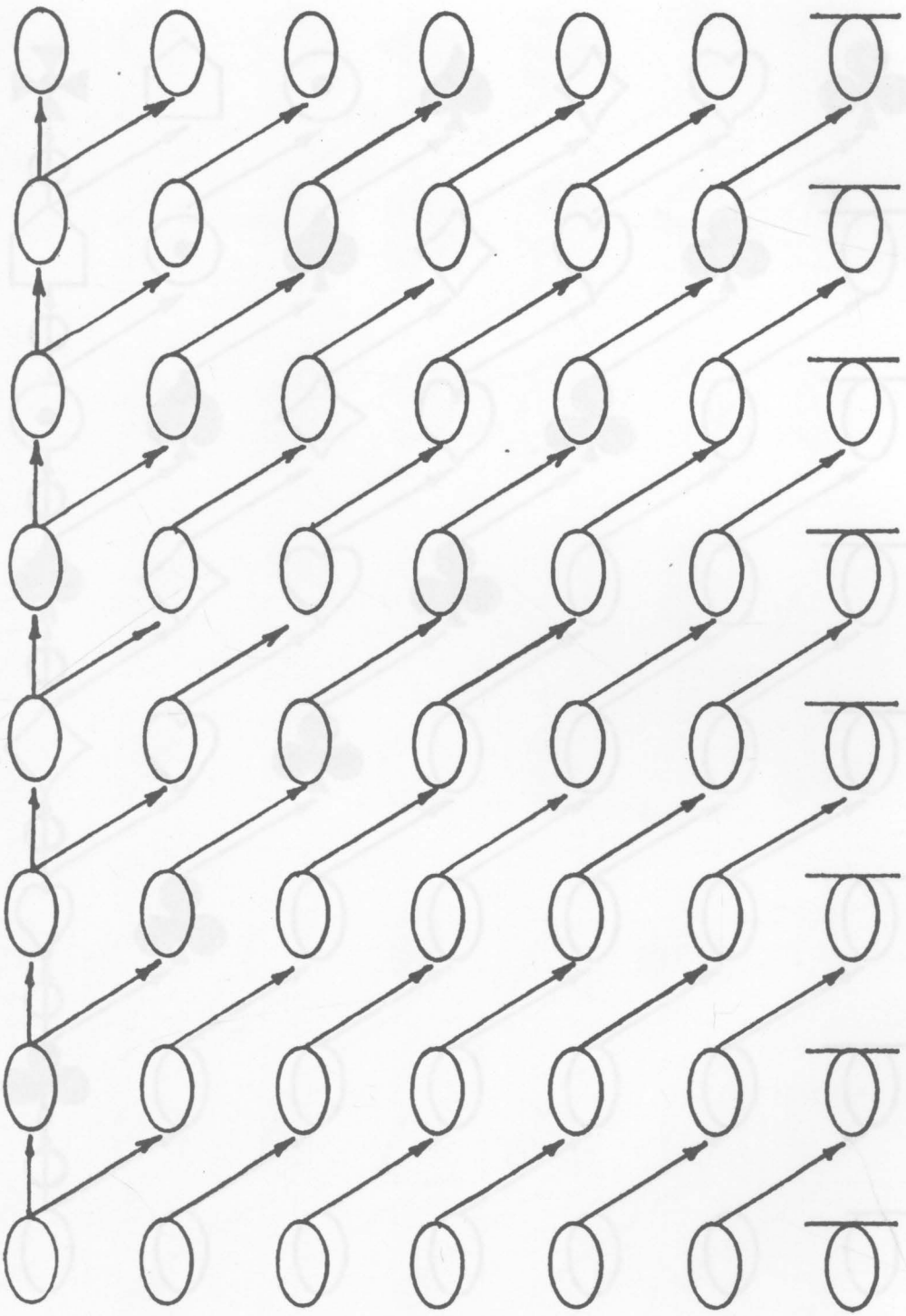


Figure 6.08: Heuristic Diagram of Random Selection Supplied with Variation by Neutral Mutation, in an Ideal Population of Seven Individuals Viewed Through Eight Generations.

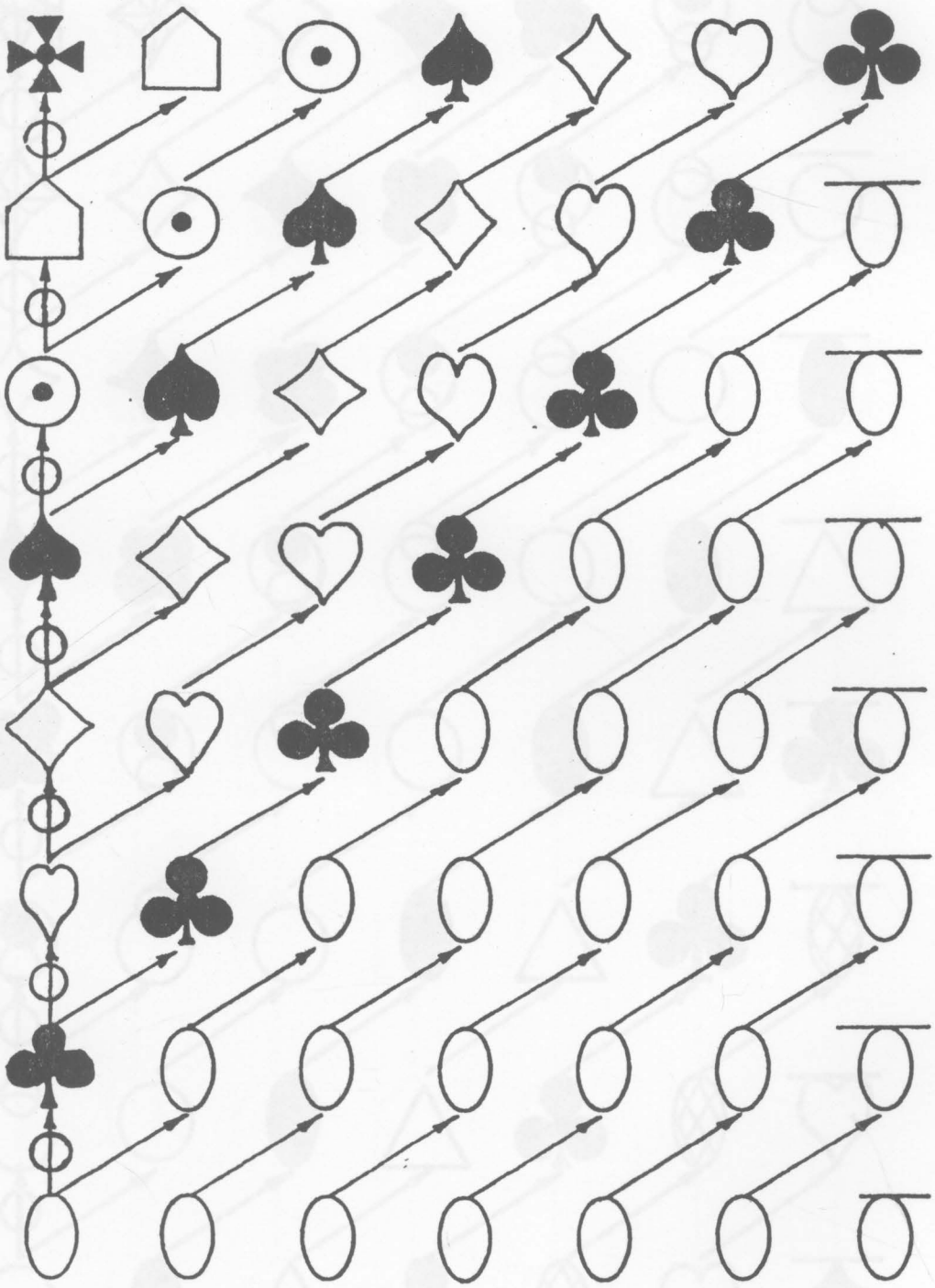


Figure 6.09: Heuristic Diagram of Continuing Drift or Random Selection Supplied with Variation by Neutral Mutation, in an Ideal Population of Seven Individuals.

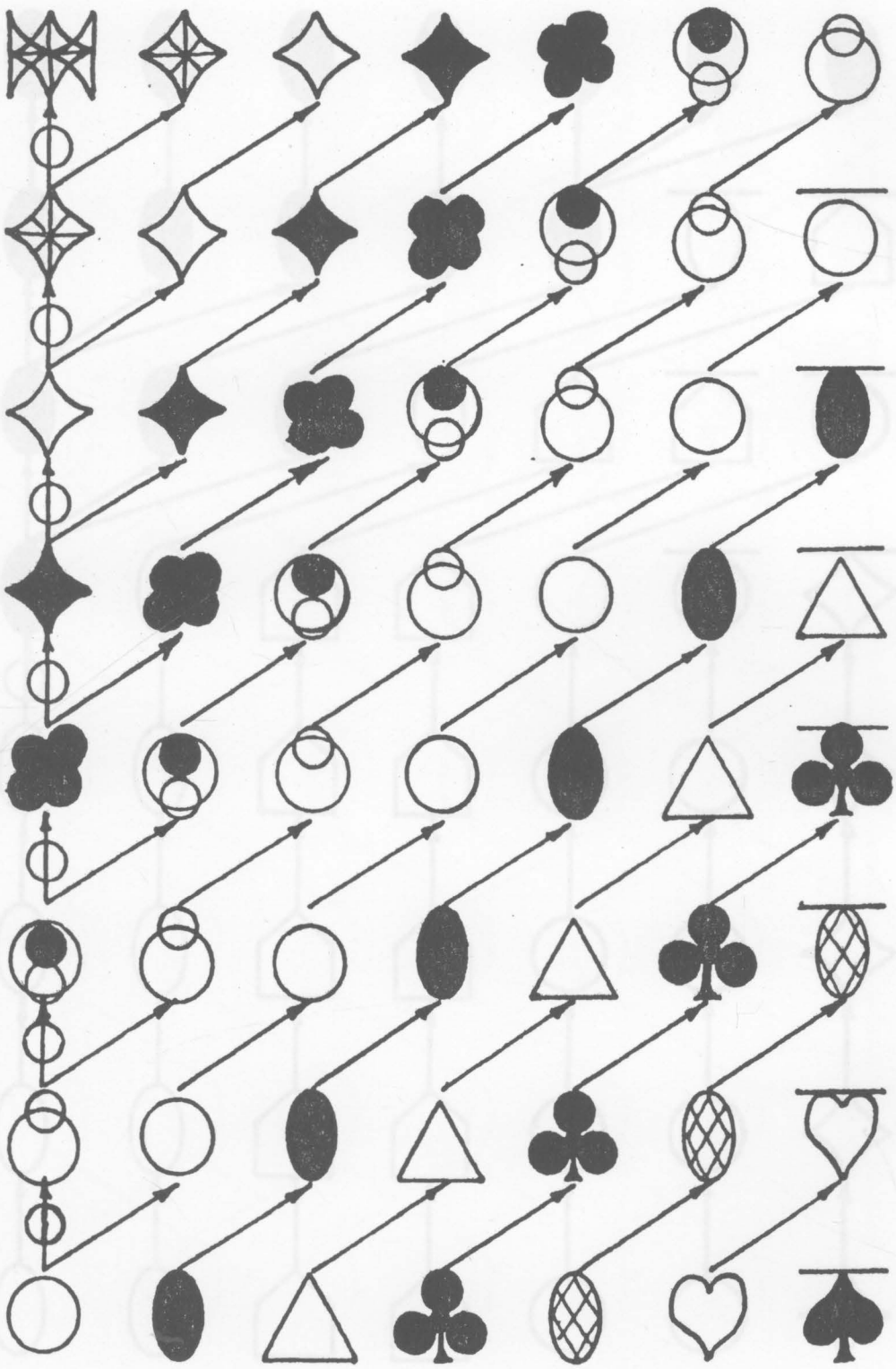


Figure 6.10: Heuristic Diagram of the Random Selection Associated with Darwian Adaptation, in an Ideal Population of Seven Individuals.

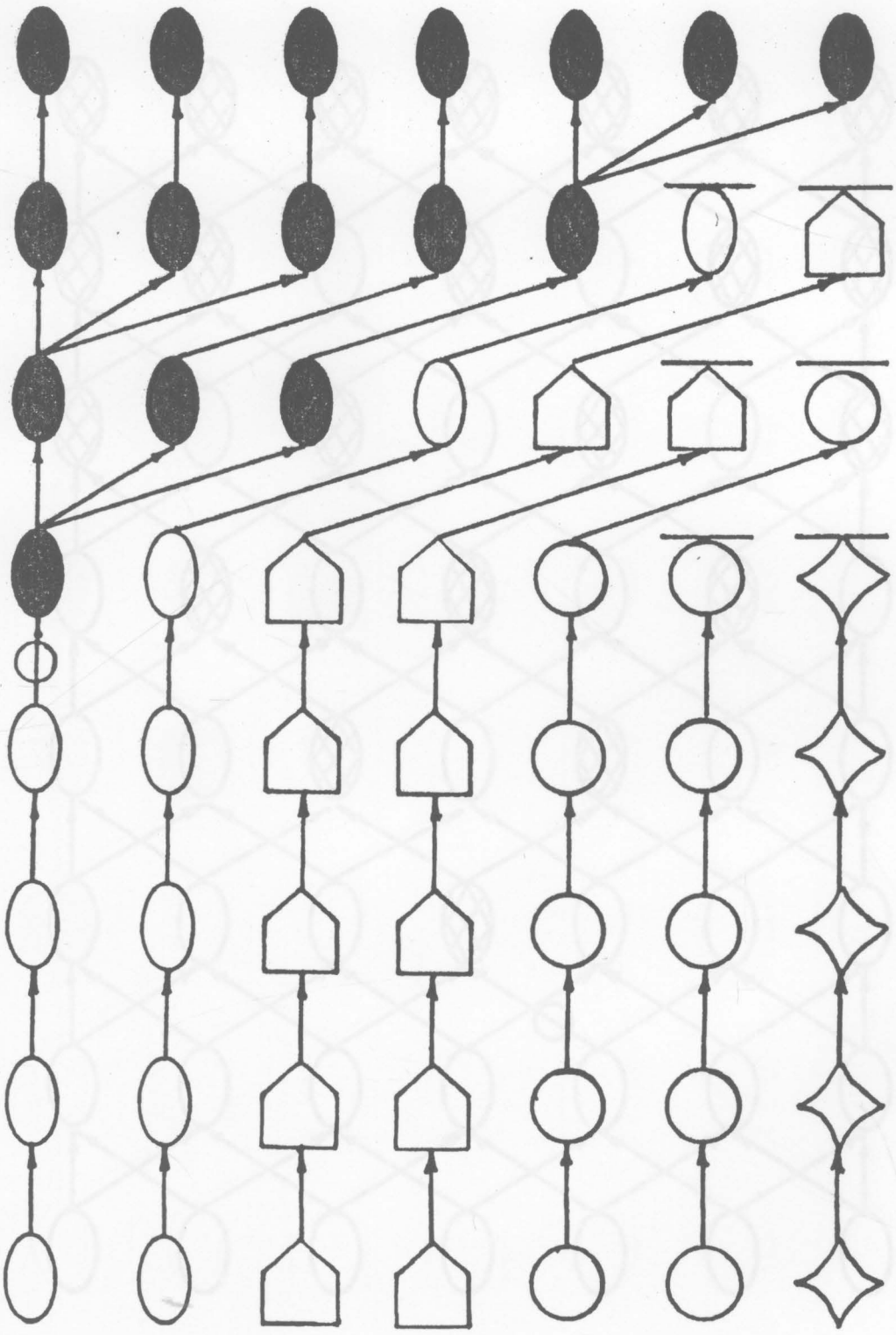


Figure 6.11: Heuristic Diagram Illustrating the Potential Spread of a Mutation in an Idealised Sexually Reproducing Population of Seven Individuals.

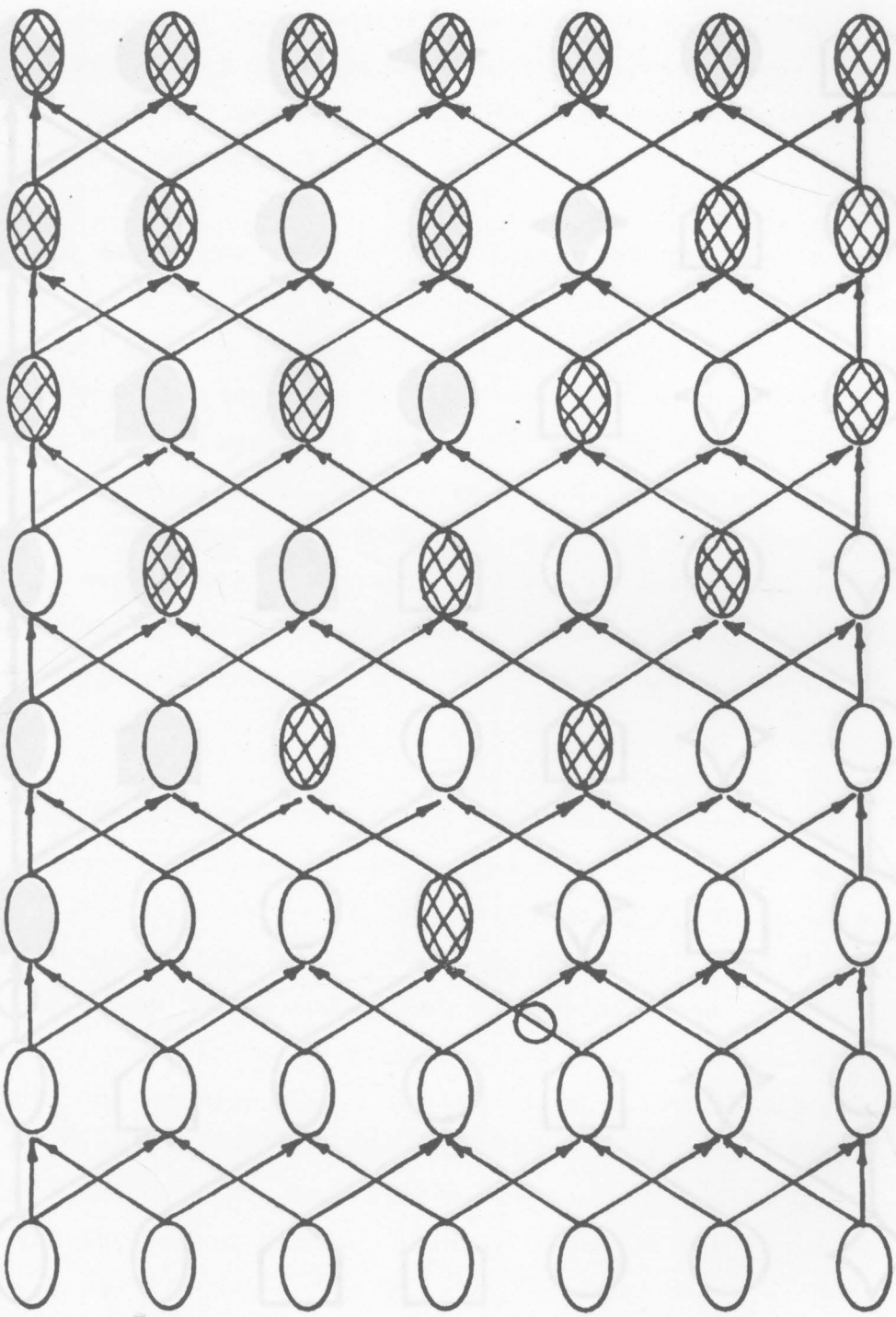
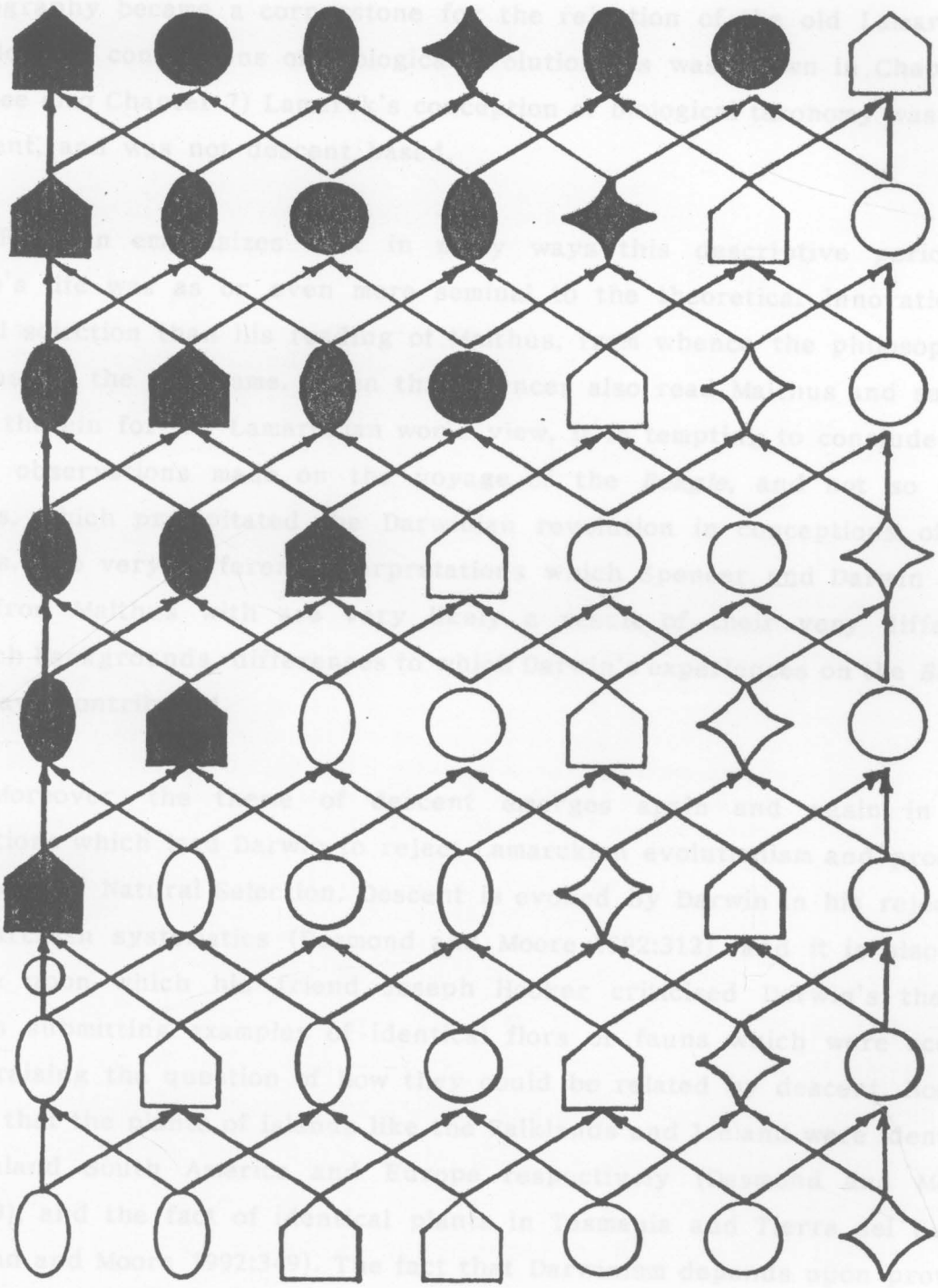


Figure 6.12: Heuristic Diagram Illustrating How the Potential Spread of a Mutation in an Idealised Sexually Reproducing Population Reduces Random Selection Due to Adaptation.



> become modified; and the subject haunted me." (Barlow 1958:118-119 in Freeman 1974:211).

Not only did these patterns lead Darwin to a selectionist conception of what was to ultimately become genetic process; Darwin's articulation of biogeography became a cornerstone for the rejection of the old Lamarckian orthodoxy in conceptions of biological evolution. As was shown in Chapter 2 (and see also Chapter 7) Lamarck's conception of biological taxonomy was very different, and was not descent based.

Freeman emphasizes that in many ways this descriptive period of Darwin's life was as or even more seminal to the theoretical innovation of natural selection than his reading of Malthus, from whence the philosophical stimulus for the idea came. Given that Spencer also read Malthus and saw no threat therein for his Lamarckian world view, it is tempting to conclude that it was observations made on the voyage of the *Beagle*, and not so much Malthus, which precipitated the Darwinian revolution in conceptions of life process. The very different interpretations which Spencer and Darwin came away from Malthus with are very likely a result of their very different research backgrounds, differences to which Darwin's experiences on the *Beagle* must have contributed.

Moreover, the theme of descent emerges again and again in the ruminations which lead Darwin to reject Lamarckian evolutionism and produce the theory of Natural Selection. Descent is evoked by Darwin in his rejection of Lamarckian systematics (Desmond and Moore 1992:312), and it is also the premise upon which his friend Joseph Hooker criticised Darwin's theory, through submitting examples of identical flora or fauna which were oceans apart, raising the question of how they could be related by descent. Hooker argued that the plants of islands like the Falklands and Iceland were identical to mainland South America and Europe respectively (Desmond and Moore 1992:319), and the fact of identical plants in Tasmania and Tierra del Fuego (Desmond and Moore 1992:349). The fact that Darwinism depends upon proving that these widely distributed flora are related by common descent is reflected in Darwin's experiments with the viability of plant seeds which had spent long

periods in saltwater (Desmond and Moore 1992:422). These were designed to prove that plants such as those in Tasmania and Tierra del Fuego might well have had a common ancestor (given that Darwin had no recourse to plate tectonics at this time).

Freeman actually places the moment of insight in Darwin's life at 1837, about a year and a half before he read Malthus for the first time, with these words:

> "On this evidence the genesis of Darwin's theory of the origin of species, may be traced with precision to the month of March, 1837, when after studying them in Cambridge he was "greatly struck" by the significance of the array of facts which, as a naturalist, he had collected during the voyage of the *Beagle*, and which, as he testified, were the origin of "all" his views. These facts were solely concerned with biological, geological, and cognate natural phenomena, and it was not until 18 months later that Darwin's thinking was decisively influenced by his reading of Malthus." (Freeman 1974:212).

Freeman goes on to detail how Darwin's reading of Malthus was:

> "...a maieutic stimulus, and not a pervasive ideological influence of the kind Harris has conjured up. When Darwin read Malthus, in September, 1838, he had already been conducting his "systematic enquiry" into the phenomena of descent with modification for some 15 months. Having begun these enquiries Darwin "soon perceived" (as recorded in his Autobiography) "that selection was the keystone of man's success in making useful races of animals and plants"; and further, as De Beer (1969:57) has documented in his analysis of Darwin's research notebooks for 1837-1838, Darwin had become aware of the existence of a natural process directly comparable to artificial selection some months before he read Malthus." (Freeman 1974:212).

Desmond and Moore also indicate that Darwin was exploring "selectionesque" concepts before reading Malthus, inventing the word "descent" (Desmond and Moore 1992:241), and noting a range of aspects of selective breeding in domesticated animals and plants (1992:240) and (1992:247-248). Darwin's theory was not, therefore, merely a result of literary inspiration from Malthus. Although this did in fact play an auxiliary role (Freeman 1974:213), the idea had already all but emerged in the process of Darwin's own "systematic enquiries".

Furthermore, the thematic generalisation which Darwin made about patterns of continuity and change in biogeography is often expressed in the short quoted phrase "descent with modification". Since change itself, the fact of evolution, was nothing new and had been well articulated in the theories of Lamarck and Erasmus Darwin, it can be seen that the pivotal word of this phrase, that which makes it distinctively Darwinian, is "descent". The phrase itself is rarely considered as a technical term which requires explanation; the fact that the words "descent" and "modification" are meant to denote "common sense" meanings of these words gives the phrase a simplicity beyond normal biological terminology. The continuity of inheritance and/or transmission (in contrast to the environmentally directed transformation often mistakenly assumed to exemplify Darwinism) is the most important element of this concept, even in regular usuage, and non-technical discussion:

> "descent, n. ...being descended, lineage; single generation *(lineal succession of four descendants)*; transmission of property, title or property, title, or quality, by inheritance." (The Concise Oxford Dictionary 1964:330).

> "descent n. ...**3a** derivation from an ancestor; birth, lineage **b** transmission of an estate by inheritance **c** a transmission from a usually earlier source; a derivation." (The New Penguin English Dictionary 1986:249).

Even the noun for change which Darwin selected, "modification", suggests limited rather than fundamental change:

> "modify, v.t. Make less severe or decided, tone down; make partial changes in; (gram.) qualify sense of (word etc.); change (vowel) by umlaut." (The Concise Oxford Dictionary 1964:778).

> "modify /v.t. **1** to make less extreme **2** to limit in meaning; qualify **3a** to make minor changes in **b** to make basic changes in, often for specific purpose - vi to undergo change." (The New Penguin English Dictionary 1986:591).

The point to make is simply that the much-quoted Darwinian phrase "descent with modification", which Darwin himself favoured so much, cannot be vaguely interpreted as simply meaning "descent with change", where the changes induced by the environment are profound changes which erase descent. Rather, descent is coupled with limited, partial or minor changes, and therefore remains the primary theme of the process.

Heritage constraint, not environmental determinism, is the fundamental signature of neo-Darwinian process. Obviously, the environment is disqualified from this status, since environmental influence *of some sort* is common to both neo-Darwinism *and Lamarckism*. Phylogenetic constraint is an integral part of selectionist theory itself, and moreover, those phenomena which display the "descent with modification" pattern played an important part in the development of the theory in the first place.

Logically, then, a "preliminary test" of cultural selectionism should involve the elucidation of a fundamental pattern of heritage or "descent with modification" in cultural data. This will presumably involve an analytical procedure somewhat akin to Darwin's post-voyage "systematic enquiry" into his detailed descriptions of animals and plants around the world.

Today, however, the analysis of large bodies of descriptive information can be greatly aided through the use of computerised multivariate analysis. A post-fieldwork analysis of the kind that took Darwin many years to complete can be performed in a fraction of that time.

In the next and final section of this chapter (section 6.4 below), multivariate analysis is used to analyze the structure of large globally distributed samples of bone measurements, to show that the neo-Darwinian signature of heritage constraint can quickly be illustrated by comparing the morphology and diversity of collections of crania from different locations on the globe.

6.4 A Three Hour Voyage in an Electronic Beagle

The "descent with modification" pattern pervades the biogeography of all faunal assemblages. The aim of this section, however, is to demonstrate the heritage or descent pattern with respect to a global sample of skeletal material with a roughly similar geographic distribution to culture, in order to establish a particular neo-Darwinian hypothesis for the cultural test of Chapter 8. The

most convenient and frequently available data of this kind proved to be human remains.

The general procedure incorporated the use of a computer for the multivariate comparison of collections of human crania of known phylogenetic heritage, and the analyzing of the nature and diversity of skull morphology in each group *purely with respect to that known restricted heritage.* The collections, by and large, were derived from communities or areas into which there had been (at the time of collection) very little European colonial influence (see further discussion below). Overall, the aim is simply to show that groups of skulls from communities with restricted phylogenetic heritage (in other words, groups of people whose ancestors are not a random sample of all nationalities) show evidence of heritage constraint. Constraints in each group are reflected in the tendency for a variety of skull shapes to be virtually absent in any one area of the world. Whenever a sample of crania from a group with restricted phylogenetic origins is taken, a range of distinctive shapes are present, and another range of distinctive shapes, which are present in other groups, are absent. In other words, human remains exhibit a scaled down version of what Darwin discovered in faunal assemblages; on any particular continent, a range of animals are present, and a range of other animals are absent. The network of presences and absences is largely a function of the phylogenetic heritage of the animals on different continents.

The database used for this study is the 2524 crania, 33 variable matrix compiled by W.W.Howells (see Howells, 1973, Cranial Variation in Man: a Study by Multivariate Analysis). Other studies have made use of this data, but usually for the purpose of identifying new crania as they are found. Kamminga and Wright (1988) for example, use the data to discuss the morphology of a particular skull. A full description of the nature of the variables can be found in Howells' original work, but only a brief description is needed here, as the focus will be on the general occurrence of shape constraints in some form or other in all populations, not on the specific shapes involved in each case.

All the reader need know for the purposes of the analysis is that each of the 33 variables is a measurement between two particular features of the human cranium. These measurements might be the height of the eye socket (orbital) measured from base to top at the centre, the maximum breadth of the cranium, or the height of the cranium from the chin to the nose, etc. Each skull is thus described, not as a series of convex and concave surfaces, but as an assemblage of lines, each of a particular length. Metaphorically speaking, any given skull is represented as a kind of "cross hatched sketch" where each "hatch" or line is a particular measurement taken from that particular skull.

The data matrix (consisting of 2524 crania represented in terms of 2524 x 33 measurements) was then subjected to a Principal Components Analysis (PCA). A more technical explanation of this technique can be found in R.V.S. Wright's multivariate software package (see Wright 1989 Doing Multivariate Archaeology and Prehistory: Handling Large Data Sets with MV-Arch). Basically what the computer does is to compare each skull with each of the others, first in terms of one variable, then two, then three, and so on until all variables have been considered. The skulls are ranked from smallest to largest in terms of the first variable, and then the computer moves on to the next variable. The first variable may have been, for example, the width of the left orbital, and the skulls may have been ordered from those with the broadest orbitals to those with the narrowest. The computer then moves to variable 2, skull 1, which may be the height of the left orbital, and compares all the skull scores for this measurement, once again arranging them in order from smallest to largest, and so on for the remaining variables. Once this is done the computer can place the skulls in order according to their combined performance across all the variables, and the profiles of different variables can be compared. Groups of variables with similar profiles, which covary, or which vary as the inverse of each other are (to put it crudely) "combined" to form synthetic "supervariables" or Principal Components. The first "principal component" emerges as the "supervariable" for which the skulls vary most, or which accounts for the most variance in the cranial data. The second Principal Component is the component or "group of variables" which together account

for the next largest amount of variance in the data, the third accounts for the next largest, and so on.

When Kamminga and Wright (1988) undertook a series of multivariate analyses of this cranial data, they noticed (Wright *Pers. Com.*) that the results were very similar no matter how many variables (within reason) were employed in the analysis. The very same regularities of what is termed the "phylogenetic heritage constraint pattern" below emerged when the number of variables was cut from 33 to 20, from 20 to 10, and then even, finally, when the variables were cut from 10 to 5. Thus it appeared that phylogenetic constraint was so robust that it was manifest in analyses which employed only a few variables.

In data of this kind, or "continuous variables", and where there is considerable difference in the sizes of the objects being compared, size is usually the first Principal Component extracted from the matrix. The size component is the largest group of variables which covary, and this group is so large it includes all the variables (the size of each variable tends rise with the overall size of the skull). It is customary to exclude the size component from an analysis where phylogenetic interrelationships are to be the focus of attention, as size is strongly affected by a variety of non-hereditary factors. The skulls will therefore be graphed in terms of the second and third principal components, which, as will be shown, are components not of size, but of shape.

Before the heritage constraints in the cranial data are brought out, however, a brief description of the variables is offered, to help in the visualisation of the nature of the "space" in which the skulls will be graphed. In the tables below the measurements of the Howells cranial data are listed, along with their codes, and the measurements taken for each variable from one sample skull, for the purposes of visualising the dimensions involved with each variable (see Table 1).

The crania of the Howells collection consist of 26 groups, each with 30-50 male skulls and 30-50 female skulls. There are a further 3 collections of 30-50 skulls, from China (Anyang), New Zealand (Maori) and Siberia (Buriat), which consist of skulls from males only. The position of each group of crania within the Howells data matrix is also listed below, along with brief details of the human groups from which they were derived (see Table 2).

TABLE 1: THE CRANIAL VARIABLES

AND THEIR VALUE FOR A SAMPLE CRANIUM

Variable	*1*	*GOL*	*Glabello-occipital length 206*
Variable	*2*	*NOL*	*Nasio-occipital length 199*
Variable	*3*	*BNL*	*Basion-nasion length 109*
Variable	*4*	*BBH*	*Basion-bregma height 133*
Variable	*5*	*XCB*	*Maximum cranial breadth 144*
Variable	*6*	*XFB*	*Maximum frontal breadth 125*
Variable	*7*	*STB*	*Bistephanic breadth 120*
Variable	*8*	*ZYB*	*Bizygomatic breadth 143*
Variable	*9*	*AUB*	*Biauricular breadth 137*
Variable	*10*	*WCB*	*Minimum cranial breadth 73*
Variable	*11*	*ASB*	*Biasterionic breadth 122*
Variable	*12*	*BPL*	*Basion-prosthion breadth 107*
Variable	*13*	*NRH*	*Nasion-prosthion height 76*
Variable	*14*	*NLH*	*Nasal height 58*
Variable	*15*	*OBH*	*Orbital (left) height 34*
Variable	*16*	*OBB*	*Orbital (left) breadth 45*
Variable	*17*	*JUB*	*Bijugal breadth 127*
Variable	*18*	*NLB*	*Nasal breadth 33*
Variable	*19*	*MAB*	*Palate breadth 69*
Variable	*20*	*MDH*	*Mastoid height 31*
Variable	*21*	*MDB*	*Mastoid breadth 17*
Variable	*22*	*ZMB*	*Bimaxilliary breadth 104*
Variable	*23*	*FMB*	*Bifrontal breadth 108*
Variable	*24*	*EKB*	*Biorbital breadth 110*
Variable	*25*	*DKB*	*Interorbital breadth 22*
Variable	*26*	*WNB*	*Simotic chord 10*
Variable	*27*	*IML*	*Malar length, inferior 47*
Variable	*28*	*XML*	*Malar length, maximum 65*
Variable	*29*	*WMH*	*Cheek height 27*
Variable	*30*	*FOL*	*Foramen magnum length 38*
Variable	*31*	*FRC*	*Frontal chord 119*
Variable	*32*	*PAC*	*Parietal chord 115*
Variable	*33*	*OCC*	*Occipital chord 102*

(Table from Kamminga and Wright 1988).

TABLE 2: THE 29 CRANIAL COLLECTIONS
AND THEIR CULTURAL ORIGINS

Crania	*1-55*	*Norse*	*Norway*	*male*
Crania	*56-108*	*Zalavar*	*Hungary*	*male*
Crania	*109-164*	*Berg*	*Austria*	*male*
Crania	*165-197*	*26-30 Dyn.*	*Egypt*	*male*
Crania	*198-244*	*Teita*	*East Africa*	*male*
Crania	*245-299*	*Dogon*	*West Africa*	*male*
Crania	*300-351*	*Zulu*	*South Africa*	*male*
Crania	*352-396*	*Bushman*	*South Africa*	*male*
Crania	*397-452*		*South Australia*	*male*
Crania	*453-503*		*Tasmania*	*male*
Crania	*504-552*	*Tolai*	*New Britain*	*male*
Crania	*553-609*	*Mokapu*	*Hawaii*	*male*
Crania	*610-651*		*Easter Island*	*male*
Crania	*652-702*	*Moriori*	*Chatham Islands*	*male*
Crania	*703-757*	*Arikara*	*Dakota*	*male*
Crania	*758-812*	*Santa Cruz Is.*	*California*	*male*
Crania	*813-862*	*Youyos*	*Peru*	*male*
Crania	*863-907*	*Hokkaido*	*North Japan*	*male*
Crania	*908-936*	*Kyushu*	*South Japan*	*male*
Crania	*937-986*	*Hainan*	*China*	*male*
Crania	*987-1016*	*Atayal*	*Taiwan*	*male*
Crania	*1017-1074*		*Phillipines*	*male*
Crania	*1075-1115*	*Latte period,*	*Guam*	*male*
Crania	*1116-1150*		*Andaman Is.*	*male*
Crania	*1151-1198*	*Ainu*	*North Japan*	*male*
Crania	*1199-1253*	*Buriat*	*Siberia*	*male*
Crania	*1254-1306*	*Eskimo*	*Greenland*	*male*
Crania	*1307-1348*	*Anyang*	*China (Shang)*	*male*
Crania	*1349-1368*	*Maori*	*New Zealand*	*male*
Crania	*1369-1423*	*Norse*	*Norway*	*female*
Crania	*1424-1468*	*Zalavar*	*Hungary*	*female*
Crania	*1469-1521*	*Berg*	*Austria*	*female*
Crania	*1522-1571*	*26-30 Dyn.*	*Egypt*	*female*
Crania	*1572-1623*	*Teita*	*East Africa*	*female*
Crania	*1624-1669*	*Dogon*	*West Africa*	*female*
Crania	*1670-1718*	*Zulu*	*South Africa*	*female*
Crania	*1719-1760*	*Bushman*	*South Africa*	*female*
Crania	*1761-1814*		*South Australia*	*female*
Crania	*1815-1863*		*Tasmania*	*female*
Crania	*1864-1900*	*Tolai*	*New Britain*	*female*
Crania	*1901-1951*	*Mokapu*	*Hawaii*	*female*
Crania	*1952-1978*		*Easter Island*	*female*
Crania	*1979-2029*	*Moriori*	*Chatham Island*	*female*
Crania	*2030-2084*	*Arikara*	*Dakota*	*female*
Crania	*2085-2116*	*Santa Cruz Is.*	*California*	*female*
Crania	*2117-2157*	*Youyos*	*Peru*	*female*
Crania	*2158-2195*	*Hokkaido*	*North Japan*	*female*
Crania	*2196-2213*	*Kyushu*	*South Japan*	*female*

Crania	*2214-2240*	*Hainan*	*China*	*female*
Crania	*2241-2293*	*Atayal*	*Taiwan*	*female*
Crania	*2294-2342*		*Phillipines*	*female*
Crania	*2343-2377*	*Latte period, Guam*		*female*
Crania	*2378-2415*		*Andaman Is.*	*female*
Crania	*2416-2469*	*Ainu*	*North Japan*	*female*
Crania	*2470-2524*	*Eskimo*	*Greenland*	*female*

By plotting the skulls in terms of any two principal components, a huge amount of information can be represented, but in a highly "digestible" and visual (rather than numerical) form. The position of a skull on the graph is a function of its values for two groups of variables; that group which vary most along the component allocated to the x axis, and that group which make up the principal component along the y axis. In short a great deal of multidimensional information about shape is reflected in a skull's position on the graph, and a great deal of comparitive information is reflected in the distance between any two skulls on the graph. As stated above, no attempt will be made to reconstruct the actual shapes of the human crania. This is because the human shapes of these particular skulls are quite irrelevant to this particular task of extracting a neo-Darwinian hypothesis for later examinations of cultural data; any group of bone assemblages would have suited the analysis equally well.

Whether a particular set of bones has a tendency toward any particular shape or shapes is not important, what is important is that every group of skulls has a tendency toward some particular shape or shapes, regardless of what those shapes may be. The sole purpose of the Principal Components Analysis is to demonstrate this tendency.

Thus the plot of the variables in terms of the second and third principal components **(FIGURE 6.13)**, will not be used in the customary manner of Principal Components Analyses, i.e. as a "shape key" to reconstruct actual shapes in later object plots. It is presented merely to show that the variables are very widely spread, and that the multidimensional space in within which the skulls are to be compared is a function of many variables, rather than

Figure 6.13: Plot of all cranial variables in terms of the 2nd and 3rd Principal components.

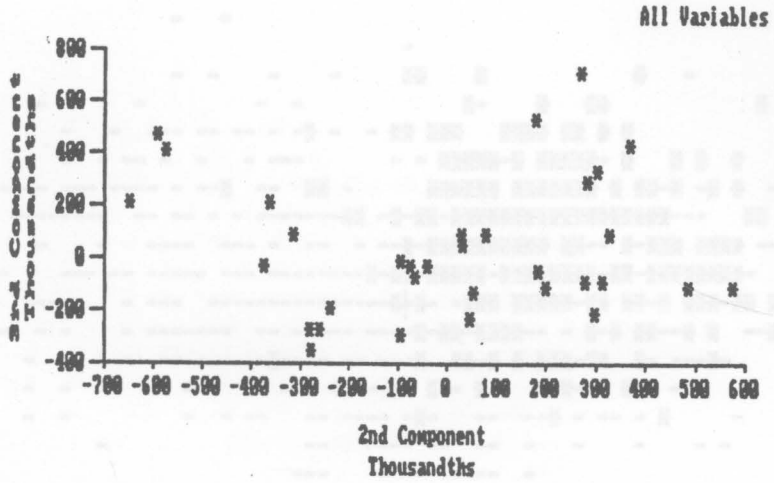


Figure 6.14: Plot of all crania in terms of the 2nd and 3rd Principal components.

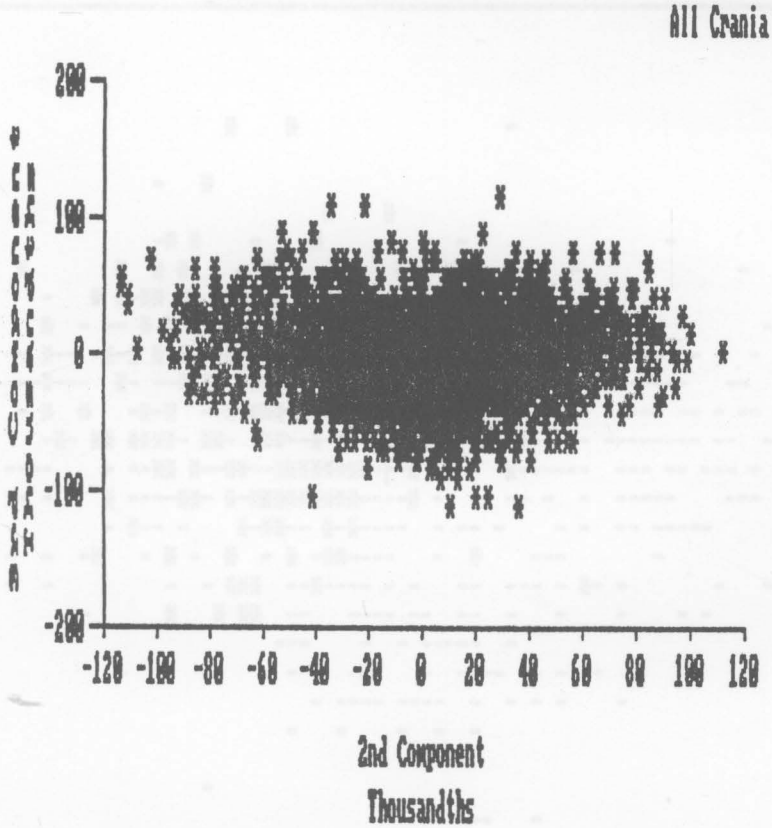


Figure 6.15: Distribution of sub-saharan African crania (*) within the plot of all crania (-), in terms of the 2nd and 3rd components.

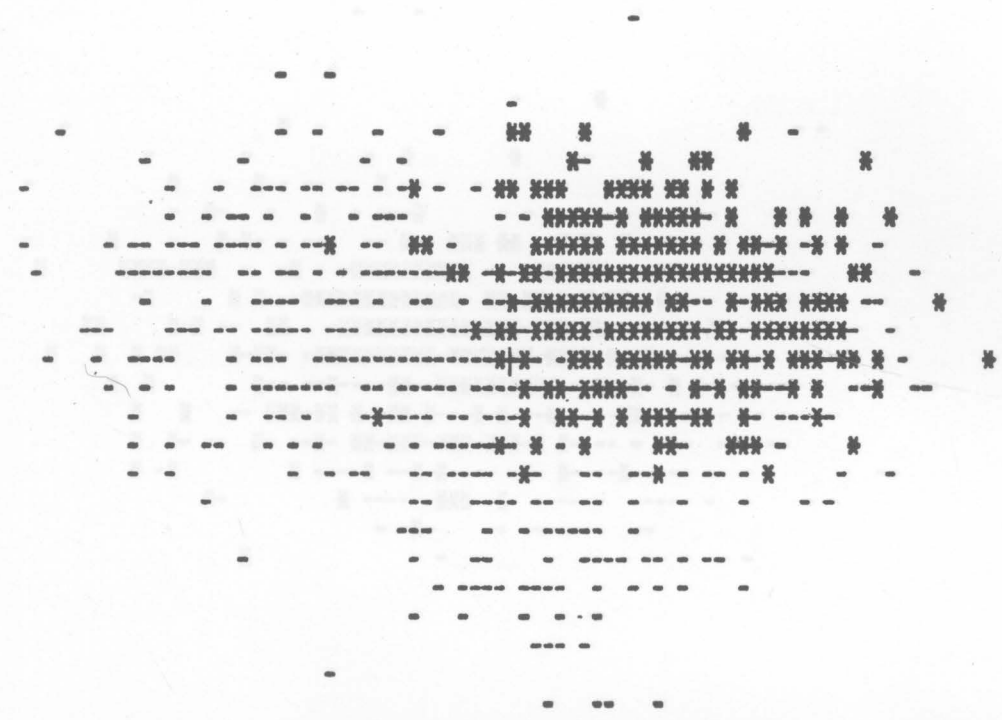


Figure 6.16: Distribution of European crania (*) within the plot of all crania (-), in terms of the 2nd and 3rd components.

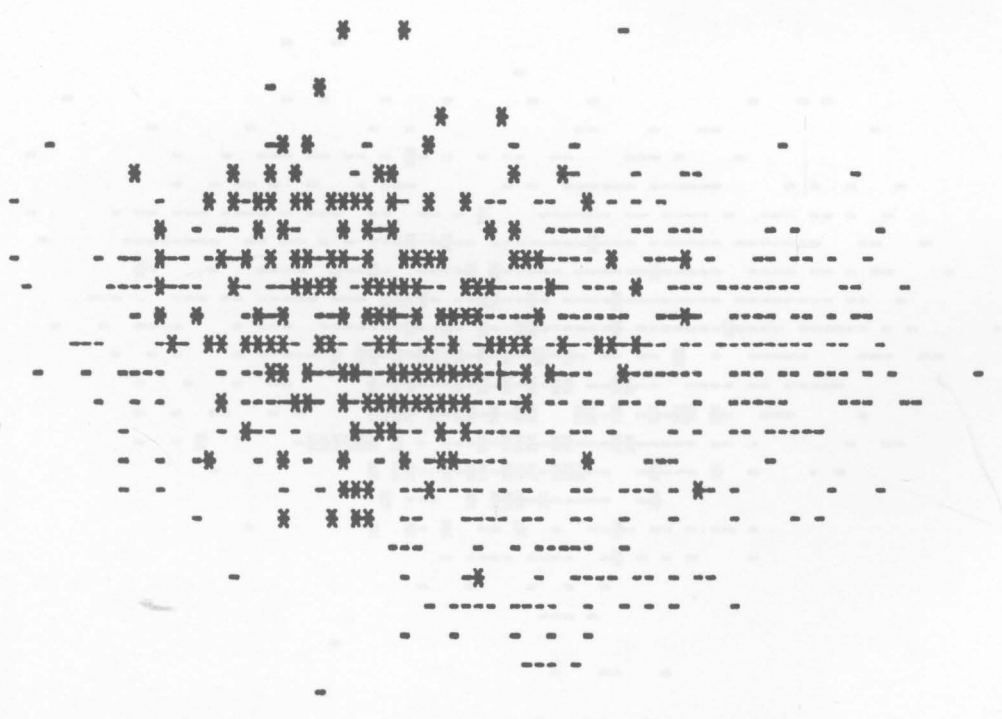


Figure 6.17: Distribution of Asian crania (*) within the plot of all crania (-), in terms of the 2nd and 3rd components.

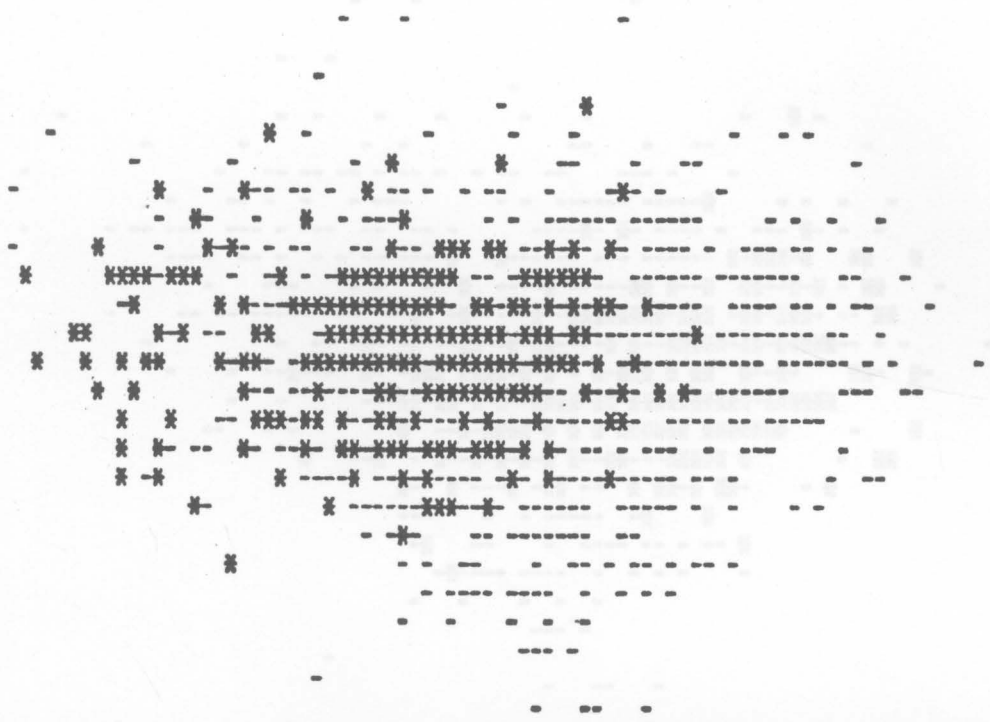


Figure 6.18: Distribution of Malayo-Polynesian and Andaman crania (*) within the plot of all crania (-), in terms of the 2nd and 3rd components.

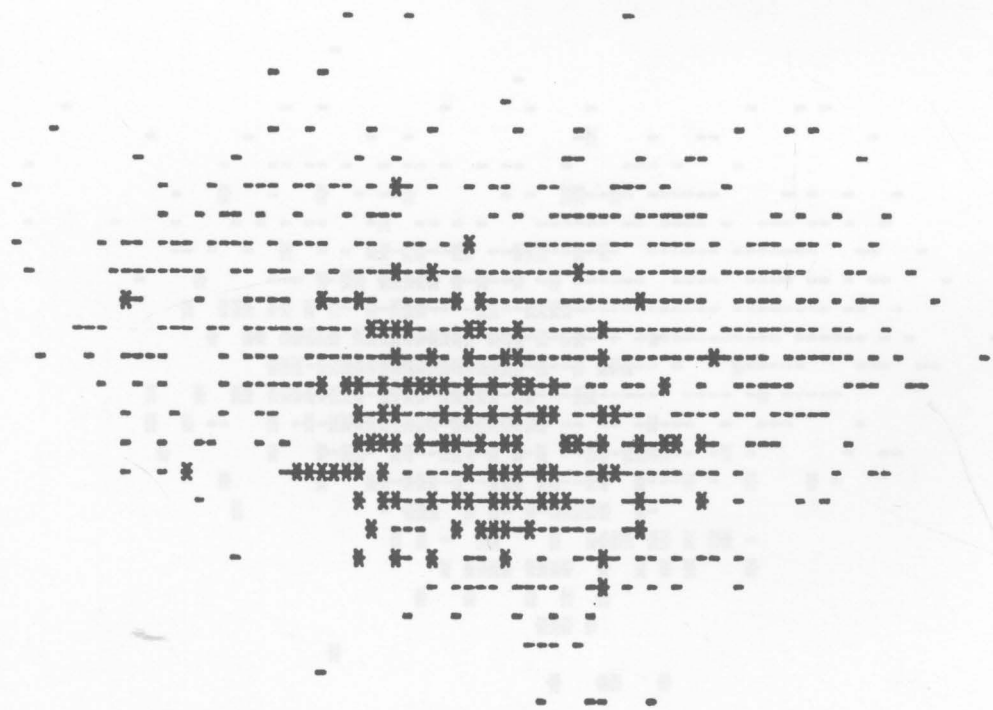


Figure 6.19: Distribution of Papuan and Australian crania (*) within the plot of all crania (-), in terms of the 2nd and 3rd components.

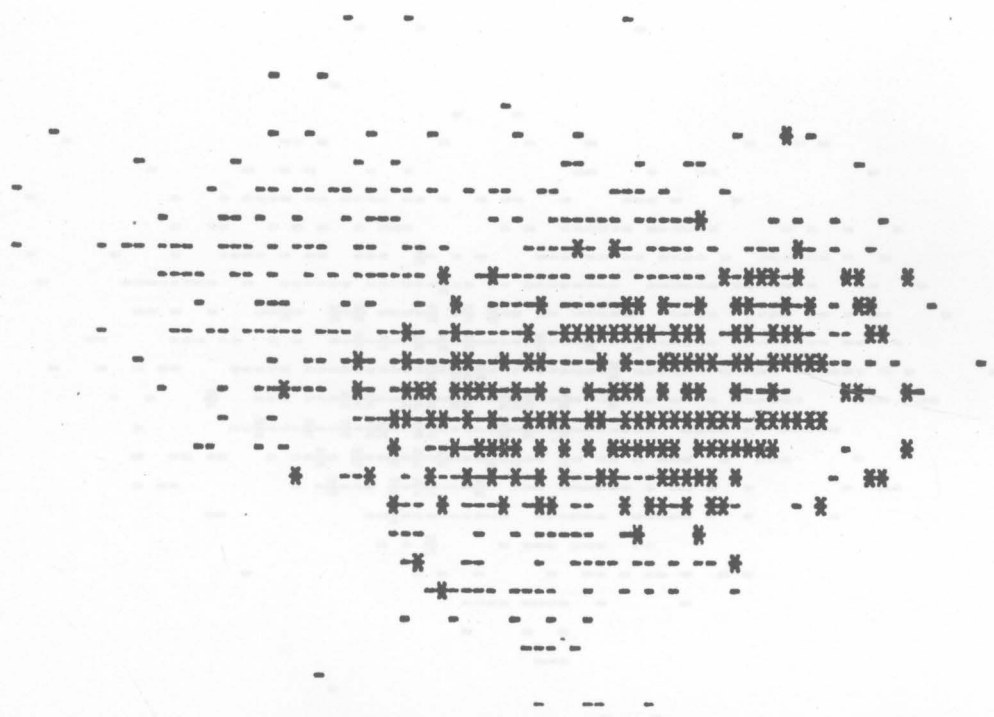


Figure 6.20: Distribution of North American and Greenland crania (*) within the plot of all crania (-), in terms of the 2nd and 3rd components.

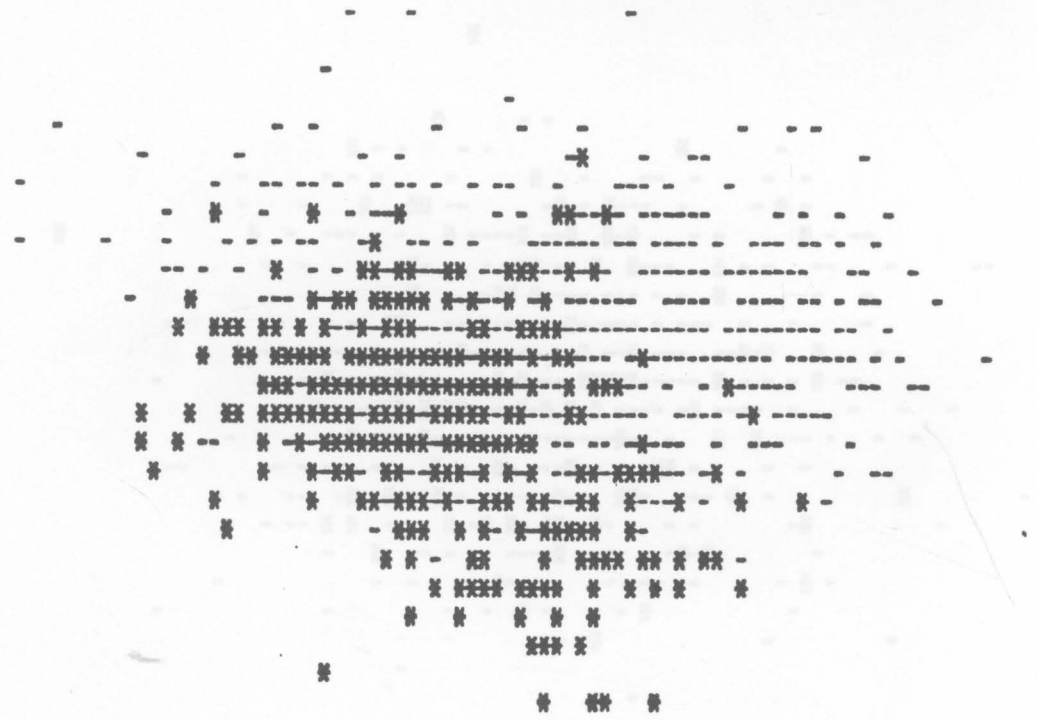


Figure 6.21: Distribution of South American crania (*) within the plot of all crania (-), in terms of the 2nd and 3rd components (one collection only from continent).

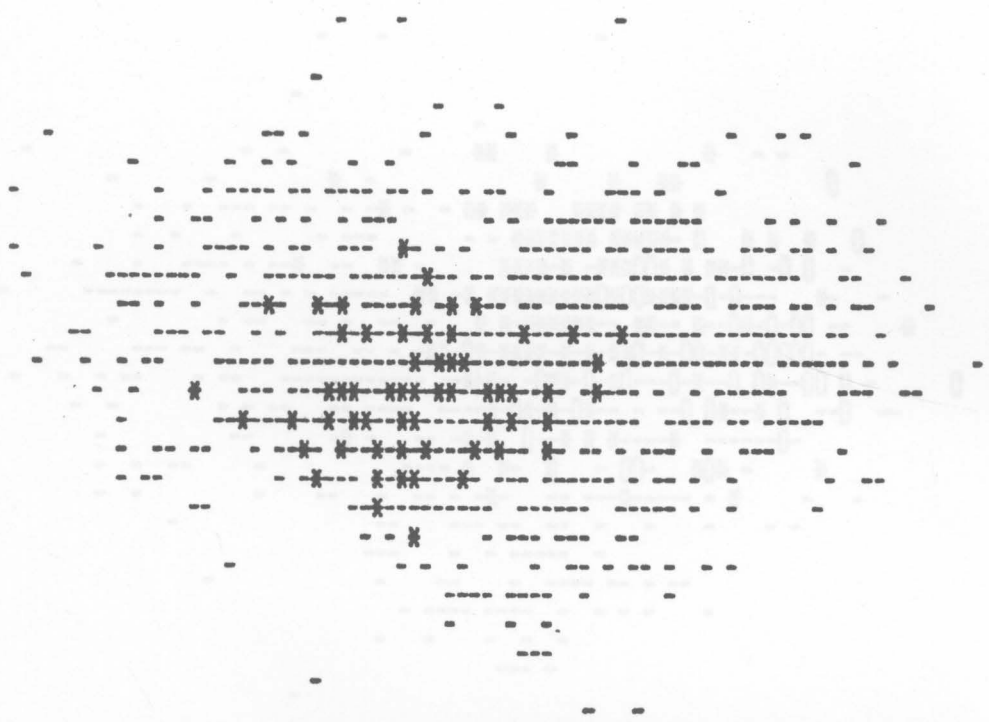


Figure 6.22: Distribution of a typical subset (*) within the plot of all objects (-), in an analysis of random numbers, in terms of the 2nd and 3rd components.



Figure 6.23: Distribution of Zulu crania (0) within the African distribution (*), and the plot of all crania (-), 2nd and 3rd components.



Figure 6.24: Distribution of Berg crania (0) within the European distribution (*) and the plot of all crania (-), 2nd and 3rd components.



Figure 6.25: Distribution of Hainan Chinese crania (0) within the Asian distribution (*), and the plot of all crania (-), 2nd and 3rd components.



Figure 6.26: Distribution of Maori crania (0) within the Polynesian distribution (*) and the plot of all crania (-), 2nd and 3rd components.

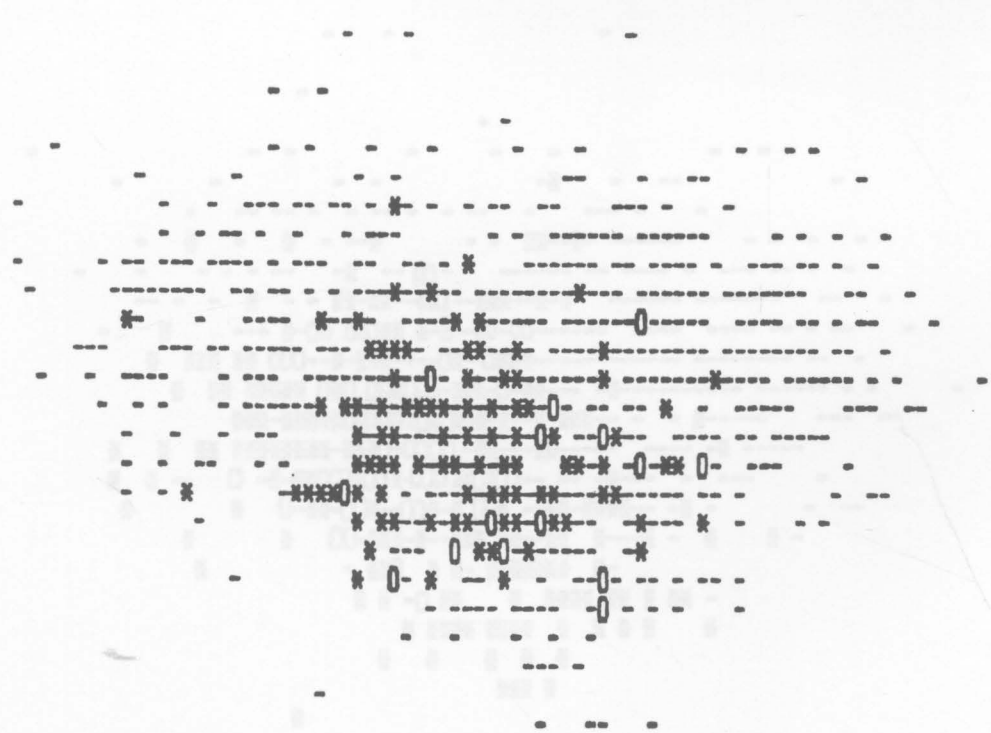


Figure 6.27: Distribution of South Australian crania (0) within the southwest Pacific distribution (*) and the plot of all crania (-), 2nd and 3rd components.



Figure 6.28: Distribution of Peruvian crania (0) within the American/Greenland distribution (*) and the plot of all crania (-), 2nd and 3rd components.



Figure 6.29: Distribution of Arikara (Dakota) crania (0) within the American/Greenland distribution (*) and the plot of all crania (-), 2nd and 3rd components.



Figure 6.30: Distribution of a typical small subset (0) within the distribution of a larger subset (*) and the plot of all objects (-), 2nd and 3rd components, in a random number matrix.

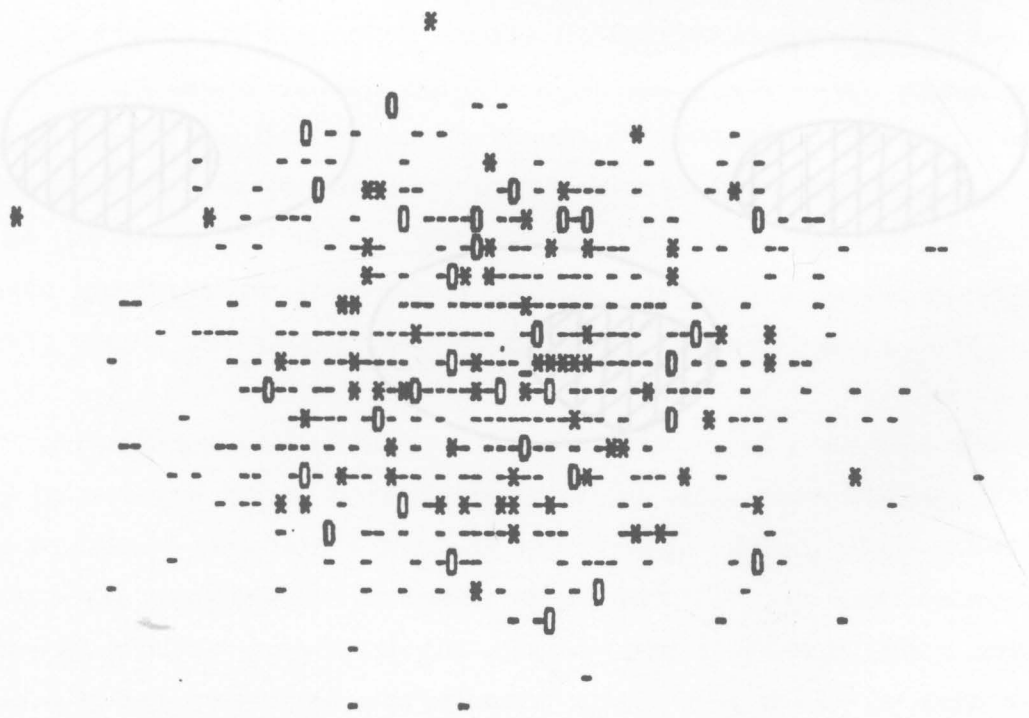
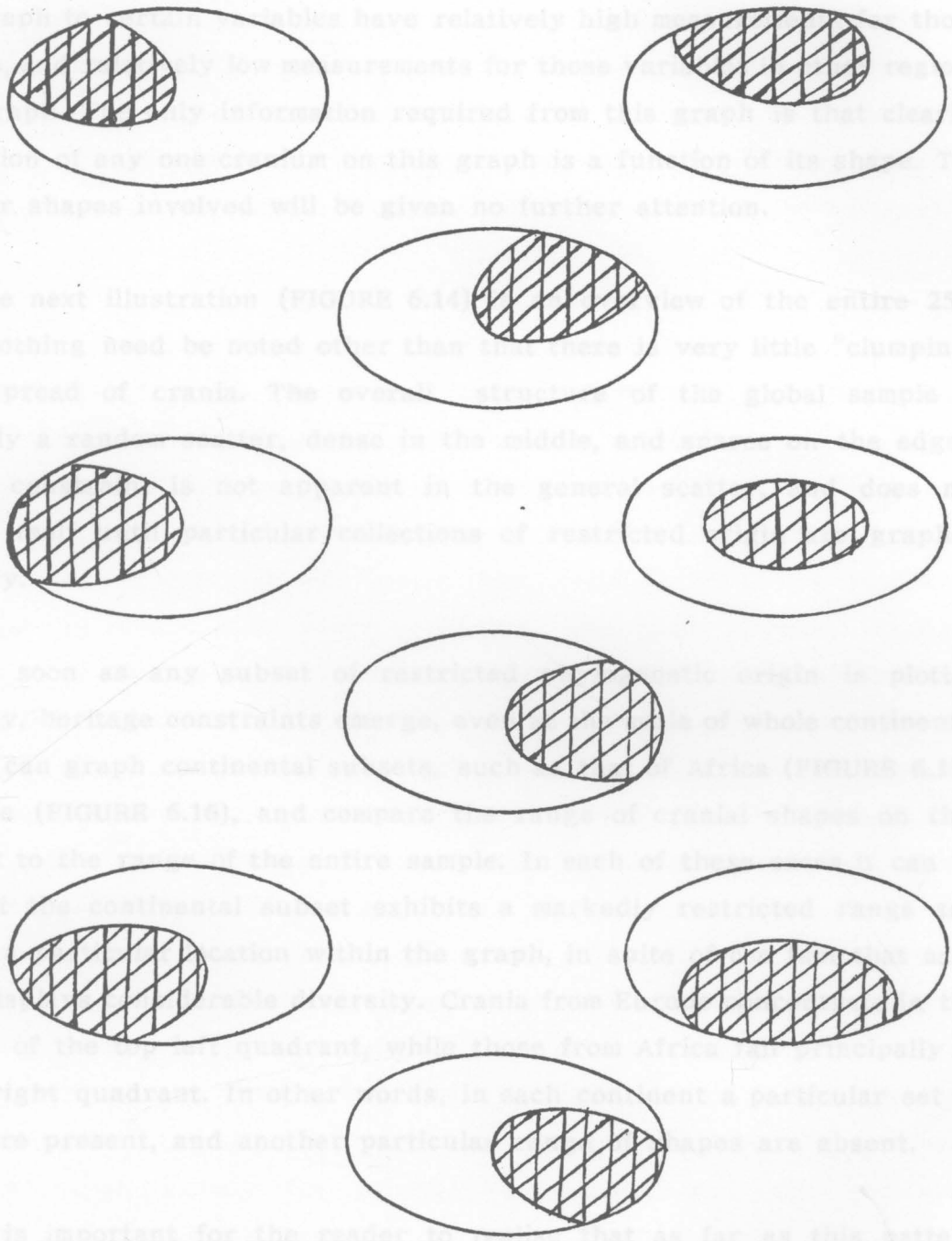


Figure 6.31: Idealised hypothesis for heritage Constraint in the Products of a Darwinian Process. Outer Oval = Plot of all objects, Shading = Plot of a limited heritage subgroup.



just a few. It can also be seen that particular variables are associated with particular areas of the graph. Crania which are positioned in similar regions of the graph to certain variables have relatively high measurements for those variables, and relatively low measurements for those variables in other regions of the graph. The only information required from this graph is that clearly, the position of any one cranium on this graph is a function of its shape. The particular shapes involved will be given no further attention.

The next illustration **(FIGURE 6.14)** is an overview of the entire 2524 crania. Nothing need be noted other than that there is very little "clumping" in the spread of crania. The overall structure of the global sample is essentially a random scatter, dense in the middle, and sparse on the edges. Heritage constraint is not apparent in the general scatter, and does not manifest itself until particular collections of restricted origin are graphed separately.

As soon as any subset of restricted phylogenetic origin is plotted separately, heritage constraints emerge, even at the scale of whole continents. Thus we can graph continental subsets, such as that of Africa **(FIGURE 6.15)**, or Europe **(FIGURE 6.16)**, and compare the range of cranial shapes on that continent to the range of the entire sample. In each of these cases it can be seen that the continental subset exhibits a markedly restricted range and occupies a particular location within the graph, in spite of the fact that each subset displays considerable diversity. Crania from Europe concentrate in the direction of the top left quadrant, while those from Africa fall principally in the top right quadrant. In other words, in each continent a particular set of shapes are present, and another particular range of shapes are absent.

It is important for the reader to realise that as far as this pattern (which will be pursued below) is concerned, factors other than heritage which contribute to cranial variation - such as any number of individual selective constraints in the environment, or habits which influence the development of musculature at various points on the skull - are irrelevant. Each group plotted has a known restricted phylogenetic origin, and it can be seen that

they also display a degree of corresponding morphological diversity. The pattern is purely qualitative; here *is* a restricted subgroup, and we see there *is* heritage constraint. There is no need to ascertain the relative contribution of phylogeny to cranial shape, compared to the many other factors involved, as we would expect the relative contribution of phylogeny to be low. The object of this chapter is merely to establish a procedure for "seeing" heritage constraint (however well hidden it may be) in the products of a neo-Darwinian process.

As other continental subsets are taken, the pattern continues. Asian crania **(FIGURE 6.17)** and Polynesian crania **(FIGURE 6.18)** exhibit similarly restricted distributions within the global sample, but with a tendency more toward the bottom left quadrant, and the centre of the distribution, respectively. Australian and Melanesian crania **(FIGURE 6.19)**, plotted together under the generic category of "South West Pacific", occupy a restricted distribution in the bottom right quadrant. The most dramatic heritage constraint of all the continental subsets is that shown by the American crania **(FIGURES 6.20 and 6.21)**. Occupying the bottom left quadrant, along with the Asian crania, the crania of both North and South America are strikingly similar and even less diverse than the other groups. Many of the shapes of the top left, top right, and bottom right quadrants, which are common in other groups, appear to be totally absent in the American collections. For comparison, a similar sized subset, taken from a matrix of random numbers subjected to PCA, is presented for comparison with all the subsets **(FIGURE 6.22)**. The total lack of the heritage constraint subsetting pattern in a matrix of random numbers (the random null hypothesis), is explored fully in the next chapter, along with the Lamarckian null hypothesis. This single plot is offered in the meantime, merely for intuitive comparison with the restricted distributions of the cranial subsets.

It can be seen that each continental assemblage of crania has conspicuous absences in its distribution. Each distribution is non-randomly positioned in relation to the overall distribution of the global sample. Given that the range of environments (and the presumably greater range of

individual selective constraints and adaptive challenges) on each continent is immense, the most economical explanation of this phenomenon is that certain skull structures lie outside the phylogenetic constraints of the human population of each continent; each population exhibits a kind of large scale "founder effect" at the intercontinental level.

The pattern continues on a smaller scale, when intracontinental subsets are graphed within the distributions of each continental subset. These smaller subsets are usually the 100 skull collections from particular localities. Each particular cranial collection tends to have a skull shape distribution which is more restricted again than the range of shapes in that continent. In other words, each individual group within the continent exhibits further phylogenetic constraints of its own. So, for instance, if one of the five individual African collections is plotted **(FIGURE 6.23)**, it can be seen that the range of shape in a particular group of people is considerably less than the range of the continent upon which they are situated. Here the range of shapes in the Zulu collection can be seen to exhibit a very distinctive distribution within the range of skull shape in Africa as a whole.

Once again a similar pattern emerges throughout the Howells cranial data. If the Berg collection from Austria is graphed within the European range **(FIGURE 6.24)**, the range of shapes exhibited is only half that of the overall European range. Moreover, it is located in the left upper half, at the border of the European distribution, so that all of the European shapes of the right side of the European distribution are absent from the collection. Similarly, the shape distribution of the Hainan Chinese collection **(FIGURE 6.25)** lies well within the general range of Asian crania. While some of the other Asian collections typically include a range of shapes in the bottom left corner of the global distribution, these are absent in the Hainan distribution, where shapes are concentrated toward that part of the Asian distribution closest to the origin. In **(FIGURE 6.26)**, the pattern continues. Maori cranial variation occupies a restricted distribution within the overall Polynesian distribution. Although exhibiting considerable diversity, this New Zealand collection consists

principally of skull shapes which plot in the bottom right quadrant, and are therefore concentrated within this part of the Polynesian range.

Individual collections from the southwest Pacific and America also show limited ranges within their respective continental ranges **(FIGURES 6.27 and 6.28)**. All the South Australian crania have shapes which plot in the right half of the southwest Pacific range, lacking some of the shapes present in the Tasmanian and New Britain collections which plot in the bottom right quadrant, and near the origin. The Youyos collection from Peru is similarly located well within the total American range. However, perhaps because the overall American range is so dramatically constrained at the continental level **(Refer back to FIGURES 6.20 and 6.21)**, and does in fact have a continental diversity smaller than some individual collections elsewhere, certain American collections have almost all of the American shapes. Nonetheless, some of the North American cranial collections, such as that from the Arikara of Dakota, **(FIGURE 6.29)**, display and even more restricted distribution than the North American/Eskimo distribution, in keeping with the overall pattern of restricted origin = restricted diversity. Finally, in **(FIGURE 6.30)**, a similar sized subset (100 objects), taken from a totally random data matrix, is presented. As stated above with **(FIGURE 6.22)**, this single plot is offered only for brief intuitive comparison, as the random null hypothesis is given more complete attention in the next chapter. For the moment, however, it can be seen at a purely intuitive or visual level that the relationship between small subset distributions and overall distributions in random number matrices bear no obvious similarity with the distributions of cranial collections graphed above.

6.5 Concluding Remarks

In the final illustration of this chapter **(FIGURE 6.31)** an idealised summary of structure in the products of a neo-Darwinian process is presented. If we take a series of groups of restricted origin, and plot them

against the distribution of all the groups put together, the restricted ancestry of each group is reflected in its restricted morphology. Examination of the biogeography of human crania has thus provided a refutable hypothesis about the geographic patterns that should be found in cultural assemblages, if cultural change is a fundamentally selective process. If, on the other hand, this pervasive pattern of heritage constraint cannot be found in cultural data, Cultural Selectionism will have been refuted, at least in terms of the local parameters established within the study. Before turning to cultural data to test the heritage hypothesis, however (see Chapter 8), it is important to establish several alternative hypotheses, or "null" hypotheses; and in the next chapter alternative patterns of diversity (to that of heritage constraint) are explored.

The reader is reminded once again that the cranial analysis of this chapter is not intended as any sort of analysis of human diversity, such as those of Durham (1990, 1991) or of Cavalli-Sforza et al (1988) or Cavalli-Sforza (1990). The Howells cranial data has been employed merely to yield an example of biogeography in a neo-Darwinian system, one which can then be employed as a hypothesis in later tests using data from cultural phenomena.

7

PATTERNS WITHOUT HERITAGE

Overview

In this chapter some "non-selective geographies" are given attention. There are at least two other patterns which cultural variation might conform to, two patterns which if demonstrated to exist would refute Cultural Selectionism. These two alternative patterns are referred to as "Lamarckian environmental determinism" and the "Random Evolution" hypothesis. To demonstrate the first of these patterns, the inorganic environments of a globally distributed set of human communities are subjected to Principal Components Analysis. By analyzing the geographic distribution of environmental conditions we can envisage what the geography of a system determined by these environments might look like. Although the variables used offer only a crude description of the environments in question, it still becomes clear that there is no equivalent of the "heritage constraint" pattern to be found in any of the inorganic phenomena under focus. To generate the third "Random Evolution" hypothesis, an artificially generated data matrix composed entirely of random numbers was analyzed, and shown to display a third kind of structure, one quite different to the structure of the "Lamarckian" and "neo-Darwinian" hypotheses.

7.1 A Lamarckian Signature

The logical antithesis to the geographic patterning produced by neo-Darwinian process would be those patterns produced by Lamarckian process (ie. a system in which causal factors actually directed and thereby determined structural variation, rather than merely exerting selective pressure). If, somehow, a definitive signature can now be elucidated from a system in which Lamarckian process operates, it could be used as a convenient "null hypothesis" in the "preliminary test" of Chapter 8. In Chapter 2, it was shown

that although Lamarck denied and dismissed the idea of the environment producing direct modifications in animals, environmental determinism is still a logical corollary of his particular mode of adaptation. Lamarckian concepts were linked up with the modern critique of Cultural Evolutionism, to show that this approach represents an implicit Lamarckian orthodoxy in cultural fields of enquiry. This situation is sustained by the persistence of cultural equivalents of the two main elements of Lamarckism (Progressionism and environmental determinism) in Cultural Evolutionism, and one of them (a form of environmental determinism) in Sociobiology (see Chapters 2 and 3 for a full explanation).

To unite these two approaches under a Lamarckian banner, it was necessary to broaden the definition of "Lamarckism", to the point where any evolutionary process that was fundamentally determined and directed by some "force" was classed as Lamarckian. Thus, both Orthogenesis (Progressionism) and environmental determinism could be considered to jointly constitute a coherent pre-Darwinian evolutionary perspective. It was shown that this broader definition of Lamarckism, far from destroying the essential qualities or spirit of a Lamarckian approach, was in fact more in keeping with Lamarck's original world view than the modern and more narrow definition of Lamarckism, which essentially includes environmental determinism only. In Chapters 4, 5 and 6, the neo-Darwinian view was contrasted with the Lamarckian position, and was seen to be a process where the effects of external forces were far more indirect, and where those forces could only select from within a range of internally and blindly generated alternatives.

The previous chapter might then be considered an exercise in predicting the general patterns one might expect to find in the products of an ongoing selective process. An examination of human cranial variation showed that there is a distinctive and pervasive pattern of diversity which one can consider to be a signature of neo-Darwinian process. The challenge here is to find a corresponding set of general empirical predictions for Lamarckian process models; to find some diagnostic signature of the fact that a Lamarckian process is at work.

Finding a "Lamarckian signature" will involve the same general two-stage procedure as that used in the finding of a "neo-Darwinian signature" in the previous chapter. Firstly, in the next section, the nature of Lamarckian theory is explored in terms of a series of "process diagrams", in order to demonstrate the essential theoretical elements of the broad Lamarckian world view outlined in Chapter 2. This is an intermediate step in the process of translating Lamarckian theory into a testable "Lamarckian reality", a step in which the theory is first translated into the "ideal reality" of a population of seven asexually reproducing lineages viewed through eight generations. The Lamarckian principles discussed in purely verbal terms in Chapter 2 are shown to imply a view of the life sciences that is paradoxically *inorganic*, and as a consequence they imply the kind of geography which would be found in an inorganic system. The nature of this "Lamarckian signature" is then further explored with reference to a body of inorganic data from Douglas White's Maptab programme and the Ethnographic Atlas. The inorganic data was derived from the descriptions of the climatic and topographic conditions which surround particular human communities in the Atlas. The geographic patterns to be found in climatic and topographic data cannot be considered to be "Lamarckian" until these patterns are shown to be a logical empirical outcome of Lamarckian processes. Undertaking this task occupies the next section.

7.2 The Nature Of Lamarckian Process

In Chapter 6 the nature of neo-Darwinian process was illustrated in a series of diagrams, each of which represented an idealised population of seven individuals viewed through eight generations, as seven lineages **(Refer back to FIGURE 6.01 onwards)**. The same heuristic diagrams will now be used to illustrate the nature of Lamarckian process, and to demonstrate its fundamentally different logistics.

A more extensive literary discussion of Lamarckian philosophy can be found in the last 3 sections of Chapter 2. There the cultural selectionist critique of Cultural Evolutionism (and not Cultural Evolutionism itself) is analyzed. It was shown that the neo-Darwinian critique of Cultural Evolutionism can be extended beyond the general demonstration of the "non-Darwinian" nature of prevailing theories of cultural change. Prevailing theories of cultural change were shown to be more than just *not* neo-Darwinian; they can be demonstrated to *be* Lamarckian, in all senses of the word.

So the basic argument of Chapter 2 was that the common non-Darwinian elements of prevailing theories of cultural change, drawn out by the Cultural Selectionist critique, could be brought together and shown to constitute an implicit Lamarckian orthodoxy. However, in that chapter the ontological similarities of modern theories of cultural change and Lamarckian biology were presented in purely verbal form. Here the essential qualities of Lamarckism will be presented in a purely visual form, although not without some annotation.

Although Lamarck's statement *against* direct environmental effects (as highlighted by Freeman 1974:213, see also Chapter 2 in this study) is unambiguous, it was shown in Chapter 2 that other statements made in *Zoological Philosophy* actually imply the opposite. In the strictest of Lamarckian terms, animal structures are determined by habits ("the effects of use and disuse"), and not by the environment. However, as discussed in greater detail in Chapter 2, use and disuse are not the generative causes in and of themselves, but are caused in turn by "needs". As it is difficult to define needs except in relation to the environment, one can deduce that "animal structures determined by animal needs" must be necessarily determined by the environment.

Nonetheless, environmental phenomena are completely excluded from the diagrams, so whether the environment produces direct modifications in the shape of animals or not is largely tangential to the arguments conveyed by

the visual motifs below. The illustrations depict only the internal logistics of evolving systems, and, correspondingly, a comparison of the diagrams of this chapter with those of the last should allow the reader to grasp, through contrasting the visual motifs of this and the previous chapter **(FIGURES 6.01 - 6.12 with 7.01 - 7.05 below)**, the fundamentally different logistics of neo-Darwinian and Lamarckian change.

Let us assume, then, that Lamarckian adaptation can be logically reduced to a form of environmentally determined adaptation. Using the same blank format of **(FIGURE 6.01)**, the first visual motif of this chapter, **(FIGURE 7.01)**, is a visual representation of Lamarckian adaptation. Its neo-Darwinian opposite, for visual comparison, is the representations of neo-Darwinian adaptation in Chapter 6 **(FIGURES 6.05 and 6.06)**. The endpoint, a full transformation of the population from white objects to black objects, is the same in each case. The different courses taken toward the same endpoint represent the important differences between Lamarckian and neo-Darwinian adaptation. The change from light to dark can be considered as generated by either internally defined (Lamarckian progressionist) or externally defined (Lamarckian adaptationist) needs, as needs are not represented in the visual motif. All we see is the effect of an unspecified Lamarckian need on a population.

What is important in the first depiction, **(FIGURE 7.01)**, is that no differential reproductive success - no constriction of the heritage of the population - is involved in the transformation. As the change is generated by new needs which are shared by all the lineages, the adaptive response occurs simultaneously in each. There is no single unpredictable "event" or mutation required before the change can be set in motion. As the needs are the generative force, they begin to take effect as soon as they appear, actually producing the changes which a selective system requires to be produced by some other means. This kind of effect is represented by the gradual synchronised changes in shading - from white to black - through which each lineage passes. The morphology of the final generation is thus not a function of the differential success of a mutant lineage; it is a function only of

prevailing needs and the immediate heritable changes they produce. And as each and every change imparted is heritable, no *differential* heredity is required for whole populations to change in concert, as a result of all the isolated changes occurring in their constituent lineages.

The change represented in the first Lamarckian diagram **(FIGURE 7.01)** involves, therefore, no change in the complexity of the prevailing forms of the population. As a consequence it depicts the logistics of the Lamarckian principle of Acquired Characteristics, or environmental determinism (those traits generated by needs), and this principle alone (see Chapter 2, section 2.3). The next illustration **(FIGURE 7.02)** depicts a second element of Lamarckian process, that of orthogenesis and progressionism, where the "essential fluid" of each animal lineage becomes "more complex and animalised" (see Chapter 2, section 2.4). Once again the diagram expresses the fundamentally isolated manner in which this essential process independently takes effect in each lineage. No differential reproduction is involved; the changes are a result of an "in built complexity clock". The final complex state reached by the last generation is a result of the fact that this force operates independently on each lineage to produce the same results. The similarities of the seven individuals in this final generation is not a function of common phylogenetic heritage, but of common influence. They are similar as a result of being dictated by common laws, not by virtue of common origin.

These two representations of Lamarckian forces can be combined to produce a visual motif of the nature of process in the Lamarckian world. Firstly, by combining **(FIGURE 7.01)** and **(FIGURE 7.02)**, the interaction of orthogenesis and Lamarckian adaptation can be represented **(FIGURE 7.03)**. The effects of prevailing "needs" never override the ongoing process of orthogenesis, and increasing complexity therefore proceeds throughout any one episode of environmentally generated adaptation. Orthogenesis is however a much longer term process than adaptation; while structures produced by Lamarckian adaptation merely reflect prevailing environmental conditions, the degree of complexity reached reflects the antiquity of the lineage.

Over a long period of orthogenesis, then, the external conditions of a lineage might pass through many fluctuations, while orthogenesis is gradually increasing the complexity of the organism **(FIGURE 7.04)**. If the diagram is interpreted on a much larger scale, by allowing each row of seven organisms to represent not one but many generations in a condensed form, the appropriate adjustments can be made. The eight generations can now crudely summarise the entire history of a group of lineages. Orthogenesis has a cumulative effect on the complexity of the population, while the environmental conditions (represented by the degree of shading) can fluctuate back and fourth. So, for example, the modern whale lineage may, as it passed along the chain of being, have experienced a number of different periods of adaptation to underwater conditions, interspersed with periods of adaptation to land conditions, so accumulating the effects of conflicting environmental effects. Beginning, like all organismic lineages, in a simple form, it may have consisted of micro-organisms such as plankton living in the sea, which evolved into insects which became adapted to land, then back into fish which required further adaptation to water, into reptiles and mammals which had to adapt to land again, before finally returning to the water as mammals and adapting again to water.

So, in the Lamarckian world view, different ways of life, different habits and living conditions can only produce limited variation, since they cannot override the fundamental structural determinant of organismic morphology (progressive orthogenesis). How then does Lamarck account for the obvious fundamental orthogenetic differences that exist between the major organismic classes in the modern world? Lamarck's solution was to argue that all lineages began at different times (again see Chapter 2, section 2.4, for a purely verbal discussion) with an event of spontaneous generation, in which a very simple organism was produced from inorganic phenomena. Depending on the antiquity of lineage, i.e. how long it had existed since that original spontaneous event, the longer the lineage had been exposed to the effects of orthogenesis. Thus **(FIGURE 7.05)**, the profound structural variations of modern animals – such as the differences between insects and mammals, or between worms and echinoderms – are a result of different lineages having different antiquities.

Thus this diagram summarises the entire Lamarckian world view, and illustrates how Lamarck's "chain of being" can extend into the past and encompass the present all at once. Each modern form is the result of a orthogenetic development, which proceeds at a fixed rate from the moment of spontaneous generation **(the vertical dimension of FIGURE 7.05)**. However, some lineages have been around longer than others, and as a consequence, some have attained greater complexity than others (hence the variation along the horizontal dimension of the final generation). This is the Lamarckian taxonomic system often referred as the "typological approach", and the diagram attempts to capture the fact that rigid "types" are not created by punctuations in orthogenesis (as this is a very gradual process) but by the different ages of particular lineages. As orthogenesis proceeds at a fixed rate, all lineages of the same age, subjected to the same material conditions, constitute a "type", and all lineages of different ages necessarily belong to different types, regardless of the conditions to which they are adapted. The necessary punctuations between types, which occur in the "typological approach", are represented in this diagram by the structural differences in the shapes of the final generation, each substantial difference being a function of *antiquity*.

It can be seen that none of the above Lamarckian principles involve any necessary influence of phylogenetic heritage upon morphology. None of the fundamental "forces" of the Lamarckian world have, in fact, anything to do with reproduction or heredity at all, and could therefore be as easily applied to an inorganic system in which reproduction was, by definition, absent. In a Lamarckian world, reproduction only serves the purpose of a ensuring that organisms (and their acquired adaptations and orthogenetic increased complexity) continue to exist. In other words, reproduction serves only to preserve the *status quo* through counteracting the effect of death; it plays no role in the actual process of change. If biologists were to discover that there was no such thing as reproduction in biology, and that each organism was actually an immortal being that was billions of years old, there would be no need to alter any part of the Lamarckian paradigm to accomodate the new discovery.

As a consequence there is really no fundamental difference between pre-Darwinian theories of biological evolution, and notions of inorganic evolution in disciplines such as geology and astronomy. There is insufficient space within the study to pursue this theme of the fundamentally inorganic nature of Lamarckism in all its aspects. Proving that all non-Darwinian western science is united by a single non-selective evolutionary paradigm would be a study all of its own. It is worthwhile providing a brief outline of this argument, however, in order to illustrate an important point which can be made about Lamarckian evolutionism. The fact that this kind of philosophy is inappropriate for cultural fields of enquiry is not a result of it being unscientific (as Dunnell 1980:38 has asserted), but is rather a result of it being *too inorganic* in its attempt to be scientific, and therefore basically "lifeless", as it were. The aim of the next part of this section, then, is not to *convince* the reader of the inorganic nature of Lamarckian theory, but merely to show the surprising degree to which this would *appear* to be so.

Some evidence for the notion can be found in the fact that some Lamarckians explicitly incorporated inorganic phenomena under the umbrella of their evolutionary philosophy. Freeman, for example, has comprehensively documented that the most famous and productive Lamarckian, Herbert Spencer, eventually advanced the Lamarckian principle of progressionism as a universal law of both organic and inorganic fields of enquiry. In a scholarly combination of quotes and text, Freeman writes:

> "During the next few years Spencer became increasingly preoccupied with the implications of his Lamarckian hypothesis. His *Principles of Psychology*, on which he began work in August, 1854, while on vacation at Treport, sprang from the implication (Spencer 1904 [1882], vol. 2.10) "that not only had bodily organisation been naturally evolved, but mental organisation too," and was based on the Lamarckian premise (Spencer 1855:526) that "a modified form of constitution produced by new habits of life, is bequeathed to future generations." And it was at Treport while engaged in his audacious attempt (1904 [1882], vol. 2:11) to trace out in Lamarckian terms "the genesis of mind in all its forms, sub-human and human" that Spencer (p. 167) was "suddenly led to the perception" (reaching far beyond his "hypothesis" of 1852) that a change "from the homogenous to the heterogenous" is a "universal trait of progress, inorganic, organic and super-organic," and to the realization, which he felt to be

> momentous, that the multiplication of effects is everywhere the cause." (Freeman, 1974:215).

Although this universal scheme has been attacked along various fronts, the fact that Spencer himself saw fit to extend his theory to inorganic phenomena reflects the fact *that the theory did not rely on any process which was unique to life*, such as reproduction. Spencer's theory - being Lamarckian not just with respect to the principle of environmentally directed invention (the manner in which Freeman uses the epithet "Lamarckian") - but in every way (see last section of Chapter 2), did not incorporate any principle of reproductive success. Spencerian evolution, therefore, is not subject to phylogenetic heritage constraint, anymore than is evolution in inorganic phenomena. It is, then, perhaps no accident that Spencer chose to idolise the quintessential inorganic determinism of Newton. As Freeman writes, Spencer hoped that one day his own work would achieve the wide acceptance that the work of Newton had at that time:

> "His conclusions, he determined (Duncan 1911:547), he would set forth in an essay entitled "The Cause of all Progress," and his work, as he confided to his father soon afterwards (1904 [1882], vol. 2:75), would, he believed, "ultimately stand beside Newton's Principia." (Freeman 1974:215).

Due to Spencer's nervous breakdown the essay was not published until 1857 in the Westminster Gazette (Freeman 1974:215). The basic point to be made is that Spencer's theory was both explicitly Lamarckian, and explicitly extended to inorganic phenomena. Moreover, inorganic and organic order were seen to be the result of a single, omnipotent force. As Freeman details, no empirical correspondent for this force was offered, nor even sought:

> "that "the transformation of the homogenous into the heterogenous, in which all progress, organic or other, essentially consists, is consequent on the production of many effects by one cause - many changes by one force." The "persistence" of this "force," Spencer maintained, made progress inevitable. At no point, however, either then or later, was the nature of this "force" and its "persistence" explained in empirical terms. His "ultimate principle" of the "persistence of Force," so Spencer declared (1904 [1882], vol. 1:554), was an aspect of a "Power" that "transcends human knowledge and conception."" (Freeman 1974:215).

To sum up the brief outline of this argument, perhaps Spencer's grand synthesis from the organic *back to the inorganic*, reflected the fact that the

Figure 7.01: Heuristic Diagram of Lamarckian Adaptation Illustrating a Synchronised Response to Environmental Conditions.

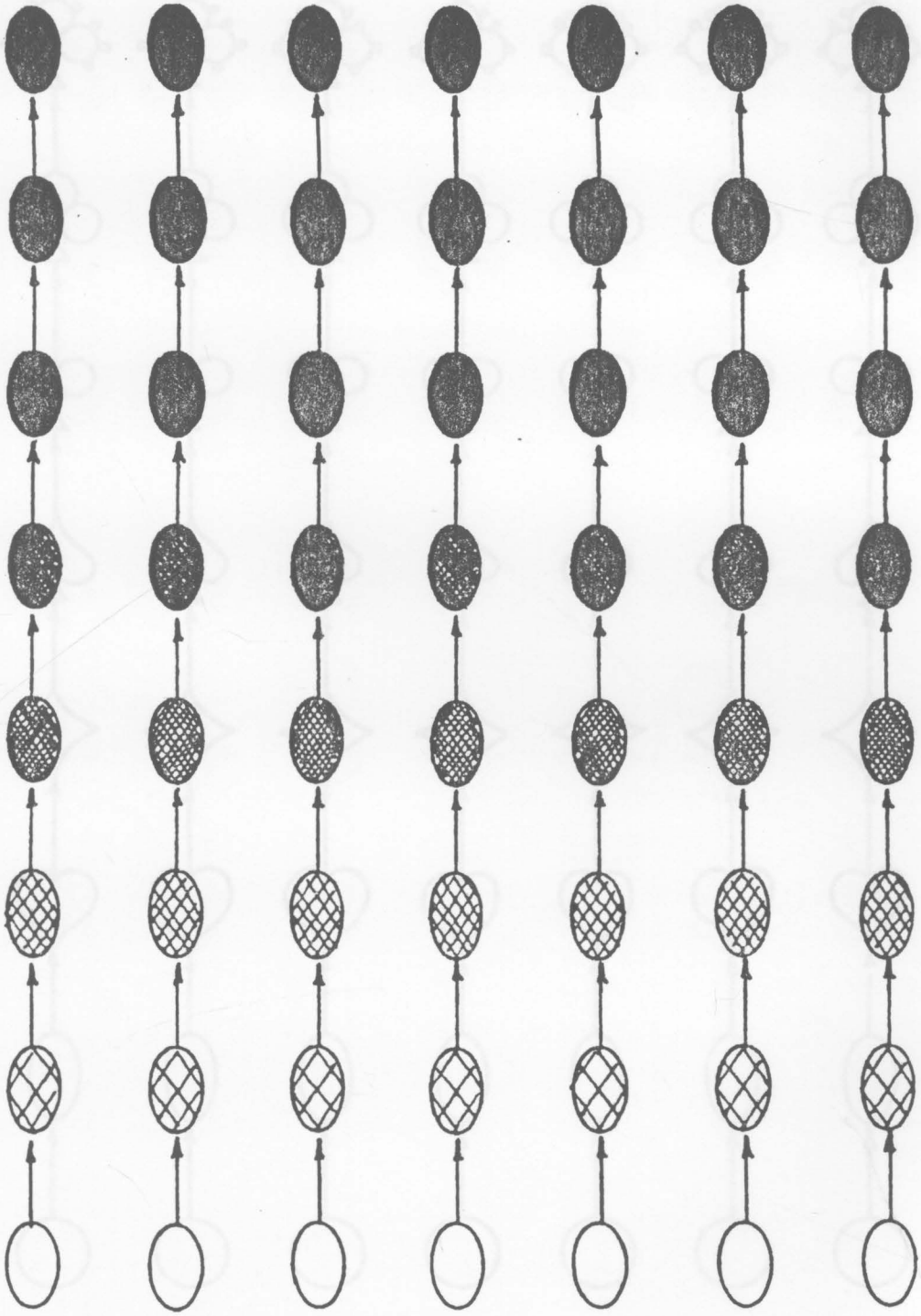


Figure 7.02: Heuristic Diagram of Lamarckian Orthogenesis Illustrating a Synchronised Tendency Toward Increasing Complexity.

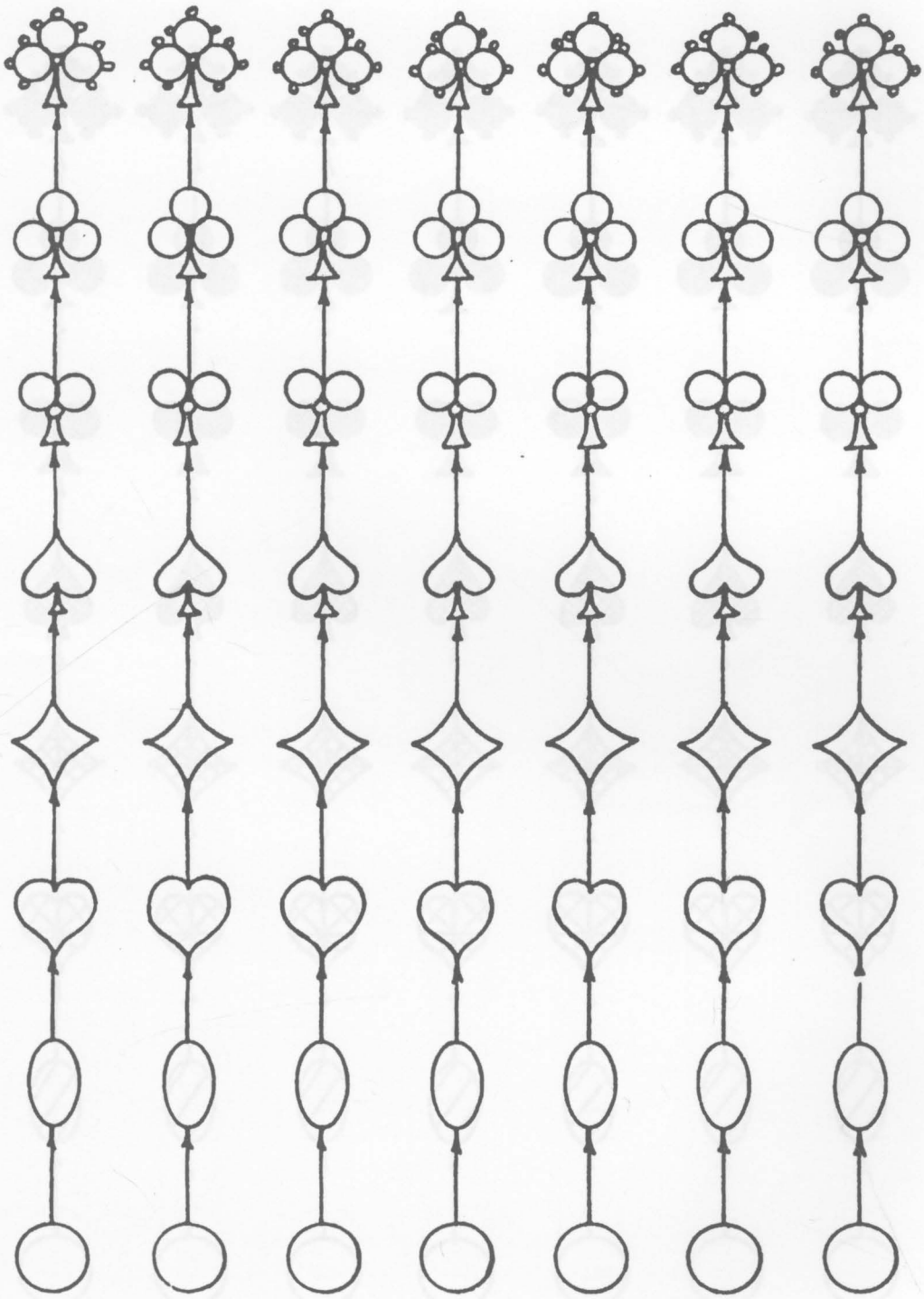


Figure 7.03: Heuristic Diagram of Combining Lamarckian Adaptation and Orthogenesis Illustrating a Synchronised Tendency Toward Increasing Complexity, and Environmental Response.

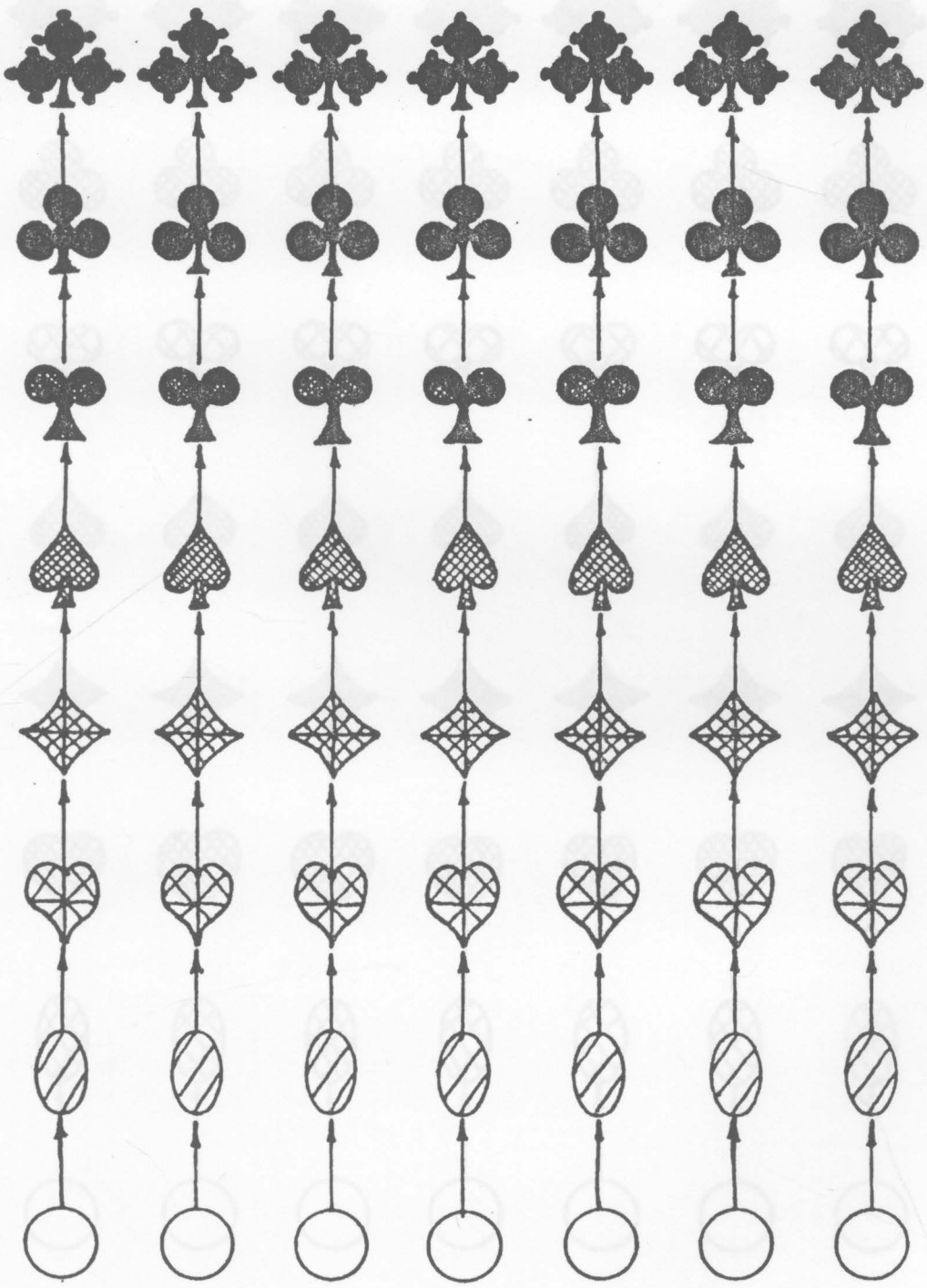


Figure 7.04: Heuristic Diagram Combining Lamarckian Adaptation and Orthogenesis on a Larger Time Scale, Illustrating Long Term Oscillation of Environmental Conditions.

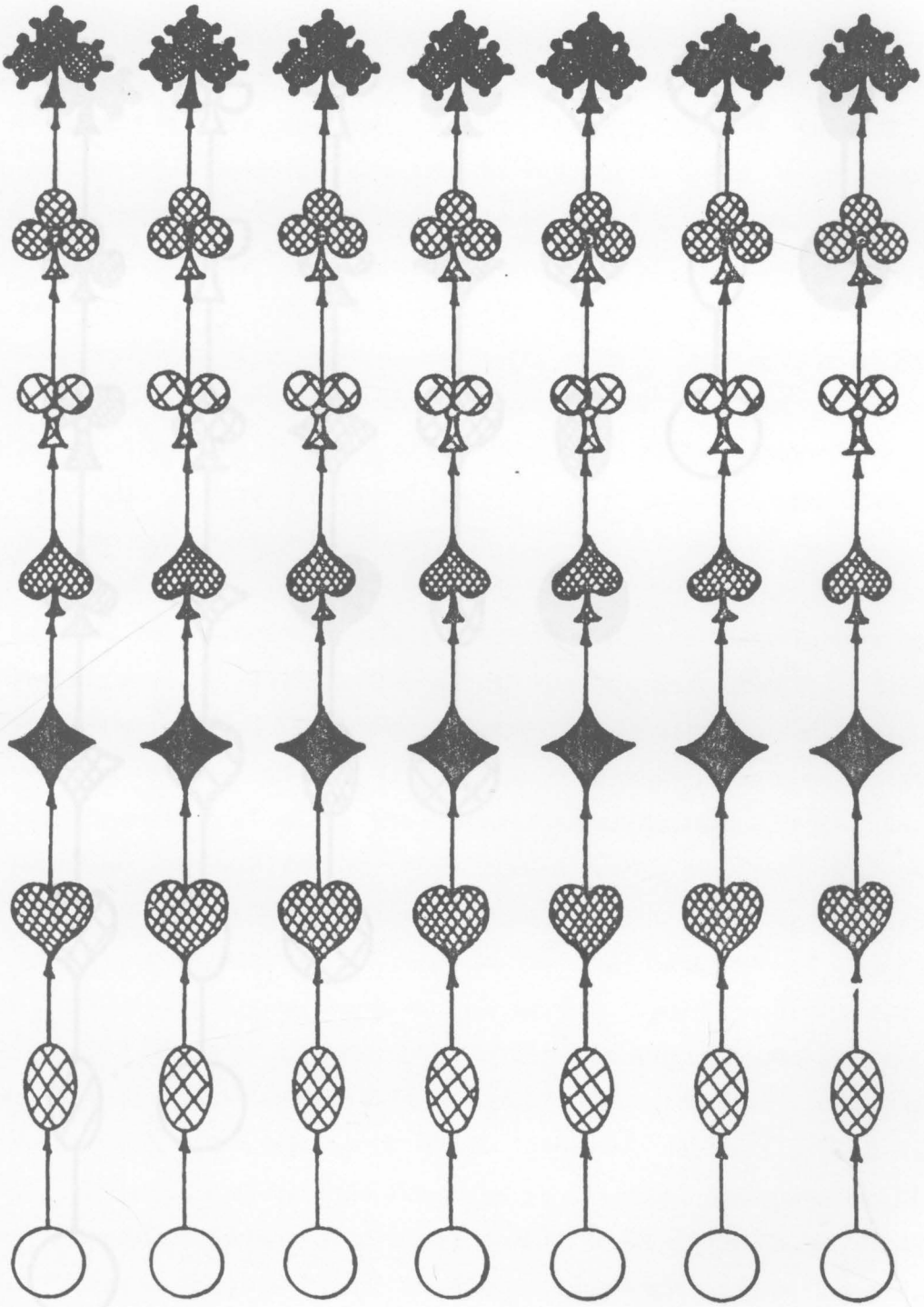
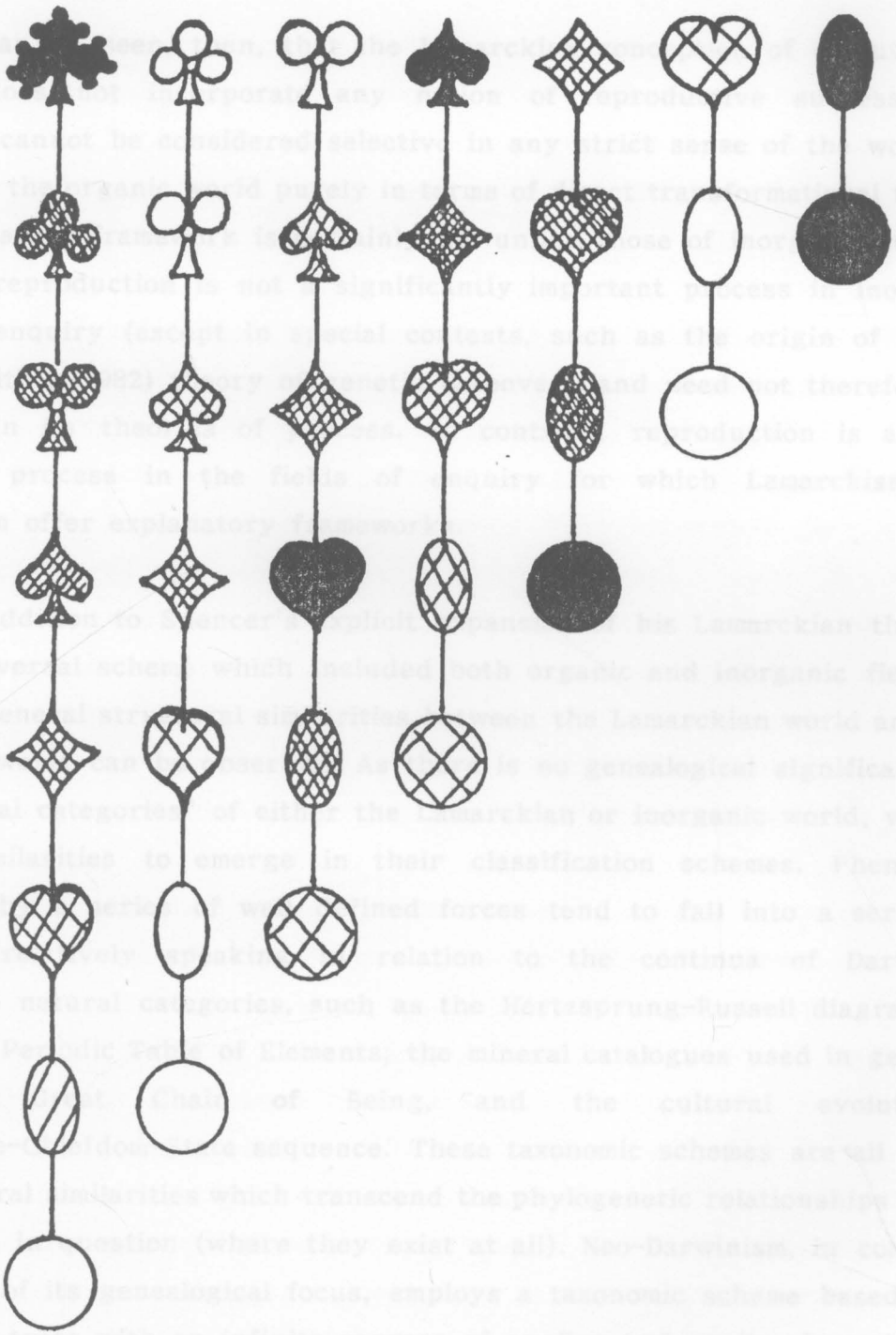


Figure 7.05: Heuristic Diagram Illustrating How Orthogenesis and Lamarckian Adaptation Combine to Produce the Lamarckian Great Chain of Being.



invention of the original Lamarckian world view was an extension which occurred *in the opposite direction*, and which then did not transcend its original inorganic heritage.

It can be seen, then, that the Lamarckian conception of evolutionary process does not incorporate any notion of reproductive success, and therefore cannot be considered selective in any strict sense of the word. In explaining the organic world purely in terms of direct transformational forces, the explanatory framework is certainly not unlike those of inorganic science. However, reproduction is not a significantly important process in inorganic fields of enquiry (except in special contexts, such as the origin of life in Cairns-Smith's (1982) theory of genetic takeover), and need not therefore be included in its theories of process. In contrast, reproduction is a very important process in the fields of enquiry for which Lamarckism and Spencerism offer explanatory frameworks.

In addition to Spencer's explicit expansion of his Lamarckian theories into a universal scheme which included both organic and inorganic fields of enquiry, general structural similarities between the Lamarckian world and the inorganic world can be observed. As there is no genealogical significance to the "natural categories" of either the Lamarckian or inorganic world, we can expect similarities to emerge in their classification schemes. Phenomena produced by a series of well defined forces tend to fall into a series of discrete (relatively speaking in relation to the continua of Darwinian categories) natural categories, such as the Hertzsprung-Russell diagram for stars, the Periodic Table of Elements, the mineral catalogues used in geology, Lamarck's Great Chain of Being, and the cultural evolutionist Band-Tribe-Chiefdom-State sequence. These taxonomic schemes are all based on structural similarities which transcend the phylogenetic relationships of the phenomena in question (where they exist at all). Neo-Darwinism, in contrast, by virtue of its genealogical focus, employs a taxonomic scheme based on a number of trees with an infinite regress of smaller and smaller branches.

Stars, for example, are categorised into a series of "evolutionary categories", which occur in a fixed order from the birth of the star. Like spontaneous generation in Lamarckism, where the simplest organisms in the Great Chain of Being are generated from non-organismic phenomena, each star lineage begins with the generation of the first category of stellar classification from non-stellar phenomena. This occurs when each star is formed out of diffuse clouds of interstellar medium, which principally consists of hydrogen and helium in the ratio of ten to one, with the addition of some heavier elements.

Once stars are formed, they then pass through one of several different evolutionary sequences (depending on their size), in a manner reminiscent of the concept of orthogenesis depicted in the second diagram of this chapter **(FIGURE 7.02)**. Which sequence they pass through is largely dependent upon the size of the star. Stars burn until they have exhausted their nuclear energy supply, and the larger the star the larger that supply. If a star is large enough, it may pass through stages such as those called "red giant", a supernova, a white dwarf (dense white stars with diameters comparable to planets), a neutron star (extremely dense, sometime with a diameter of only 20 kilometres), and if the star is large enough it may eventually collapse into a black hole.

In addition to this general development through a set sequence of stages, there are interference factors which, like the Lamarckian adaptation "force" of the first diagram of this section **(FIGURE 7.01)**, does not override or reverse stellar evolution (which largely depends on the initial size of the energy source and its subsequent steady decrease). Nonetheless, a star at any particular stage of its life will be affected by local conditions. No two stars are exactly the same, as local formation conditions vary. Young stars formed from more recent interstellar medium, for example, have higher proportions of the heavier elements, while the oldest stars were originally formed entirely from hydrogen and helium. This is because the interstellar medium now contains matter from old stars which have exploded, releasing the heavy elements produced during their lifetime into interstellar space. So aspects of

local conditions also affect the structure of stars, although the primary determinant still involves the interaction of size with age during stellar evolution.

Finally, there is even a "stellar equivalent" of the Lamarckian "typological approach" **(FIGURE 7.05)**. The theory of stellar morphology and evolution makes it possible to determine the age of a star from its "type". Some types only occur late in the sequence, while others only occur at the beginning. Stars continue to shine as long as energy is released from nuclear reactions in their interiors, and as this energy resource is gradually depleted changes in their structure can be observed. This is the basis on which the stars at the centre of our galaxy are judged to be much older than those in the spiral arms.

All in all, it can be seen that like Lamarckian taxonomy, stellar taxonomy (predictably and more justifiably) uses categories not of common descent, but common "age since birth or spontaneous generation", embellishing this basic evolutionary lineage with contingent local factors such as size and substance proportions. And finally, stars fall into a number of fairly discrete categories in the present, as a result of sharing common structure, but due to sharing the same generative causal forces and ages, not as the result of some common stellar "ancestor".

Although the issue has only been given the briefest attention, a superficial comparison of the theoretical structures of stellar evolution and the Lamarckian world view reveals a striking similarity. When this fact is added to other points made in this section, such as the absence of any significant use of the notion of differential reproduction or phylogenetic heritage constraint in Lamarckism, and the fact that Spencer saw no difficulty in extending the theory to inorganic fields of enquiry,1 Lamarckism would seem a highly inappropriate philosophy to apply to any living system, and particularly one in which differential reproductive success, with small amounts of "copying error", is one of the basic ordering forces.

In any case, a evolutionary framework which is arranged around the concept of needs, whether they be intrinsically created by orthogenesis, or extrinsically created by changing environmental conditions, should be refutable by an analysis of global geography. If the theory is correct, the biogeography of organismic morphology should principally reflect the geography of those needs. Related organisms exposed to the influence of different needs should reflect this fact, and become substantially different; the structural signatures of common heritage should be steadily erased by structures generated out of necessity.

Lamarckian evolutionism would therefore predict a distribution of animal or cultural diversity which directly reflects the distribution of climatic and geological conditions, and basic qualities of habitats which are not directly dependent upon the phylogeny of other organisms (for a hypothetical example, the general presence of a category like "large herbivores", irrespective of whether they are placental mammals, marsupials, birds or reptiles such as giant tortoises; or a vegetation category like "rainforest" or "steppe" - two rainforests, for example might contain completely unrelated forms of trees such as eucalypts and beeches, respectively, yet offer a similar range of resources when considered as human habitats).

For this purpose, a Principal Components Analysis of some of the inorganic, and "nonphylogenetic biotic" (such as qualities of general "resource availability" just mentioned above) conditions surrounding a global sample of human communities is undertaken in the next section. It will be shown that inorganic/nonphylogenetic geography exhibits very different patterns of distribution to that found in selective systems such as the cranial data analyzed in the last chapter. Predictably, descriptions of non-phylogenetic aspects of the environment turn out to exhibit a far more discrete clustering pattern - to a certain extent a "periodic table of environments" emerges (although with a lot of isotopes!), and from this pattern, as will be discussed below, a Lamarckian hypothesis for the geography of cultural assemblages is elucidated.

The reader is reminded that the inorganic data used in the next section is actually *doubly* Lamarckian. It is firstly Lamarckian in the sense that any body of inorganic phenomena, due to the arguments presented above, can be expected to display similar geographic distributions to those we might imagine to exist in a hypothetical Lamarckian system. But the environmental variables used below are doubly Lamarckian since they are recordings of the inorganic and non-phylogenetic conditions which constrain a global sample of human communities, the very same communities from which the global sample of cultural assemblages used in the next chapter are taken. Being inorganic, they can be expected to possess Lamarckian patterning in and of themselves; being of the kinds of factors which define human *needs*, they can be expected to further *impose* a Lamarckian pattern on the cultural assemblages they surround.

7.3 Elucidating A Lamarckian Geography From Climatic, Topographic, and Nonphylogenetic Biological Data

If it were possible to find a globally distributed class of phenomena in which structure was governed by Lamarckian rather than neo-Darwinian process, it would be possible to elucidate the spatial patterns produced by such processes simply by investigating the actual patterns present in any such system. This procedure would be equivalent to the way in which spatial patterns derived from selective process were elucidated in the last chapter. Unfortunately, there is no such system in which Lamarckian environmental determinism is generally agreed to occur.

However, the simple relationship between environment and a given adapting entity which is specified in any Lamarckian explanatory framework makes it possible to define the nature of Lamarckian biogeography via a different conceptual route. As suggested above there is no known Lamarckian biological system within which biogeographic patterns can simply be empirically demonstrated after the manner of Chapter 6. Yet any deterministic

relationship between a class of causes (in the "extra-cultural" Lamarckian case environmental conditions) and their corresponding class of effects (biological structures) presupposes complete co-occurrence of the two. Therefore, the biogeography produced by Lamarckian process can be inferred via an empirical investigation of the geographic patterns in the *evironmental causes* themselves.

Which inorganic or non-phylogenetic environmental phenomena are used to define a Lamarckian hypothesis is irrelevant. The aim is rather to find a different kind of geographic patterning to the neo-Darwinian one, one entirely without phylogenetic constraint, *in any set* of non-phylogenetic environmental phenomena. Biological resource information, such as "the presence of large herbivores", can be used to define a Lamarckian geographic hypothesis, but phylogenetic assertions, such as "the presence of Kangaroos" cannot be used, as such statements would allow the implicit phylogeny of biogeography to lead the analysis back to a neo-Darwinian pattern. A series of statements about environmental conditions are required which contain no phylogenetic imformation about animals or plants.

In other words, the determinative nature of causal relationships between environment and adapting structure in Lamarckian theory allows the extrapolation of a "Lamarckian" hypothesis from an investigation of the environmental factors themselves. An investigator does not have to scour the earth in order to find some "real" Lamarckian system in order to discover the geographic signatures associated with it, since these signatures would only be reflections of the external forces which produced them in any case. Ideally, what is required is an hypothesis of Lamarckian geographic patterning which corresponds closely to the spatial distribution of the cultural test cases used in the next chapter. If the implicit Lamarckian orthodoxy of cultural fields of enquiry is correct we can expect the geographic variation in such an environmental data set to correspond closely to variation in the cultural data set (although the reader is reminded that such a procedure only tests the materialist half of the Lamarckian orthodoxy). Fortunately, it was possible to compile a data set of "material conditions" for the same human communities

amongst which patterns of cultural diversity are examined in the next chapter. All of the environmental data was derived (with some translations) from Douglas White's Maptab Demonstration Diskette. White's "climate" variable was broken into six "presence or absence" categories (see description below). A final variable, distance from the equator, was added as a crude indicator of the seasonality of available light. It is important to realise that no attempt is made to represent the environments in their entirety - all that is important is that the few variables selected are non-cultural, non-genetic, and therefore non-Darwinian in character, so that a least one example of a non-selective pattern will emerge. If a heritage constraint pattern emerges, then this would refute any notion of heritage being some kind of *de facto* Darwinian signature.

The variables are listed below and described briefly, in the final order into which they were arranged for the Principal Components Analysis. In plots of the variables below, these numbers become the codes for the variables. Some non-cultural

TABLE 3: THE ENVIRONMENTAL VARIABLES

Variable 1: Dryness, measured via the number of dry months each year (variable number 7 in Maptab).

Variable 2: Agricultural potential of the vicinity. A synthesis of a number of variables, including topography, soil types, and climate scales, a grading from 4 - 23 (variable number 8 in Maptab).

Variable 3: Population Density, a grading from 1 person per five square miles to over 500 people per square mile.

Variable 4: Dominant vegetation/ecological structure of environment conforming to the "Polar" climate type (Derived from variable 7 in Maptab).

Variable 5: Dominant vegetation/ecological structure of environment conforming to the "Desert" or "Cold Steppe" climate types - combined because of similar lack of access to "Rich Ecological Resources" as defined by White. (Derived from variable 7 in Maptab).

Variable 6: Dominant vegetation/ecological structure of environment conforming to the "Tropical Rainforest" climate type (Derived from variable 7 in Maptab).

Variable 7: Dominant vegetation/ecological structure of environment conforming to the "Moist temperate" climate type (Derived from variable 7 in Maptab).

Variable 8: Dominant vegetation/ecological structure of environment conforming to the "Tropical savanna" climate type (Derived from variable 7 in Maptab).

Variable 9: Dominant vegetation/ecological structure of environment conforming to the "Tropical highlands" climate type (Derived from variable 7 in Maptab).

Variable 10: Distance from equator in degrees - as a crude estimate of the seasonality of available light (Derived from the map co-ordinates listed in the Ethnographic Atlas).

biological constraints, such as vegetation and human population density, are also included, to give a more complete description of the material conditions surrounding each of the 157 cultural assemblages analyzed for the purposes of testing Cultural Selectionism in Chapter 8. There are ten "environmental"

variables in all, a relatively small number, but several of them are actually a synthesis of quite a number of variables (See Table 3).

Measurements or descriptions for these variables were compiled for the environments of 157 societies in the Standard Cross Cultural Sample. A more complete list of these societies is provided in the relevant section of Chapter 8. Here they will be listed in their continental and language subgroups only, in order to provide a brief reference list for interpreting the results. These results are presented as a series of figures a little further on.

The series of environments described and compared in the Principal Components Analysis of this chapter surround the series of societies listed in Table 4 below. They are numbered according to their positions in the original data matrix subjected to PCA. They are classified according to the cultural heritage of the societies they surround (a combination of geographic origin and language affiliation - see sections of next chapter, 8.2 to 8.3). In other words, they are classified below (see Table 4) into groups in which the *cultural* assemblages of the next chapter are also classified in order to test for the heritage constraint pattern. If these environmental phenomena were produced by selective process, then these are the groups in which "environmental" heritage constraint would be most likely to be manifest, if it is to be present at all. As will be seen, no such pattern emerges, and the notion that heritage constraint is a property of selective process remains unrefuted. There is more information on these societies to be found in Chapter 8, where the cultural data is itself discussed (sections 8.2 to 8.3).

The 157 environmental descriptions were then subjected to the same Principal Components transformations applied to the cranial data of Chapter 6. Parallel metaphorical accounts of the data manipulations involved, in terms of the particular kind of data used, can be found in each of the three data chapters, of which this is the second.

Basically what the program does is to compare each string of environmental readings with each of the others, firstly in terms of the first

variable, and then in terms of variables one and two, eventually comparing the profiles in their entirety. The environments are ranked in order from the smallest to the largest readings for a particular variable (thus it ranks the environments from those with the smallest number of dry months to the largest), then the programme moves to the next variable (ranking the environments from those with the greatest potential for agriculture to those with the least), and the next, and so on.

Figure 7.06: Plot of all environmental variables in terms of the 4th and 5th principal components.

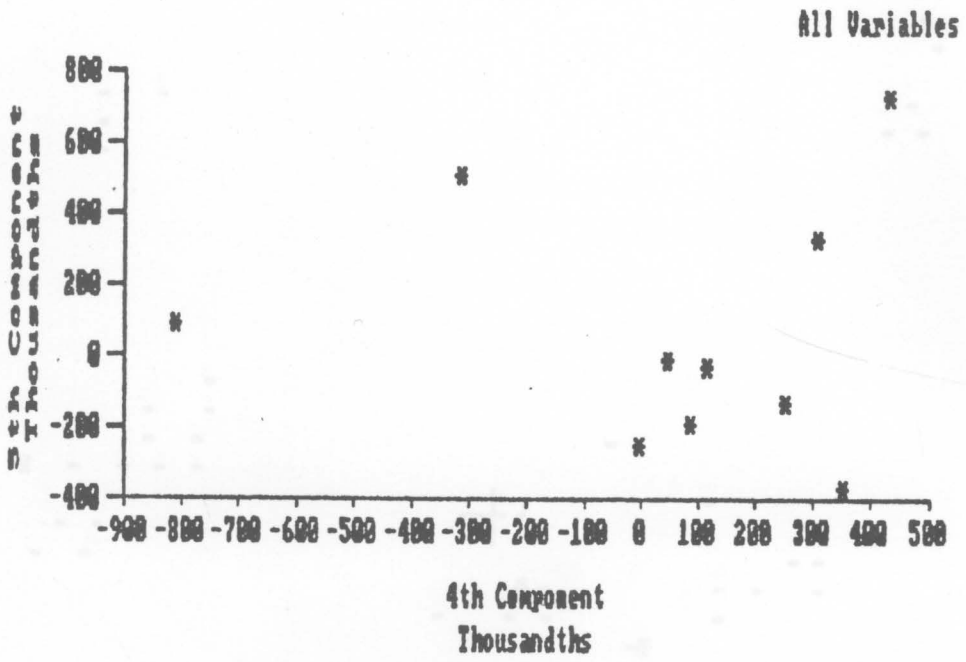


Figure 7.07: Plot of all environments in terms of the 4th and 5th principal components.

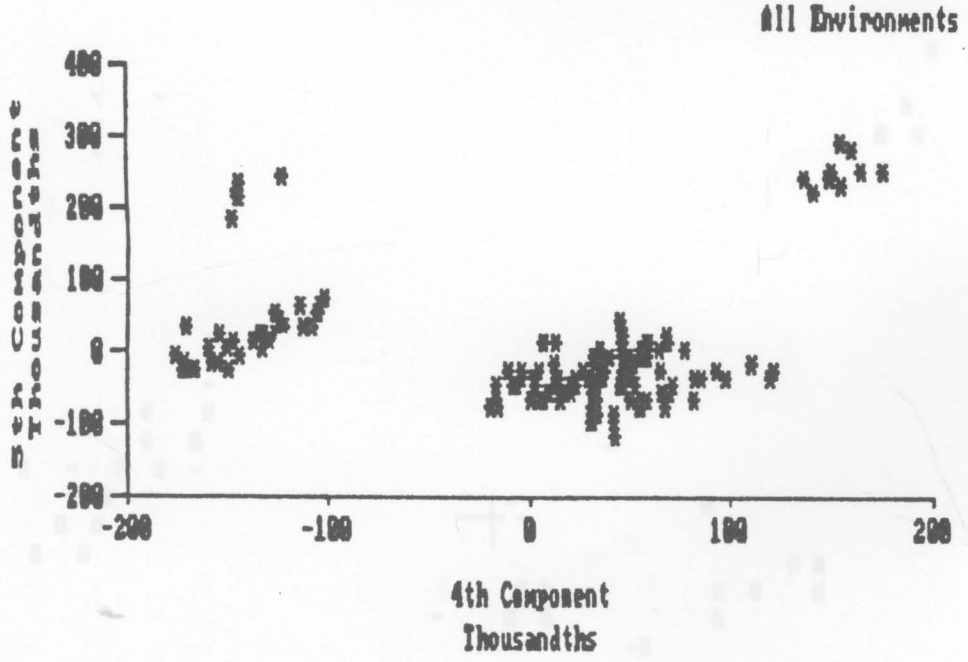


Figure 7.08: Distribution of Khoisan enviroments (*) within the plot of all environments (-), in terms of the 4th and 5th components.

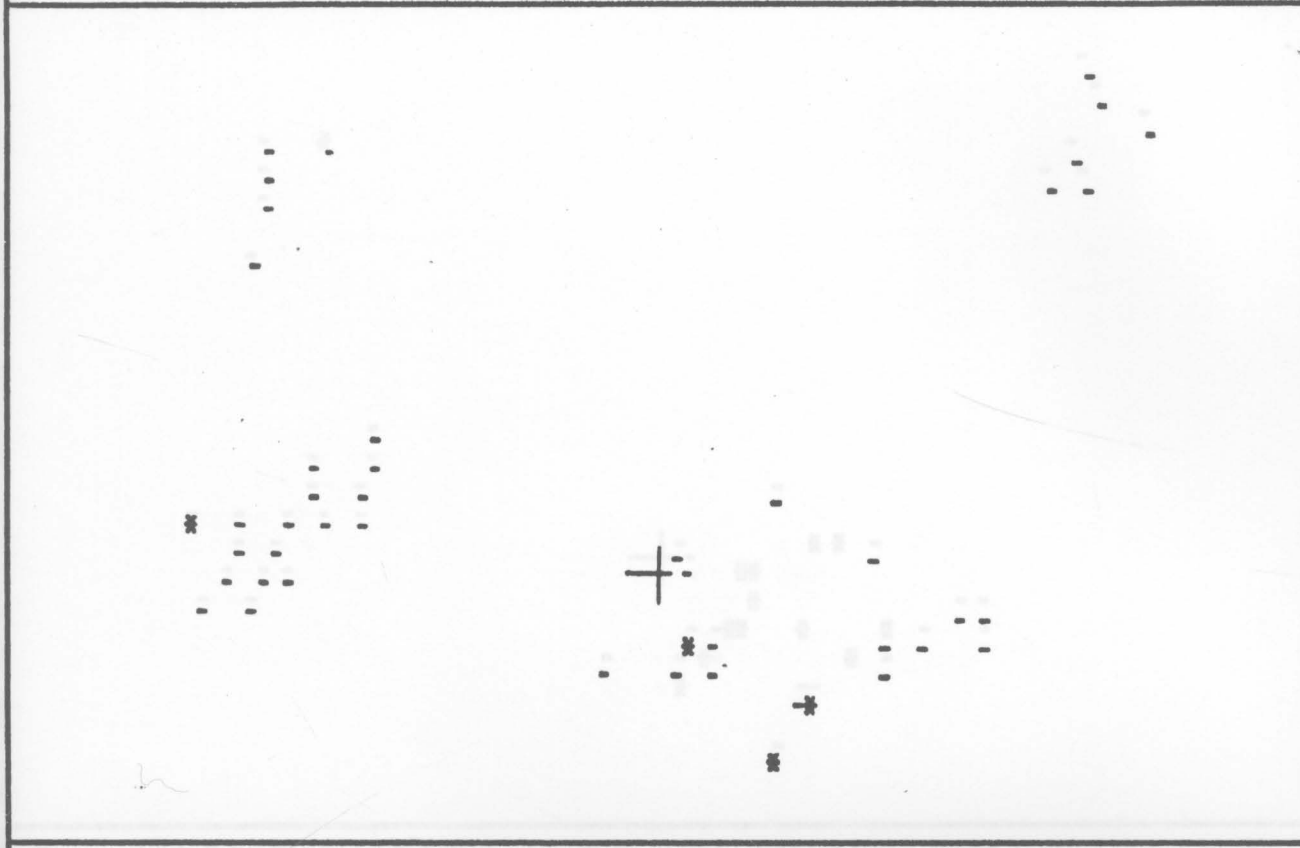


Figure 7.09: Distribution of African environments (*) within the plot of all environments (-), in terms of the 4th and 5th components.



Figure 7.10: Distribution of west Eurasian environments (*) within the plot of all environments (-), in terms of the 4th and 5th components.

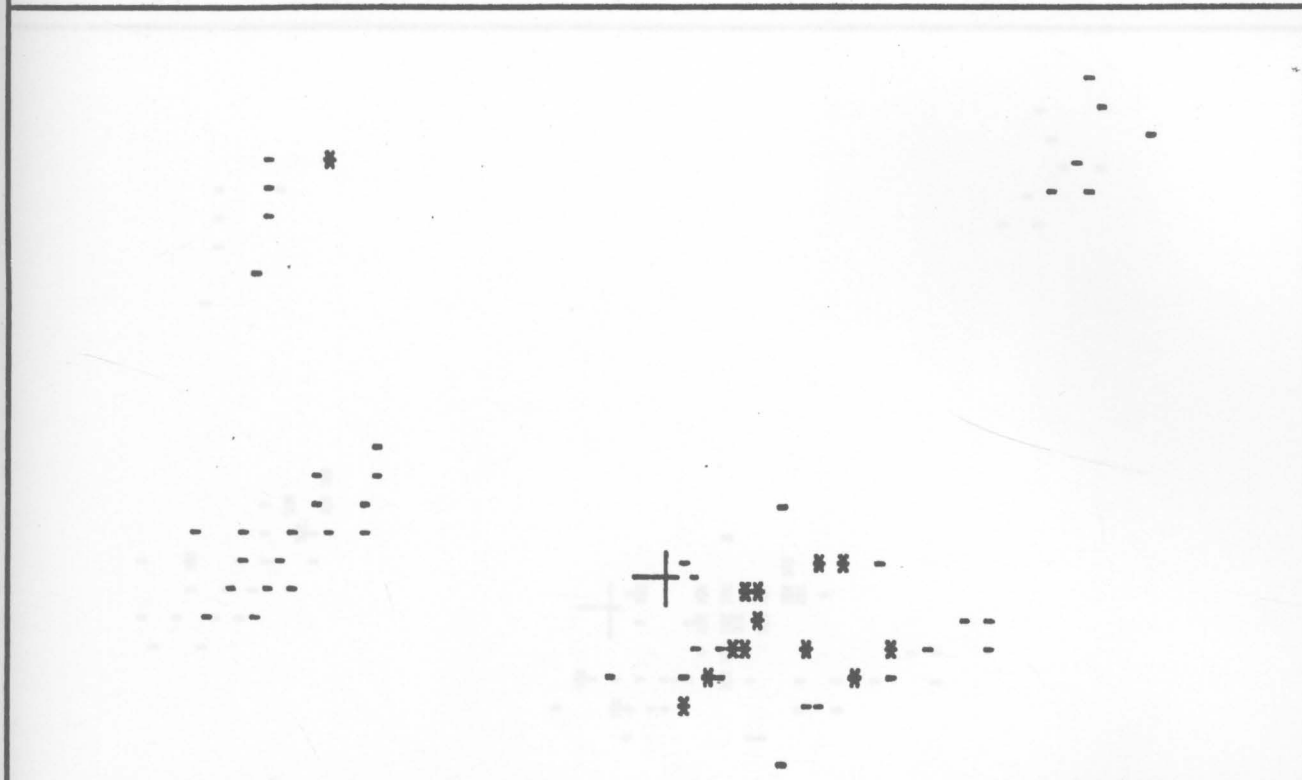


Figure 7.11: Distribution of Asian environments (*) within the plot of all environments (-), in terms of the 4th and 5th components.

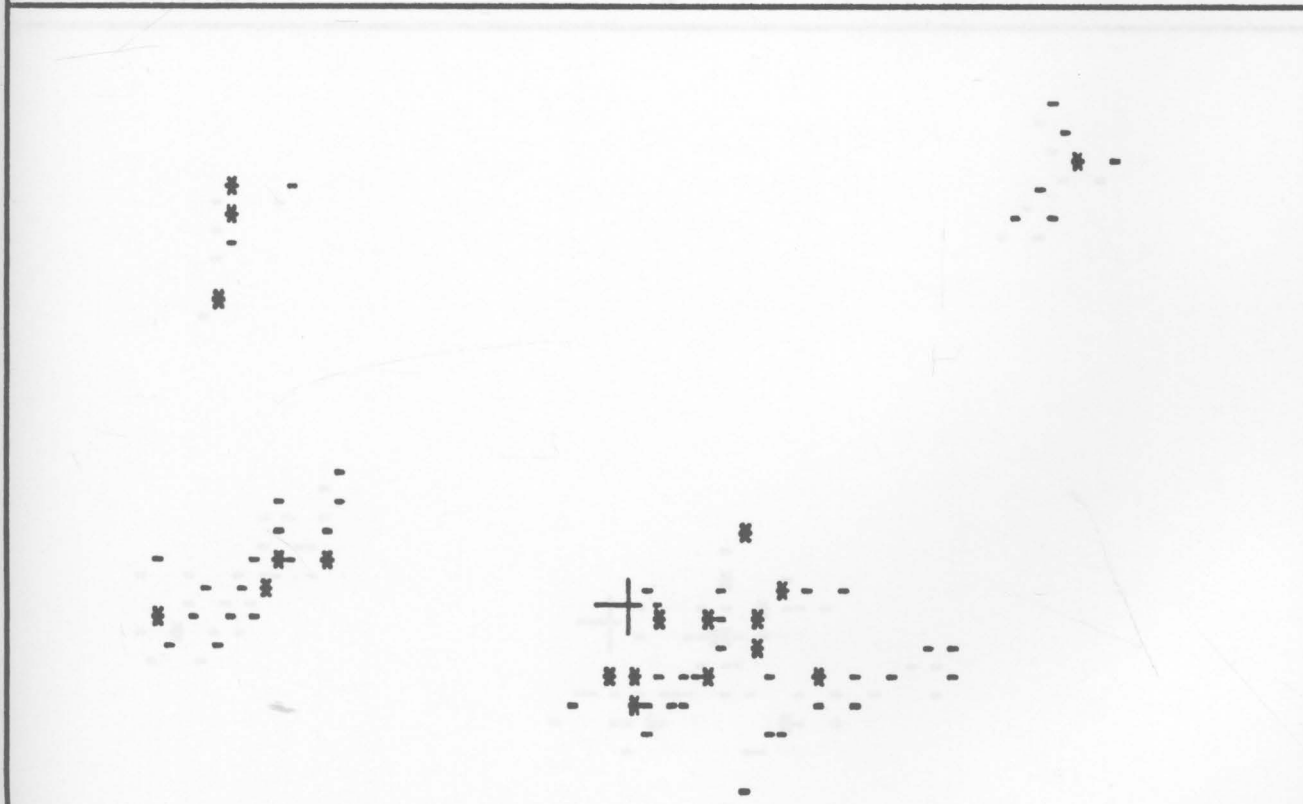


Figure 7.12: Distribution of Malayo-Polynesian environments (*) within the plot of all environments (-), in terms of the 4th and 5th components.



Figure 7.13: Distribution of Australian Papuan environments (*) within the plot of all environments (-), in terms of the 4th and 5th components.



Figure 7.14: Distribution of North American environments (*) within the plot of all environments (-), in terms of the 4th and 5th components.

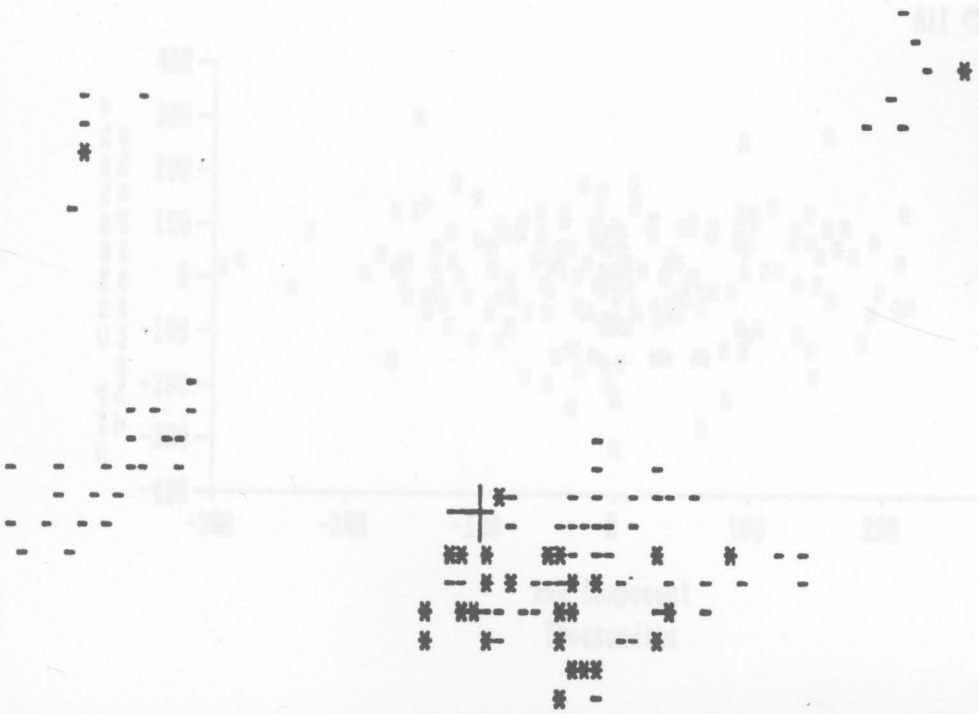


Figure 7.15: Distribution of South American environments (*) within the plot of all environments (-), in terms of the 4th and 5th components.



Figure 7.16: Plot of all objects in terms of the 2nd and 3rd components, from a 200 object matrix of random normal deviates.

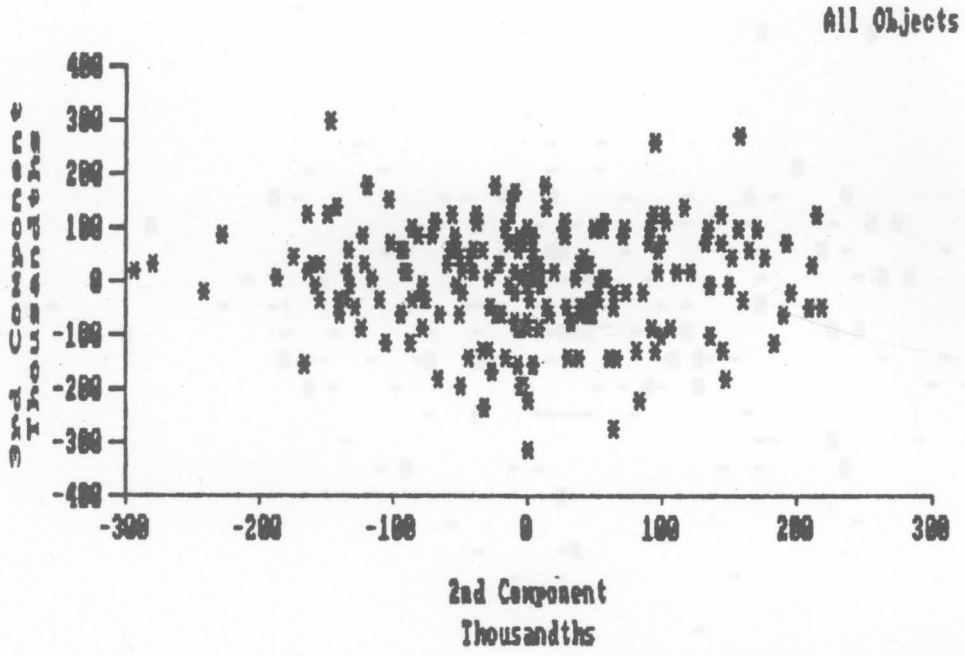


Figure 7.17: Distribution of a 100 object random subset (*) within the plot of all objects (-), in terms of the 2nd and 3rd components, from a 200 object matrix of random normal deviates.

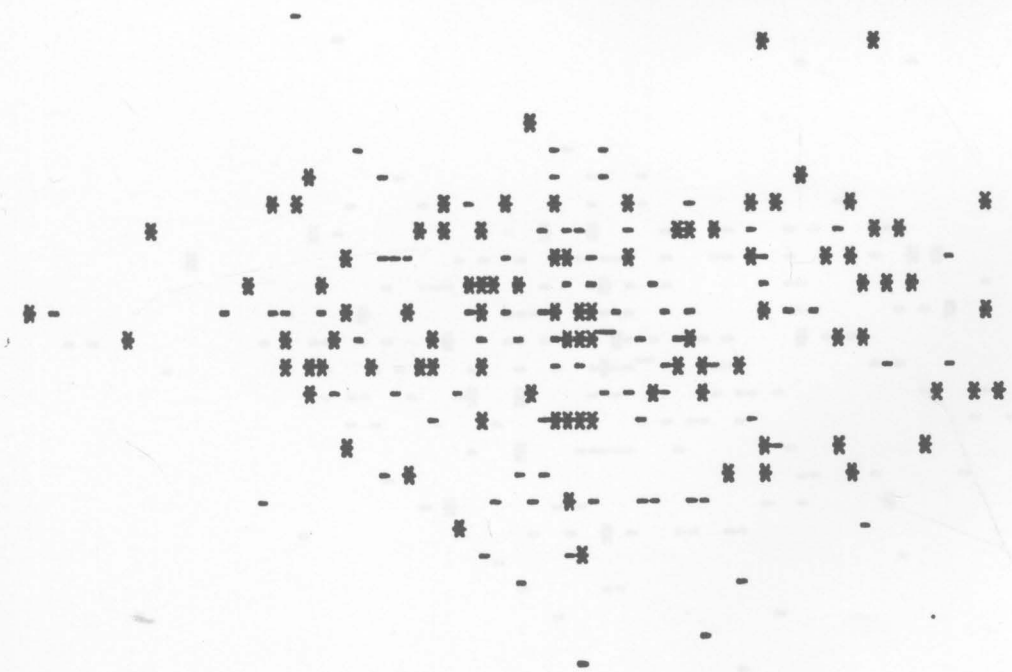


Figure 7.18: Distribution of a 50 object random subset (*) within the plot of all objects (-), in terms of the 2nd and 3rd components, from a 200 object matrix of random normal deviates.

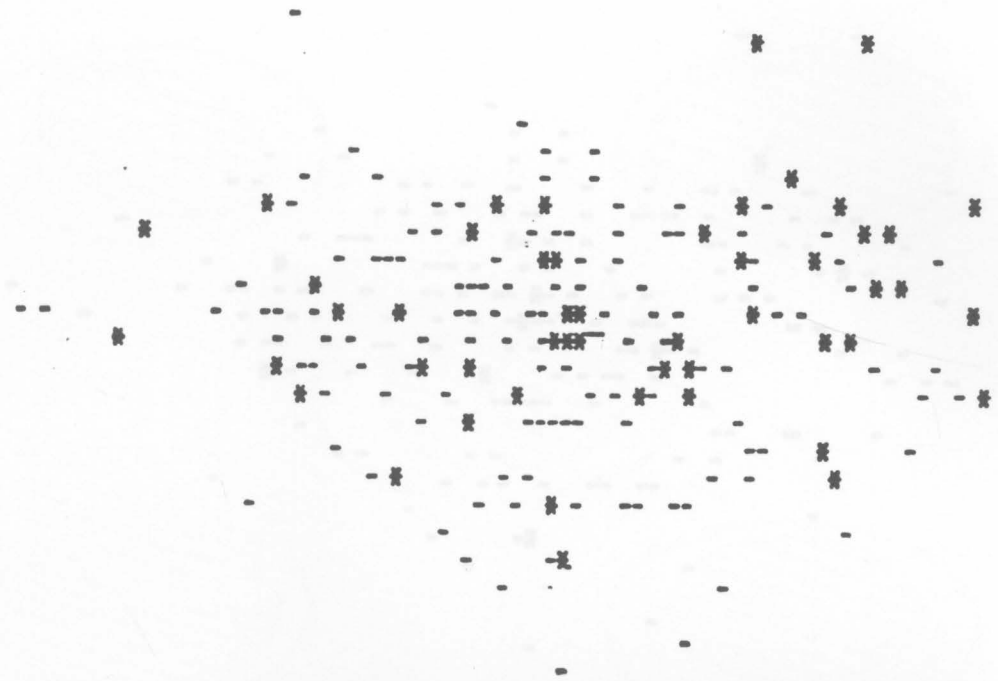


Figure 7.19: Distribution of a 20 object random subset (*) within the plot of all objects (-), in terms of the 2nd and 3rd components, from a 200 object matrix of random normal deviates.

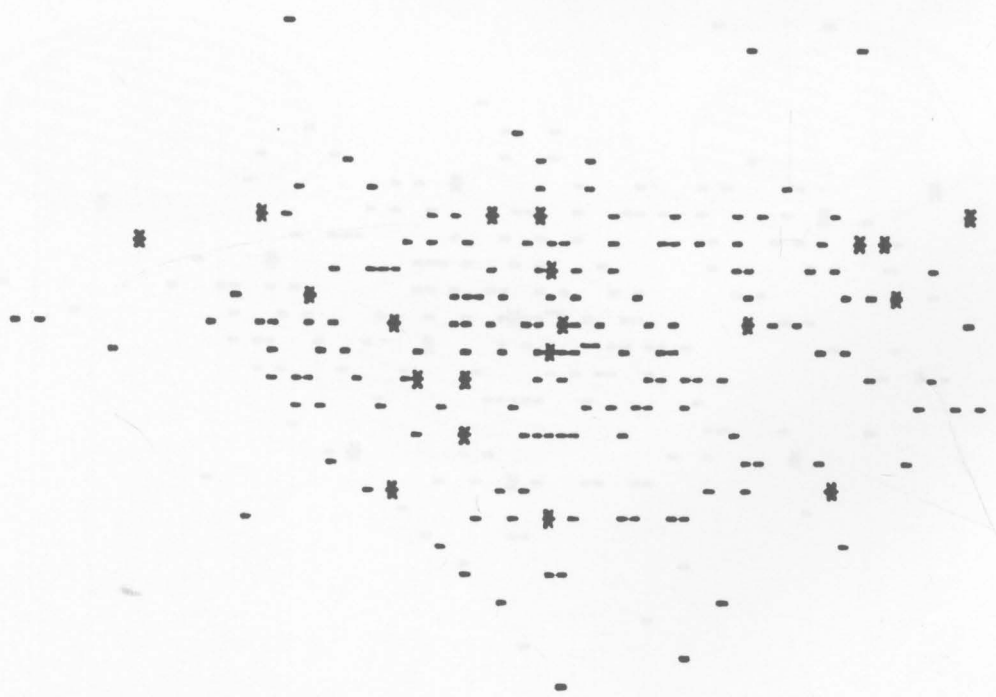


Figure 7.20: Distribution of a 10 object random subset (*) within the plot of all objects (-), in terms of the 2nd and 3rd components, from a 200 object matrix of random normal deviates.

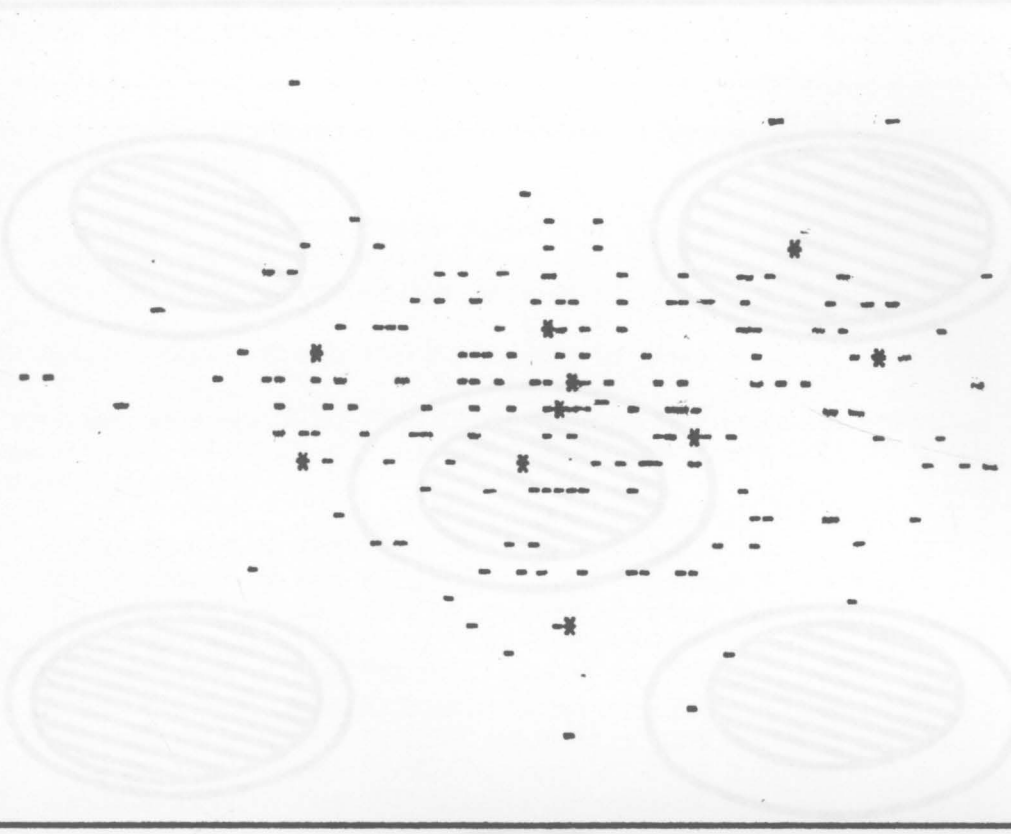


Figure 7.21: Distribution of a 10 object random subset (*) within the plot of all objects (-), in terms of the 2nd and 3rd components, from a 200 object matrix of random normal deviates.



Figure 7.22: Idealised hypothesis for the patterns generated from a matrix of random numbers, the random null hypothesis. Outer Oval = Plot of all objects, Shading = Plot of a Subgroup.

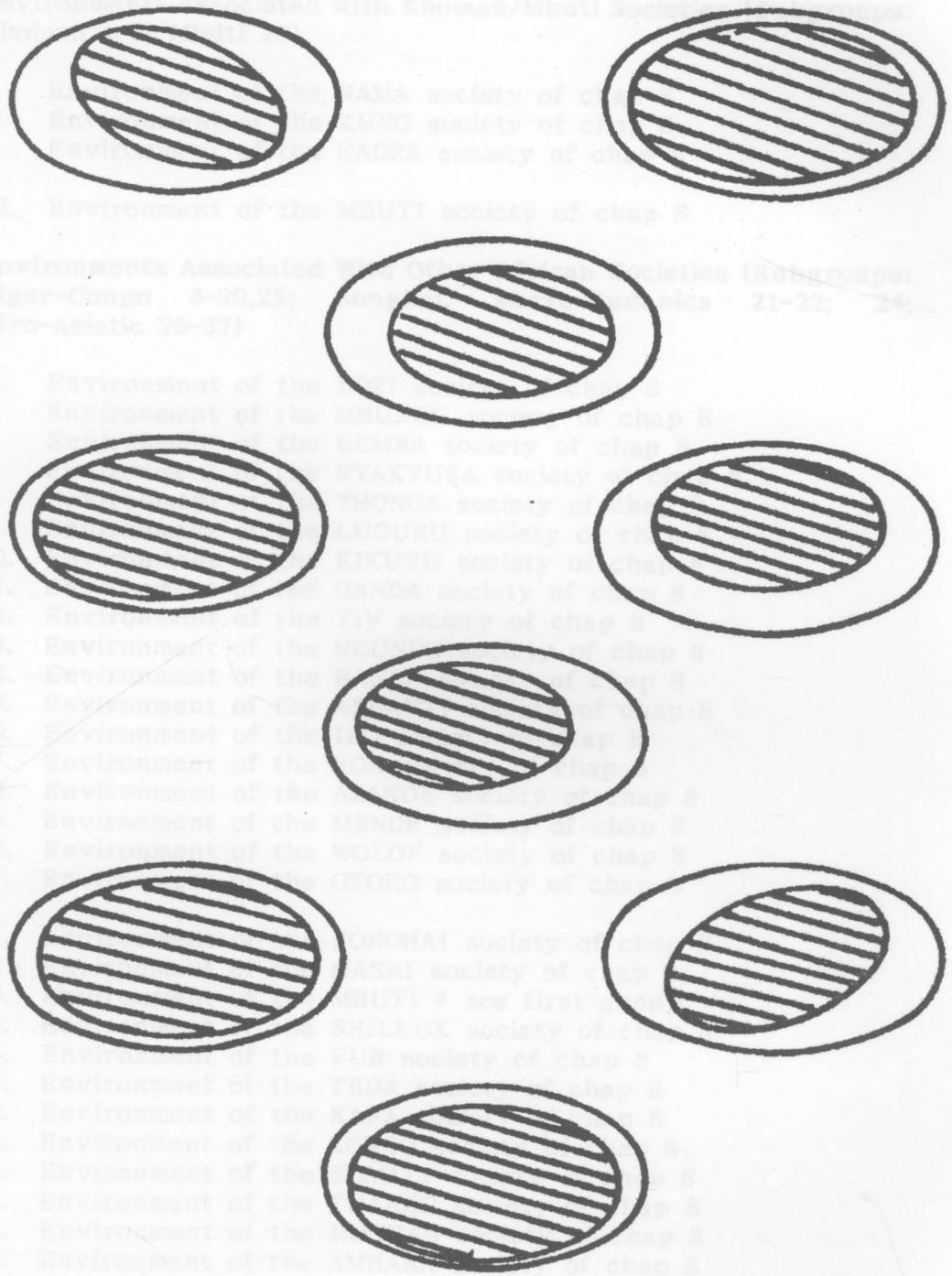


TABLE 4: THE 157 ENVIRONMENTS INCLUDED IN THE ANALYSIS

Environments Associated With Khoisan/Mbuti Societies (Subgroups: Khoisan 1-3; Mbuti 23)

1. Environment of the NAMA society of chap 8
2. Environment of the KUNG society of chap 8
3. Environment of the HADZA society of chap 8

23. Environment of the MBUTI society of chap 8

Environments Associated With Other African Societies (Subgroups: Niger-Congo 4-20,25; Songhai, Macro-Sudanics 21-22; 24; Afro-Asiatic 26-37)

4. Environment of the LOZI society of chap 8
5. Environment of the MBUNDU society of chap 8
6. Environment of the BEMBA society of chap 8
7. Environment of the NYAKYUSA society of chap 8
8. Environment of the THONGA society of chap 8
9. Environment of the LUGURU society of chap 8
10. Environment of the KIKUYU society of chap 8
11. Environment of the GANDA society of chap 8
12. Environment of the TIV society of chap 8
13. Environment of the NKUNDO society of chap 8
14. Environment of the BANEN society of chap 8
15. Environment of the ASHANTI society of chap 8
16. Environment of the IBO society of chap 8
17. Environment of the FON society of chap 8
18. Environment of the AZANDE society of chap 8
19. Environment of the MENDE society of chap 8
20. Environment of the WOLOF society of chap 8
25. Environment of the OTORO society of chap 8

21. Environment of the SONGHAI society of chap 8
22. Environment of the MASAI society of chap 8
23. Environment of the MBUTI * see first group
24. Environment of the SHILLUK society of chap 8
26. Environment of the FUR society of chap 8
27. Environment of the TEDA society of chap 8
28. Environment of the KAFA society of chap 8
29. Environment of the KONSO society of chap 8
30. Environment of the SOMALI society of chap 8
31. Environment of the TUAREG society of chap 8
32. Environment of the RIFFIAN society of chap 8
33. Environment of the AMHARA society of chap 8
34. Environment of the EGYPTIAN society of chap 8
35. Environment of the HEBREW society of chap 8
36. Environment of the BABYLONIAN society of chap 8
37. Environment of the BEDOUIN society of chap 8

Environments Of Western Eurasian Societies (Subgroups: Indo-European 38-46; Uralic 47-48; Altaic 49-51)

38. Environment of the ALBANIAN society of chap 8
39. Environment of the ARMENIAN society of chap 8
40. Environment of the ROMAN society of chap 8
41. Environment of the IRISH society of chap 8
42. Environment of the RUSSIAN society of chap 8
43. Environment of the KURDS society of chap 8
44. Environment of the BASSERI society of chap 8
45. Environment of the VEDDA society of chap 8
46. Environment of the SARAMACCA society of chap 8
47. Environment of the LAPP society of chap 8
48. Environment of the SAMOYED society of chap 8
49. Environment of the TURKISH society of chap 8
50. Environment of the KAZAK society of chap 8
51. Environment of the KHALKA society of chap 8

Environments Of Eastern Asian Societies (Subgroups: Japanese/Korean 52-53; Ainu And North Asian Isolates 54-57; Dravidian 58-59; Mon-Khmer and Andamanese 60-65; Tibetan 66-70)

52. Environment of the KOREAN society of chap 8
53. Environment of the JAPANESE society of chap 8
54. Environment of the AINU society of chap 8
55. Environment of the GILYAK society of chap 8
56. Environment of the CHUKCHEE society of chap 8
57. Environment of the YUKAGHIR society of chap 8
58. Environment of the GOND society of chap 8
59. Environment of the TODA society of chap 8
60. Environment of the SANTAL society of chap 8
61. Environment of the LAMET society of chap 8
62. Environment of the VIETNAMESE society of chap 8
63. Environment of the SEMANG society of chap 8
64. Environment of the NICOBARESE society of chap 8
65. Environment of the ANDAMANESE society of chap 8
66. Environment of the LOLO society of chap 8
67. Environment of the LEPCHA society of chap 8
68. Environment of the GARO society of chap 8
69. Environment of the LAKHER society of chap 8
70. Environment of the BURMESE society of chap 8

Environments Of Macro-Austronesian Societies (Subgroups: Siamese 72; Malayo-Polynesian 73-93)

71. Environment of the SIAMESE society of chap 8
72. Environment of the RHADE society of chap 8
73. Environment of the TANALA society of chap 8
74. Environment of the JAVANESE society of chap 8
75. Environment of the BALINESE society of chap 8
76. Environment of the IBAN society of chap 8
77. Environment of the BADJAU society of chap 8

78. Environment of the ALORESE society of chap 8
79. Environment of the MANUS society of chap 8
80. Environment of the NEW IRELAND society of chap 8
81. Environment of the TROBRIANDERS society of chap 8
82. Environment of the TIKOPIA society of chap 8
83. Environment of the AJIE society of chap 8
84. Environment of the MAORI society of chap 8
85. Environment of the MARQUESAN society of chap 8
86. Environment of the WEST SAMOAN society of chap 8
87. Environment of the GILBERTESE society of chap 8
88. Environment of the MARSHALLESE society of chap 8
89. Environment of the TRUKESE society of chap 8
90. Environment of the YAPESE society of chap 8
91. Environment of the PALAUAN society of chap 8
92. Environment of the IFUAGO society of chap 8
93. Environment of the ATAYAL society of chap 8

Environments Of South West Pacific Societies (Subgroups: Papuan 94–98; Australian 99–100)

94. Environment of the OROKAIVA society of chap 8
95. Environment of the KIMAM society of chap 8
96. Environment of the KAPAUKU society of chap 8
97. Environment of the KWOMA society of chap 8
98. Environment of the SIUAI society of chap 8
99. Environment of the TIWI society of chap 8
100. Environment of the ARANDA society of chap 8

Environments Of North American Societies (Subgroups: Nadene 100–106; Eskimo 107–108; Macro–Algonkian 109–113; Salishan, Hokan, Siouan 114–119; Uto–Aztecan, Zuni 120–125; Macro–Penutian, Kutenai, Pawnee, Iroquoian, Natchez–Muskogean 126–132)

101. Environment of the INGALIK society of chap 8
102. Environment of the SLAVE society of chap 8
103. Environment of the KASKA society of chap 8
104. Environment of the EYAK society of chap 8
105. Environment of the HAIDA society of chap 8
106. Environment of the CHIRICAHUA society of chap 8
107. Environment of the ALEUT society of chap 8
108. Environment of the COPPER ESKIMO society of chap8

109. Environment of the MONTAGNAIS society of chap 8
110. Environment of the MICMAC society of chap 8
111. Environment of the SAULTEAUX society of chap 8
112. Environment of the GROS VENTRE society of chap 8
113. Environment of the YUROK society of chap 8

114. Environment of the BELLACOOLA society of chap 8
115. Environment of the TWANA society of chap 8
116. Environment of the HAVASUPAI society of chap 8
117. Environment of the EASTERN POMO society of chap 8
118. Environment of the OMAHA society of chap 8
119. Environment of the HIDATSA society of chap 8

120. Environment of the N. PAIUTE society of chap 8
121. Environment of the COMMANCHE society of chap 8
122. Environment of the PAPAGO society of chap 8
123. Environment of the AZTEC society of chap 8
124. Environment of the HUICHOL society of chap 8
125. Environment of the ZUNI society of chap 8

126. Environment of the KLAMATH society of chap 8
127. Environment of the LAKE YOKUTS society of chap 8
128. Environment of the KUTENAI society of chap 8
129. Environment of the PAWNEE society of chap 8
130. Environment of the HURON society of chap 8
131. Environment of the NATCHEZ society of chap 8
132. Environment of the CREEK society of chap 8

Environments Of South American (Sub-Mexican) Societies (Subgroups: Mayan/Cariban 134-136; Macro-Chibchan 137-139; Equatorial 140-146; Ge-Panoan 147-152; Andean 153-157)

133. Environment of the QUICHE society of chap 8
134. Environment of the CALLINAGO society of chap 8
135. Environment of the CARIB society of chap 8
136. Environment of the MISKITO society of chap 8

137. Environment of the CUNA society of chap 8
138. Environment of the WARRAU society of chap 8
139. Environment of the CAYAPA society of chap 8

140. Environment of the GOAJIRO society of chap 8
141. Environment of the MUNDURUNCU society of chap 8
142. Environment of the CUBEO society of chap 8
143. Environment of the JIVARO society of chap 8
144. Environment of the SIRIONO society of chap 8
145. Environment of the TRUMAI society of chap 8
146. Environment of the TUPINAMBA society of chap 8
147. Environment of the AMAHUACA society of chap 8
148. Environment of the NAMBICUARA society of chap 8
149. Environment of the TIMBIRA society of chap 8
150. Environment of the AWEIKOMA society of chap 8
151. Environment of the LENGUA society of chap 8
152. Environment of the ABIPON society of chap 8

153. Environment of the INCA society of chap 8
154. Environment of the AYMARA society of chap 8
155. Environment of the MAPUCHE society of chap 8
156. Environment of the TEHUELCHE society of chap 8
157. Environment of the YAGHAN society of chap 8

The programme then tries to place the environments in the order to which the greatest number of individual variable orders conform to. This

order becomes the first Principal Component, a synthesised "supervariable" for which each environment is given a value, from the "smallest" environment for the first Principal Component, to the "largest". If the original data matrix of 157 environments can be imagined as a cloud of points in the multi-dimensional space of the variables (length as a function of dry months, breadth as a function of agricultural potential, height as a function of population density, time as a function of the presence of a Polar climate, and so on), the first principal component can then be imagined as a straight line drawn through the longest dimension of that cloud of points. Each point (environment) is then collapsed onto that line, and its position on that line becomes its value for this new synthesised variable. The "second principal component" is another line drawn through this cloud of points, at a right angle to the first component, at a position along that line where the cloud of points has its greatest breadth, the cloud is collapsed onto this line, and each environment allocated a value or co-ordinate according to its collapsed position on this line. The third component measures the greatest height of the cloud, at a right angle to the other two, and so on.

The environments can now be efficiently compared in terms of any two principal components. When plotted on a graph according to their co-ordinates along two components, their position is now a function of their values for a number of variables, and not just two. The degree to which this is so can be seen in the variable plots **(FIGURE 7.06)**. The fourth and fifth principal components were chosen due to the fact that the "Lamarckian" signature was in its simplest and most pronounced form on these graphs (see discussion of the environments immediately below for further explanation). As in the other analyses of this study **(see discussion of FIGURE 6.13, Chapter 6, section 6.4)**, all that need be noted about the variable plot is that most of the variables are significantly removed from the origin. This means that all of the variables (with the exception of Variable 10 - distance from the equator) are contributing to the plotted positions of the environments (although to a varying extent). In other words, the distance between any two environments on the biplots below is a function of their differences and similarities across nine variables. As already mentioned above in the description of the variables,

most of these variables are themselves quotients of a number of simpler observations. Consequently an environment's position on the graph is affected by a large number of factors.

If the results are now presented according to the same formula as the cranial data - where groups of environments with proximate geographic origins are graphed separately, and their distributions compared with that of the whole dataset - a very different pattern emerges. The distinctive pattern is dramatically apparent even when the results are compared at the gross level of the overall plot, when all the objects of the analysis are plotted on the same graph **(Compare FIGURE 6.14 with FIGURE 7.07)**. While the human cranial descriptions show a perfectly random structure at this gross level of the data (a dense "shotgun" scatter about the origin), the environmental descriptions **(FIGURE 7.07)** exhibit just the opposite. There are four very tight clusters, each well distinguished from the other, and there is no general scatter around the origin (although one cluster is near the origin). In other words, in spite of the fact that three of the variables affecting graph position are continuous rather than discrete variables (Variables 1-3), the environments are tending to fall into a limited number of discrete categories. True to the general expectations defined in the last section about inorganic phenomena and hypothetical Lamarckian systems, a "periodic table" of environments seems to emerge.

The specific form involved in each cluster is not important to the point at hand. The nature of this "periodic table of environments" (although potentially very interesting) need not be explored, as it is tangential to the study. All that need be noted for the purpose of the argument is that the structure of the two data matrices is very different even at this general level of the data, and that most of the environments around the target societies fall into three main clusters, with a small additional cluster in the top left quadrant associated with polar environments.

When subsets of common origin are plotted independently, further differences, latent in the general plot of all the objects, begin to emerge.

Even extremely small subsets, like the four environments from around the Khoisan/Mbuti societies **(FIGURE 7.08)**, occupy extremely broad distributions with respect to the total range of human environments, in contrast to the societies which they encompass. Given that the vast majority of human environments plot in one of the three main clusters, the four environments of this group display a distribution that is almost two thirds of the global range of human environments in the analysis.

As soon as a larger group is plotted, such as all of the African environments together **(FIGURE 7.09)**, all of the three main clusters are well represented. It can be seen that the range of human environments in Africa is (polar environments excluded) very similar to the global range, with respect to these particular environmental factors. This is in direct contrast to the total range of shapes in African crania **(refer back to FIGURE 6.15)**, which showed a much more restricted distribution with respect to the rest of the world. African environments, then, are not as distinctive (in relation to the rest of the world) as the organisms which inhabit them. In other words, African environments span a greater area of the distribution of world environments (in terms of these descriptions) than do African crania, in relation to the distribution of world crania. While all the environments fall into discrete groups, the range of discrete groups that we find in Africa are the very same as those found elsewhere, and are, therefore, not distinctive *to Africa.*

The absence of heritage constraint continues on an even more dramatic level throughout the rest of the data. The Eurasian environments display a range of material conditions which are essentially identical to that of the rest of the world. Although the west Eurasian human environments **(FIGURE 7.10)** are limited to two of the clusters, and only one of the main clusters, Eurasia as a whole displays environments belonging to all four categories. This is due to the fact that the other Eurasian environments **(FIGURE 7.11)**, those from Asia alone, display the full range of environmental variability. Even though Europe displays a narrow distribution **(back to FIGURE 7.10)**, it should be noted that this narrow distribution is located in the large central cluster,

which is well represented on every continent. As a consequence, even this narrow distribution is not due to the presence of distinctive environments, but quite the opposite; the absence thereof. Moreover, the range of environments in Europe and Asia together, as already pointed out, contains all basic types. Yet in the last chapter it was shown that the *range* of Eurasian crania was quite distinctive **(FIGURES 6.16 and 6.17)**. Thus, in similar manner to Africa, Eurasia also displays a distinctive range of organisms in an otherwise indistinctive *range* of environments. (NB: The reader is reminded that it is important to distinguish between different levels of diversity within the data, different levels at which "distinctiveness" may be manifest. The environments, *unlike* the crania, fall into discrete or distinctive groups/types/clusters; however, none of these groups are unique to Eurasia, and therefore the *range* of Eurasian environments, in contrast to the environments themselves, is quite indistinctive. The range of Eurasian crania, in contrast, is quite distinctive, although no distinctive *types* are found *within* this range).

Thai and Malayo-Polynesian environments **(FIGURE 7.12)**, and the environments of Australian and Papuan societies **(FIGURE 7.13)**, also display the pattern. While the human crania of these societies each occupied distinctive distributions within the Howells data **(FIGURES 6.18 and 6.19)**, the two basic types of environment they inhabit are very common throughout Africa and Eurasia (and also throughout America as will be seen below). What is becoming apparent is that the "heritage constraint" pattern is in no way caused by environments, as environmental phenomena do not exhibit this pattern.

American environments display the lack of heritage constraint particularly well. North American material conditions **(FIGURE 7.14)** principally fall in the large central cluster, the general set of conditions so common throughout the rest of the world, along with one tropical highland environment (Aztec), and one polar environment (Copper Eskimo). Three of the four clusters, and an even greater proportion of the area of the overall distribution, is thus spanned. No distinctive environment is present in America

lbut absent elsewhere, nor is the reverse situation apparent. And yet, as was shown in the last chapter, **(FIGURE 6.20)**, the cranial morphology of this continent is strikingly narrow, occupying a corner of the world's range to which the ranges of certain other groups never extended.

South America similarly contains no unique sets of inorganic conditions whatsoever, and has a very similar range to the rest of the world **(FIGURE 7.15)**, with all of the three main clusters present in the overall plot **(FIGURE 7.07)**. Indeed when both North and South America are considered together, the distribution would be difficult to distinguish from the range of environments in the world as a whole. And yet, once again, the range of crania in America was so obviously distinctive **(FIGURES 6.20 and 6.21)**, and so obviously smaller than the range of crania to be found in the world as a whole. So while the range of American environmental conditions, in relation to the global range, is large, the range of American cranial shapes is relatively small in relation to the range of all the crania.

A quick overview of material conditions reveals that such phenomena display a very different geographic distribution to that of a "neo-Darwinian system". Environments tend to fall into one of a small number of discrete identifiable categories, in contrast to the "continuous fabric" of organismic populations, and the millions upon millions of organismic species. As the forces determining material conditions are universal, and not subject to phylogenetic constraint, little heritage constraint can be observed in the range of physical conditions on any particular continent.

For the reasons outlined earlier in the chapter, this pattern can now be employed as a Lamarckian hypothesis (and as a null hypothesis to the Cultural Selectionist hypothesis) for cultural assemblages. The above global sample of phenomena taken from inorganic and Lamarckian systems therefore displays two distinctive and observable characteristics - a series of discrete, readily identifiable "types", and a dramatic lack of heritage constraint, due to the fact that continental and sub-continental subsets tend to incorporate a relatively large number of these types. If cultural process is not selective in

nature, we might expect cultural assemblages to display a pattern such as this.

7.4 The Random Evolution Pattern

Having made an attempt to define a "Lamarckian" hypothesis, a third and less complicated hypothesis (or another null-hypothesis to Cultural Selectionism) will be briefly presented. For this purpose, patterns of variation in a completely unstructured, random system will now be analyzed. This hypothesis has already been briefly referred to in the previous chapter **(FIGURE 6.30)**, where a single general plot was offered as a brief intuitive comparison with the cranial data subsets.

To a Darwinian anthropologist with little knowledge of prevailing views in post-Processual Archaeology or Cultural Anthropology, any notion of random evolution may seem to be ridiculous, a straw man hypothesis. Some Darwinian anthropologists are so isolated within Darwinian discourse that they appear to hold the opinion that no-one believes that cultural evolution is essentially random. Yet the essentially structuralist notion of the completely "arbitrary" symbol, which is not determined by anything external whatsoever (and therefore, the philosophical opposite of environmental Lamarckism), is often raised when Darwinian principles are discussed in dialogue with cultural anthropologists who are not well acquainted with Darwinian theory. Consider, for example, Sahlins (1976 *passim*) or Hodder's discussion (1989:69, and further discussion of this in Chapter 1, Section 1.6 of this study) of how meaning is constituted merely through associations between sets of signifiers and/or contexts. Moreover, the equivalent position in Psychology, which Edelman terms "Nominalism" (Edelman 1989:31-32), is still used enough in this discipline for it to be considered a basic alternative to his position (see Chapter 1, Section 1.2).

A Random Evolution hypothesis was engineered through the generation of a data matrix which was of similar dimensions to the cultural case study of the next chapter, but composed purely of random numbers. A matrix of 200 objects and 50 variables was created, where an object's measurement for each "variable" was "drawn out of hat", as it were; the "hat" in this case being a set of "z-scores" with a mean of zero and a standard deviation of one. The process is sometimes called "generating random normal deviates".

The random number matrix was then analyzed in precisely the same way as the data used for defining the neo-Darwinian and Lamarckian hypotheses was analyzed. Technically speaking (for those who have themselves used Wright's MV-Arch), the matrix was not first transformed into a matrix of presences and absences, by making all the values above a certain standard deviation threshold into one. Also, in order to replicate the same conditions of analysis as the other studies, the option "correlation matrix" (in BIGPCA2) was chosen, as this was used in the other analyses to give variables of different magnitude equal weight. The spirit of experimental replication in this "control" analysis was thus adhered to, in spite of the fact that all variables tend to be of similar magnitude already in a random number matrix.

The results were then graphed and presented in the same way as results of the "real" data have been presented so far. A plot of all 200 objects is briefly described, and then a series of subset plots of diminishing size are offered, so that the reader can contemplate "the nature of non-structure" in Principal Components Analysis, and compare these results with the Darwinian and Lamarckian structures defined so far.

At first glance, the overall structure of a random number matrix looks very similar to a Darwinian system, and utterly different to a Lamarckian system **(FIGURE 7.16, then refer back to FIGURES 7.07 and 6.14)**. A plot of all 200 objects displays a random "shotgun" scatter, dense toward the origin, and sparse toward the margins (reflecting that the origin numbers were drawn from a random distribution). The only feature which distinguishes the random number matrix from the cranial data at this gross level is that the former is

somewhat more sparse again on the margins, and somewhat more dense again at the centre.

However, when the now standard procedure is replicated further, and subsets are plotted, the random numbers yield a very different series of sub-distributions **(FIGURES 7.17, 7.18, 7.19, 7.20, and 7.21)**. Of course, these random number subsets cannot have any real "geographic" or "phylogenetic" origin. The aim is to demonstrate that the "heritage" pattern is not a mere artefact of the data manipulations associated with Principal Components Analysis and graphed subsets, without delving into an extensive discussion of the mathematical principles involved. It can be seen below that while a similar procedure of graphing smaller and smaller subsets was employed, the heritage constraint pattern did not emerge.

It can be seen that the typical signature of a random number matrix is a series of subsets in which objects are strongly congregated around the origin (just as most numbers in a random distribution are centred upon the central point). While the distribution of any subset, even that of the larger subsets **(FIGURES 7.17 and 7.18)**, will tend to lie within the very sparse borders of the overall plot, the smaller plots sometimes display correspondingly smaller distributions **(FIGURE 7.20)**. In contrast to the smaller plots of a neo-Darwinian pattern, however, small random number plots tend to be firmly located on and about the origin. Thus in **(FIGURE 7.20)**, the small scatter is nonetheless centred upon the origin of the graph. Other small subsets, such as the twenty object subset of **(FIGURE 7.19)**, and the ten object subset of **(FIGURE 7.21)** show comparable distributions to larger subsets.

In general, then, a series of subsets from a random number matrix display a remarkably repetitive profile, in contrast to a neo-Darwinian system, where each subset tends to display a unique distribution. On the other hand, in having a repetitive subset profile, the random number system displays a superficial similarity to the Lamarckian system analyzed above. However, as a comparison of the overall plots of these two kinds of system reveal **(FIGURES**

7.07 and 7.16), the overall structure of a Lamarckian system is completely different, in displaying a series of discrete, well defined categories, a pattern totally absent from the "shotgun" scatter of the overall plot in the Random Evolution hypothesis **(FIGURE 7.07)**.

7.5 Concluding Remarks

Thus two null hypotheses have been explored in this chapter. They provide two different intuitive impressions of the general structure to be found in global samples of data from systems where selective process is not operating. These are two patterns which do in fact occur in certain kinds of data where selective process is absent; they are not just "straw man" patterns which have no "real world" referents. Each of these null hypotheses is a general pattern which could conceivably be found in cultural geography, and if either pattern were to be found, Cultural Selectionism in its current unamended form would be refuted.

The basic aspect of each null hypothesis to remember (for the "preliminary test" of Cultural Selectionism in the next chapter) is the general absence of heritage constraint in subgroups of common origin taken from "non-selective systems". When assemblages of "material conditions" are broken up into subgroups from particular continents, and the distribution of these subgroups is graphed in relation to the distribution of the global sample, a complete or near complete sample of the global range in material conditions is usually found. This pattern cannot be idealised in terms of a series of subset oval distributions within the larger oval of the plot of all objects, as the plot of all objects does not display an ovoid "football" shaped cluster in the first place **(FIGURE 7.07)**, but rather a series of discrete clusters with very little internal variation. From this graph the reader can develop a "subliminal image" of the pattern to look for in cultural phenomena, if the latent Lamarckian orthodoxy is to remain unrefuted.

As far as the other "Random Evolution" null hypothesis is concerned, any series of subsets taken from a general matrix of random numbers follow the profile idealised in the final illustration of the chapter **(FIGURE 7.22)**. This final image (summarising Figures 7.18 to 7.21) provides the reader with a third general pattern which could possibly emerge in the data.

In general the two alternative hypotheses display a lack of heritage constraint in that small groups, when graphed, display the distribution which would be expected if the subset had been randomly chosen (which, in the case of the third hypothesis or second null hypothesis, it actually had been). But this pattern occurs even in a Lamarckian system when all members of a subset are derived from a single continent; their distribution is still basically similar to what it would have been had we graphed a randomly chosen subset of the same size. This is because almost all the basic environmental types occur on any one continent. Where the two null hypotheses differ, of course, is in the nature of the global distribution. The overall distribution of world environments is very different to that of a similar sized set of random numbers in that the environments tend to fall into a restricted number of "types", whereas there are no obvious clusters (nor even any of those generated accidentally) in the random number matrix.

8

HERITAGE CONSTRAINT

IN CULTURAL PHENOMENA

Overview

As shown in Chapter 6, heritage constraint is the natural and logical result of selective process. The diffusionist assumption that similarity reflected common origin is an extreme form of this pattern. Moreover, as shown in Chapter 7, heritage constraint is *not* a logical result of *Lamarckian* process. Therefore the presence of heritage constraint in cultural data can be invoked in support of the general propositions of Cultural Selectionism. Yet since the presence of heritage constraint in archaeological and anthropological data is already generally accepted, part of the test has already been achieved merely through pointing out that this "common sense" archaeological knowledge can actually be considered as solid evidence for the selective nature of cultural process. However, non-Darwinian anthropologists who are aware of the presence of heritage constraint in cultural data may not realise how ubiquitous this pattern really is, or, more importantly, the fact that the pattern can be evoked in support of Cultural Selectionism.

8.1 Heritage Constraint Pervades Many Levels

In the last two chapters geographical and biogeographical data were examined in order to illustrate the basic signatures left by selective process in living systems, and also the basic signatures which might be produced by other processes. The patterns brought out in Chapter 6 can be considered to be basically the same as those patterns intuitively described by Darwin during the voyage of The Beagle, but on the scale of a single species rather that of

whole faunal and floral assemblages (see Chapter 6, Sections 6.3 and 6.4). Human crania were shown to display the same basic pattern of "descent with modification" as that which pervades the biogeography of other organisms. Moreover, since all the human crania were, by definition, members of the same species, the phylogenetic heritage constraint which emerged in the analysis was thereby demonstrated to exist within one species, and indeed one sub-species; and therefore within one of the most specific levels of biological taxonomy. When we couple this fact with the generally accepted notion of intercontinental phylogenetic constraints, at the family level and above (for example, Ceboid monkeys are only found in South America, Cercopithecoids are only found in the Old World, Prosimians are found primarily on Madagascar, there are no primates whatsoever in Australia, etc.) it becomes clear that phylogenetic constraint is a general quality of many levels of selective systems.

The all pervading nature of heritage constraint is due to the fact that selective pressures have no direct influence on the course of mutation. We find very different crania in similar environments, and very similar crania in different environments, precisely because those environments are not *determining* (as they would be in a Lamarckian process) cranial shape. Rather, various environmental factors exert a selective pressure, which only produces differential reproductive success of some *existing* traits at the expense of other existing traits. Even if a human population was subjected to a set of selective pressures for which a kangaroo was the perfect adaptation, it is doubtful whether a form which even remotely resembled a kangaroo would emerge. Each human population has a unique phylogenetic heritage, and therefore offers a unique suite of diverse forms (and derivative mutations) to those similar selective forces.

This same principle (for an additional example) is embodied in the forms of the dominant herbivores which Darwin encountered during his voyage. Large herbivores such as kangaroos, zebras, donkeys, horses, antelopes, deer, bison, camels and llamas can all be found in very similar grassland or open woodland environments. Moreover, they are often reliant upon similar food

resources, and hunted by broadly similar predators such as dogs and cats. So their considerable diversity cannot be purely due to selective pressures. A marsupial gene pool will "throw up" very different derivative mutations to an antelope gene pool, which would be different again to the mutations "thrown up" by a camel (llama) gene pool, and so on. Each population has a unique phylogenetic heritage which constrains the forms that animals can take in that population.

Thus in Chapter 6 it was established (Sections 6.1 to 6.4) - with reference to Freeman's exhaustive historical analysis of nineteenth century evolutionary theory (Freeman 1974), and the work of Desmond and Moore (1992) - that the "descent based" *empirical* contribution of Darwin's voyage and indeed other "descent based" descriptive work was crucial in the ultimate "switch" from Lamarckian to neo-Darwinian orthodoxy. Darwin's claim to the establishment of Selectionism in Biology lies not so much in some special claim to precedence (although there are elements of this in his work), but in the immense empirical and interpretive contribution made during his voyage and later life. Ultimately, it was Darwin's voyage around the world (and his exhaustive monographs such as those on barnacles) which enabled the wide ranging demonstration of the pervasive tendency toward "descent with modification" in biological systems, a pattern which in turn exemplifies the fact that Natural Selection does not wholly erase an organism's ancestry.

In light of this it is curious that no corresponding account of cultural phenomena has really been achieved, or at least not systematically evoked in support of Cultural Selectionism. By this I mean that while a number of programmatic statements about the neo-Darwinian nature of cultural process have been made, there has not, to my knowledge, been any corresponding attempt to demonstrate the pervasive occurrence of heritage constraint in some synthesized, globally distributed set of cultural phenomena, nor to point out the significance of this underlying pattern for verifying Cultural Selectionism itself. Studies which do synthesize cultural data over wide geographic areas or large chronological time scales, such as Cavalli-Sforza (1991), Cavalli-Sforza et al (1988) or Foley (1987), do not draw out the

implications of the selective patterns in culture *for the nature of cultural process itself*. Rather, they employ the cultural data concerned to elucidate or embellish certain primary arguments made in the papers about genetic process. Moreover, the above studies seek to interrelate genetically and culturally inherited data, rather than to focus exclusively upon the intrinsic patterns of the cultural data, as will be attempted below. Other studies, such as Durham (1990), link cultural descent with Cultural Selectionism, but below an explicit attempt is made to advance cultural heritage constraint as a ubiquitous verification for Cultural Selectionism, and as a refutation of competing accounts of cultural change.

The many attempts to advance the cause of a selective model of cultural process (ie. those of Chapters 4 and 5) all lack what could be considered the most vital component; some cultural equivalent of the voyage of *The Beagle* and its resulting globally distributed biogeographic data. The equivalent act for anthropology would have been a similar voyage, probably around the same time or earlier, in which cultural phenomena, not organisms, were the focus of attention. Some cultural observations were, of course, taken down during Darwin's voyage; for an interesting example consider his comparison of the Gauchos of eastern South America and the Guasos of Chile (Darwin 1889:258-259). Another more famous example would be his detailed description of the Tierra del Fuegians (Darwin 1889:212-216 and 218-230). For a variety of reasons which I will not delve into, however, these observations did not lead Darwin to elucidate a "Theory Of Cultural Selection" to lay alongside his theory of Natural Selection.

Such voyages, whether they be organismic or cultural in focus, would no longer yield simple, geographically defined "ecounits" of common heritage. Large amounts of colonisation and cross-colonisation have now occurred in faunal, floral and cultural systems, such that geography no longer closely correlates with phylogenetic heritage. Even if a modern Darwin were to march thousands of kilometres into central Australia he would find as many European artefacts and ideas as he would donkeys, horses, camels and pigs. In other words, the Australian faunal assemblage would not appear to be as distinctive

in the eyes of an explorer as it did in Darwin's day, given that it now includes so many old world placentals. It is not just indigenous *cultural* assemblages which have been invaded, and made to include structures of European origin.

It is important to stress that the modern process of cross-colonisation does not, in any sense, violate the fundamental rule that shape reflects phylogeny, nor does it corrupt the general tendency of Selective systems toward heritage constraint. All that is corrupted is the secondary correlation of geography with the primary correlation between shape and phylogeny. For example, the introduction of Ostriches (an indigenous species of Africa) around the South Australian city of Adelaide, means that there are now two quite different large flightless birds (ratites) on that continent; both Emus and Ostriches. Yet this does not mean that the morphology of Australian flightless birds has escaped the phylogenetic constraints of past, or that their morphology is no longer the key to that phylogeny. Each Ostrich gene pool still has its genetic constraints, as does each Emu gene pool. The new ratite assemblage simply has a broader heritage, combining the phylogeny of both Emus and Ostriches, and this breadth of heritage is reflected in a correspondingly wider range of morphologies. The range of forms in the new ratite assemblage is still overwhelmingly narrow with respect to the total range of avian forms, with a dramatic absence of Eagles, Wrens, Ducks, and Egrets, etc., and even if a group of Ostriches and Emus were exposed to selective pressures for which, say, an Eagle was the perfect solution, it is difficult to imagine such forms arising through mutation, even on a geological time scale. The phylogenetic heritage of Australian ratites may no longer be restricted purely to Emu genes, but it still has a very restricted phylogeny with respect to the rest of the bird world.

Biogeography no longer directly reflects phylogenetic heritage, or at least not in the simple linear sense that it once did. Colonisation has by no means been symmetrical - different places have had very different colonising inputs - but without some general knowledge of the history of world colonisation, a voyage around the world would yield some very peculiar

biogeography, and, for that matter, some very peculiar ethnography. If anything, this problem would be the cause of even more confusion in the course of a *culturally* focused voyage. What would a modern ethnographer sailing down the coast of the Americas make of the mixed up sequence of Amerindian, Western European, Eastern European, Asian and West African cultural phenomena he or she observed, without being privy to a great deal of historical information?

Thus it can be said with some certainty that there can now never be an exact equivalent of a "Darwin of cultural fields of enquiry", in so far as there can never be a re-enactment of a cultural equivalent of the voyage of *The Beagle*. Moreover, a second Darwinian revolution in cultural fields of enquiry would, by definition, be very different to the first by virtue of post dating it; simply by virtue of being the second, and not the first. In biology, selectionism began as a minority position with no established disciplinary "territory" elsewhere, and was eventually successful enough to colonise virtually all of that discipline - a "rags to riches" story of a kind. Selectionism is now generally accepted orthodoxy for all but one of the millions upon millions of species on this planet. The theory is even well established as the explanatory framework for the non-cultural aspects of our species. Cultural phenomena are thus the only area of the study of life left unconquered by selective explanatory frameworks, and this area consists of the psychology and behaviour of just one of the many millions of animals and plants on earth. Viewed from this perspective, the second revolution would seem more like "the icing on the cake" than a major conquest. However, it is also true that the cultural part of this species is nonetheless differentially studied, and accounts for a much larger proportion of academic discourse (under the umbrella of the "Humanities") than does the behaviour of all the other species put together. Couched in these terms, the second Darwinian revolution could actually be seen to be *more* significant than the first, in that it would finally involve that original and monolithic body of so many universities, the Arts faculty.

Either way, the "second" revolution, should it ever occur, would be a very different neo-Darwinian revolution indeed. It could also, perhaps, prove to be a more socialistic enterprise (as a corporate achievement of a large number of people, and not a solitary "Darwin"), given the relatively widespread awareness that exists of the potential for such a revolution, and the relatively large number of participants already involved. It would be a far more self-conscious revolution this time. But what may be required, if the revolution is to proceed, is a change in focus away from theoretical manifestos and case studies of particular areas, a change toward more general aspects of cultural data. Darwin's voyage around the world cannot be replicated ethnographically, but it can give cultural selectionists some idea of the scale and nature of the cultural geography required to verify their approach.

8.2 Heritage Constraint in the Standard Cross Cultural Sample

While in some sense a cultural equivalent of Darwin's voyage would now be too late, in another sense it is no longer necessary, at least as far as the voyage itself is concerned. In over a century of ethnographic description and interpretation, anthropologists have amassed far more information about the world's diverse cultural phenomena than one person could ever collect in a single voyage. So a voyage is not what we need - we have instead a plethora of notes, produced by different people for a variety of different reasons; the problem is more akin to a post-voyage compilation and synthesis. The body of notes to be synthesized is so vast and recorded in so many different forms, however, that analyzing and coordinating the data is perhaps beyond the capacity of any unaided human being. It is important to remember that heritage constraint does not have to be singular to exist; a society may have a number of different influences in its history, but it is still jointly constrained by its joint heritage. It will be important, however, to arrive at a method by means of which the primary origins and/or influences involved in a given society's history may be estimated with reasonable reliability.

Yet as became clear in Chapters 6 and 7, the use of computers makes it possible to review large bodies of data with great speed and accuracy. The basic "descent with modification" or "phylogenetic heritage constraint" pattern was quickly and economically elucidated from a database of 2500 crania with the aid of the Principal Components variety of multivariate analysis, and this pattern appeared to emerge again and again when the analysis was repeated with smaller and smaller numbers of variables. It was also remarked that in other studies employing the same data (for example Wright and Kamminga 1988) a similar phenomenon had been discovered (Wright *Pers. Com.*); that this basic pattern would emerge even when the number of variables included in the analysis were reduced from 33 to as few as 5 (Chapter 6, Section 6.4). Such computer analysis would presumably have been an incredible advantage to any nineteenth century biologist wanting to find latent structure in a vast body of faunal or floral descriptions, as this form of analysis makes it possible to "see" complex structure in huge bodies of data. A *Beagle* voyage through ethnography is no longer possible, but multivariate analysis should at least allow us to contemplate the patterns in cultural data at a very general level.

So we are in a position to perform any number of computer aided "thought experiments", in which we can develop some idea of what a cultural selectionist ethnographer might have experienced in a voyage around the world. These thought experiments have an added advantage; that the cultural data within which they are performed is derived from "the world at large", so that we can perform the thought experiment with very real data.

The task is not so much to "prove" that heritage constraint exists in assemblages of cultural phenomena, as assumptions of local heritage constraint in regional archaeological or ethnographic sequences are already implicit in anthropological "practice" at the descriptive level of the discipline. Many empirically focused studies in both social anthropology and archaeology work on the assumption of some sort of fundamental historical continuity in regional sequences.

Rather, the role of the test in this chapter is to facilitate a more comprehensive and synthetic understanding of the pervasive and global occurrence of this pattern, and then to point out (particularly to the non-Darwinian archaeologist or anthropologist) that this ubiquitous pattern can be evoked in support of Cultural Selectionism, in the same way that biogeography can be evoked in support of neo-Darwinian genetics.

8.3 The Cultural Data

One problem with any attempt to prepare a wide ranging geographic sample of ethnographic descriptions involves the non-equivalence of ethnographic studies from different parts of the world. This is due to the tendency for theoretical traditions to focus on a particular region or set of regions for their case studies. In other words, to use a very simplistic but hopefully heuristic turn of phrase, in comparing cultures the analyst is not comparing Bantu with Irish but rather "the Marxist anthropologist's Bantus" with "the Freudian anthropologist's Irish" or *vice versa* as the case may be. For this reason the cultural diversity of different regions of the world is not only a function of actual differences (as perceived subjectively by anthropologists) in the cultures being studied, but also a function of considerable diversity amongst anthropologists in terms of methodology, interpretation, and focus of attention. However, the second class constitutes a greater obstacle for the elucidation of large scale geographic patterns, since the first (the actual diversity) is a dimension of the basic subject of study (ie. is a natural reflection of variation in the field of enquiry anyway), and should be explicitly incorporated into any comparative anthropological approach, while the latter (the diversity in the nature of ethnographic methodology) is a potential obfuscation to comparison. It is within the actual diversity that the heritage pattern is predicted to exist by the Cultural Selectionist perspective, while the process of anthropological representation of this diversity presumably has the potential to exaggerate or diminish the actual diversity. Fortunately, however, the problem of diversity amongst

anthropologists is, for the purposes of this study, a red herring. However great the cultural diversity that might exist amongst anthropologists, it must, presumably, be less than that empirically encountered in the field of enquiry, since anthropologists themselves have a more restricted cultural heritage than the cultures which they study. Thus even a diverse group of anthropologists would in fact *reduce* the scope of the diversity to be found in the field, since the anthropologists bring a broadly similar set of Western "subjectivities" to the "foreign" cultures which they then attempt to describe. Thus, despite the fact that theoretical diversity exists within anthropology, the restricted cultural background of most anthropologists would lead them to (inadvertently) reduce the likelihood of finding the neo-Darwinian pattern of cultural heritage constraint, which resides in the "historical particularism" of each culture. Western subjectivities presumably distort ethnographic description in the direction of Western structures, thus reducing the degree of difference perceived by the ethnographer. The fundamentally European heritage of anthropology, would, therefore (despite the philosophical diversity which exists amongst anthropologists) bias the study *away* from the primary selective hypothesis (that is, away from the actual and historically specific diversity toward simplified European universalities), and so the question of theoretical diversity within anthropology cannot be used to assail the results.

It is also important to bear in mind, in conclusion to the brief discussion above, that the Selectionist hypothesis of pervasive heritage constraint is unlikely to emerge as a result of the two basic kinds of error which presumably occur in large bodies of ethnographic data. Errors in this sort of data presumably fall into two main categories: either (A), where random inaccuracies are created through human error, or (B), where more systematic inaccuracies are created through the cultural or theoretical bias of the original anthropologist who made the observations. Both these general classes of error would be expected to corrupt the heritage constraint pattern, rather than create or exaggerate it. Errors of the first sort would be expected to gradually corrupt the original structure of the data, making it more and more like a random number matrix than it originally was, and so pushing it toward the "Random Evolution" hypothesis of the previous chapter. Errors of the

second sort, for the reasons mentioned in the last few pages, could be expected to reduce the distinctive elements of different cultures, and "Europeanize" them, thereby diluting the heritage constraint pattern, and making its appearance in the data less and less pronounced.

With respect to the methodology of the analysis, and the multivariate algorithms used in the large scale comparisons of bodies of information, non-equivalence of ethnographies derived from different anthropological traditions also presents practical difficulties. The basic practical difficulty is that it is more difficult to find mathematically commensurate descriptions of cultural systems. In fact, soon after beginning this part of the project it became clear that it was beyond the bounds of the study to compile a cultural data set of similar proportions to the W.W. Howells human cranial data. To actually compile a table of ethnographic descriptions of the cultural assemblages of 2400 communities and code them for 33 variables might well be more than a lifetime's work. Moreover, in many cases additional fieldwork would be required to obtain the missing data. As will be seen below, however, and may be seen in the environmental data of Chapter 7, often the variables that are recorded combine a large number of different observations, thereby presenting the information in a very "concentrated" form, as it were.

Non-equivalent theory and method in descriptive studies thus cause the main problem with the synthesis of the cultural data which is already available. Even in the largest databases, which may reach as many as 1200 societies coded for as many as 1000 variables, there was a problem in that no one society is represented in all the variables, and no one variable has been recorded for all the societies. At present, the Principal Components Analysis algorithm available for this kind of study requires a completely full matrix of numbers (including zeros, but no blank spaces). It thus requires every society to be represented in terms of every variable and vice versa. A huge database of 2000 x 1000 usually contains many blank spaces where a society's status with respect to that variable is unknown. As a consequence, the largest number of societies which shared complete records for the same variables turned out to be considerably smaller than the 2000 x 1000 dataset source, as

a direct result of the missing data problem. At the time of the analysis, PCA algorithms which could cope with missing data were not available. The other basic problem, occurring in both ethnographic and artefact data, is that Principal Components Analysis requires that the data be coded in the form of numerically meaningful ordinal scales. As a result, the data had to be comprehensively translated, and the details of those translations are laid out in Tables 5-8. The two basic data sources used were Murdock's *Ethnographic Atlas* (from the 1968 issues of the journal *Ethnology*), and the Demonstration Diskette for White's *Maptab*. This second source is one of three cross-cultural data sets published as electronic journal software by the World Cultures Quorum (Vol 1, Number 1). The demonstration diskette consists of 172 variables coded on a variety of topics for the 186 societies in what is termed the *Standard Cross Cultural Sample*. Topics span areas as diverse as factors of climate, subsistence and economics, sex and reproduction, family and social organisation, demography, politics and religion, child training and personality. Data from some categories was obviously not cultural in nature; only categories like subsistence and economics, family and social organisation, and politics and religion could offer cultural variables. From these sources, coded data for 25 common ethnographic variables was gathered. They are listed below, firstly in their original format on the *Maptab* Demonstration Diskette, and then in their final translated form. Added to these ethnographic variables are a further 41 artefact variables, derived from information in the atlas about house shape and raw material. These are described in some detail a little later, along with the translation procedures to which they were subjected.

After numerous eliminations of societies or variables, due to many examples of missing data, a "clean" or "full" data matrix of 157 societies and 66 variables was gathered. Thus the analysis essentially consists of the multivariate comparison of 157 cultural assemblages from all around the world. A brief list of the societies, and a brief explanation of the variables chosen and the manner of their coding will now be offered. All 157 societies used in the analysis come from a particular well used 186 society subset of the ethnographic atlas referred to as the Standard Cross Cultural Sample. This sample was created to mitigate a number of problems which occurred with

much larger data sets, outlined in the following words in the World Cultures Quorum, Volume 1, Number 1:

> "Many social scientists prefer to use large samples such as the Atlas to test hypotheses about human behaviour. However, a major problem of sample design effects arises from using multiple cases of each basic cultural type such as the 186 identified by Murdock and White. In any sample, duplicates lead to underestimates of variances. An "autocorrelation" problem exists to the extent that neighbouring societies tend to be similar. In a recent unpublished paper on "Design Effects in Standard Samples", Malcolm Dow shows that the use of the Ethnographic Atlas as a sample may lead to underestimates of variance by orders of magnitude of thirty to one hundred. In contrast, standard deviations estimated from the Standard Sample are often estimated at 70% or more of true magnitude compared to estimates from the Atlas at only 10% of true value. To test hypotheses it is thus essential to minimize design effects by using such designs as the Standard Sample." (World Cultures Quorum, Volume 1, Number 1, page 5).

Thus, this sample was specially chosen to minimise the very factor of cultural systems which causes the "heritage" or "descent with modification" pattern. An attempt was made to compensate for the fact that "neighbouring societies tend to be similar". Thus it can assumed that a whole level of the data in which heritage constraint was in its most dramatic form has been removed. If heritage constraint can still be found in the 157 cultural assemblages, it will be in spite of sampling procedures which were specifically designed to take it out.

Elucidating phylogenetic heritage constraint in any body of data taken from the products of a selective process often requires some kind of prior knowledge of phylogeny itself. In the case of the Howells cranial data of Chapter 6, the genealogical origin of each skull was already known within broad parameters. Each skull collection consisted of a group of skulls of known origin. The cultural assemblages of this chapter were classified into "common and/or restricted heritage" groups on the basis of common linguistic affiliation. Although linguistic affiliation is also highly correlated with geographic origin, it was thought that the language spoken by a group of people would be a reasonably reliable indicator of their cultural heritage. As will be seen in the groups below, however, some subsets are made up of societies of geographic proximity, when the languages spoken by the societies

display no obvious relationship. This is because the cultural assemblages have been included in the restricted heritage subgroups on the grounds of either language spoken or geographic origin, and often both. The 157 cultural assemblages are listed in the order in which they occur in the analysis (these are not their usual numbers in the Standard Cross Cultural Sample), and they are derived from the groups of people listed alongside (see Table 5). The first 25 variables (which are principally "non-material") employed in the analysis are coded and described (see Table 6, after Table 5), much as they are in Douglas White's Maptab Demonstration Diskette Codebook, although some of them were altered from arbitrary numerical codings to "quantity related" numerical or ordinal scales, as explained in detail immediately after the list below.

TABLE 5: THE CULTURAL ASSEMBLAGES

INCLUDED IN THE ANALYSIS

Cultural Assemblages From Khoisan And Mbuti Societies (Subsets: Khoisan 1-3; Mbuti 23):

1.Nama.1860. Gei/Khauan Hottentot.
2.Kung.1950. Nyai Nyae Bushmen.
3.Hadza.1930. Tribe.

23.Mbuti.1950. Epulu nethunters Ituri Forest.

Cultural Assemblages From Other African Societies (Subsets: Niger-Kordofanian 4-20,25; Miscellaneous 21-22, 24; Afroasiatic 26-37):

4.Lozi.1900. Ruling Luyana.
5.Mbundu.1890. Bailundo subtribe.
6.Bemba.1897. Zambia branch.
7.Nyakyusa.1934. Mwaya & Masoko Age villages.
8.Thonga.1865. Ronga subtribe.
9.Luguru.1925. Morogoro District.
10.Kikuyu.1920. Fort Hall or Metume district.
11.Ganda.1875. Kyaddondo district of Kampala.
12.Tiv.1920. Tar of Benue Province.
13.Nkundo.1930. Ilanga group.
14.Banen.1935. Ndiki subtribe.
15.Ashanti.1895. Kumasi State.
16.Ibo.1935. East Isu-Ana group.

17.Fon.1890. Abomey city and environs.
18.Azande.1905. Yambio Chiefdom.
19.Mende.1945. Bo town and vicinity.
20.Wolof.1950. Upper and lower Salum, Gambia.
25.Otoro.1930. Nuba Hills.
21.Songhai.1940. Bamba division.
22.Masai.1900. Kisonko S.Masai of Tanzania.
24.Shilluk.1910. Kingdom.

26.Fur.1880. Jebel Marra.
27.Teda.1950. Nomads of Tibesti.
28.Kaffa.1905. Kingdom.
29.Konso.1935. Buso town.
30.Somali.1900. Dolbahanta clan or subtribe.
31.Tuareg.1900. Ahaggaren tribe.
32.Riffians.1926. Entirety: Moroccan.
33.Amhara.1953. Gondar district.
34.Egytians.1950. Silwa town and environs.
35.Hebrews.621BC. Kingdom of Judea.
36.Babylonians.1750BC. Babylon city and environs.
37.Bedouin.1913. Rwala: Unspecified.

Cultural Assemblages From West Eurasian Societies (Subsets: Indo-European 38-46; Uralic 47-48; Altaic 49-51):

38.Albanians.1910. Mountain Gheg of N/Albania.
39.Armenians.1843. Vicinty of Erevan city.
40.Romans.110. Rome city and environs.
41.Irish.1932. County Clare.
42.Russians.1955. Viriatino village.
43.Kurd.1951. Rowanduz town and environs.
44.Basseri.1958. Nomadic branch.
45.Vedda.1860. Danigala Forest hunters.
46.Saramacca.1928. Upper Suriname River (South America).
47.Lapps.1950. Konkama District.
48.Samoyed.1894. Yurak.
49.Turks.1950. Northern Anatolian Plateau.
50.Kazak.1885. Great Horde.
51.Khalka.1920. Narobanchin Territory.

Cultural Assemblages From East Asian Societies (Subsets: Japanese/Korean 52-53; Far North Asian 54-57; Dravidian 58-59; Mon-Khmer/Austroasiatic 60-65; Tibetan 66-70):

52.Koreans.1947. Kanghwa Island.
53.Japanese.1950. Southern Okayama.
54.Ainu.1880. Saru Basin in Hokkaido.
55.Gilyak.1890. Sakhalin Island.
56.Chukchee.1900. Reindeer Division.
57.Yukaghir.1850. Upper Kolyma River.

58.Gond.1938. Hill Maria.
59.Toda.1900. Tribe.

60.Santal.1940. Bankura and Berghum Districts.
61.Lamet.1940. Tribe, Northwestern Laos.
62.Vietnamese.1930. Red River Delta in Tonkin.
63.Semang.1925. Jehai Group or subtribe.
64.Nicobarese.1870. Car Nicobar of N. Islands.
65.Andamanese.1860. Aka-Bea of South Andaman.

66.Lolo.1910. Liang Shan & Taliang Shan.
67.Lepcha.1937. Lingthem and vicinity.
68.Garo.1955. Rensanggri village.
69.Lakher.1930. Tribe.
70.Burmese.1965. Nondwin village.

Cultural Assemblages From Macro-Austronesian Societies (Subsets: Thai-Kadai 71; Malayo-Polynesian 72-93):

71.Siamese.1955. Bang Chan village.
72.Rhade.1962. Ko-Sier village.
73.Tanala.1925. Menabe subtribe.
74.Javanese.1954. Pare town and environs.
75.Balinese.1958. Tihingan village.
76.Iban.1950. Ulu Ai Group.
77.Badjau.1963. Tawi-Tawi & adjacent islands.
78.Alorese.1938. Abui of Atimelang village.
79.Manus.1937. Peri village.
80.New Ireland.1930. Lesu village.
81.Trobrianders.1914. Kiriwina Island.
82.Tikopia.1930. Ravenga District.
83.Ajie.1845. Neje Chiefdom.
84.Maori.1820. Nga Puhi Tribe.
85.Marquesans.1800.Te-i'i Chiefdom SW NukuHiva.
86.West Samoans.1829.Aana, Western Upolu Island.
87.Gilbertese.1890. Makin & Butiritari Isles.
88.Marshallese.1900. Jaluit Atoll.
89.Trukese.1947. Romonum Island.
90.Yapese.1910. Island.
91.Palauans.1947. Ulimang village.
92.Ifuago.1910. Kiangan Group.
93.Atayal.1930. Tribe (but excluding Sedeq).

Cultural Assemblages From South West Pacific Societies (Papuan 94-98; Australian 99-100):

94.Orokaiva.1925. Aiga subtribe.
95.Kimam.1960. Bamol village.
96.Kapauku.1955. Botukedo village.
97.Kwoma.1960. Hongwam subtribe.
98.Siuai.1939. Northeastern group.
99.Tiwi.1929. Tribe: Melville Island.
100.Aranda.1896. Alice Springs and environs.

Cultural Assemblages From North American Societies (Subsets: Nadene 101–106; Eskimo 107–108; Macro–Algonkian 109–113; Salishan, Hokan, and Siouan 114–119; Uto–Aztecan and Zuni 120–125; Macro–Penutian, Kutenai, Caddoan, Iroquoian, Natchez–Muskogean 126–132):

101.Ingalik.1885. Shageluk village.
102.Slave.1940. Lynx Point band.
103.Kaska.1900. Upper Liard River Group.
104.Eyak.1890. Tribe.
105.Haida.1875. Masset town.
106.Chiricahua.1870. Central band.
107.Aleut.1800. Unalaska Branch.
108.Copper Eskimo.1915. Coronation Gulf.
109.Montagnais.1910. Lake St.John & Mistassani.
110.Micmac.1650. Mainland Division.
111.Saulteaux.1930. Berens River band.
112.Gros Ventre.1880. Tribe.
113.Yurok. 1850.Tsurai village.
114.Bellacoola.1880.CentralGroup,lowerB.C.River.
115.Twana.1860. Tribe.
116.Havasupai.1918. Tribe.
117.Eastern Pomo.1850.Cignon village,Clear Lake.
118.Omaha.1860. Tribe.
119.Hidatsa.1836. Village.
120.Northern Paiute.1870.Wadadika,Harney Valley.
121.Commanche.1870. Tribe.
122.Papago.1910. Archie division.
123.Aztec.1520. Tenochtitlan city & environs.
124.Huichol.1890. Tribe.
125.Zuni.1880. Pueblo.
126.Klamath.1860. Tribe.
127.Lake Yokuts.1850. Tulare Lake.
128.Kutenai.1890. Lower or eastern branch.
129.Pawnee.1867. Skidi Band or subtribe.
130.Huron.1634. Bear and Cord Subtribes.
131.Natchez.1718. Kingdom.
132.Creek.1800. Upper division in Alabama.

Cultural Assemblages From South American Societies (Subsets: Mayan, Cariban 133–135; Macro–Chibchan 136–139; Equatorial 140–146; Ge–Panoan 147–152; Andean 153–157):

133.Quiche.1930. Chichicastenango town.
134.Callinago.1650. Dominica Island.
135.Carib.1932. Barama River.

136.Miskito.1921. Cape Gracias a Dios vicinity.
137.Cuna.1927. San Blas Archipelago.
138.Warrau.1935. Winikina of Orinoco Delta.
139.Cayapa.1908. Rio Cayapas Basin.
140.Goajiro.1947. Tribe.
141.Mundurucu.1850. Cabrua village.
142.Cubeo.1939. Village on Caduiari River.
143.Jivaro.1920. Tribe.

144.Siriono.1942. Vicinity of the Rio Blanco.
145.Trumai.1938. Vanivani village.
146.Tupinamba.1550. Rio de Janeiro hinterland.
147.Amahuaca.1960. Upper Inuya River.
148.Nambicuara.1940. Cocozu Group.
149.Timbira.1915. Ramcocamecra or Canella.
150.Aweikoma.1932. Duque de Caxias Reservation.
151.Lengua.1889. Vicinity of mission.
152.Abipon.1750. Vicinity of mission.
153.Inca.1530. Cuzco city and environs.
154.Aymara.1940. Chucuito Clan community.
155.Mapuche.1950. Vicinity of Temuco.
156.Tehuelche.1870. Equestrian.
157.Yaghan. 1865. Eastern and central.

(White, D. 1989)

The codebook is not quoted but rather paraphrased in such a way as to exclude any information other than that which concerns the nature of the variables themselves, as any additional information is superfluous here. Some explanatory notes are added for the purpose of this study (usually in parentheses). Where the original coding system was nominal or arbitrary (that is, where the coding symbol system had no representational element, such as when 1=donkey, and 2=Zebra, given that Zebras are not "larger" or "more" than Donkeys, but merely "different to"), the numbering system could sometimes be rearranged or simplified to produce a meaningful numeric series (for example, variables 14, 15, and 19).

Thus information for Principal Components Analysis has to be expressed in numeric form, and in the form of ordinal scales, where the first state of that variable, (whether it be 0 or 1), has to represent the total absence of the variable; the next state (1 or 2) has to represent a small amount of that variable; the next state (2 or 3) had to represent a larger amount; and so on. The numeric scale has to reflect *increasing quantity*, or the results of the Principal Components Analysis would be indeterminate. In most of the variables from the maptab codebook laid out below, the coded format already articulates an ordinal scale. Few changes had to made, since the coding was already meaningful in terms of increasing quantity.

TABLE 6: THE 25 ETHNOGRAPHIC VARIABLES

DERIVED FROM WHITE'S MAPTAB

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1. Writing and Records
- 1 = None
- 2 = Mnemonic devices
- 3 = Nonwritten records
- 4 = True writing; no records
- 5 = True writing; records

2. Amount of Gathering
- 0 = 0-5% dependence
- 1 = 6-15% dependence
- 2 = 16-25% dependence
- 3 = 26-35% dependence
- 4 = 36-45% dependence
- 5 = 46-55% dependence
- 6 = 56-65% dependence
- 7 = 66-75% dependence
- 8 = 76-85% dependence
- 9 = 86-100% dependence

3. Amount of Hunting
(Coded in the same manner as 2)

4. Amount of Fishing
(Coded in the same manner as 2)

5. Amount of Animal husbandry
(Coded in the same manner as 2)

6. Amount of Agriculture
(coded in the same manner as 2)

7. Animals and Plow Cultivation
- 1 = Absent (no plow animals)
- 2 = Not aboriginal but well established at period of observation. (altered to 0 in this study)
- 3 = Prior to contact.

8. Food Surplus via Storage
- 1 = None or very little
- 2 = Moderate
- 3 = Complex or more than adequate

9. Fixity of Settlement
1 = Migratory
2 = Seminomadic - alternatively fixed and migratory
3 = Rotating among two or more fixed points
4 = Semisedentary - fixed core, some migratory
5 = Impermanent - periodically moved
6 = Permanent

10. Compactness of Settlement
1 = Dispersed
2 = Spatially separated subsettlements
3 = Partially dispersed with central core
4 = Compact

11. Money (media of exchange) and Credit
1 = No media of exchange or money
2 = Domestically usable articles as media of exchange
3 = Tokens of conventional value as media of exchange
4 = Foreign coinage or paper currency
5 = Indigenous coinage or paper currency

12. Economic dependence on trade
0 = 0-4% dependence
1 = 5-9% dependence
2 = 10-14% dependence
3 = 15-19% dependence
4 = 20-24% dependence
5 = 25-29% dependence
6 = 55-59% dependence
7 = 65-69% dependence

13. Female Contribution to Subsistence (Average of three scores)
0 = 0-5%
1 = 6-15%
2 = 16-25%
3 = 26-35%
4 = 36-45%
5 = 46-55%
6 = 56-65%
7 = 66-75%
8 = 76-85%
9 = 86-99%

14. Technological Specialization
1 = None
2 = Pottery only
3 = Loom weaving only
4 = Metalwork only
5 = Smiths, weavers, potters.

As this coding was non-ordinal, it was changed to an ordinal scale based on "degree of technological specialization" as follows:

0 = None

1 = Specialization in one trade only, whether smiths, weavers, or potters
2 = Multiple numbers of specialist artisans

15. Land Transport (especially of food)
1 = Human Carriers
2 = Pack Animals
3 = Draft Animals (sleds, travois)
4 = Animal Drawn Wheeled Vehicles
5 = Motorised vehicles

(The apparently non-ordinal or arbitrary code system of variable 15 was retained, and judged to have an indirectly ordinal quality, as an ordinal scale of "increasing similarity to motorised vehicles")

16. Social Stratification (Degree of "vertical" differentiation)
1 = Egalitarian
2 = Hereditary
3 = 2 social classes, no castes/slavery
4 = 2 social classes, castes/slavery
5 = 3 social classes or castes, with or without slavery

17. Slavery (Degree to which it is hereditary)
1 = Absence or near absence
2 = Incipient or nonhereditary
3 = Reported but type not identified
4 = Hereditary and socially significant

18. Direction of Gift Exchanges in Marriage (An ordinal scale of increasing cost of wedding to bride's family)
1 = Bride-Price or -Wealth, to bride's family from husband's family
2 = Bride-Service, to bride's family from husband's family
3 = Token Bride-price
4 = Gift exchange, reciprocal
5 = Sister or female relative exchanged for bride
6 = Absence of consideration (recoded as 4)
7 = Dowry to bride from her family

19. Marital Residence with Kin (An ordinal scale of increasing patrilocality)
0 = Nonestablishment of Common Household, or Neolocal and located independently of kin
1 = Matrilocal: with wife's unilineal kin group
2 = Uxorilocal: with wife's parents
3 = Optional avunculocal or uxorilocal
4 = Bilocal: with either kin group
5 = Avunculocal: with husband's mother's brother
6 = Optional avunculocal, virilocal, or patrilocal
7 = Virilocal: with husband's parents
8 = Patrilocal: with husband's unilineal kin group

20. Household Form (An ordinal scale of

increasing household segregation)
1 = Large communal structures
2 = Multi-family dwellings
3 = Single family dwellings
4 = Family homestead
5 = Multi-dwelling households, each with married pair
6 = Multi-dwelling households, husband rotates among wives
7 = Mother-child households, husbands separate
8 = Multi-dwelling households, each dwelling occupied

21. Family Type (An ordinal scale of more and more extended family structure)
1 = Nuclear Monogamous
2 = Nuclear Polygynous
3 = Stem Family
4 = Small extended
5 = Large extended

22. Presence or Absence of Corporate Kinship
0 = Bilateral - not a corporate kin group
1 = Matrilineal - through female line
1 = Double descent - separate groups through male and female lines
1 = Patrilineal - through one parent in each generation
1 = Ambilineal - through one parent in each generation

23. Community Leadership (Degree of Complexity)
1 = No centralised local leadership
2 = Higher level only
3 = Single local leader
4 = Dual/plural headmen
5 = Single local leader and council
6 = Local councils
7 = Single local leader and subordinates
8 = To complex to be coded

24. Degree of Intercommunity Marriage
1 = Local endogamy 90-100%
2 = Local endogamy 61-89% (agamous)
3 = Local endogamy 60-60% (agamous)
4 = Local endogamy 11-29% (agamous)
5 = Local endogamy 0-10% (exogamous)

25. Levels of Sovereignty
1 = Stateless society
2 = Sovereignty 1st hierarchical level up
3 = Sovereignty 2nd hierarchical level up
4 = Sovereignty 3rd or higher hierarchical level

(White, D. 1989)

However, as all of the 25 variables above were ethnographic, attention was then turned to some of the more "ethnoarchaeological" variables recorded in the ethnographic atlas. This sort of data, particularly in the ethnographic atlas, was coded alphabetically rather than numerically, and as a result required a large amount of translation. A global sample of any kind of artefact was now all that was required - it was irrelevant whether longbows, stone tool technologies, or kitchen stoves were used. A cross-cultural sample of houses were chosen, houses being one of the better represented artefacts in the ethnographic atlas, and the hope of finding house information for a large number of the Standard Cross Cultural Sample societies was likely. Using the same sources as those used for the ethnographic data set of the last chapter, a database of house shape and building material was compiled. Where possible, the very same sample communities were used; otherwise a random draw was taken from amongst the same language group as the community for which that particular kind of information was missing (this turned out to be necessary in only one case). At first the data was collected from the *Ethnographic Atlas* in its original form (see Murdock, George Peter, 1968). The most common dwelling form was represented by five letters, each letter representing the form of that dwelling with respect to each of five general categories of shape, construction, or material. These five letters form columns 80-84 of the atlas, and the information these columns convey are outlined on page 168 of the atlas, and presented in Table 7 below).

TABLE 7: THE ARTEFACT VARIABLES IN THEIR ORIGINAL FORM

Column 80: Ground Plan of Dwelling. The symbols used are the following:

C Circular.
E Elliptical.
P Polygonal.
Q Quadrangular around (or partly around) an interior court.
R Rectangular or square.
S Semicircular.

Column 81: Floor Level. The symbols used are the following:

E Elevated slightly above the ground on a raised platform of earth, stone, or wood.
G Floor formed by or level with the ground itself.
P Raised substantially above the ground on piles, posts, or piers.
S Subterranean or semi-subterranean, ignoring cellars beneath the living quarters.

Column 82: Wall Material. The symbols used are the following:
A Adobe, clay, or dried brick.
B Bark.
F Felt, cloth, or other fabric.
G Grass, leaves, or other thatch.
H Hides or skins.
M Mats, latticework, or wattle.
O Open walls, including cases where they can be temporarily closed by screens.
P Plaster, mud and dung, or wattle and daub.
R Walls indistinguishable from roof or merging into the latter. Cf. Column 84.
S Stone, stucco, concrete, or fired brick.
W Wood, including logs, planks, poles, bamboo, or shingles.

Column 83: Shape of Roof (or of walls and roof where not distinct). The symbols used are the following:
B Beehive shaped with pointed peak.
C Conical.
D Done shaped or hemispherical.
E Semi-hemispherical.
F Flat or horizontal.
G Gabled, i.e., with two slopes.
H Hipped or pyramidical, i.e., with four slopes.
R Rounded or semi-cylindrical.
S Shed, i.e., with one slope.

Column 84: Roofing Material. The symbols used are the following:
B Bark.
E Earth or turf.
F Felt, cloth, or other fabric.
G Grass, leaves, brush, or other thatch.
H Hides or skins.
I Ice or snow.
M Mats.
P Plaster, clay, mud and dung, or wattle and daub.
S Stone or slate.
T Tile or fired brick.
W Wood, including logs, planks, poles, bamboo, or shingles.

(Murdock,G. P. 1968:168-169).

Before these artefact variables could be added to the analysis, the data had to be translated from the letter codes into a meaningful series of ordinal scales, in a similar manner to the ethnographic data. This also proved to be

a long and involved process (however this process was considerably shorter than were the estimates made of the time it might take to create a similar cross-cultural sample of second hand (leave alone first hand) archaeological phenomena). Basically, the information had to be expressed in numeric form, where the number allocated to a house type for a particular variable had to be a meaningful expression of the degree, quantity, or amount of the variable in question, possessed by that house. Because of this, the initial variables had to be broken up. It was not possible to take, say, "Column 80: Ground Plan of Dwelling" and then code Circular as 1, Elliptical as 2, Polygonal as 3, and Quadrangular as 4, since the computer would interpret this as meaning that circular ground plans were "small" ground plans, elliptical plans not so small, polygonal plans somewhat larger, quadrangular plans as larger still, and so on. Instead each individual trait and its associated symbol was made into a single variable, with only two states of that variable being possible - either it is present or it is absent. The new database has 41 variables instead of 5 (added to the 25 above for a total of 66). Presence is given the numeric code of one, and absence is given the numeric code of zero. This kind of solution is a happy compromise; all the original information is preserved, and yet it is perfectly meaningful to say that the presence of mud walls in a house implies a greater quantity of "mud wallness" than the absence of mud walls in another house. In other words, the implicit ordinal values of the codes 1 and 0 do not conflict with the ontological world of the data.

Table 8 displays the final order in which the remaining 41 house shape/raw material variables (the main "artefact" variables) were arranged in the analysis, after the above recoding procedure.

The raw data matrix consists of 157 societies each coded for a total of 66 variables. Moreover, although no data from actual archaeological sites was included, a considerable amount of the data involves descriptions of artefact variables, and the nature of the cultural assemblages is therefore at least "ethnoarchaeological". Each of the 157 societies could now be compared multivariately with each of the others, in terms of each of the 66 variables,

and it remained to be seen whether, or to what degree, the selective pattern of cultural heritage constraint would emerge.

TABLE 8: THE 41 ARTEFACT VARIABLES

IN THEIR FINAL FORM FOR THE ANALYSIS

Variable 26: Presence of Circular Ground Plan in Most Frequent House Type
0 = Absent
1 = Present

Variable 27: Presence of Elliptical Ground Plan in Most Frequent House Type
0 = Absent
1 = Present

Variable 28: Presence of Polygonal Ground Plan in Most Frequent House Type
0 = Absent
1 = Present

Variable 29: Presence of Quadrangular Ground Plan in Most Frequent House Type
0 = Absent
1 = Present

Variable 30: Presence of Rectangular Ground Plan in Most Frequent House Type
0 = Absent
1 = Present

Variable 31: Presence of Semicircular Ground Plan in Most Frequent House Type
0 = Absent
1 = Present

Variable 32: Presence of Elevated Floor Level in Most Frequent House Type
0 = Absent
1 = Present

Variable 33: Presence of Ground Level Floor in Most Frequent House Type
0 = Absent
1 = Present

Variable 34: Presence of Floor Raised on Piles, Posts, or Piers in Most Frequent House Type
0 = Absent
1 = Present

Variable 35: Presence of Subterranean or semi Subterranean Floor in Most Frequent House Type
0 = Absent
1 = Present

Variable 36: Presence of Adobe, Clay, or Dried Brick Walls in Most Frequent House Type
0 = Absent
1 = Present

Variable 37: Presence of Bark Walls in Most Frequent House Type
0 = Absent
1 = Present

Variable 38: Presence of Felt, Cloth, or Other Fabric Walls in Most Frequent House Type
0 = Absent
1 = Present

Variable 39: Presence of Grass, Leaves, or Thatch Walls in Most Frequent House Type
0 = Absent
1 = Present

Variable 40: Presence of Hides or Skin Walls in Most Frequent House Type
0 = Absent
1 = Present

Variable 41: Presence of Mats, Latticework, or Wattle Walls in Most Frequent House Type
0 = Absent
1 = Present

Variable 42: Presence of Open (or Temporarily Closed) Walls in Most Frequent House Type
0 = Absent
1 = Present

Variable 43: Presence of Plaster, Mud and Dung, or Wattle and Daub Walls in Most Frequent House Type
0 = Absent
1 = Present

Variable 44: Presence of Walls which are Indistinguishable from Roof in Most Frequent House Type
0 = Absent
1 = Present

Variable 45: Presence of Stone, Stucco, Concrete, or Fired Brick Walls in Most Frequent House Type
0 = Absent
1 = Present

Variable 46: Presence of Wood Walls in Most Frequent House Type
0 = Absent

1 = Present

Variable 47: Presence of Beehive Shaped Roof in Most Frequent House Type
0 = Absent
1 = Present

Variable 48: Presence of Conical Roof in Most Frequent House Type
0 = Absent
1 = Present

Variable 49: Presence of Dome Shaped or Hemispherical Roof in Most Frequent House Type
0 = Absent
1 = Present

Variable 50: Presence of Semi-hemispherical Roof in Most Frequent House Type
0 = Absent
1 = Present

Variable 51: Presence of Flat Roof in Most Frequent House Type
0 = Absent
1 = Present

Variable 52: Presence of Gabled (Two Slope) Roof in Most Frequent House Type
0 = Absent
1 = Present

Variable 53: Presence of Pyramidal (Four Slope) Roof in Most Frequent House Type
0 = Absent
1 = Present

Variable 54: Presence of Rounded (Semi-cylindrical) Roof in Most Frequent House Type
0 = Absent
1 = Present

Variable 55: Presence of "Shed" (One Slope) Roof in Most Frequent House Type
0 = Absent
1 = Present

Variable 56: Presence of Bark Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

Variable 57: Presence of Earth/Turf Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

Variable 58: Presence of Felt, Cloth, or other Fabric Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

Variable 59: Presence of Grass, Leaves, Brush or Thatch Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

Variable 60: Presence of Hide or Skin Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

Variable 61: Presence of Ice/Snow Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

Variable 62: Use of Mats as a Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

Variable 63: Presence of Plaster, Clay, Wattle and Daub, or Mud and Dung Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

Variable 64: Presence of Stone or Slate Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

Variable 65: Presence of Tile or Fired Brick Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

Variable 66: Presence of Wooden Roofing Material in Most Frequent House Type
0 = Absent
1 = Present

8.4 Analysis And Results

A "metaphorical" explanation of the data manipulation will now be offered, with the aim of conveying an intuitive understanding of the algorithmic "assembly line" through which the cultural data listed above was passed. It is a similar series of metaphors to those used in Chapters 6 and 7,

only this time the process is discussed with reference to the peculiarities of the data of this chapter. Those readers who require a more mathematically specific description of PCA might begin with R.V.S. Wright's discussion (Wright 1989, BIGPCA 1-5), for a lucid summary and an introductory bibliography.

As Wright has remarked (BIGPCA page 3), PCA is not a mysterious method at the intuitive level. The computer is able to compare each of the 157 cultural assemblages with each of the others in terms of the 66 variables, and similarly compare each of the 66 variables with each of the others in terms of the 157 cultural assemblages. Groups of cultural variables which covary - i.e. are highly correlated - are thus discovered. These groups of variables, very loosely speaking, are then synthesized into "supervariables" or principal components, variables which are constituted from far more information than the original single variables. The cultural assemblages can then be graphed bivariately in terms of any two principal components, where the position or co-ordinates of a cultural assemblage on that graph is a function of its entire 66 variable profile, in contrast with the usual bivariate graph, where co-ordinates are a function of only two variables. The first Principal Component is the "supervariable" which articulates the largest dimension of variation in the assemblages, the second the component which accounts for the next largest dimension of variation, and so on.

Thus the analysis can provide a series of bivariate plots in which each 66 variable cultural assemblage can be intuitively compared with each and all of the others, with a degree of comprehensive precision far beyond that of unaided human observation of a raw data matrix. Furthermore, various subgroups can be graphed separately, and their position and range compared with that of other subgroups, or with the range of the entire 157 assemblages.

When the neo-Darwinian "phylogenetic heritage constraint" pattern was defined in Chapter 6, with reference to the "known neo-Darwinian system" of human cranial data, skulls thought to have common ancestry were graphed together. The diversity and morphology of these groups of limited heritage was shown to be "constrained", in the sense that each group displayed a

distinctive range of morphologies and a relatively constricted diversity. The identical procedure will now be followed with the cultural data. It should be noted that the cultural "objects" being compared are not "cultural crania" - in the sense of single objects or artefacts - but are instead the "cultural equivalent" of "faunal assemblages". In this sense, this chapter is addressing a larger scale of the system in question than does the cranial data in Chapter 6. However, the heritage constraint pattern is equally pervasive at all levels of a neo-Darwinian system (see Chapter 6, Sections 6.3 and 6.4, also Section 7.1 of this chapter) and so the level of the system addressed is irrelevant. Moreover, the level at which Darwin defined the biogeography which was so important to the advance of selectionism in biology was in fact the faunal assemblage, not the level of a single globally distributed species. So the cultural assemblage is, after all, perhaps the most appropriate level at which to look for a general manifestation of heritage constraint in culture. A global comparison of cultural phenomena described at this level should be, in any case, at least *approximately* equivalent to the level of biological systems tapped by Darwin's general observations on the voyage of *The Beagle*.

What can now be produced, then, is a graph of any particular group of cultural assemblages according to its range of "shapes", in the same way that groups of crania were graphed according to their range of shapes.

Firstly, however, the space in which the cultural assemblages are to be plotted has to be characterised in terms of the original variables. It is customary in a PCA to discuss the plots of the variables and objects together, so that each is characterised and explained in terms of the other. But in this study the variables will be dealt with briefly, as the purpose of this analysis is to illustrate one aspect of the data only: *the phylogenetic heritage constraint implied by the restricted distribution of groups of cultural assemblages with restricted heritage*, when graphed on PCA biplots. Those readers with some specialist interest in Principal Components Analysis may choose to note, however, that in the analysis the data was first transformed into a correlation matrix, not a covariance matrix (Wright 1989 BIGPCA page

10). This option changes variables of different sizes into variables of the same size. Wright offers the following explanatory example:

> "Imagine and analysis of femurs in which femur head diameters are inherently about 1/10 of the size of femur length. We are surely just as interested in relative variation in femur head diameter as we are in variation of femur length. Yet if we choose the covariance matrix femur length will swamp the analysis by a factor of 10." (Wright 1989 BIGPCA p.10).

As the cultural variables listed above were of different magnitudes, the *correlation matrix* option was used here. It was also used in *every other analysis*, in order that the hypotheses derived from these other analyses would be commensurate with the cultural case study of this chapter.

A short additional discussion of the variables is necessary, if only to briefly describe the "fabric" upon which the restricted distributions of particular groups will be plotted **(FIGURE 8.01)**. As with the variable plot of the cranial data **(FIGURE 6.13)**, and of environmental conditions **(FIGURE 7.06)**, all that need be noted is that the variables are well spread. This means that the space in which the cultural assemblages will be plotted in succeeding diagrams is a function of most of the variables and not just a few. As explained in previous chapters, variable plots are usually discussed in detail in principal components analysis, variable by variable, in order to reconstruct the different object structures characterised in various regions of the plot. But, as emphasised throughout the data chapters, principal components analysis is used in this study purely to illustrate very general aspects of structural variability. The structure under investigation is not the structure of individual crania or cultural assemblages, but the structure of structural variation itself, and by inference, the structure of the evolutionary process which produced that variation. Therefore the variable plot itself will be given no further attention.

The general plot of the entire sample of cultural assemblages **(FIGURE 8.02)**, is the first plot in which the results of the "preliminary" test begin to emerge. The general pattern produced can be compared with the overall plots of the three alternative hypotheses **(FIGURES 6.14, 7.07, 7.16)**. Like the "neo-Darwinian" and "Random Evolution" hypotheses **(FIGURES 6.14 and 7.16)**, the

overall plot of cultural assemblages displays a random "shotgun scatter". No obvious divisions or clusters can be discerned - we can see immediately that there is no "periodic table of cultures" or rigid typological sequence to be found in cultural assemblages. The results at this level are consistent with other general analyses of cultural data by cultural selectionists such as Leonard and Jones (1987:207-210), in which it was shown that rigid typological schemes for cultures can be easily deconstructed. The graph clearly demonstrates the fundamentally continuous nature of variation within the standard cross-cultural sample. At this general level of observation, then, the Lamarckian hypothesis can be tentatively considered as having been refuted. There is an obvious absence of any discrete clusters of the kind which were shown to exist in human environments analyzed in Chapter 7 **(FIGURE 7.07)**. Cultural assemblages, faunal assemblages, and random numbers, on the other hand, all resemble each other at this general level. However, there is already some indication that the faunal and cultural plots are the most similar - like the human cranial data, the cultural assemblage plot is not sparse along all margins. As remarked in the discussion of the general random number plot **(FIGURE 7.16)**, a principal components analysis of "random normal deviates" tends to produce a plot which is sparse along all edges of the distribution. But although both the cranial and cultural plots are sparse along parts of their edges, each of them displays a more even distribution of objects throughout the plot - with fewer objects concentrating around the origin, and more objects in the outer regions of the distribution - than random numbers.

As the established procedure of the previous analyses is continued, and limited heritage subsets are plotted, more and more striking correspondence between the neo-Darwinian hypothesis and cultural geography can be observed. Compare, for example, the plot of African cultural assemblages **(FIGURES 8.03 and 8.04 combined)** with the distributions of African crania **(FIGURE 6.15)**, and the "Lamarckian' or "environmental determinist" null hypothesis distribution of African environments **(FIGURE 7.09)**. Although the African subset - the first 37 cultural assemblages - is centred upon the origin, it will become clear below that it is rare in this regard. It was seen, moreover, in the cranial data of Chapter 6, that some subset plots were

centred upon the origin. What was characteristic of the random number null hypothesis was that nearly all the plots were centred upon the origin. Moreover, if the African subset is broken into two smaller subgroups which one might reasonably expect to have somewhat different heritage in terms of cultural origins and influences, two typically nonrandom distributions are produced. Khoisan and Mbuti societies occupy a tight region in the top left quadrant of the world distribution (**FIGURE 8.03**), while the other African cultural assemblages are strongly concentrated in the right hand side of the distribution, occupying the top of the lower right quadrant, and the lower area of the top right quadrant (**FIGURE 8.04**).

The heritage constraint pattern has so far been illustrated purely in terms of ranges and distributions. But as cultural assemblages are the focus of the test it is interesting to note some additional statistics associated with these groups. The limited range of morphology associated with the cultural assemblages of a particular continent can be represented in terms of other characteristics of the subsets.

The total distribution of the African subset, for example, is nonrandom with respect to other more restricted distributions in the study, but is in and of itself not obviously nonrandom. If, however, we compute the mean score of the entire group for each component, we find that the point to which the mean corresponds is not located on the origin, but well into the top right quadrant (0.07,0.10; the middle of this quadrant, for comparison, being located around 0.20,0.20). Most African cultural assemblages, then, are located more to the right of the origin than the distribution would suggest. Its apparent "pararandom" distribution is a function of a small number of unusual assemblages, not of typical structure.

The tendency of the African assemblages to occupy the top right quadrant is further exemplified when the "upper and lower 95% confidence limits" of the group are computed. These limits exclude the most atypical members of the subset (the most peripheral 5%), and when this is done the distribution falls entirely into the top right quadrant, as even the lower

confidence limits along both components (0.01,0.07) are positive, with the point defined by the upper confidence limits lying well into this quadrant (0.14,0.15). In other words, the vast majority of cultural assemblages within Africa display even more dramatic heritage constraint than the plots would indicate. Like African crania, they are more distinctive than the (the inorganic and general biotic aspects of) the environments in which they are found. The means and 95% confidence limits will be computed for the other subset distributions graphed below, to illustrate that heritage constraint is not only qualitatively present in the continental and subcontinental distributions of the subsets, but quantitatively present in their internal structure.

Before moving on to a discussion of the other continental and subcontinental subsets, the meaning of these statistics can be further clarified through a brief comparison with such group statistics in the random "Random Evolution" null hypothesis of Chapter 7. Similar statistics for ten subsets (sizes ranging from 4 to 20 objects) taken from the random number study of Chapter 7, were computed for the purpose of comparison. The mean co-ordinates of these subsets tended to approach the origin (the ten means being [0.00,0.02]; [0.02,-0.02]; [0.00,-0.01]; [0.05,-0.02]; [-0.03,-0.01]; [0.04,-0.03]; [0.00,0.02]; [0.00,0.00]; [0.00,0.00]; and [-0.01,0.05] on axes which ranged from [-0.20,-0.22] to [0.24,0.22]), in contrast to the tendency of subsets from faunal and cultural analyses to depart from the origin (for example the African mean above [0.07,0.11], and those discussed in greater detail below, such as western Eurasia [0.22,0.13]; northern Asia [-0.17,0.12] and South America [-0.10,-0.06] on axes which range from [-0.43,-0.27] to [0.58,0.51]).

Random distributions were most easily distinguished from nonrandom distributions, however, when 95% confidence limits were calculated for each. African cultural assemblages (above) were able to yield positive lower 95% confidence limits on both components [0.01,0.07], and upper limits of [0.14,0.15]. These are typical cultural (and selective system) statistics, in that the two points do not span the origin (in having the same sign), due to the distinctiveness of the subset, as will become clear in the discussion of other limited heritage subsets below.

On the other hand, the lower and upper confidence limits of ten random number subsets from the Chapter 7 matrix invariably spanned the origin on both axes. Thus the lower 95% confidence limits are invariably negative on both axes, while the upper 95% confidence limits are positive. The actual computations for the ten random subsets were as follows: [-0.04,-0.01] and [0.05,0.06]; [-0.03,-0.06] and [0.06,0.03]; [-0.05,-0.04] and [0.04,0.03]; [-0.03,-0.09] and [0.12,0.04]; [-0.06,-0.06] and [0.02, 0.04]; [-0.02,-0.10] and [0.11,0.04]; [-0.03,-0.01] and [0.02,0.04]; [-0.08,-0.08] and [0.08,0.08]; [-0.02,-0.03] and [0.02,0.02]; [-0.12,-0.07] and [0.11,0.19].

The average upper and lower 95% confidence limits for a random subset, derived from this sample of ten, were [-0.05,-0.05] and [0.06,0.06]. The average mean of the ten random subsets was [+/-0.02,+/-0.02]. These figures provide an additional indication of the general tendency of the random null hypothesis of Chapter 7 to yield subset distributions centred upon the origin. Conversely, these statistics are employed in the cultural data of this chapter in order to provide an additional illustration of the tendency of limited heritage subsets to be centred, not upon the origin, but on a unique part of the graph.

Moving on from Africa, the distributions of west Eurasian cultural assemblages are graphed in the next illustrations **(FIGURES 8.05 and 8.06)**. The distribution can seen to be concentrated toward the right hand quadrants, away from the margins of the left side of the world distribution, a somewhat broader distribution on the y axis, but narrower on the negative side of the x axis, due to its lack of any cultural assemblages resembling those of the click languages in the African distribution. In a similar manner to Africa, a small number of societies, themselves sharing a common heritage which is more limited again, extend the west Eurasian distribution. Thus, Indo-European and Uralic (Lapp and Samoyed) cultural assemblages **(FIGURE 8.05)** tend to congregate in the bottom right quadrant, but when the three Altaic (Turks, Kazak, and Khalka) cultural assemblages are added **(FIGURE 8.06)**, the distribution occupies both right hand quadrants, extending over the origin.

Figure 8.01: Plot of all cultural variables in terms of the 1st and 2nd principal components.

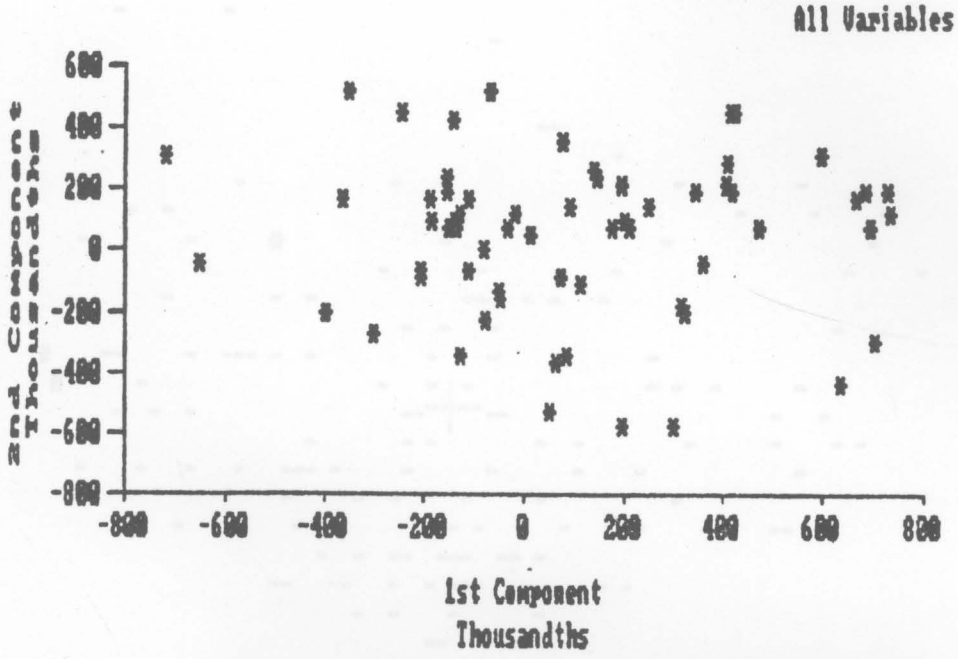


Figure 8.02: Plot of all cultural assemblages in terms of the 1st and 2nd principal components.

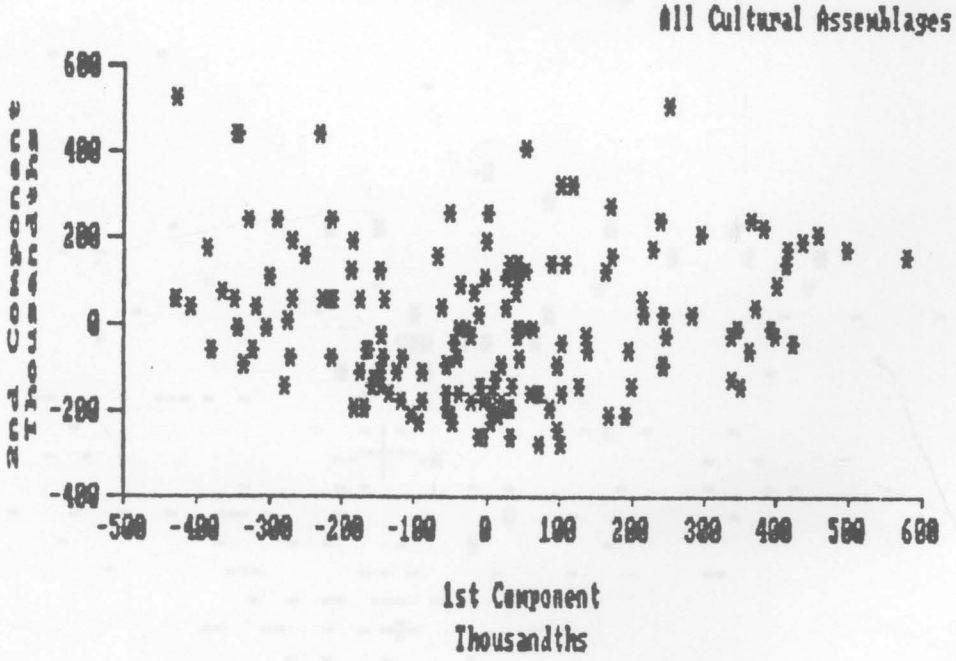


Figure 8.03: Distribution of Khoisan/Mbuti cultural assemblages (*) within the plot of all cultural assemblages (-), in terms of the 1st and 2nd components.



Figure 8.04: Distribution of non-Khoisan/Mbuti African cultural assemblages (*) within the plot of all cultural assemblages (-), in terms of the 1st and 2nd components.

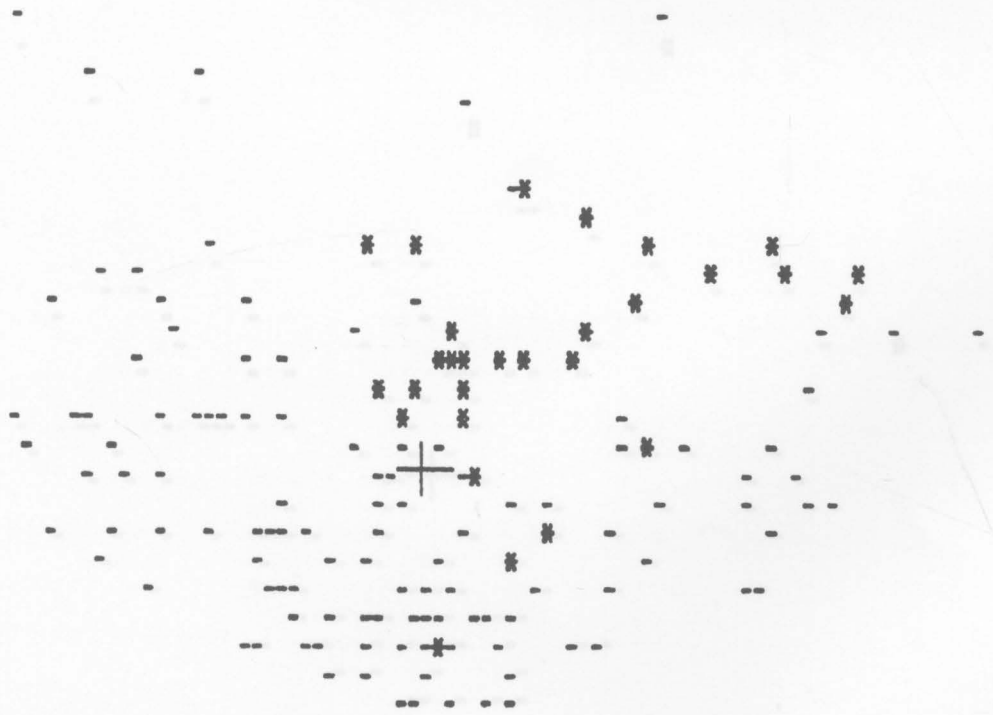


Figure 8.05: Distribution of Indo-European and Uralic (Lapp, Samoyed) cultural assemblages (*) within the plot of all cultural assemblages (-), in terms of the 1st and 2nd components.



Figure 8.06: Distribution of Altaic (Turkish, Kazak, Khalka) cultural assemblages (*) within the plot of all cultural assemblages (-), in terms of the 1st and 2nd components.

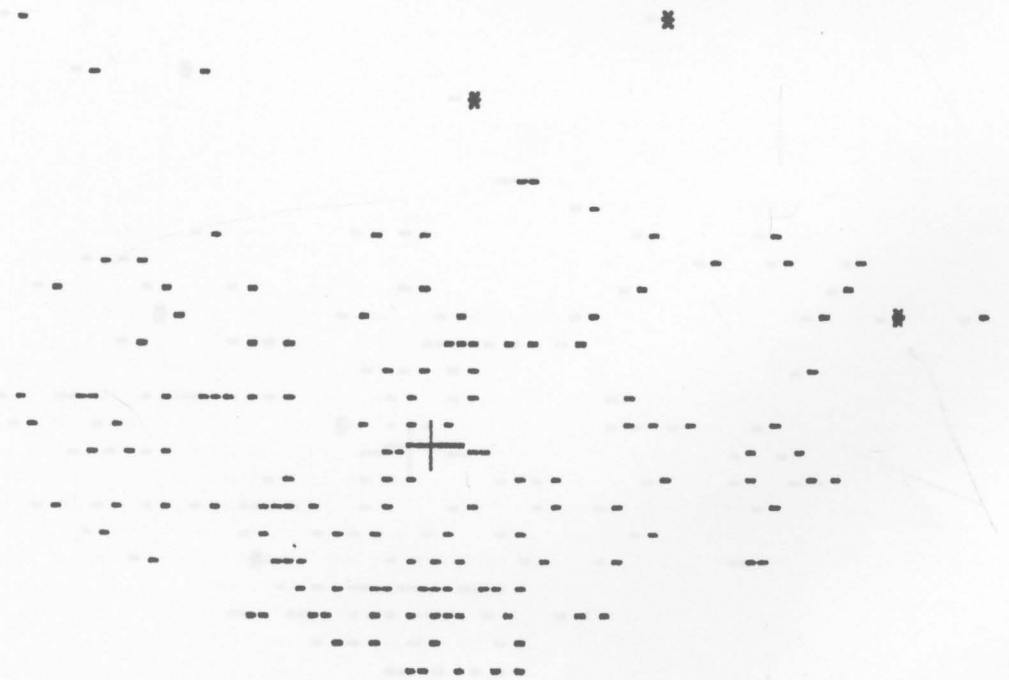


Figure 8.07: Distribution of east Asian cultural assemblages (*) within the plot of all cultural assemblages (-), in terms of the 1st and 2nd components.

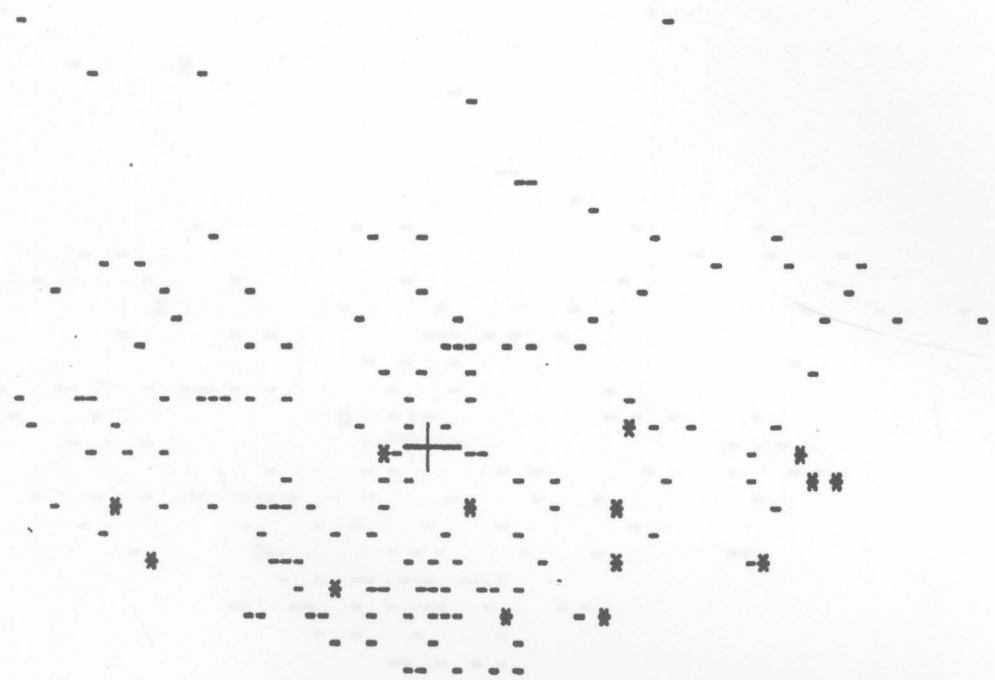


Figure 8.08: Distribution of north Asian (Ainu and Siberian) cultural assemblages (*) within the plot of all cultural assemblages (-), in terms of the 1st and 2nd components.

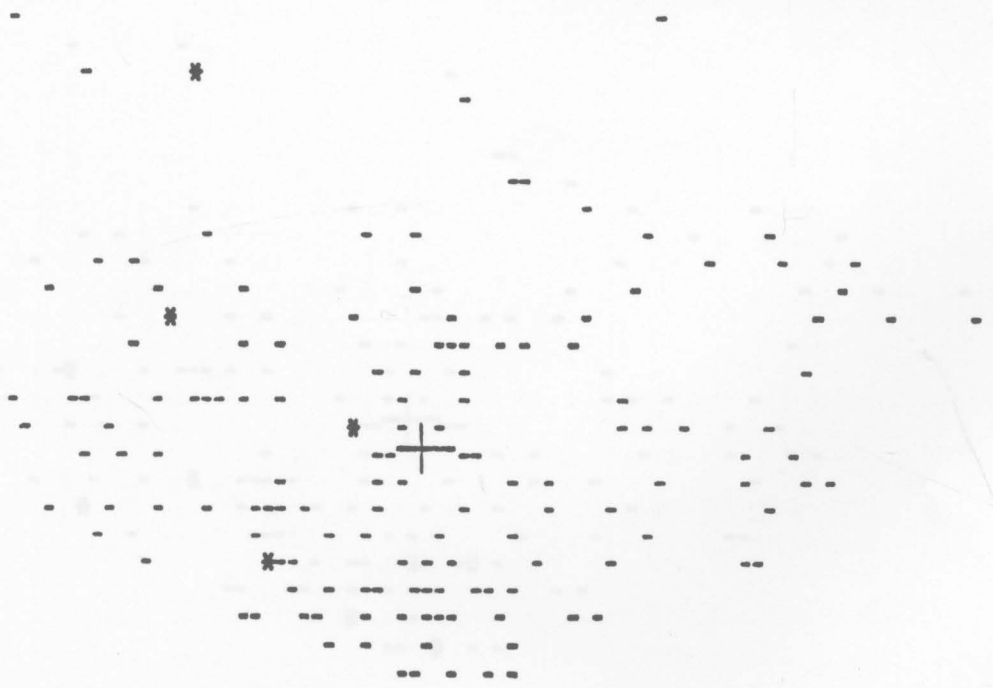


Figure 8.09: Distribution of Malayo-Polynesian and Thai-Kadai cultural assemblages (*) within the plot of all cultural assemblages (-), in terms of the 1st and 2nd components.

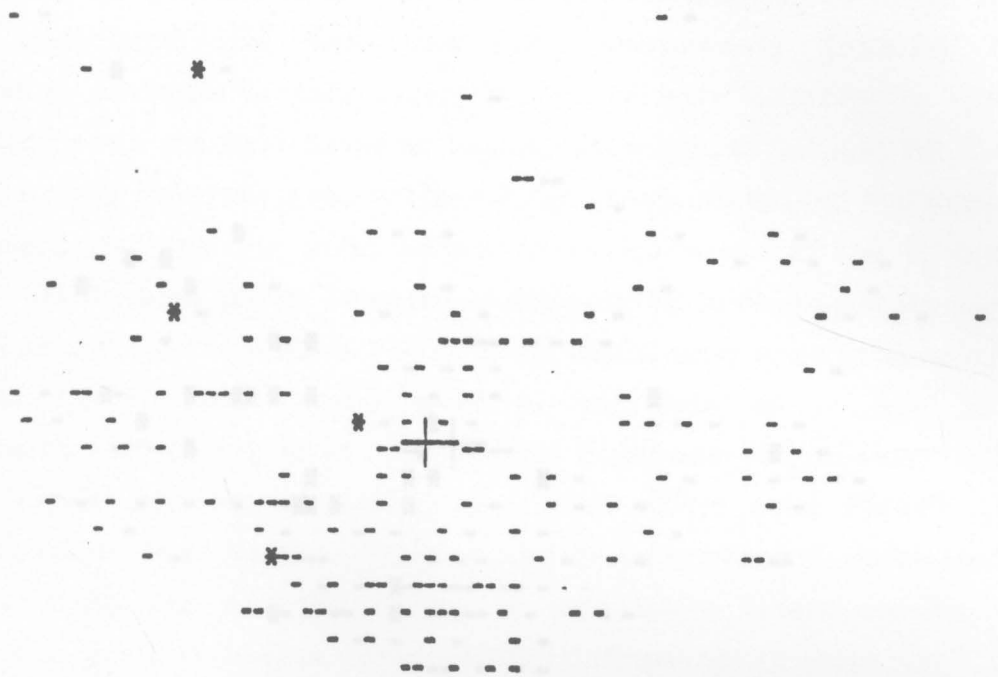


Figure 8.10: Distribution of Australian and Papuan cultural assemblages (*) within the plot of all cultural assemblages (-), in terms of the 1st and 2nd components.

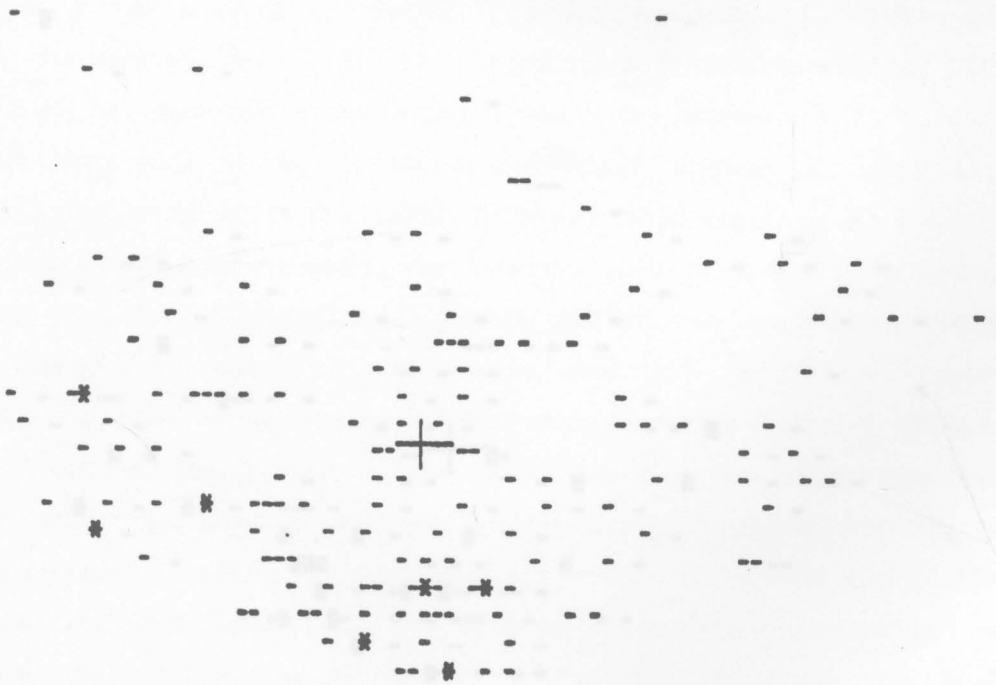


Figure 8.11: Distribution of North American cultural assemblages (*) within the plot of all cultural assemblages (-), in terms of the 1st and 2nd components.

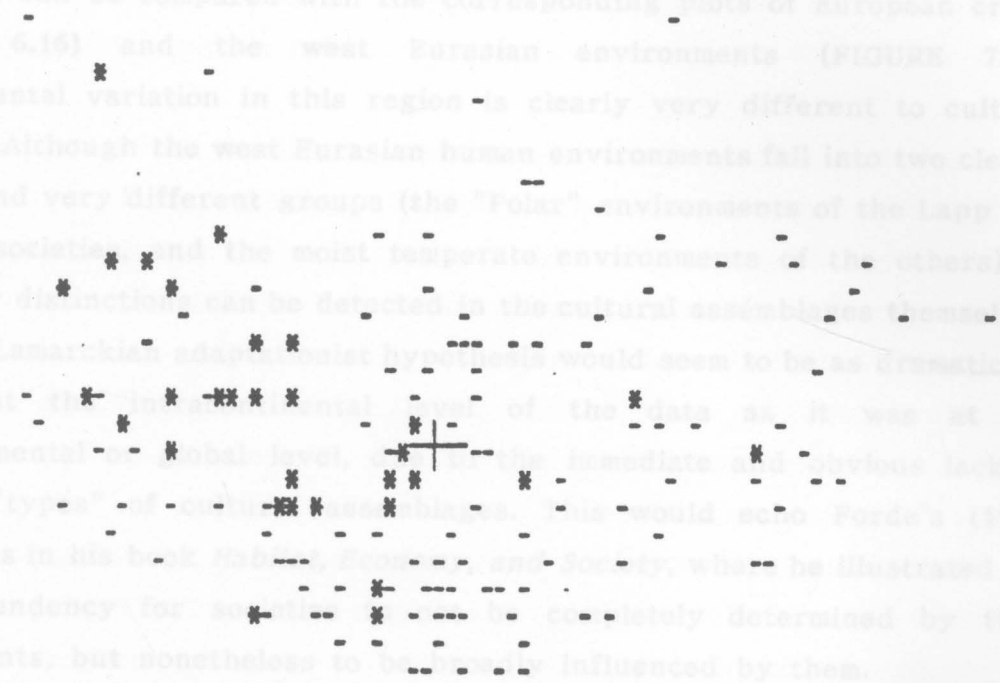
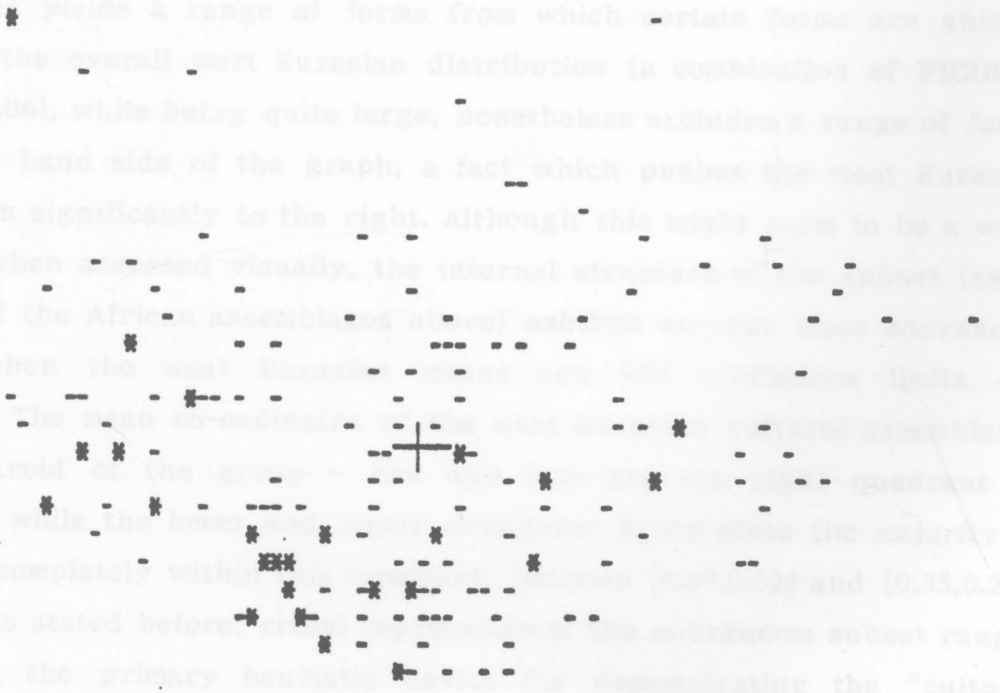


Figure 8.12: Distribution of South American cultural assemblages (*) within the plot of all cultural assemblages (-), in terms of the 1st and 2nd components.



The combined west Eurasian distribution **(FIGURES 8.05 and 8.06 together)**, can be compared with the corresponding plots of European crania **(FIGURE 6.16)** and the west Eurasian environments **(FIGURE 7.10)**. Environmental variation in this region is clearly very different to cultural variation. Although the west Eurasian human environments fall into two clearly defined and very different groups (the "Polar" environments of the Lapp and Samoyed societies, and the moist temperate environments of the others) no such clear distinctions can be detected in the cultural assemblages themselves. Thus the Lamarckian adaptationist hypothesis would seem to be as dramatically refuted at the intracontinental level of the data as it was at the intercontinental or global level, due to the immediate and obvious lack of discrete "types" of cultural assemblages. This would echo Forde's (1934) conclusions in his book *Habitat, Economy, and Society*, where he illustrated the general tendency for societies to not be completely determined by their environments, but nonetheless to be broadly influenced by them.

Like the African assemblages, each of the two plotted west Eurasian subsets **(FIGURES 8.05 and 8.06 again)** conform to the neo-Darwinian pattern in that the general ranges are nonrandom. Although there are as many "types" as societies - and therefore no types at all - each subset of cultural assemblages yields a range of forms from which certain forms are absent. Moreover, the overall west Eurasian distribution **(a combination of FIGURES 8.05 and 8.06)**, while being quite large, nonetheless excludes a range of forms in the left hand side of the graph, a fact which pushes the west Eurasian distribution significantly to the right. Although this might seem to be a weak tendency when assessed visually, the internal structure of the subset (as in the case of the African assemblages above) exhibits an even more nonrandom pattern, when the west Eurasian means and 95% confidence limits are calculated. The mean co-ordinates of the west Eurasian cultural assemblages - the centroid of the group - lies well into the top right quadrant at [0.22,0.13], while the lower and upper confidence limits place the majority of the group completely within this quadrant, between [0.09,0.02] and [0.35,0.24]. Although, as stated before, visual impressions of the nonrandom subset ranges will remain the primary heuristic device for demonstrating the "cultural

heritage constraint" pattern in the cultural assemblages, the means and 95% confidence limits serve to embellish the intuitive impressions derived from the subset plots with accepted indices of significance.

Subset distributions of the remaining assemblages begin to "migrate" away from the African and west Eurasian areas toward other parts of the global distribution, but each exhibits a visibly restricted distribution. In the next two figures **(FIGURES 8.07 and 8.08)**, Asian cultural assemblages are seen to occupy quadrants other than the top right quadrant, within which most of the African and European assemblages fell. East Asian assemblages **(FIGURE 8.07)** show a very distinctive and narrow distribution in the two lower quadrants, while the north Asian subset (Ainu, and Siberian isolates Yukaghir, Chukchee, and Gilyak) **(FIGURE 8.08)** occupies the top left quadrant. These distributions have means well removed from the origin - [0.10,-0.10] and [-0.17,0.12] respectively, compared to the averaged mean of [+/-0.02,+/-0.02] for the random subsets above. Such restricted and distinctive morphology cannot be a function of restricted environmental conditions, as a comparison of the Asian cultural assemblages with the human environments of Asia from the previous chapter **(FIGURE 7.11)** will reveal. All of the basic types of environment are present in this region, and yet, in keeping with the neo-Darwinian hypothesis, the limited heritage of this cultural group prevents certain cultural forms from being present. And like Asian crania, Asian cultural assemblages are far more distinctive than the Asian environments which surround them.

Pacific cultural assemblages also display dramatically restricted morphology in relation to global diversity. The Thai-Kadai and Malayo-Polynesian assemblages **(FIGURE 8.09)** display a similar range of structures to Asian societies, but with a more narrow distribution which excludes the assemblages of the lower left. The Malayo-Polynesian assemblages are thus contained almost wholly within the lower right quadrant, as though they are an Asian offshoot to the right. Thus the mean of this group, [0.07,-0.16], falls well within this quadrant, as do both the lower [0.01,-0.20] and upper [0.12,-0.12] 95% confidence limits.

While Malayo-Polynesian assemblages occupy a restricted area on the right of the Asian range, Papuan and Australian cultural assemblages (**FIGURE 8.10**) occupy a very restricted area to the left of the Asian range. Once again, this is in spite of the fact Pacific environments fall into two of the four very different and well defined groups, as shown in Chapter 7 (**FIGURES 7.12 and 7.13**). Common heritage clearly overrides the effects of these different environments, as the Papuan and Australian cultural assemblages display no sign of falling into two well defined groups, occupying the lower left area of the world distribution. The group mean, at [-0.12,-0.14], corroborates this intuitive impression, and even the lower and upper 95% confidence limits, at [-0.21,-0.23] and [0.05,-0.04] respectively, lie almost completely in the quadrant, extending only marginally into the lower right quadrant.

North and South America complete the preliminary test, both of them displaying the same limited and distinctive range of cultural phenomena (**FIGURES 8.11 and 8.12**), in spite of the immense range of American environments, which approximates the world environment range (**refer back to FIGURES 7.14 and 7.15**). American cultural assemblages are characterised by broad ranges, which extend into the top left quadrant to include distinctive morphologies only shared by Ainu and isolated Siberian cultural assemblages; these are forms which are distinctively present in both North and South America. Distinctive absences in the American distributions are certain cultural structures which were so distinctively present on the other side of the Atlantic, in the African and European cultural assemblages. As stated several times already, what forms these distinctive structures take is not the focus of the present study, emphasis is placed only on the fact that distinctive forms of whatever description, or their absence, characterise the faunal and cultural assemblages of particular regions.

The group statistics for the American assemblages provide further verification of the distinctive nature of Amerindian cultural phenomena. The North American mean of [-0.15,0.05] indicates that, on average, the forms of these cultural assemblages are intermediate between the far north Asian assemblages (**FIGURE 8.08**) which have a mean of [-0.17,0.12], and those of

South America, which have a mean [-0.10,-0.06] - as all of these means fall on the left side of the global distribution. Both American means are thus, like all the other subsets of the study, well removed from the origin. They are also well removed from most of the other group means (with the exception of the Papuan/Australian and Ainu/Siberian plots), which all occur in either the top right or lower right quadrants.

The lower and upper 95% confidence limits of the American cultural assemblages allow no further doubt as to their distinctive nature. When the confidence limits of the North American distribution **(FIGURE 8.11)** are calculated, the majority of assemblages yield an even more restricted distribution than the continental plot - with the points [-0.21,-0.0] and [-0.09,0.10] articulating a narrow territory well to the left of the distribution, in the lower part of the top left quadrant. When the same statistics are calculated for the South American assemblages, the internal distribution of **(FIGURE 8.12)** proves to be more nonrandom again than the overall range. The South American mean, at [-0.11,-0.07], falls well into the lower left quadrant, indicating that, on average, there is a thematic resemblance between these cultural assemblages and the Papuan/Australian assemblages **(back to FIGURE 8.10)**, in addition to the general resemblance to North America. So consistently ldo the majority of South American assemblages fall into the lower left quadrant, that even the upper 95% confidence limit of the distribution [-0.03,-0.01] falls into this quadrant, as must, therefore, the lower, at [-0.18,-0.13].

The restricted morphology of the American assemblages is all the more remarkable when the wide range of environmental conditions on these two continents is considered **(refer back to FIGURES 7.14 and 7.15)**. All the basic environmental types of Africa and Europe, where the cultural assemblages were so different, are present, and yet these conditions are quite unable to override the peculiar heritage constraints of Amerindian societies.

8.5 Further Discussion

A global cross-cultural sample has thus been compiled and subjected to the same series of mathematical analyses as the non-cultural data of Chapters 6 and 7. The geography of cultural assemblages displayed a remarkably similar general pattern to the cranial data of Chapter 6, and this kind of result may be summarised in terms of idealised "football" shaped clusters, as in the final illustration of that chapter. Cultural variation clearly conforms to the idealised "neo-Darwinian" hypothesis defined in Chapter 6 with reference to cranial data **(FIGURE 6.31)**. On the other hand, both the "Lamarckian" and "Random Evolution" null hypotheses of Chapter 7 **(FIGURES 7.22 and 7.23)** seem to be refuted, as neither of these two patterns emerged in the cultural data.

It should be noted that the neo-Darwinian patterns in cultural assemblages simultaneously refute sociobiology, and leave Cultural Selectionist perspective unrefuted: it refutes one Darwinism, and paves the way for another. Sociobiology is refuted due to the fact that in the approach, as shown in Chapter 3 (see Sections 3.5, 3.6, and 3.7), cultural assemblages are seen to plastically reflect human adaptive necessities - as "man's extrasomatic means of adaptation". In other words, to take up the notion that the only order to be found in history is that imposed upon it by the human genome, or, for that matter, that that order is imposed upon it by anything external to the cultural inheritance system itself, is to take up a Lamarckian position. To propose that culturally reproduced phenomena merely exist for human benefit is non-Darwinian, without serving their own benefit, is to violate the basic Darwinian [Darwin 1872 (1859)] and neo-Darwinian (Dawkins 1989) principle of selfishness (Cullen 1990). As a consequence, the Sociobiological approach to culture does not predict a heritage constraint pattern to appear in cultural data, since human needs, unlike human crania (broadly speaking), are defined by things external to human genes, and are not, therefore, constrained by human phylogenetic heritage. A cultural system which simply reflected human needs would, correspondingly, display a *geography* which simply reflected the *geography* of those needs. General qualities of ecosystems,

such as the mere presence of absence of resources for food (of *some* kind) and shelter (of *some* kind), are not a function of phylogenetic heritage.

It may also be noted that cultural diversity **(FIGURES 8.03 –8.12)** is not "synchronised with", or "codetermined by", human genetic diversity, as it might be in the obsolete, genetic determinist sociobiological framework for culture, or as the behaviour of an animal without any culture at all would be. Rather, cultural assemblages appear to vary as though they are an independent selective system; as though cultural assemblages consist of a plethora of separate but sympatric species of different socially transmitted phenomena, all of which are genealogically independent from the human genome. Both plots display the general pattern of heritage constraint, and are equally selective, but the pattern is not the same in its specifics. In other words, it is clear that the overall plot of human crania **(FIGURE 6.14)** and its internal structure **(FIGURES 6.15 onwards)** is no means the same pattern as that displayed in the cultural data, either in the overall plot **(FIGURE 8.02)** or its internal structure **(FIGURES 8.03 onwards)**, even if we rotate one of the plots about the origin in an attempt to find a position in which it may be perfectly imposed upon the other. There is no such position. For example, if we follow the larger subsets in the cultural data in a circle, starting with the African societies, the clockwise sequence (with overlap) appears as follows: African **(FIGURES 8.03, 8.04)**; European **(FIGURES 8.05, 8.06)**; Asian **(FIGURE 8.07)**; Malayo-Polynesian **(FIGURE 8.09)**; Australian and Papuan **(FIGURE 8.10)**; South American **(FIGURE 8.12)**; and North American **(FIGURE 8.11)**, and then to Africa again. If we follow the cranial data in a clockwise direction, starting with African crania, the sequence appears to be something like this: African **(FIGURE 6.15)**; Australian and Papuan **(FIGURE 6.19)**; Malayo-Polynesian **(FIGURE 6.18)**; North American **(FIGURE 6.20)**; South American **(FIGURE 6.21)**; Asian **(FIGURE 6.17)**; and European **(FIGURE 6.16)**, and then to Africa again. The two patterns are equally Darwinian (as shown in other sections), but they are not the same. In other words, it appears as though each set of phenomena are constrained by a pattern of phylogenetic or genealogical constraints, but that the two genealogies are independent of each other. This is entirely consistent with the correlation between human genetic and linguistic groups

found by Cavalli-Sforza (1991) and Cavalli-Sforza, Piazza, Menozzi, and Mountain (1988), which is far from perfect due to independent gene or language replacement. It is also consistent with the more generalised correlations between early hominid remains and certain artefacts described by Foley (1987), and see Chapter 10 for further discussion.

We may conclude that while cultural phenomena correlate with human populations, the correlation is very "temperamental". It appears that cultural phenomena correlate with human crania only after the manner of domesticate or commensal fauna might be expected to do so (such as dogs or mice), as an independent but equally neo-Darwinian system. Like cultural phenomena, such animals also covary with human bone morphology, due to the tendency of their gene pools to share similar corridors of (and barriers to) gene flow, as due to their dependence upon humans they have virtually sympatric distributions. Yet such organisms are, obviously, not reproduced via human genes, but via their own. To summarise this point another way, there may be distinctive "African dogs" and distinctive "African humans", and "European Dogs" and "European humans", which co-occur on their respective continents, but we know that this is not a result of the influence of human genes on dog phenotypes (or, for that matter, the influence of dog genes on human phenotypes). It is rather a corollary of the fact that human interaction takes place in a context of certain cultural and geographic constraints, and that these constraints have parallel effects upon the size and degree of isolation of both human and dog gene pools. Perhaps populations of cultural phenomena, then, are affected by much the same barriers and corridors to information exchange as are humans and their domesticates, and therefore tend to develop as a third class of neo-Darwinian phenomena, but as one which is as disarticulated from the first two as those two are from each other. Moreover, and for similar reasons, one would expect a large number of disarticulated lines of descent to occur within cultural assemblages (for example, pottery and metallurgy), just as multiple lines of descent would occur within a community of domesticates (for example, cows and sheep).

8.6 Concluding Remarks

The principal objective of this chapter was to provide a general indication of the degree to which cultural diversity displayed the distinctive pattern of heritage constraint, and it appears that this is indeed the case. The implication is that cultural process is primarily selective in nature, and that human innovation tends not to transcend a cultural tradition, or, at least, not on a grand scale. In other words, a cultural tradition would appear to place fundamental constraints either on the nature of innovations, and/or on the uptake of those innovations. It seems that new ideas are derived not from the contemplation of "necessity", but from old ideas.

It is also interesting to note that if anything the neo-Darwinian pattern is stronger in cultural assemblages than it is in "known" neo-Darwinian systems. Heritage constraint seems to be an even more dramatic phenomenon in cultural process than it is in genetic process, when the global geography of faunal assemblages is compared with that of cultural assemblages. This is precisely what would be expected to be the case if cultural evolution was every bit as selective as genetic evolution, only very much faster. Comparison of human crania demonstrates the heritage pattern intraspecifically; yet, given the rate of cultural evolution, in comparing cultural assemblages one may be comparing phenomena at the interspecific, intergenus, interfamily or even interphylum level.

One might then pose an interesting question: If cultural process is so perfectly and dramatically neo-Darwinian in its logistics, and yet so much faster and therefore more observable (on the scale of human perception), why was it not the first selective process to be discovered? Perhaps because the patterns in culture could always be explained away as some kind of "spin off" from human genetic variation. But as already outlined above, there is no need to evoke genetic constraint; intrinsic neo-Darwinian logistics within cultural process itself are able to explain the general correlation. Moreover, unlike the genetic constraint position, a cultural selectionist position can also explain the few instances of non-correlation (gene or culture replacement) which also

occur. Like the "Punctuated Equilibrium" explanation in Palaeontology (Eldredge and Gould 1972), Cultural Selectionism predicts the "gaps" in the genes/culture correlation as well as the correlation patterns that *are* there:

> "Why should genetic and linguistic evolution correspond so closely? The answer lies not in genetic determinism but in history: genes do not control language; rather the circumstances of birth determine the languages to which one is exposed. Linguistic differences may generate or reinforce genetic barriers between populations, but they are unlikely to be the leading cause of the correlation. Human evolution is punctuated by the splitting of populations into parts, some of which settle elsewhere. Each fragment evolves linguistic and genetic patterns that bear the marks of shared branching points. Hence, some correlation is inevitable." (Cavalli-Sforza 1991:77).

In any case, the pervasive occurrence of cultural heritage constraint makes one thing clear. Building a general theory of cultural change should involve some kind of appeal to notions of selective process. In other words, it would appear that Cultural Selectionism *of some kind* is required if a general theory of cultural change is to be fully developed.

The next question to ask is "What sort of Cultural Selectionism is most appropriate?" The next chapter addresses this question, and (building on the critique of the first five chapters) attempts to argue for a particular form of Cultural Selectionism termed the Cultural Virus Theory.

PART III

A PROVISIONAL OUTLINE

OF THE CULTURAL VIRUS THEORY

9

INTERDISCIPLINARY FOUNDATIONS OF A VIRUS PERSPECTIVE

Overview

Given the results of the previous chapter, it would appear that the explanation of cultural process will require a selective metaphysic of some kind. In the final two chapters of the thesis the potential of virus analogies to provide a selective framework is assessed. Firstly, in this chapter, a wide range of interdisciplinary foundations for the virus perspective are explored. No explicit definition of Cultural Virus Theory is attempted. Section 9.1 provides a general introduction to use of virus metaphors in popular discourse. Section 9.2 introduces principles of parasite ecology from modern evolutionary biology and outline some implications of these principles. Sections 9.3 and 9.4 summarise aspects of Edelman's Neural Darwinism. Finally, in Section 9.5, a brief outline is provided of how the Dawkins and Edelman arguments fit together. The main aim of the chapter is to introduce a series of elements from other disciplines which will be brought together to form a synthesis in the next chapter.

It is emphasized that Cultural Virus Theory itself, in the form of a precise and cultural definition of the nature of a viral phenomenon and larger entities composed thereof, is not outlined here in Chapter 9, but in Chapter 10, Section 10.1.

9.1 Virus Metaphors and Popular Discourse

The word virus is frequently used in modern discourse, and usually in either of two basic contexts. On one hand, there are a range of diseases

caused by microbial viruses which present some of the most frustrating challenges modern medicine has ever faced, from common colds to serious immune system deficiencies. On the other hand, we now have electronically transmitted computer viruses, designed by human programmers, which spread throughout a computer system and then cause parts of the information network to come crashing down.

Yet the concept which the word virus denotes easily encompasses both phenomena. Although the origin of the use of the word "virus" to describe certain kinds of pernicious computer programmes was presumably metaphorical, such electronically transmitted phenomena fit the specification of a virus remarkably well. Viruses can be broadly defined as phenomena which belong to a grey area in between organic and inorganic fields of enquiry, somewhere between the living and the dead. They are not living in the sense that organisms which breathe, metabolise, and reproduce, are living, since, while outside their hosts, they carry out none of these basic processes (although some virologists prefer to view viruses as living when inside their hosts, e.g. Slap 1991:668). Nor yet are they dead or truly inorganic in the sense of a pebble or a vial of helium, since unlike these phenomena a virus can undergo reproduction with the help of an agent such as a larger organism or a computer. Viruses are not elemental or natural products of inorganic forces, and would therefore cease to exist if there were no agents or hosts to reproduce them.

A virus can be alternatively defined as a freelance fragment which needs to "borrow" a body in order to reproduce. Unlike other predators a virus does not use organisms simply as fuel for its own autonomous metabolic processes (as it has none), but instead causes part of its host to manufacture copies of itself - it diverts an existing replicative operation to its own ends. RNA retroviruses, of which HIV is an example, are not capable of direct self reproduction, as they have to borrow the DNA and the DNA-instructed cellular machinery of other organisms (in an undigested state) in order to reproduce (Delius 1989:45; 1991:83). This reliance on the phenotypes of other organisms obviates the need for complex, life sustaining metabolic processes usually

associated with (and often considered definitive of) life. Due to this periodic borrowing of cellular machinery, they either never had to develop - or else have been able to dispense with - a range of structures which would be required in order to be more self sufficient.

Moreover, there is no need to assume that a viral entity will always be parasitic, although most modern usage of the term usually implies some pernicious effect. In fact, under many circumstances one might envisage that a viral entity would be best advised not to hinder, or perhaps even to help its host, and this may be the best way to ensure its own reproductive success. But the "benefit" conveyed may not be a reproductive one, and therefore not a genuine "Darwinian" benefit at all. For example, it may be in the interests of a parasite to help host survival rather than host reproduction. Certain parasites of snails (see below in Section 9.2) cause their hosts to divert energy normally channelled into producing offspring into thickening and strengthening their shells, thereby prolonging the life of their hosts, but at the expense of their reproductive success (Dawkins 1989:241). It is important that such parasites keep their hosts alive, but it is not in their interests to let the host waste energy in reproduction (again see Section 9.2 for expansion).

The great value of the generalised definition of a virus is that it provides an alternative avenue for the "Darwinization" of cultural fields of enquiry, as outlined more fully in the form of the notion of viral phenomena as defined in the first section of the next chapter. Anthropologists and archaeologists who wish to reconcile the humanities with neo-Darwinian evolutionary theory are no longer obliged to incorporate culturally reproduced phenomena into the human phenotype, that is, to consider cultural phenomena as parts, traits, or organs of the genetically emergent dimension of the human individual. Nor do we have to go to the other extreme (which Foley 1987:280; Levi-Strauss 1963:4; and Leonard and Jones 1987:215 have disparaged), and view artefacts as self reproducing organisms in their own right. The virus concept provides the archaeologist or, for that matter, any scholar of cultural evolution, with a way out of the quandary of having an uncomfortable seat

between modern evolutionary genetics and the superorganic view of culture. Cultural phenomena may be viewed as both totally biological, and totally extra-genetic; cultural fields of enquiry are thereby grounded in actual biological processes, and yet no disciplinary identity is lost (Cullen 1991:219). They are a class of psychobiological structures (and behavioural and artefactual products) which are reproduced independently from genes. Rather than speaking of microbial viruses with genes and phenotypic products, we are speaking of culturally transmitted viral phenomena, as memes with phenotypic products. In this sense, the two positions are neatly complementary, where each viral phenomenon consists of a group of memes and their combined phenotypic product (see Chapter 10, Section 10.8).

A viral perspective would be better described as an heuristic simplification of Cultural Selectionism, not as a radical departure from it. The shock value of the word "virus" forces the reader to envisage populations of artefacts or ideas as evolving in a Darwinian fashion, without the latent concomitant assumption where such phenomena were assumed to be human traits. The term "viral phenomenon" contains no explicit reference to Darwinism or Selectionism, enabling the concept to "slip through" the defences of a reader who has a pronounced prejudice against selective concepts as a result of their more frequent contextual association with Sociobiology. At the same time, a selective metaphysic of some kind is implied by the fact that viruses are normally explained in terms of selective frameworks. A term such as "viral phenomenon", unlike the term "trait", immediately evokes a mental picture of something that is independent from human genes, yet totally neo-Darwinian in and of itself. Thus the virus concept has heuristic value for cultural selectionists wishing to defend themselves against the occupational hazard of being lumped with cultural Sociobiology, despite the fact that Cultural Selectionism has a substantially different research agenda which is focused around cultural reproduced phenomena, rather than genetically reproduced phenomena.

Nonetheless, as will become clear, the concept also has some ontological value. Human behaviour and artefacts can be viewed as the phenotypic

products of a class of neuronal structures (sets of strengthened synapses) which are reproduced via the process of cultural transmission (which is defined in the most general sense as socially mediated learning). The word virus simultaneously excludes or attracts attention away from the old Darwinian discourse focused on the adaptation of populations of human individuals, toward a new discourse focused on the adaptation of populations of ideas, while also avoiding any suggestion that ideas are sophisticated organisms in their own right.

Culturally reproduced phenomena do not, therefore, require the complexity or autonomy we normally associate with the word "organism" in order to have their own unique adaptive history and genealogy, or to demand the status of a Darwinian individual, at least within the framework of Darwin's principles of adaptive selfishness (See also Chapter 3, Section 3.5; and Chapter 5, Section 5.4). Any phenomenon which is reproduced independently of the reproduction of its host, in other words which displays "genealogical independence", qualifies as an "individual" with regard to the principle of adaptive selfishness, and therefore must demand treatment as an independent lineage with its own idiosyncratic fitness criteria and separate adaptive polarity. "Adaptive polarity" means that what is adaptive to such phenomena may be maladaptive for the body in which it resides, while conversely what is maladaptive for such phenomena may be adaptive for the body in which it resides. In any case, as a consequence of the genealogical independence of (an otherwise highly dependent) cultural entity, it is quite possible to develop a genuinely Darwinian view of history, one based not on human virility or fecundity, but instead based on "cultural virility" or "fecundity". Propagation is still the key concept, but the general patterns of history are interpreted in terms of a cultural structure's ability to reproduce itself in new minds before the death of the mind or minds in which it is currently maintained.

The terms "virus" or "viral phenomenon" are also intended to capture the passive reproductive nature of phenomena such as artefacts, ideas, and rituals. Like viruses, none of these phenomena are capable of reproducing themselves. Yet the ideas in someone's mind, the recipes of various cookbooks

and cuisines, or the technical knowledge of how to make a hand axe need not be connected to who that person's parents were. An individual can acquire an idea or acquire a recipe from someone with whom they have no genetic relationship, in fact, from almost anyone that seems to be worthy of imitation, or who is willing to demonstrate and explain how to do something. So there is a kind of human behaviour that does not really "belong" to a particular family of genetically related people in the way that genetic traits such as eye colour or the shape of ears does.

The critical point however is that although the influenza virus that someone catches is not inherited from that individual's grandparents via his or her parents, the influenza virus itself has a very solid genealogy of its own. It is a copy of another virus that existed, say, in the body of another individual, a virus which in turn was a copy of a virus that this particular individual was exposed to on an international trip, and so on all the way back to the ancestor of all influenza viruses. And, like any viral phenomenon, ideas also have a genealogy of their own which is usually quite different from the genealogy of the person whose mind they occupy. Consider that behind the many different contemporary Marxist thinkers there is a long chain of other Marxist thinkers leading all the way back to Marx. Yet very few Marxists are genetically related to each other; the process of cultural transmission by means of which Marxism spreads is a result of a learning process which proceeds independently of human reproduction. Marx's ideas left his body via a very different route to his genes, at very different times and places, and consequently ended up, more often than not, in different people. Thus Marx's cultural descendants and his genetic descendants are two very different subsets of modern humanity.

Once cultural phenomena are included in the broader notion of "viral phenomena", the category encompass not only microbial viruses and computer viruses, but all computer programmes, all computers, all artefacts, and all other culturally transmitted phenomena such as scientific or religious concepts, behaviour patterns, rituals and political ideology. All these phenomena display genealogical independence from the human phenotype, and

as a result they are not necessarily obedient extrasomatic "organs". Unlike true "organs" and "traits", their reproduction is not guaranteed by the reproduction of their hosts. At best, if properly controlled, they can be considered as analogous to human domesticates, such as pets or livestock, which have separate genealogies from their human companions, but are entrenched in a mutually beneficial "symbiotic" relationship (see Delius 1989:48 and 1991:85, and Heylighen 1992a for parallel arguments in the meme discourse). In fact, all cultural phenomena are domestic in the sense that they are dependent upon humans for reproductive success. Yet neither benevolent nor malevolent viral phenomena need present any difficulty to a selectionist world view. To survive, no idea or practise need enhance the reproductive fitness of anything but itself. And even the most helpful or reassuring ideas and rituals, such as a belief in life after death or the morning mass, cannot possibly continue to help human communities unless those communities "reward" them with the ultimate of Darwinian rewards, reproductive success. In other words, even symbiotic or beneficial ideas and artefacts must be passed on to new hosts if they are not to cease to exist with the death of the people who currently maintain them.

As shown in previous chapters, other attempts to reconcile selectionist thought with cultural fields of enquiry have each contained a serious logical flaw which "deconstructed" their attempts to develop a genuinely selectionist view of cultural process. Social "Darwinism" was simply shown to be social Lamarckism in disguise; the flaw in this attempt was a misunderstanding of the nature of selective process. Sociobiology's flaw was less obvious. Its grasp of the nature of selective process was not the limiting factor; the fundamental flaw lay in sociobiology's conception of cultural phenomena as "organs" rather than as suborganismic parasites and symbiotes (guests), despite the fact that cultural phenomena undoubtably display genealogical independence from the human body. Through mistaking a complicated ecological assemblage (i.e. a human individual) for a Darwinian individual, cultural phenomena were viewed as traits of the human phenotype, and viewed as though they were ordered by reproductive events which occurred outside cultural lines of inheritance. The explanatory framework thereby violates the basic Darwinian principle of

reproductive selfishness, and thereby degenerates into Lamarckism. Thus the two alternative evolutionary approaches to Cultural Selectionism, Cultural Evolutionism and cultural Sociobiology, can be dismissed as being explicitly Lamarckian and inadvertently Lamarckian respectively.

As shown in Chapter 5, the Phenotypic form of Cultural Selectionism retains Sociobiological definitions of individual, population, phenotype, environment, and adaptation. But the general notion of viral phenomena opens up the possibility of a fourth approach which could reconcile the modern humanities with modern selectionist notions of process. The virus notion of cultural phenomena allows a definition of cultural fields of enquiry as unique, and as lying beyond the boundaries of established biological discourse in certain important ways, but potentially classifiable as a unique branch of biology. The approach thoroughly deconstructs the nature/nurture, biology/culture, and body/mind distinctions. However, one distinction is judged to be very important indeed. This is the distinction between modes of transmission (genetic versus cultural or social). Thus in the virus account of culture (within humanity) "all is nature but not all is nurture", "all is biology, but not all culture", and "all is body, but not all is mind". In other words, cultural phenomena are defined as perfectly biological, in the sense that they are a life form (although not actively self reproducing through the medium of DNA). This notion will not be more explicitly developed until the next chapter.

Microbial parasite and symbiote analogies are have been discussed *vis a vis* the Meme concept by those such as Ball (1984), Delius (1989, 1991), Heylighen (1992a), and Moritz (1990) among others. Notions of the parasitic qualities of cultural phenomena were appearing quite widely in popular and anthropological discourse in the 1970s and 1980s. Elsewhere (Cullen 1990:64) a number of studies which consider the possibility of cultural phenomena having either viral or meme-like qualities - in addition to the above mentioned Dawkins (1976:207) - have been listed; these include Ball (1984 *passim*); Cloak (1975:172); and Delius (1989 *passim*). Heylighen (1992a) and Moritz (1990) may be added to this list. There is also the "co-evolutionary" concept which implies the potential for some kind of parasitic quality, as in Durham (1990,

1991), and (less explicitly parasitic) Freeman (1983). Secondly, there are references to "less viral" concepts of parasitism or negativity (again see Cullen 1990:64) - uses of words like "disease", "maladaptive", "susceptibility", or "epidemic" with reference to cultural phenomena, such as those notions used by Cavalli-Sforza and Feldman (1981:342) Fletcher (1977:46); Richerson and Boyd (1985:83); and Sperber (1985). Use of the virus metaphor even occurs in the context of implicitly arguing *against* a Darwinian approach to culture (Deleuze and Guattari 1992[1987]:10). In much of this literature cultural phenomena with pernicious qualities are viewed as a class of maladaptive traits. Moreover, Delius lists a wide range of popular references to such notions in general discourse in an epigraph to a paper:

> "Colleague at Bochum University: "You have set a flea in my ear". Discjockey on German radio: "The song is a proper earwig". Report in an English newspaper: "The idea was obviously infectious". Passenger on an international flight: "What a contagious, nonsensical habit!". American performance artist: "Language is a virus"."
>
> (Delius 1989:26)

The recording of the performance artist referred to by Delius (Laurie Anderson "Language is a Virus") was released in 1986. Given the completely different terminology used in most of the above mentioned studies, the tendency toward a lack of inter-referencing, and the wide ranging occurrence of "viralesque" terminology in non-academic contexts indicated by the Delius epigraph, it would appear that, in fact, independent invention has been more the rule than the exception in the origin of these ideas. It should therefore come as no surprise that the Meme and Virus positions, were conceived and developed independently of each other. There may well be others.

9.2 Foundation Arguments from Parasite Ecology

The first response of a "traits" or Phenotypic cultural selectionist (or, for that matter, a sociobiologist) to the virus argument is usually based not on an objection to the basic viability of the analogy, but on doubt as to whether the analogy has any heuristic value which would make it superior to the more established Darwinian positions. David Rindos (*pers. comm.* 1988), William Irons (*pers. comm.* 1990), and Robert Boyd (*pers. comm.* 1992) for example, have all expressed this kind of reservation. Their objection would be expressed something like this: we begin to see how cultural phenomena may be viewed as viral phenomena without logical absurdity, but what explanatory advantages does the position offer?

If we already accept that the host/virus analogy for humans and their associated culturally transmitted structures is viable, then the challenge to a virus perspective becomes that of demonstrating the potential and value of the perspective. Any demonstration of heuristic superiority depends upon finding some fundamental differences between the logistics of evolutionary process in populations of "individuals" which represent "single" phenotypes (hosts or parasites/symbiotes), and populations of individuals each of which represents a "multiple" phenotype (an host/parasite combination, or "host/guest" relationship in terms used by Delius 1989:48).

A population consisting of individual structures constituted from more than one phenotype really consists of two or more populations occupying the same space, unless all those structures of which any given individual is constituted reproduce synchronically. In fact an individual is properly defined as any group of structures which are ultimately reproduced in a single

reproductive event. In the argument developed below, definitions of individuality and notions of "a phenotype" are shown to be crucial to selectionist explanation. A virus perspective provides the basis for a more appropriate model of "individuality" for cultural process than the implicit "human being" unit of Sociobiology and the implicit or partially declared "human being" unit of Cultural Selectionism. The key to this argument lies in the special issues of host/guest evolution.

In response to the concern about demonstrating the heuristic superiority of a virus perspective over one based around traits, a general discussion of host/guest ecological relationships will now be used to develop an argument for the necessity of the viral independence approach to cultural process. An appeal will be made to several summaries of host/parasite conflicts of interest presented by Dawkins in the final chapter of the new edition of *The Selfish Gene* (1989). In this edition, another chapter has been added in which the basic arguments of another book, *The Extended Phenotype* (1982), are summarised. Dawkins examines the conditions under which host/guest relationships demand complete cooperation, and conversely, those conditions under which host/guest relationships are likely to produce a situation of conflict. In indirect opposition to some statements from Durham (1982:304), but ultimately in partial accordance with other statements of this study (see Durham 1982:309; and below in this chapter), I will argue that the human/culturally transmitted entity relationship is one where the potential for host/guest conflict arises. This argument involves the extrapolation of Dawkins's discussion of the extended phenotype from the genetic context to the cultural one.

In his notion of the "extended phenotype", Dawkins explores how genes can have a variety of effects on bodies other than that in which they reside. Just like ordinary phenotypic effects, such factors can either aid or impede a gene's or group of gene's chances of successful reproduction. In other words, variation in the extended phenotype can provide raw material for natural selection, and a given trajectory in a population of organisms produced by changes in gene frequencies may well be due to effects the favoured genes have on some other group of organisms altogether. Genetic variation for this characteristic may be completely invisible in the phenotypes of the bodies they produce, and manifest only as a "distortion" of the phenotypes of another species. The particular interest that the evolutionary genetics of host/guest relations has for the "human individual versus virus" issue begins with the fact that genes may have extended phenotypic effects in bodies other than the one in which they reside. This is because part of a guest's extended phenotype includes those effects it has on the phenotype of its host, and part of the host extended phenotype includes all the influences its genes may have upon the phenotypes of its guests.

The form any particular body takes is not merely the outcome of the information stored in its particular genotype interacting with the inorganic environment, but also influenced by other genes in other genotypes via the structures and molecules that those other genotypes produce. Given that the interests of both guest and host can involve a body taking a particular form, it might be interesting to pose the question: "Do these interests always coincide, and if not, when are they likely to diverge?". Quite possibly, the form a body takes may in fact be a convoluted mixture of quite separate genetic influences, each based on somewhat different reproductive interests.

Dawkins, in three examples which will be summarised briefly below, shows how the best interests of guest and host may often subtly or dramatically diverge, and that this conflict of interest may well be resolved in favour of the guest rather than the host, or vice versa, rather than always manifesting itself as a symbiotic compromise.

The first example concerns certain kinds of snail which are parasitised by certain flukes (flatworms). One of the effects the guest have on the snail is to thicken its shell. This thicker shell has the effect of making the snail more resistant to shell damage (presumably), and so we have a situation where (apparently) it is actually advantageous for a snail to be infected with flatworms. Dawkins (1989:241), however, points out that this begs the question of why (given that shells of greater thickness are obviously possible) do snails not normally grow shells of this thickness? His answer lies in the economic cost of shell production to the owner:

> "Making a shell is costly for the snail. It requires energy. It requires calcium and other chemicals that have to be extracted from hard won food. All these resources, if they were not spent on making shell substance, could be spent on something else such as making more offspring. A snail that spends lots of resources on making an extra-thick shell has bought safety for its own body. But at what cost? It may live longer, but it will be less successful at reproducing and may fail to pass on its genes. Among the genes that fail to be passed will be the genes for making extra thick shells. In other words, it is possible for a shell to be too thick as well as too thin." (Dawkins 1989:241).

The fundamental issue is whose physical resources are being used to manufacture the extra shell thickness. If the fluke uses its own metabolic processes and energy resources to add to the thickness of the snail's shell, then all is fair and square. The snail enjoys the added safety of a thicker shell, and is in a position to translate its longer life into increased reproductive success. Each fluke is in a position to do likewise, while enjoying the security of a longer living host. If, however, each fluke is able to exert subtle chemical influences on the snail, such that the host is forced to use its

own valuable energy resources to lengthen its life at the expense of its reproductive success, then the guests become the sole beneficiaries of the transaction. The chemical which the guests exude constitutes an aspect of their extended phenotype which allows them to be more reproductively successful than other flukes who cannot influence their hosts in a similar way. Fluke genes with shell thickening extended phenotypes (ie. genes with the potential to collaborate with certain fluke genotypes in such a way as to produce an organism which secretes the appropriate chemical) have undergone differential reproduction in the respective fluke population due to their ability to divert host energy away from host reproduction toward their own. Dawkins sums up the situation as follows:

> "Both snail genes and fluke genes stand to gain from the snail's bodily survival, all other things being equal. But survival is not the same thing as reproduction and there is likely to be a trade off. Whereas snail genes stand to gain from the snail's reproduction, fluke genes don't. This is because any given fluke has no particular expectation that its genes will be housed in its present host's offspring. They might be, but so might those of any of its fluke rivals. Given that snail longevity has to be bought at the cost of some loss in the snail's reproductive success, the fluke genes are "happy" to make the snail pay that cost, since they have no interest in the snail's reproducing itself. The snail genes are not happy to pay that cost, since their long terms future depends upon the snail reproducing. So, I suggest that fluke genes exert an influence on the shell-secreting cells of the snail, an influence that benefits themselves but is costly to the snail's genes. This theory is testable, though it hasn't been tested yet." (Dawkins 1989:242).

In the second example of host/guest reproductive conflict (where two genetic interests are competing for influence of one body), guests exert an influence over the development of their hosts. A species of single celled animal (protozoan) called *Nosema* lives in the bodies of flour beetle larvae, and has the capacity to control the timing of the onset of maturity in its host by exuding its own copies of one of the beetle's hormones. This particular hormone, called "juvenile" hormone, is secreted in the body of a juvenile insect and has the effect of maintaining the organism in its larval state. Pupation, and ultimately adulthood, are later stages brought about by the cessation of "juvenile" hormone production in the larva's body. The *Nosema* parasites are able to prevent the onset of adulthood by secreting a close chemical analogue of the "juvenile" hormone, such that the larva continues to

grow way beyond the normal size at which the larval/adult transition occurs. As the larva is not, by definition, sexually mature, it cannot reproduce, and therefore spends all its time eating and growing, supporting its strange assemblage of guests.

The final example which Dawkins uses is that which involves the exploitation of crabs by *Sacculina*. In an extraordinary example of host control, a certain kind of parasite similar to a barnacle is actually able to castrate its crab host, enabling it to continue to thrive but preventing it from translating its stored energy resources into reproductive success. Dawkins describes the situation with vivid imagery:

> "*Sacculina* is related to barnacles, though you would think, to look at it, that it was a parasitic plant. It drives an elaborate root system into the tissues of the unfortunate crab, and sucks nourishment from its body. It is probably no accident that among the first organs that it attacks are the crabs testicles or ovaries; it spares the organs the crab needs to survive - as opposed to reproduce - till later. The crab is effectively castrated by the parasite. Like a fattened bullock, the castrated crab diverts energy and resources away from reproduction and into its own body - rich pickings for the parasite at the expense of the crabs reproduction." (Dawkins 1989:243).

These examples lead Dawkins to a definitive statement about the conditions under which the development of host/guest conflicts in reproductive success become likely:

> "I suggest that the most important question to ask about any parasite is this. Are its genes transmitted to future generations via the same vehicles as the host's genes? If they are not, I would expect it to damage the host, in one way or another. But if they are, the parasite will do all it can to help the host, not only to survive but to reproduce. Over evolutionary time it will cease to be a parasite, will cooperate with the host, and may eventually merge into the host's tissues and become unrecognizable as a parasite at all." (Dawkins 1989:243).

To illustrate this dynamic, Dawkins adds a final example in which host and parasite genes do use a common exit, and in which reproductive and therefore adaptive interests essentially coincide. Wood-boring ambrosia beetles harbour bacterial parasites which, unlike the parasites of the previous examples, actually use the beetle eggs as their means of transport to a new host. In other words, their interhost transmission takes place in the body of

the current host during the gamete production process, and all new hosts infected by the bacteria must therefore be direct descendants of the old host. As an evolving entity the beetle/bacteria combination becomes practically indistinguishable from the relationship beetles and other organisms sustain with intracellular structures such as ribosomes which possess their own genetic material which reproduces independently of human chromosomes. Providing that parasite and host use the same vehicles for reproduction, what is adaptive for the parasite and what is adaptive for the host should amount to the same thing. As Dawkins puts it:

> "The two sets of genes can be expected to "pull together" for just the same reasons as all the genes of one individual organism normally pull together. It is irrelevant that some of them happen to be "beetle genes", while others happen to be "bacterial genes". Both sets of genes are "interested" in beetle survival and the propagation of beetle eggs, because both "see" beetle eggs as their passport to the future." (Dawkins 1989:244).

In fact, the two parties do a lot more than just pull together. The beetles are haplodiploid; fertilized eggs develop into females while unfertilized eggs develop into males. However, unfertilized eggs are incapable of developing into male beetles without the intervention of the bacteria. Without the help of their bacterial parasites, these beetles would be incapable of sexual recombination.

We can take Dawkins's rule of parasite ecology and apply it to culturally transmitted phenomena. Clearly cultural reproduction is not synchronised with human reproduction (cultural transmission does not occur exclusively between parents and children), and we can, therefore, expect conflicts of adaptive interest to emerge between artefacts and the people who reproduce them. I will briefly sketch out the cultural implications of this particular foundation aspect of a virus perspective through a brief discussion of the example of the behaviour of a workaholic business employee (Cullen 1992b), although a more complete discussion will be offered in Chapter 10, Sections 10.2 and 10.3. Let us say the whole act of working nine to five, having tea breaks and half an hour for lunch, working late, and wining and dining clients and colleagues at dinner is all a daily ritual which the woman or man concerned replicates each day. Let us say that this person has a divorced spouse and two children in their early twenties living on another continent. Finally, let us say that this

person is also actively involved in developing the skills of a number of junior partners, and is transferring to them the elements of a disciplined daily regime.

From the point of view of classical sociobiology, the actions of the workaholic would be interpreted as the maximisation of genetic reproductive success. The workaholic pursues economic, emotional, aesthetic and social rewards, which he or she then translates into alimony payments, emotional support, and advice for his or her children, being careful to put a little aside for the possibility of attracting future suitors, having more children, and building up a reasonable inheritance for the family in the event of the person's death. There is an optimum trade-off between the necessity of continuing to live, and the necessity of providing for one's past and future children, just as in snails there is an optimum trade off between shell quality and numbers of young snails. Strictly speaking, individuals in a business should attempt to manipulate their investment in the workplace in order to increase their chances both of producing genetic offspring, and providing resources, not for only for their children, but for other family members also. In some circumstances, the only way to do this will be through working for the general good of the company, so that all employees and especially the partners benefit from general productivity and increased profits. Under other circumstances, a worker's family may be benefited through more dishonest practices, but the temptation of these must be weighed up against other factors, such as the possibility for self-criticism at a later date via "morals" or "one's conscience", an assessment of the physical or emotional attributes required to successfully carry out a particular strategy, the threat of public humiliation, prosecution, and so on.

Human individuals are able to use their intelligence to understand and weigh up the value of these complicated alternatives (however fallibly), although they will almost certainly not think about their lives in these terms. The general information processing capacities of the brain must be basically adaptive, or such qualities could never have evolved so comprehensively in the first place (Mithen 1989, Rindos 1986b). But as non-reductionists such as

Dawkins have so carefully pointed out time and time again (despite unfair accusations of being both determinist and reductionist), genes can only *create* organisms, they cannot *determine or control* them (Dawkins 1989:20, 271). Once created, the human capacity for culture, like any other organismic structure, may then be "hijacked" by potentially parasitic guest structures which colonise it after it has begun to develop. Yet, the end result of these cultural capacities must still be a net gain, or individuals with no learning capacities would begin to outreproduce other individuals.

Nonetheless, from the point of view of a virus perspective, the whole adaptive equation becomes more complex. The information processing system, which evolved due to the general advantages it conveyed to its bearers, is by no means perfect. The worhaholic's capacity to translate economic rewards into children can be seriously compromised by his or her fascination with the daily ritual of the work experience, as work rituals of different kinds persist in companies longer than any one employee precisely because of their capacity to sustain appeal amongst their participants. Knowledge of and commitment to this daily ritual constitutes a cultural phenomenon distributed in bits and pieces throughout the minds of the people in the company. Some daily activities tend to be far more appealing than others, due to the degree of satisfaction they offer, the degree of escape from stress, and so on. Everyday we spend in the workplace we encounter rituals with a survival pedigree; they are still around precisely because no employee or employer has been able to prevent their cultural transmission from senior employee to junior employee over the decades.

Whether the person concerned views themselves as working because "they enjoy it", in order to "gain experience", or "make money", is not the issue. The main issue is that only two general classes of phenomena have the potential to live on after the individual concerned has died; genes and memes. The question becomes one of attempting to estimate whether the individual's genes, or cultural phenomena, are benefiting from the individual's behaviour. It is possible to imagine a course of behaviour which would lead toward maximum numbers of genetic offspring; conversely, at the other extreme, one

can imagine a course of behaviour which would lead to maximum "teaching" of certain cultural phenomena and their corresponding behaviour patterns. Most human behaviour presumably lies somewhere in between these two extremes. Cultural selection will favour work rituals which help the worker to survive, but which have the effect of distracting the attention of this increasingly successful individual away from his or her family, and toward the workplace. In other words, a lethal or highly dangerous yet exciting work practise will tend to have a shortened lifespan, unless it can spread very rapidly indeed. Generally, a meme can be far more successful if it actually *promotes* health, but reduces family interests.

So the work ritual may well tip the workaholic's behaviour in favour of its own cultural reproduction, in the same way as the liver fluke influences the snail to build its shell stronger than it ought. Of course, the workaholic is praised for contributing to company productivity, and may receive financial bonuses and a plump expenses account. In short, the workaholic's life is enhanced and perhaps extended, as was the snails. However, from a Darwinian point of view, the work ritual itself may, under certain circumstances, be deriving a great deal more reproductive benefit from the workaholic's actions than are his or her genes. The energy channelled into the teaching of new employees and helping existing employees, into maintaining and replicating the rituals and activities of the company, may come to vastly outweigh the amount of energy the workaholic channels into children or family. Far from being arranged around genetic reproduction, the workaholic's life is in fact arranged around cultural reproduction. He or she has, so to speak, fallen prey to a viral neuronal structure, and one which has the hypnotic effect of channelling human energies into its replication.

The workaholic example was included at this early stage only to outline the general direction that a cultural equivalent of this general rule would take. The basic rule may be summarised as follows: parasite/host combinations = many lines of inheritance = adaptive conflicts. The converse rule would be this: single organism = single (or double, in the case of sexual reproduction) line of inheritance = adaptive co-operation. All the reader need conclude from

this is that cultural transmission = many lines of inheritance, and that "human individuals" are therefore really mini ecological assemblages, and host/parasite or host/guest combinations, with adaptive conflicts.

However, the translation of such rules into a cultural context in the next chapter will require additional foundation arguments which demonstrate how cultural reproduced information is equivalent to DNA in neo-Darwinian terms. In the next two sections, an attempt is made to outline some recent developments in psychobiology which facilitate the conception of cultural phenomena as physical structures in nervous systems which provide templates for imitation and instruction, processes which in turn bring about the reproduction of such structures in new nervous systems. These advances provide background assumptions which strengthen the virus perspective.

9.3 Foundation Arguments from Edelman's Psychobiology

Recent advances in the study of the psychobiology of the brain indicate that it is valid to speak of learned "structures" as material realities (Dawkins 1989:323; Delius 1989:43-47, Edelman 1989:300-303). Edelman has extended this materialist argument to include not only learned information, but consciousness also. It is important to note in advance at this point that recent well publicised "critiques" of Edelman by Crick (Crick 1994:234, 282, 284; as quoted in Turney 1994; see also Horgan 1994:73) do not attack or even address the aspects of Edelman's ideas discussed below. Horgan (1994) reports that "Crick has accused Edelman of dressing up ideas that are not terribly original in an idiosyncratic and obscure jargon". In fact, Edelman is mentioned in only three places in Crick's book:

> "In neural terms this might imply that reentrant pathways...are essential, as Gerald Edelman has suggested." (Crick 1994:234).
> "Edelman is a molecular biologist who now works on developmental biology and theoretical models of the brain...Some of Edelman's stories, while very familiar to his friends, do not sit so well on the printed page." (Crick 1994:282).
> "He is rather overfond of his own concepts (such as the theory of neuronal group selection, neural Darwinism, and reentrant

> loops). An enthusiast, noted more for his exuberance than for his clarity." (Crick 1994:284).

It can be seen from these quotes, which exhaust all the references to Edelman in Crick's book, make no specific attack on Edelman's logic or the actual structure of his arguments whatsoever. Such "critiques" do not, therefore, undermine the use of Edelman's concepts in the context in which they will be used below, namely to support the notion that culturally reproduced information can be envisaged as actual biological structures existing in the brain and nervous system.

If we employ this assumption, a meme is no less a material structure than is a gene, and can be described as a group of synapses which have passed from a "cold" state to a "hot" state (Delius 1989:43), a definition which will be expanded upon below. Such patterns form units of highly variable size, and so the word "meme", like the words for "big" or "old", has no fixed size, much like the word "gene" in biology (Dawkins 1989:33). Indeed, there is a whole range of brain structures which can be related to the whole range of corresponding structures in DNA, all the way from "nucleotide", "cistron", "chromosome", "crossing over", "sexual recombination", and "genome", although there is insufficient space to discuss cultural analogues of all of these. As Delius has remarked (1989:70), and as supported by the early chapters of this study, analogies between biological (I prefer the word "genetic", as "biological" implies that "cultural" is non-biological) and cultural evolution have been explored for a long time. However, the cultural virus position is a particularly specific analogy, as the microscopic structure of the brain displays a range of structures and processes which support the notion of indirectly replicating viral neuronal patterns.

Edelman has shown that both brains and immune systems carry out selective processes in somatic time, and indeed the immune system can sometimes be employed as a simplified analogy for Edelman's models of brain development (Levine 1988). The immune system, like any selective system, is unable to predict the pathogens which an organism will encounter in its lifetime. But it has evolved a system of development where prediction is not

always necessary. Although a whole range of common antibodies are always produced during immunodevelopment, an extremely broad range of other antibodies are also produced in small quantities, with the result being that whatever diseases are encountered, some antibody will usually be found. Once any particular antibody turns out to "match" a new invader, it is thereby "selected", and the cells of this type undergo differential reproductive success within the immune system, at the expense of other kinds of cells. The key to successful immunity, then, is not prediction, but diversity. The greater the range of antibodies produced, the greater the chance that one or more of them will be able to combat a particular pathogen.

The same rules apply to the ontogeny of neural networks. During the development of the brain, an immense range of neuronal groups are produced, forming what Edelman calls the *Primary Repertoire*:

> "We may define a primary repertoire as a diverse collection of neuronal groups whose extrinsic connectivities and potential functions in given brain region are prespecified to some extent during ontogeny and development. Without further qualifications at this point, we shall assume that, if a neuronal group responds with characteristic electrical output and chemical change more or less specifically to an input consisting of a particular spatiotemporal configuration of signals, there is a "match" between that group and the signal configuration." (Edelman 1989:47).

The range of neuronal groups in this basic repertoire is truly immense, and some estimate of this space can be gained from the possible existence of 10^{15} variable synapses (Delius 1989:44). This means that the psychocultural equivalent of what Dawkins has termed "genetic space" (Shennan 1989:340 after Dawkins 1986; meaning the full range of organismic structures which DNA is capable of coding for), a concept which might be termed the "neuronal group space", "memetic space" or "synaptic space", is, like "genetic space", unimaginably large. It also appears that at some levels the variation in this primary repertoire is completely random, and that its formation is something like a simulation of the history of life on earth in somatic time, only in the absence of the selective elimination afforded by Natural Selection. In other words, the developing neuronal configurations simply "mutate for all they are worth", creating many different neuronal structures of unknown potential. The

object of brain development is to yield as much variation as possible, in preparation for a largely unpredictable world. In other words, an immense range of potential memes and memetic motor products are formed during development, but they are not subjected to selective influences until the organism begins to experience sensory signals of various kinds. The brain functions in somatic time in a manner which is no less Darwinian than the manner in which the earth's biomass evolves in geological time, except that the processes of mutation and selection are fundamentally separated in time. Brain development is like an hypothetical planetary biomass which is able to go on replicating and mutating for a long period of time without any selective constraints, such that when selective criteria finally do impinge on the variability produced, there will be enough diversity for a large number of forms to "match" as many niches as possible.

While genetic activity places a range of constraints on brain development, and these constraints vary from species to species, within such parameters the specific structures finally attained are unpredictable and apparently unique to each individual. Moreover, it appears that there is no precise point-to-point wiring within the various localities of neuronal maps (Edelman 1989:34-39).

One of the basic characteristics of the primary repertoire of neuronal groups, this vast array of preformed symbols awaiting sensory experience, is *degeneracy*. This is Edelman's term for the fact that more than one neuronal group is likely to yield a match with a given stimulus, and each symbol is capable of yielding a match with more than one stimulus. The word "redundant" is avoided, since two symbols or neuronal groups which excite a match with the same sensory phenomenon are usually not the same themselves; indeed, they may be substantially different. Redundancy implies that there is more than one of the same thing, while degeneracy implies that there is more than one kind of thing capable of performing the same function. The difference between the two concepts can be illustrated using the analogy of twin births in animals; Edelman's concept of degeneracy is more consistent

with the phenomenon of *fraternal* twins, while redundancy fits the phenomenon of *identical* twins.

9.4 The Biological Nature of the Learning Process: Cultural DNA?

The degree to which the primary repertoire operates as the starting point for a selective system is surprisingly complete. It seems that for every level of the genetic code in DNA-based reproduction, there is an equivalent coding level in human psychobiology for cultural reproduction. We can start with the basic building blocks of DNA, nucleotides, and state that the corresponding level of culturally transmitted structure is the synapse. Both of these entities contain little information in and of themselves, as they can only be in one of two states. A given nucleotide may either be present or absent, while a given synapse may be either "on" (that is, strengthened so as to facilitate the passage of an electrical signal), or "off". Nucleotides come in only four basic types (adenine, thymine, cystosine, and guanine). At any locus along a DNA strand, one of the four is present and the rest are absent; genetic diversity is essentially a function of "what order" this small number of different nucleotides are arranged into. Diversity does not have much to do with the qualitative presence or absence of particular nucleotide bases in a DNA strand, but it has everything to do with the order in which these presences and corresponding absences are arranged. Unlike the computer memory bits, and like nucleotides, synapses also appear to occur in more than one basic type, as there is considerable variation in the structural constituents of neurons, such as in their intracellular structures, axoplasmic flow, transmitter type, and membrane receptors (Edelman 1989:57). In other words, there is quite a number of different psychobiological "nucleotides" (synapse types), which can be present or absent in the neuronal equivalent of a nucleotide sequence. However, it would appear that neurons are not arranged in any sort of linear sequence, but in three dimensional, "bush" like patterns. Neurons with different qualities yield different kinds of "synapses"

(points of connection between different neurons within a neuronal group). Synapses then become the basic information elements which are either switched on or off in a particular symbol configuration.

We can then move to the levels above that of the single nucleotide base in DNA, which is the level of a single "codon" (3 nucleotides) which codes for a single amino acid, while a certain number of codons then constitute a "cistron", a sequence of nucleotides which codes for a single protein, one enzyme, or one poly-peptide (see Chapter 5, Section 5.2). Cistrons have been called "genes" in a number of contexts (Poirier 1981:43), but as shown with reference to Dawkins (1989:33 again see Section 5.2, Chapter 5), this is a rather conservative definition, and it is better to define the "gene" concept as a much larger and highly variable generic unit of DNA, retaining the word "cistron" for the smaller, single protein coding units. A gene is best defined as a length of DNA of any size which is seldom divided, as discussed below. The relationship between the two categories is one of scale; a gene would normally encompass one or more cistrons.

Logically, then, the psychocultural equivalent of a codon or of a cistron should be the most basic level at which an assemblage of neurons can register what Edelman refers to as a "match" with some external stimulus (Edelman 1989:47). We are looking for some level of the brain where there are natural, or "objective" physical boundaries, actual physical punctuations or limitations in the symbolling capacity, as in the case of codons corresponding to single amino acids, or cistrons corresponding to single protein molecules. At the same time, we should hope, that like cistrons in relation to genes, the physical punctuations of these neuronal units would not necessarily define the physical boundaries of memes, such that a meme may include any number of these smaller units. A good candidate for a psychological equivalent of a "codon" or "cistron" would therefore appear to be one of Edelman's "neuronal groups" (It is very important that the reader reads Neuronal Group Selection as the selection of a neuronal-group, *not* as the "group selection" of populations of neurons, as if the theory were some kind of psychological equivalent of the Wynne-Edwards fallacious "group selection" argument). It appears that

Edelman's "neuronal group" is "informationally" or "logistically" similar to a codon or cistron, in that these, by reversing the comparison, could almost be called "nucleotide groups" instead of "neuronal groups". We might also parsimoniously define the neuronal group, like a cistron which may be referred to as the smallest kind of gene, as the smallest unit capable of functioning as a meme. Edelman defines a neuronal group as follows:

> "For the present, we may consider a neuronal group to be a collection of cells of similar or variant types, ranging in numbers from hundreds to thousands, that are closely connected in their intrinsic circuitry and whose mutual dynamic interaction may be further enhanced by increases in synaptic efficacy. The set of cells constituting a group can vary dynamically as a result of changes in input or output requirements or of competition with other groups. Such variations do not occur anatomically but rather are the result of alterations at either excitatory or inhibitory synapses." (Edelman 1989:47).

However, it would appear that one neuronal group on its own is capable of registering very little in terms of patterned signals from the outside world. The representation of categories in a certain spatial or temporal order appears to require more than one neuronal group responding in concert to different parts of the same object, separate representations which are then correlated at a higher level of brain function. Parallel signals may arrive at different localities (mapping) in the brain, each selecting a particular neuronal group, but each of these neuronal groups may then respond with characteristic outputs of signals to other localities and each other, a process which Edelman terms "reentry":

> "The constitution of a procedure or of a representation of categories in a spatiotemporal order occurs by mapping and reentry among parallel channels. While the *minimal* neuronal structure capable of carrying this out is a classification couple, in general, such couples do not act in isolation. Instead, they are embedded in large collections of parallel mapped channels, including reentry to and from nonmapped regions as well as to motor output systems." (Edelman 1989:319).

Thus it would appear that there are "natural" (but very small scale) punctuations in the symbolling capacity, such as neuronal groups and classification couples (or classification multiples, as the case may be), which, like codons and cistrons in DNA coding, provide a range of "building blocks" for incorporation into more complex representations. In DNA, codons and

cistrons are parts of genes. Correspondingly, in human psychobiology, the equivalent of a gene, in other words a meme, should preferably be composed of neuronal equivalents of codons and cistrons. Since the psychological or cultural equivalents of codons and cistrons have been defined as neuronal groups, the cultural equivalent of the gene should presumably be composed of these. In order to be incorporated into more global representations, neuronal groups must first be fixed in the secondary repertoire through selection. Neuronal groups are "selected" at the expense of other groups through synaptic modification. Edelman uses a population model of synapse modification, where whole sets of synapses are jointly modified, rather than viewing information in terms of single synapse changes:

> "In general, however, the complexity of the nervous system makes it highly unlikely that there is a simple relationship between changes at any individual synapse and changes in the behaviour of the network. It is much more likely that multiple synaptic modifications occur simultaneously at various sites in the network, reflecting its degeneracy. This prompts the consideration of synapses as populations. The importance of this consideration is obvious: modification of synaptic function serves as the principle means for the selection of neuronal groups and for their competitive interactions that lead to the formation of the secondary repertoire." (Edelman 1989:179).

A neuronal group (or groups) becomes a "meme" (conceived as a "replicatee" within CVT, rather than Dawkins's conception as a "replicator", see Section 10.1) when certain synapses are altered in such a way as to make their joint interaction more predictable and more likely. We may then define a meme as one or more neuronal groups, which, due to their internal structure and structural relationships to each other, bear a sufficient relationship to certain inputs or outputs such that they have been consistently and jointly excited by those inputs, or consistently and jointly involved in exciting those outputs. Another way to relate the appearance of a new meme to Edelman's psychobiology would be to say that a meme can only be said to "exist" in a new brain or mind once it has emerged into the secondary repertoire from the primary repertoire. The primary repertoire is so vast that any number of concepts could be expected to remain at this level, undiscovered by the organism and, therefore, absent from the secondary repertoire.

From this general picture of the psychobiology of the working brain we can build a provisional model of a meme. Concurrently, it should also be possible to define a general idea of the nature of memetic mutation or innovative learning processes. These two general sketches provide an indication of the psychobiological phenomena which underlie cultural evolution, and which should serve as a starting point for the building of an "evolutionary memetics" of archaeological and anthropological diversity.

Firstly, let us outline the psychobiological nature of the meme in greater detail. Edelman's concept of the primary repertoire could be described as a vast universe of latent memes, each of which requires a particular pattern of signals from the sensory nervous system to be "awakened", and brought into the secondary repertoire, where it may then be remembered. Presumably, a particular array of strengthened synapses can fade, and be forgotten, passing out of the secondary repertoire, and returning to the primary repertoire. Any given meme can then be viewed as consisting of one or more neuronal groups in the primary repertoire which display a particular pattern of synaptic interconnection, and which have emerged into the secondary repertoire as a result of the input of a particular pattern of signals which in turn produced synaptic change. The incoming signals found a match with this particular latent element in the primary repertoire, due to its synaptic structure, and a particular set of synapses were thereby strengthened, and brought into the secondary repertoire.

Cultural reproduction is therefore the means whereby one individual may help another to bring latent memes from the primary repertoire into the secondary repertoire, and constitutes what Edelman calls "genuine information processing" (Edelman 1989:320; and 1989:308-311). In Edelman's selective system, the reproductive dimension of teaching and imitation presumably

involves a process by means of which the behaviour of the "teacher" presents a series of signals to the "student", while these signals then enter the central nervous system and excite a "match" with a range of neuronal groups in the student's primary repertoire, thereby initiating first short term and then long term synaptic changes in these neuronal groups such that they emerge into the secondary repertoire. The teacher is essentially helping the student to find the nearest memetic equivalent of a given idea in his or her own primary repertoire.

It appears that such cultural reproduction involves a content dependent rather than address dependent equivalence (Delius 1989:44). In other words, the same meme may be stored in different parts of different brains, as the indeterminacy involved in the finer point to point wiring at particular localities in neural maps (Edelman 1989:34–58, 1989:318) ensures that the best adapted neuronal groups for a particular meme are unlikely to develop at an identical point in any neuronal map. But the overall pattern of altered synapses or "hotspots" in any two copies of a meme must be basically equivalent, in order to yield the same behaviour or the same pattern of verbal signals. Delius, discussing memes under the generic category of psychobiological "traits", has remarked:

> "Any trait (meme) that is taken over by a given individual from another individual must accordingly be thought of as the transfer of a particular pattern of synaptic hotspots within the associative networks of one brain to the associative networks of another brain. Different traits must be thought of as being coded by topologically different hotspot patterns. That is, a given cultural trait borne by an individual is encoded informationally as a particular pattern of modified synapses in his brain. Naturally the hotspot pattern that a trait has in one brain will not be geometrically arranged in exactly the same way as the pattern that the same trait has in another brain. For that, the brains of different individuals are likely to be too different. But functionally the two hotspot patterns would still be equivalent, at least to the extent that the effective memory contents representing the trait were identical. As an analogy the reader

> may think of the same text passage stored on two different kinds of floppy-discs by two different computers: spatially not identical but informationally equivalent." (Delius 1989:44).

Delius has offered similar arguments for the existence of memes as actual structures (Delius 1989:82-84). It is clear that the notion of a meme does not conflict with modern psychobiology, and that ideas exist as actual structures, and do not exist in some "twilight" world between fact and fiction.

In light of this general perspective on the nature of memes, some speculative statements on the nature of cultural mutation are offered; this is the problem of how new memes are produced. Memetic mutation would presumably involve some mechanism by means of which existing memes which have already emerged into the secondary repertoire provide some sort of clue or stepping stone to other memetic structures which are lying latent in the primary repertoire. One key concept which may provide a starting point for building a model of memetic mutation is Edelman's notion of *degeneracy* in the primary repertoire.

As described briefly above, a degenerate network is one where a number of different neuronal groups, distributed throughout different points of the primary repertoire, are capable of responding to the same set of signals. It is not accurate to call these different neuronal groups redundant, as they are by no means the same in their structure; in fact, they may be very different. This is a result of the fact that a "match" between a signal pattern and a neuronal group can, as a rule, never be perfect (Edelman 1989:47). If some half a dozen neuronal groups are capable of achieving an 80% match, each group may be matching with a different 80% of the signal, and failing to match a different 20% of the signal.

Moreover, while they may all respond to a given set of signals, this is not to say that they all respond with equal intensity. As a result of the differences, some neuronal groups respond more effectively than others, leading to their differential reinforcement, and resulting in a stronger emergence into the secondary repertoire. The strengthened synapses involved in the selection of the particular neuronal group now predispose it toward

responding again to a similar set of signals. Yet the degenerate nature of the symbolling system ensures that, particularly in the early stages of learning, a range of neuronal groups all respond to any one group of signals. In other words, memetic reproduction is not so precise that one single meme is brought out "cleanly" from primary to secondary repertoire. Rather, a whole population of competing neuronal groups is excited, and this population may be highly variable in structure at the same time as being approximately equal in their ability to respond to a signal pattern. In time, the neuronal groups with the highest level of response activity will be amplified, and become more and more likely to respond exclusively to the pattern, while others with a response level that is not quite so high may recede; yet even the receding groups are now more likely to match with new signals than before. It is possible that such ranges of degenerate groups constitute a population of partially amplified "mutant" memes which all bear a thematic relationship to the dominant meme, and which might compete more successfully with the dominant synaptic pattern for other sensory signals of a similar nature. Further elaboration of this possibility would require a deeper analysis and discussion of the degeneracy concept.

Another source of variation in neuronal repertoires is caused by the *long term pre-synaptic* changes which take place in the formation of the secondary repertoire:

> "The proposed epigenetic mechanism for the selection of such secondary repertoires invokes independent pre- and postsynaptic rules for changing synaptic efficacy over greater or lesser times. These rules operate to enable correlation of stimuli in time with receptor sheet space in such a way as to favour the responses of certain groups of neurons. At the same time, the rules also act to bias certain short-term responses by long-term responses; this bias emerges from the recursively connected neurons in groups obeying both rules. While this increases the likelihood that certain groups will respond upon repeated stimulation, the long term pre-synaptic changes can at the same time enhance the variability of synaptic efficacy within connected domains, generating additional diversity. The degree of variability is determined by the operation of the rules and is constrained by the degree of competition offered by different correlated stimuli affecting other groups in these domains. This process provides not only a basis for both novelty and generalisation but also a continuous source of variation." (Edelman 1989:319).

Long term synaptic changes therefore establish particular combinations of neuronal groups in the secondary repertoire, thereby increasing the likelihood that the same groups will respond when similar signal patterns are experienced again. In this sense the selection of one neuronal group over another eliminates certain elements from the primary repertoire and favours others, which are established as parts of the secondary repertoire, thereby reducing variation amongst populations of neuronal groups. Yet it appears that this process of reduction simultaneously enhances variability in other neighbouring or connected neuronal groups, thereby preparing parts of the primary repertoire for emergence which might otherwise have lain dormant, and which had no primary "match" with the original signal pattern. The memetic implications of such mechanisms are not obvious, but it is possible that once a certain meme is received into a brain, a whole suite of related but hitherto inconceivable memes or ideas then become conceivable, by virtue of their location in one of the "connected domains" of which Edelman speaks. If such were the case, is clear that such mechanisms would provide another source of variation, in addition to that of populations of partially amplified degenerate symbols.

It is clear that the above models of psychobiological phenomena require more discussion, analysis and synthesis before they could be incorporated into a series of processual models for an "evolutionary memetics". Yet it is equally clear that the emerging picture of the working brain does not present any obvious obstacle to the cultural virus or meme positions, and that cultural phenomena can be viewed as actual neuronal structures capable of influencing human behaviour and being socially transmitted as a direct or indirect result of that behaviour. Moreover, it can be seen that a number of Edelman concepts, such as degeneracy, reentry, and pre- and post-synaptic rules for changing synaptic efficacy, suggest a range of conceptual starting points for building models of memetic mutation. In the next section, some general implications of the above foundation arguments are discussed, in order to define the way in which the concepts from Dawkins and Edelman fit together, before their ultimate incorporation into Cultural Virus Theory at the beginning of the next chapter.

9.5 Brain Versus Virus: The Psychobiological Arms Race

A general implication of a virus perspective is that the "environments" or "niches" which define the fitness of different viral ideas or behaviours are governed purely by symbolic and psychological factors (Cullen 1990:65). It is thus *exclusively preoccupied* with the very areas of human affairs which neo-Darwinism is normally criticised for ignoring or being unable to cope with (Shanks and Tilley 1987a, 1987b; Ingold 1986, 1990). This is in contrast to more traditional definitions of neo-Darwinian "fitness", which involve both the symbolic and genetic, as fitness is defined in cultural selectionist frameworks. Rindos (1986b:321), for example, has a category of human benefit ("demic" cultural selection or CS1), where cultural entities can be selected on the grounds of the genetic fitness they convey to their bearers. Other cultural selectionists (Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981; Durham 1982; Lumsden and Wilson 1981) juggle genetic and cultural fitness in a variety of ways. But fitness must, in the new viral perspective, always be defined purely in terms of the total psychological appeal a cultural phenomenon can sustain in a wide range of minds and personalities, with genetic factors only being included to the extent that they affect this appeal. Genetic factors need never be directly discussed, since they cannot affect the psychological fitness of ideas unless they are perceived and valued, i.e. unless they are in fact *other ideas*, and not demographic or reproductive facts. If the idea is viewed to be "maladaptive yet appealing" (see also Chapter 5), then a cultural equivalent of the Wynne-Edwards "good of the species" group selection fallacy (Dawkins 1989:7,69,282), where a structure persists in a population due to its long term rather than short term adaptiveness, is produced. An idea or artefact can only be considered "fit" to the degree to which it has more symbolic appeal than any other at the time. A population of ideas cannot resist short term infiltration by another idea or artefact with more symbolic appeal, just because the original ideas convey some long term benefit to the human community that the new idea does not. If a pernicious but appealing new idea appears, it will simply replace the old one before

driving that human community and itself to extinction, unless at some critical point in the process it loses that appeal. This may occur, for example, if an individual is aware of the long term dangers of using a particular artefact, and deliberately avoids using it. However, the artefact is of course able to colonise any minds who are not aware of such consequences.

The overall challenge will be to view the human mind not as a coherent self (although this may often successfully emerge), but as a vast arena for complicated networks of collaborations and conflicts between (and amongst) on the one hand, the phenotypic expressions of genes, and, on the other hand, memes (Ball 1984:147; Cullen 1990:65). Each meme or gene then persists in a given human community according to its all round ability to influence a long succession of different people in such a way as to increase its chances of reproduction. More specifically, each meme competes against other memes for replicative success on the grounds of its total collaborative potential with hundreds of human phenotypes and many thousands of cultural phenotypes. Evolution in the human capacity for culture could then be expected to favour genes which collaborate with a range of bodies in such a way as to produce highly resistant intellects. In other words, it is in the reproductive interests of human genes to be able to indirectly influence brain ontogeny in such a way as to produce features which help the person to selectively resist pernicious enculturation, and to remain pragmatically sceptical. Symons (1979:308) has used just such an argument against virus analogies, arguing that counter adaptations in human intelligence would produce resistant individuals.

In light of this argument, a virus perspective ultimately begs the question of how the human cultural capacity could ever evolve, given that the capacity evidently carries with it a general susceptibility to socially transmitted pernicious behaviour. How could something with such a great potential for cultural parasitism be adaptive enough to become comprehensively fixed in the hominid lineage? It is obvious that in some sense the human cultural capacity must be adaptive, or else it could not have spread in the first place. Many, such as Dawkins (1989:193); Delius (1989:54); Durham (1982);

Freeman (1983); Mithen (1989:483); Rindos (1986a, 1986b); and, as already implied, Symons (1979); to name a few, have submitted various arguments which relate to this problem of where to fit "maladaptive" behaviour into a co-evolutionary framework. Yet the problem of pernicious behaviour itself carries within it the kernel of a solution. It is conceivable that pernicious behaviour, and not beneficial behaviour, has been the primary driving agent in the evolution of the human capacity for culture. To put it another way, it is possible that the Symons challenge does not so much threaten the virus position as advance it. Perhaps intellectual self defences did evolve in the human brain, and that *this is precisely what has made the human cultural capacity what it is.*

In the first place, it is possible that the cultural capacity and intelligence evolved as a series of increases which involved a corresponding increase in genetic fitness, which quickly became fixed in each population it spread to. Parasitic cultural phenomena of a given complexity require the presence of a correspondingly complex cultural capacity to replicate them. Therefore parasitic exploitation would always have to occur after the fact of an adaptive advance in the cultural capacity. Moreover, since the first pernicious, exploitive cultural phenomena would have to be stumbled upon randomly, via memetic mutation, it is conceivable that there was some time lag between the appearance of a certain level of the cultural capacity, and the hapless discovery of equally sophisticated neuronal patterns capable of exploiting the new capacity. Time enough, perhaps, for populations of hominids to move on to the new level, and become completely dependent on the symbiotic and beneficial ideas which the new capacity allowed them to pass amongst themselves. More time could be bought while memetic mutation threw up very pernicious ideas which were rejected not because their dangerous qualities were understood, but because they simply did not fit in with the belief systems prevailing in hominid cultures of the time. Groups of ideas which are co-adapted into mutually assisting complexes, called "meme-complexes" by Dawkins (1976, 1989:197), and "memomes" by Delius (1989:65; 1991:92), and a "viral assemblage" by the author (as in a eusocial viral complex Cullen 1990:66; and next chapter) present barriers to the uptake of

mutant memes which might otherwise be very well adapted. In other words, they might have intrinsic appeal individually, or have great functional potential, but be rejected by certain other concepts in the *status quo* of a given subculture. One example of this kind of problem would be the use of a new technique of swinging a baseball bat which guaranteed a home run; if played in a baseball match, it would presumably be a great success; but if played with a tennis racket on a court, the same action is unlikely to be as effective. Another example would be the Catholic ban on contraception. This meme presumably conveys genetic fitness to its bearer by preserving the link between the sexual reward system and genetic reproduction. But the "no-contraception" meme could well have less intrinsic psychological appeal than contraceptive memes (the Rhythm Method) and artefacts (pills and condoms). Yet in the appropriate religious and moral context the genetically advantageous meme also has greater psychological fitness (due to sanction by religious authority), enabling it resist replacement by its intrinsically appealing but genetically pernicious competitors.

Context dependent factors such as these might prolong the period of time during which hominids with increased cultural capacity were universally more genetically successful than their counterparts. This first adaptive step toward culture would resemble the "phylogenetically protocultural stage" when "memes must forcibly have been mutualists" (ie. beneficial) as the cultural capacity could only spread "if individuals that had that competence were biologically fitter than those which did not have it" (Delius 1989:54). Delius goes on to state that each subsequent step in the cultural capacity must depend on the same principle. Mutant genes would contribute to improved cultural capacity "if the memes they enabled resulted in an adaptive advantage for them" (Delius 1989:54). The brain develops like an "immune system" (making Edelman's analogy all the more intriguing, see previous sections), but one which is designed to pick out the most advantageous memes, and avoid all others.

But it is possible that this vast cultural immune system can best convey genetic fitness to its bearer by making the identification of dangerous ideas

the top priority, and to otherwise simply follow tradition. The other half of the "race" is on the side of viral concepts and behaviours. Selection in the cultural meme pool will favour cultural phenomena which confuse and confound the individual's struggles to remain sceptical about information, and which confuse the individual's attempts to retain any sense of self that is separate from those ideas. Sooner or later in this scenario, memetic mutation would catch up with each step forward in intelligence and cultural capacity. More sophisticated minds which were able to laugh at the pernicious habits and beliefs of simpler members of their communities would begin to fall prey to new and more sophisticated beliefs, and more complex and addictive amusements or substances. A general scenario such as this ought to be testable in the Palaeolithic archaeological record. For instance, incoming anatomically modern cultures at the time of the Upper Palaeolithic revolution may well have contained elements which Neanderthals were capable of learning, but not controlling. Such cultural practices could have had an effect similar to modern practices such as the consumption by colonised peoples of new substances such as alcohol, although in such modern cases there is obviously no genetic difference in alcohol resistance, only a cultural one.

The general pattern taken by such a scenario would constitute a variety of co-evolutionary process, involving the kinds of factors discussed by Durham (1982, 1990, 1991), Freeman (1983), and Irons (1979), and others mentioned above. But while symbiotic "mutual benefit" co-evolution occurring between hominids and their cultural phenomena would still occur, it would be a co-evolution primarily driven by conflict. The benefits would not be mutual at all; the cultural phenomena would be predators of their hominid hosts, while hominid gene pools would be principally adapting to minimise suicidal behaviour patterns, ie. adapting defensively rather than offensively.

Just as the evolutionary relationship between gazelles and cheetahs, or other predator/prey co-evolutionary relationships, can be described as an "arms race" (Delius 1989:52), it may be that this phrase also characterises the relationship between hominid genes and their culturally transmitted predators, and more accurately than the mutually beneficial variety of co-evolution.

Humans are not deriving strategic *benefit* from their relationship with culturally reproduced phenomena, so much as engaging in an unending attempt to gain ascendancy over each new invention. Co-evolution perhaps, symbiosis certainly, but are we the prey rather than the predator?

Ironically, the solution to cultural or artefactual predation would always be more cultural capacity, and more intelligence, rather than less. Due to the time delay in the evolution of new cultural phenomena capable of exploiting the new cultural capacity (time delays being implicit in all Darwinian processes), the immediate effect of improved cultural capacity or intelligence would be resistance to all the current pernicious memes in a given hominid community. In other words, it would take a while for new "mutant" ideas or practises to emerge in the culture of any given community. Perhaps the adaptiveness of a more and more intelligent cultural capacity lies in the resistance it conveys to prevailing suicidal behaviours, rather than in its conveyance of a brave new world of cultural symbiotes. The question immediately begged is this: Why not go back? Would not simpler hominid minds, with less cultural capacity, who are immune to the new pernicious cultural phenomena, begin to undergo a reproductive resurgence under such conditions? Without a great deal more research, it is only possible to speculate. Yet there is no reason to suppose that reduced cultural capacity *would* guarantee freedom from pernicious habits. Linden (1974) *inter alia* has shown how hominoid apes may be taught language by a human individual, in other words by individuals who have a greater cultural capacity than themselves. There have also been instances of primates such as those in circuses being taught health damaging behaviours, such as smoking and drinking. It is unlikely that a chimpanzee could understand the message of an anti-smoking advertisement. So it is quite conceivable that a hominid individual with a reduced cultural capacity would actually be at a *disadvantage* when it came to resisting pernicious cultural phenomena. Perhaps the only way out of the brain/virus evolutionary spiral is up. Becoming too intelligent for prevailing cultural phenomena would at least convey some respite, at least until the next generation of "superdrugs" came along.

So while pernicious imitation may be the cause of the problem at each stage of the evolution of the human cultural capacity, imitation that is more intelligent still is perhaps the only way out. While all culturally transmitted entities are dependent on human action for their reproduction, the dependence is now mutual. "We alone on earth are free to rebel against the selfish replicators" writes Dawkins (1989:201). But what do we rebel with? Is there such a thing as a "non-idea"? It seems to me that the only choice available is to choose *which* cultural phenomena we wish to propagate, not whether we use them at all. We now need cultural phenomena are much as they need us.

9.6 Concluding Remarks

A series of foundation arguments for a virus perspective have been outlined in this chapter and they can be summarised in 3 basic points.

Firstly, a generalised definition of a virus was introduced, and it was shown how such phenomena could be considered neo-Darwinian individuals in their own right. This can be done without attributing any organismic qualities to them, other than the genealogical independence they undoubtably display.

Secondly, some principles of parasite ecology which highlight the significance of genealogical independence were discussed. If a parasite or guest of another organism reproduces in synchrony with its host there is very little to distinguish it from a trait. Indeed, some structures long thought to be traits of organisms, such as mitochondria, are actually symbiotic parasites (Delius 1989:48), while these and other organismic structures could well be the result of ancient mergers between parasites and their hosts (Margulis 1981, Dawkins 1989). Not only can synchronised host/parasite combinations be viewed as single organisms; it can even be argued that this is all an "organism" ever was in the first place (Dawkins 1989:182, 243). Conversely it can be argued that structures which consist of parts which display genealogical independence from each other are *not* organisms, but

rather small ecological assemblages, which unlike single organisms, and like populations, are capable of evolution. Moreover, as ecological assemblages, the potential arises for adaptive conflicts to emerge amongst their constituent structures. Since cultural phenomena display genealogical independence from human genes it can be argued that human individuals are small ecological assemblages rather than single organisms, and this would explain both the fact that cultural evolution can occur in somatic time, and the fact that adaptive conflicts emerge between human genes and human behaviour. A brief example of a human individual caught in the midst of an adaptive conflict between cultural and genetic reproduction was offered in a hypothetical example of a "workaholic", but a more rounded discussion of such conflicts occurred in Section 9.5. There it was briefly shown how the genetic underpinnings of the "human cultural capacity" could undergo differential reproductive success, despite the susceptibility to parasitic cultural phenomena that that capacity produced.

In Sections 9.3 and 9.4, Edelman's psychobiology of learning and cultural reproduction was discussed, and it was shown that memes and cultural phenomena are just as much "real biological structures" as DNA and genes (and when this quality is coupled with genealogical independence, ideas, behaviour patterns and artefacts become biological individuals in and of themselves) yet the manner in which cultural phenomena evolve is utterly extragenetic. It cannot be overemphasised that cultural phenomena actually exist (see Edelman 1992:16–32 for a recent and convincing assertion of the material underpinnings of mind), and, by implication, as socially transmitted learned neuronal structures and their behavioural and artefact products, although these implications are not explored by Edelman. Ideas and their products are simultaneously completely biological and completely cultural. Yet many archaeologists and anthropologists seem to view culture as some kind of "nonexistent" phenomenon which transcends "biology", and freely speak of a distinction between "biology" and "culture". Others disparage the distinction, but then speak of the undeniable distinction between genes and culture as though it is equally invalid (Lee 1991; Ingold 1991). Some even warn of the dangers of "reifying" (Rindos 1989:4; Dunnell 1980) culture, as though it is

some metaphysical construct which has no real existence, and which we must refrain from treating as if it actually did exist. It seems to me that the arguments of Edelman and Delius make it clear that it does. In particular, Edelman's work constitutes a development for theories of cultural evolution akin to the discovery of DNA in genetic evolution; a basic understanding of psychobiological code structure. These sort of advances may be coupled with the "Mendelesque" role of assertions from Dawkins (1976), who explicitly suggested the possibility of "hereditary discreteness" in social transmission (implicit in his invention of the word "meme"). The aim of these metaphorical comparisons is not to equate disparate inventions of ideas, but to make it clear that there is nothing in psychobiology which presents any obvious obstacle to future developments of an "evolutionary memetics" within a virus perspective. Cultural phenomena really do exist, as wholly extragenetic but thoroughly biological "things", so there is no danger of "reification". Therefore, while the "biology versus culture" distinction is easily deconstructed in that culture is not wholly extrasomatic, the same distinction would appear to function as a metaphor for another and very valid distinction; that between "genetically reproduced" and "culturally reproduced" phenomena.

Finally, in Section 9.5, it was briefly outlined how some of the above foundation arguments could fit together logically in an evolutionary framework. Once all of the above foundation arguments outlined above are brought together it becomes clear that the virus position is no idle metaphor. It has the potential to become a coherent perspective supported by qualitative evidence and "in principle" arguments from a wide range of scientific domains. In the next and final chapter these general principles will be used as foundation arguments for a provisional outline of the Cultural Virus Theory.

10

THE CULTURAL VIRUS THEORY

Overview

Here the basic propositions of the Cultural Virus Theory (CVT) are introduced. In a very general sense, this new Darwinian perspective could be described as a reversion to a position not dissimilar to the old fashioned view of artefacts as organisms, only within a neo-Darwinian rather than a Lamarckian framework. Ironically, this is precisely the view which other Darwinian anthropologists such as Foley (1987:380) and Leonard and Jones (1987:215) believe should be avoided at all costs. But the cultural virus position can not be simply reduced to the phrase "artefacts as organisms". For, far from being autonomous, each "quasi-organism" or viral phenomenon is simple, passive, and dependent upon human agency for reproductive success in what is essentially a psychological and social environment - a "jungle of human sentiment". "People" are not, therefore, excluded from the cultural virus position, but rather redefined at a more complicated super-organismic level of neo-Darwinian categorisation; they are modelled as a dynamic ecological assemblage of one organism and many viral phenomena, evolving in somatic time, and capable of doing all the things that only ecological assemblages are able to do.

10.1 Merging with Memes

There are a number of subtle variations between the Meme and Virus perspectives, but these amount more to minor differences in emphasis than major theoretical conflicts. Such differences would be expected as a result of the fact that the two perspectives are parallel but largely independent inventions, which developed in isolation from each other (as discussed in Chapter 9, Section 9.2). Yet, at the most general level, the two positions fit together as two complementary aspects of the same basic principles, with memes corresponding to the level of the Cultural Virus Theory originally

occupied by phenomena termed "metonyms", or more generally, "ideas", while each viral phenomenon consists of one or more memes, and their motor products or phenotypes. In fact, the Virus and Meme positions complement each other in much the same way as the "virus" and "gene" concepts complement each other in biology. In modern evolutionary biology, the virus is the "whole entity", encompassing both its genes and the phenotypic products of those genes. Correspondingly, in Cultural Virus Theory, a cultural virus or viral phenomenon is the socially transmitted "whole entity", encompassing both its memetic information or "hotspot" synapse patterns, and the behavioural and artefact products of such neuronal structures. Dawkins has incorporated a great deal of variability into his definition of a meme, in much the same way that variability was also incorporated into his definition of a gene (see also Chapter 5, Section 5.2):

> "...I divided the "gene complex" into large and small genetic units, and units within units. The "gene" was defined, not in a rigid all-or-none way, but as a unit of convenience, a length of chromosome with just sufficient copying-fidelity to serve as a viable unit of natural selection. If a single phrase of Beethoven's ninth symphony is sufficiently distinctive and memorable to be abstracted from the context of the whole symphony, and used as the call-sign of a maddeningly intrusive European broadcasting station, then to that extent it deserves to be called one meme." (Dawkins 1989:195).

Like a gene, the meme is not a rigid unit, but a division of analytical convenience imposed on a quasi-continuous medium. Ball (1984:147) discusses a series of Dawkins definitions of memes, but does not highlight the quasi-continuous element to the same degree. Fletcher (1989:39) and Clegg (1978a) compare cultural and "biological" (genetic) code systems in a similar manner, arguing for equivalence without stressing the basically continuous nature of both media. Finally, Heylighen (1992b) also writes of "pieces of knowledge" or "production rules", in which the essentially quasi-continuous nature of the meme concept is not explicitly brought out. Yet as discussed in Section 4.5, and now above, there is no need to argue for discrete particles of cultural inheritance: the meme concept need be no more rigid than that of the gene.

Memes then produce the non-memetic aspects of viral phenomena, the behavioural and artefact "phenotypic aspects", through complex interactions

with other memes and with the "decision making" apparatus of the brain. Since this memetic interaction to produce the viral phenotype is complicated, a false "blending" effect can thereby be produced. Just as the particulate inheritance of vast numbers of skin colour genes at many loci can produce an even blending through the "averaging out" of all the different effects on skin colour, so it ought to be possible to produce the appearance of blending in behaviour when individuals strategically combine the effects of large numbers of their memes:

> "It is possible that this appearance of non-particulateness is illusory, and that the analogy with genes does not break down. After all, if we look at the inheritance of many genetic characters such as human height or skin-colouring, it does not look like the work of indivisible and unblendable genes...This does not mean that the genes concerned are not particulate. It is just that there are so many of them concerned with skin colour, each one having such a small effect, that they *seem* to blend." (Dawkins 1989:195).

Beyond this focus at different but complementary levels of cultural systems, the positions are basically similar in a that both the Virus and Meme position essentially casts culturally transmitted structures in the role of parasites (whether harmful or beneficial), or "symbionts" (Delius 1989:48, 1991:85), as distinct from the role of "traits" or organs. As a result of their genealogical independence from their hosts, they cannot be considered to be parts of their hosts's bodies, and therefore terminology which implies traithood or organhood is abandoned. Interpretation of behaviour in terms of adaptation of "people" therefore involves the juggling of two lines of reproductive benefit (host reproduction, versus reproduction of the viral phenomenon), and all combinations of those two lines of benefit. These would potentially include cases where only the host benefits, where only the viral phenomenon benefits, where both benefit, and where neither benefit (see also Ball 1984 for an interesting taxomony of this kind).

Thus at the most general level of comparison, the Virus and Meme positions may be considered to be two parallel conceptions of parasitic potential (or genealogical independence) in socially transmitted behaviour focusing at a different level of inclusiveness in cultural phenomena. As a consequence, I propose to use the meme concept freely when referring to the

"memetic" level of viral phenomena (as an improved term for what I originally termed either just an "idea" or "metonym"), and the more encompassing term "viral phenomenon" when referring to the "entire" entity, encompassing both meme and "phenotypic aspect". I use the word "phenotypic aspect" rather than "phenotype" or "traits" to embody the fact that, like any gene in an organism, a meme's phenotypic aspect will be highly variable and context dependent. Its expression in the behaviour of the human individual in which it occurs depends upon what other memes are present, and what other non-memetically reproduced psychological phenomena are present in the mind in which it is found. A meme does not code for any single "trait" which will be displayed by all its bearers, any more than does a gene. Rather, it constituted from neuronal structures corresponding to one or more ideas, which can be combined or recombined with other ideas, and suppressed or emphasised by different emotional states, according to the mood of the moment. Nonetheless, the structure of a particular idea should limit the number of contexts in which it can be used, and the number of other ideas with which it may be combined. Each meme, then, can be expected to have a range of highly contingent phenotypic expressions or aspects.

It is also interesting to note that unlike genes, the phenotypic aspects of memes are, strictly speaking, "extended" phenotypes, rather than immediate ones. The term "Extended Phenotype" comes from Dawkins (1982, 1989), and applies to effects that the genes of one organism may have on the body of another organism altogether. Thus the extended phenotype of the rabies virus is not the body of the virus itself, but its effects on the nervous system of the animal in which it resides, such as salivation, lack of swallowing, and hydrophobia. Similarly, the extended phenotype of the liver flukes of Chapter 9, Section 9.2, is not the body of the fluke, but certain effects the fluke has on the snail's body, such as the exaggerated hardness of the shell. The interesting thing about the Dawkins Extended Phenotype concept in the case of memes is that they have no bodies of their own on which to have effect; they have, therefore, an extended phenotype, but no ordinary phenotype. The only way memes can be transmitted is through their influence upon the nervous system of a body made by genes - or, at least, until very recently

with the advent of computers. Their effect on the human nervous system is not chemical, but structural and electrical; unlike the genes of ordinary parasites, they do not have to secrete any manipulative substance into their hosts nervous tissue. Memetic phenotypes directly involve thoughts, behaviour, and artefacts, produced by the body in which they reside. Thus each viral phenomenon consists of one or more memes and their combined extended phenotypes.

This can be imagined using stone knapping techniques as an example. The learning of a technique such as pressure flaking, where flakes are "popped" off a core via a precise exertion of pressure (pressing rather than striking) at a strategic point and angle along the margin of a core, involves the observation and imitation of someone performing the technique, who by definition already possesses that particular viral phenomenon. There are two sources of information for transmission. Firstly, there is the observation of the phenotype of the viral phenomenon, rather than its meme; this is the behaviour itself. Secondly, the "master" knapper may also be able to discuss and direct the student, through verbalising a set of other memes which he or she has developed in order to explain the experiences or sensations of the technique. These are also part of the extended phenotype of the "pressure flaking" meme, but they are even "further" away from the neuronal structure of the meme itself. The point to be made here is that even the most basic level of the phenotype of the meme, the behaviour, is already "extended", in that it consists of an affect on a body which the meme was not involved in creating.

Besides the levels of inclusiveness (viral phenomenon equals memes + phenotype of motor products) difference, there is another subtle difference in emphasis between the two perspectives. This difference can be expressed in terms of a distinction between the words replicatee and replicator (see Cullen 1992a). While at a general level the two words can be more or less equated, as in gross neo-Darwinian terms they are not vastly different in their implications, the two words also reflect a tendency in each perspective to focus attention upon different adaptive scenarios within cultural systems.

For example, human "intentions" and "emotions" are more featured as selective pressures in the Virus discourse, while physical or sensory limitations and other more unconscious psychological factors are the featured selective pressures in the Meme position. The Cultural Virus position emphasizes that cultural phenomena are the passive beneficiaries of unconscious *and conscious* attempts by human individuals to control their lives; while meme discourse places more emphasis on examples where meme benefit is a little more insidious, replicating without any great self consciousness on the part of their hosts. It is stressed that the set of cultural examples in the Virus position is perfectly in keeping with those of Meme position, and vice versa, and indeed each is implied by the other. We are speaking of two different but complementary (and indeed overlapping) sets of "cultural parasite" examples, ranging from the most intentional to the least.

It is important to note that Dawkins concept of a "replicator" (see also Ball 1984 *passim*) does not involve any notion of intrinsic "intent" or machiavellian intelligence on behalf of that replicator, either in the case of genes or memes. However, unlike the word "replicatee" of the virus discourse (Cullen 1992a), nor does it imply any necessary presence of such factors anywhere else. The replicator concept implies *self*-reproduction, leading the meme discourse, as we shall see a little later, toward scenarios in which the role of human intentions in forming the meme's environment, while never dismissed, is rarely explicitly discussed:

> "Whenever conditions arise in which a new kind of replicator *can* make copies of itself, the new replicators *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. The old gene-selected evolution, by making brains, provided the "soup" in which the first "memes" arose. Once self copying memes had arisen, their own, much faster, kind of evolution took off." (Dawkins 1989:193).

While Memes have thus been quite specifically defined as the new "*replicators*" by Dawkins, the Cultural Virus Theory has used terminology which implies that each viral phenomenon is a "*replicatee*", not a "replicator" - a concept more akin to an RNA retrovirus, and not a piece of DNA. Ideas and artefacts are not replicators in quite the same way that DNA is, because

they cannot reproduce themselves. They have no intrinsic capacity for self assembly like that of DNA. They require external reproductive agencies, such as human minds, which may include all sorts of conscious and unconscious goal-oriented behaviour, occurring in their "environment", in order to produce and transmit their offspring for them. They are selected for their capacity to become the passive beneficiaries of strategic decision making. Because of this humans may well, at times, be aware of the fact that they are reproducing cultural phenomena, and they may even have some idea of why they are doing so. But such "intentionality" presents no more of an obstacle to selective principles than does "artificial" (whether conscious or unconscious) selection in domestication, which may be viewed a special case of symbiosis, see Rindos (1980, 1984). Hence the special emphasis on the use of words like virus, and viral; these words denote that ideas and artefacts, *unlike genes*, are incapable of self reproduction, and that they are dependent upon human agency (Cullen 1990, 1992a, 1992b, 1993a, 1993b). Because the word virus denotes that ideas and artefacts are replicatees, and therefore, at best, only a "special case" of the category "replicator", CVT in no way denies human intentions, purposes, or planning; instead these become fundamental elements of the psychological landscape - the idea's "environment" - to which populations of ideas adapt.

But as already implied, the influence of human intentions on the "environment" of any given cultural phenomenon is never *denied* by any aspect of the meme discourse, and indeed it is sometimes mentioned (Dawkins 1989:193). However, it is not made such a featured part of primary theoretical statements as it is in the Cultural Virus position, and this could well be a reflection of the use in the meme discourse of terminology which implies self-replication rather than "proxy"-replication.

The "individuals" of cultural process are individual viral phenomena, and they are therefore quite different to the individual organisms of genetic process. In CVT the basic "individuals" under focus are not human individuals, but ideas or memes of highly variable size and complexity, along with their highly variable behavioural and artefact products. Any piece of information that can be learned or taught in a "single event" of

comprehension, or a series of interdependent moments of comprehension, along with its extended behavioural phenotype, can be considered an individual. A convenient term for these pieces of information would be "cultural phrase" or "symbolic phrase", "metonym", or even "idea", and I have used such terms elsewhere (Cullen 1990, 1993a, 1993b), but, as already suggested, there is no reason why the neuronal structures which underlie ideas - which constitute the genetic level of viral phenomena - may not be called memes. Phrase length, or meme length, then, in the quasi-continuous neuronal medium, may vary greatly (Cullen 1993a); the comprehensibility of a concept will vary according to who is teaching it, and who is learning it. Sometimes a phrase will be intrinsically meaningful, and might spread independently from other ideas. At other times it may only "make sense" with reference to a number of other phrases, and its "symbolic appeal", and therefore its reproductive fitness, may be entirely dependent on the success of other phrases (the parallel of this concept in the meme discourse is the notion of a "meme complex"). Without recourse to certain key definitions, a student may find many otherwise limpid ideas quite incomprehensible. The fitness of an idea or phrase may depend on the existence of half a dozen, a few hundred, or even thousands of other concepts to which it can be "symbolically connected". Thus in CVT an "individual" is a single phrase or body of phrases which are usually taught or learned together.

The individuals of more traditional Darwinian approaches, individual organisms, are therefore not considered to be appropriate units of analysis in cultural evolution, despite their undeniable relevance in genetic evolution. However, as already implied from another angle, human individuals, and both their conscious and unconscious intentions, remain the important "agencies" of cultural change. It is just that they are not the individuals being reproduced in the discharge of that agency. They are the selectors, not the selectees - the reverse of what is the case in genetic process. As such, the individual psyche constitutes the most basic unit of any viral phenomenon's environment, but not the most basic unit in the definition of a viral "self".

As mentioned in the overview of this chapter, what is being recommended is a reversion to a position not dissimilar to the old fashioned view of "artefacts as organisms" - except that here artefacts and behaviour are seen to be the extended phenotypic product of viral neuronal structures. Ironically, this is precisely the view which other cultural selectionists such as Foley (1987:380) and Leonard and Jones (1987:215) believe has brought evolutionary theory into disrepute in archaeology. However, in the virus form of the view, each cultural "organism" is so simple, passive, and dependent upon human agency for reproductive success, and its reproductive fitness so dependent upon a particular psychological and social milieu, that the resulting framework is ultimately very different to the antiquarian form of the "artefacts as organisms" analogy.

Put most simply, then, in CVT the primary "individuals" of cultural process are ideas (equivalent to Dawkins memes), and their behavioural and artefact products. However, *human* individuals remain crucial units of analysis, as the most obvious units of the viral idea's psychological and cultural *environment*. Each individual mind consists of a vast ecological assemblage of thousands of different ideas, built upon a genetically reproduced landscape of open ended instincts and susceptibilities, to which populations or groups of comparable and/or mutually exclusive ideas must adapt. In other words, the fitness of each idea is a function of its fit to a total psychological environment; this psychological environment includes both a fundamental landscape of genetically inherited capacities and propensities (which are probably shared by all humans), and the many thousands of learned concepts both viral (teachable) and inchoate (unteachable), which then "populate" this somewhat more immutable landscape. All of the terms of description of this paragraph are, however, highly metaphorical, and this form of discussion will not be elaborated upon just yet.

The aim of this section has been to provide a working definition of a cultural "gene" and a cultural "individual". The cultural equivalent of a gene is the "meme", the psychobiology of which was extensively discussed in the last section of the previous chapter, under the heading of "The Strange DNA

of the Cultural Virus". Some possible mechanisms for the means by which novel memes arise, which could be called models of memetic mutation, were also discussed in this part of the previous chapter.

The cultural individual is then defined as a single viral phenomenon, which consists of one or more memes (one or more of Edelman's "classification couples" involving pairs of neuronal groups, see Sections 9.3 and 9.4), along with their extended phenotype of behavioural, emotional, and artefact products (or its total set of potential effects on the human body). The subtle difference in the cultural "level" focused upon by the Meme and Virus positions (equivalent to the genome versus whole organism distinction) is thus perfectly complementary.

Another subtle difference has also been discussed above. This is the difference in the replicative terminology currently employed by the two positions. While the virus employs terms which reflect not only passivity, but pronounced "environmental agency" in the psychological context of ideas, in terms such as "replicatee", and indeed the term "viral phenomenon" itself, the meme position uses terms such "replicator" and "self reproduction". Once again, it is stressed that in many senses such minor differences in terminology may be purely semantic, we shall see in the next section that these mild differences in terminology have lead to correspondingly different sets of "hypothetical examples" of what are essentially parallel principles. Meme examples have more commonly explored cases where human individuals are the passive, or at least relatively inactive, recipients of new memes, rather like the ecological relationships between wild animals and wild parasites, or between humans and microbial parasites (see Ball 1984:146–153 for an excellent example of this focus). On the other hand the Virus perspective, as we shall see below, more explicitly incorporates situations where individuals plan and manipulate to the best of their abilities, deliberately attempting to control the viral phenomena they receive, and deliberating colonising other individuals with ideas, or withholding them from others, rather like the relationship between humans and their animal and plant domesticates. But the two areas of cultural transmission are really just two ends of the same continuum (as,

indeed, are wild and domestic animals, see references to Rindos 1980 and 1984 above), running from the most "conscious" forms of teaching and learning to the most "unconscious" and imitative forms. In any case, the two sets of examples have a substantial overlap, as we shall see below; the difference which I aim trying to describe is really just a quantitative difference in the amount of "conscious" input of human agency emerging in the illustrative examples employed by each approach. Thus the two sets of favoured examples, then, like the two levels of cultural systems considered, are perfectly complementary.

So *both* minor differences between notions of memes and viral phenomena are complementary facets of the same coin. The difference between the entities themselves (memes and viral phenomena) is equivalent to the difference between the genetic level, and the larger combined genotypic/phenotypic (whole organism) level of organismic systems. And the difference between scenarios involving different degrees of intentional human agency merely reflect the fact that cultural transmission itself involves varying degrees of human intentionality. In the next section, a series of meme explanations of cultural phenomena are discussed, and it is shown how Dawkins has tended to favour explanations which involve reasonably low levels of deliberate human intervention. A series of highly "intentional" viral alternatives are then discussed, and it is concluded that highly deliberate, goal oriented thinking on the part of a human host provides no guarantee that a viral phenomenon will not continue to exploit the situation. In other words, the tendency toward simple and elegant explanations in the meme discourse is to be commended; but the aim of the alternative permutations offered from the point of view of the viral discourse is to show that even the most convoluted attempts by people to exert conscious control over memetic replication can be explained in terms of "cultural symbiont" principles.

10.2 Memetic Hypotheticals

As suggested above, some of the illustrative examples employed in the meme discourse explore the potential for ideas to be successfully replicated by people who have no deliberate intention of doing so, or, for that matter, without any explicit discussion of deliberate intentions to do anything else either. A good example of this kind is the Dawkins explanation of the reproductive success of an erroneous version of the song "Auld Lang Syne". The original refrain, written by Burns, was sung as a four word phrase "For *auld land syne". However, it is now frequently sung as "For the sake of auld* lang syne" (Dawkins 1989:323). The challenge is to explain why the new "mutant" addition to the original four word phrase has greater fitness than the parent meme in certain contexts. Since the primary template for transmission is the singing of the song on New Year's Eve, not Burns's original score, the "for the sake of" addition has presumably arisen via some memetic mutation, and then spread so as to become the most common variant in the meme pool (Dawkins 1989:324). Roomfuls of guests can now be heard singing the mutant version in confident synchrony, for all the world as if it were the authentic original.

Dawkins's explanation of the fitness of the mutant "For (the sake of) auld lang syne" meme involves the "fit" of certain consonant sounds such as "s" and "k" to the peripheral nervous system. The presence of these particular consonants in the mutant form of the refrain make it more audible than the original, thereby catching the ears of someone who is unsure of the words, as they strain to pick them out from the general noise:

> "The sibilant "s" is notoriously obtrusive. Church choirs are drilled to pronounce "s" sounds as lightly as possible, otherwise the whole church echoes with hissing. A murmuring priest at the altar of a great cathedral can sometimes be heard, from the back of a nave, only as a sporadic sussuration (*sic*) of "s"s. The other consonant in "sake", "k", is almost as penetrating. Imagine that nineteen people are correctly singing "For auld lang syne" and one person, somewhere in the room, slips in the erroneous "For the sake of auld lang syne". A child, hearing the song for the first time, is eager to join in but uncertain of the words. Although almost everybody is singing "For auld lang syne", the hiss of an "s" and the cut of a "k" force their way into the

child's ears, and when the refrain comes round again he too sings "For the sake of auld lang syne"." (Dawkins 1989:324).

Here it may be seen that the intrinsic qualities of the meme form the basic agency, driving its reproductive success. The argument is very convincing, and processes like this must abound in cultural systems. But such scenarios cannot be the only mechanisms, or languages would tend to evolve toward a predominance of "s"s and "k"s. Nonetheless, it is an important aspect of memetic fitness to explore. Dawkins goes on to explain the fitness of an erroneous version of James Thompson's song "Rule Britannia" with a similar argument, using the notion of the improved "sensory prominence" of the new form in relation to the constraints of those parts of our peripheral and central nervous system responsible for hearing. In the original version the correct second line is "Britannia, rule the waves", and its meaning was presumably the imperative "Britannia, go out and rule the waves" (Dawkins 1989:324). However, a mutant form has emerged where "rule" is changed to "rules", becoming "Britannia rules the waves", substantially changing the meaning of the original line. The new meaning is indicative, stating the "fact" that Britain currently rules the waves. In this case, however, Dawkins explains fitness as a combination of the improved "meaning" of the mutant meme, as well as employing the "sensory prominence" theme as I term it:

> "Here the insistently hissing "s" of the meme is aided by an additional factor. The intended meaning of the poet (James Thompson) was presumably imperative (Britannia, go out and rule the waves!) or possibly subjunctive (let Britannia rule the waves). But it is superficially easier to misunderstand the sentence as indicative (Britannia, as a matter of fact, does rule the waves). This mutant meme, then, has two separate survival values over the original form that it replaced: it sounds more conspicuous and it is easier to understand." (Dawkins 1989:324).

From a virus perspective this explanation would seem to overemphasise the unconscious elements of the equation. All sorts of emotional and aesthetic factors must also affect the fitness of ideas. The fact that "Rule Britannia" is traditionally sung on the last night of the "Proms" makes it even more likely that the indicative mutant possesses special aesthetic appeal. The upsurge of patriotic feeling which occurs on this night of the British calendar sustains a milieu in which assertions of British power and superiority are most likely to arouse the greatest acclaim, and the least condemnation (see next section

for more elaborate "virus" hypotheticals). Factors of this kind are, however, not featured by Dawkins, although I am sure that he would not discount them, as some indication of the relevance of such factors is given in the recognition of the role that the potential meaning of a phrase may play in its psychological fitness.

The theme of the unintentional or unconscious aspects of psychological appeal emerge elsewhere in Dawkins's examples, such as his description of the spread of erroneous bibliographic information in academic research. In 1964, Hamilton wrote a pair of highly influential papers under the title "The genetical evolution of social behaviour". But a decade or so later, a number of publications such as Dawkins's *The Selfish Gene*, and Wilson's *Sociobiology*, mistakenly cited the papers under the erroneous title of "The genetical *theory* of social behaviour" (Dawkins 1989:328). Dawkins goes on to show how they both arrived at the same error independently, and the challenge becomes one of why the same mutant meme could become fixed in the minds of two different biologists. His explanation involves the similarity of the incorrect Hamilton title to the title of another important publication:

> "How could such a coincidence have happened? Once again, as in the case of "Auld Lang Syne", a plausible explanation is not far to seek. R.A. Fisher's most famous book is called *The Genetical Theory of Natural Selection*. Such a household name has this title become in the world of evolutionary biologists, it is hard for us to hear its first two words without automatically adding the third. I suspect that both Wilson and I must have done just that." (Dawkins 1989:329).

This kind of selection can presumably be considered to be a kind of "imitational camouflage" on the part of a meme. Imitation of one organism by another is a common form of camouflage. For instance, certain very edible butterflies are able to repel predators through their similarity with other, poisonous butterflies. Natural Selection (in the form of a predator) cannot "see" the edibility (the potential for elimination) which is actually there in the butterfly, since it does not look like an edible insect ought to look. Similarly, the mutant Hamilton title is being selected due to its resemblance to another title altogether. It fools our "checking system" since although it is "strange" or "wrong" in relation to the original Hamilton title, it is "familiar" in relation

to an even more famous one. Its erroneous nature is concealed by its resemblance to another title altogether. Yet it is clear that on both occasions the selection of the erroneous title was largely inadvertent, since Dawkins and Wilson would naturally want their bibliographic information to be as accurate as possible.

So it can be seen that theme of the self-propagating meme, ie. propagation without any special role for deliberate human agency, occurs again and again. Ball has extended this theme to its logical conclusion, remarking "that civilisation or culture is only a meme's way of propagating and perpetuating itself" (Ball 1984:153). The "sensory prominence" argument, as a special case of this kind of explanation, is a particularly passive way to pick up a meme. "Sensory prominence" arguments are not so pervasive in Dawkins's explanations of the fitness of certain "meme complexes" involved in religious systems. Yet the kinds of cultural transmission discussed are still relatively "unwitting".

Consider the meme complex associated with some forms of Christianity. Dawkins focuses on five different elements of this meme complex, under the nouns "God", "immortality", "hellfire", "faith" and "celibacy". Speculative explanations are offered for the psychological factors which increase the fitness of these various memes, and each meme is one of the factors which increases the appeal of each of the others:

> "Consider the idea of God....What is it about the idea of a god that gives it its stability and penetrance in the cultural environment? The survival of a god meme in the meme pool results from its great psychological appeal. It provides a superficially plausible answer to deep and troubling questions about existence. It suggests that injustices in this world will be rectified in the next. The "everlasting arms" hold out a cushion against our own inadequacies which, like a doctors placebo, is none the less effective for being imaginary. These are some of the reasons why the idea of God is copied so readily by successive generations of individual brains. God exists, if only in the form of a meme with high survival value, or infective power, in the environment provided by human culture." (Dawkins 1989:193).

Here we see how two memes can increase each other's appeal. The idea of God and the idea of immortality are mutually supporting, such that someone

who feels convinced of the value of one meme is more likely to be receptive to the other. Dawkins has not relied upon any "sensory prominence" arguments, and speculates about the nature of appeal. The theme emphasised is that of the appeal an idea has for a potential recipient; why someone would want to *learn* an idea, and then to hold on to it. Dawkins has not, however, chosen to speculate about the roles that the conscious attempts by priests and other religious people to "convert" new individuals play in the process, nor the reasons why some people may have a strong desire to do this, possible additional factors which will be discussed in the next section in a which a series of "viral permutations" of Dawkins's hypotheticals will be presented. The main elements contributing to psychological appeal in Dawkins's meme examples so far are the intrinsic qualities of the meme itself, and its phenotypic manifestations, rather than all the emotional, intentional, and aesthetic factors which make up the meme's psychological niche.

This theme is pursued when Dawkins moves on to other elements in the Christian meme complex. He discusses another idea which occurs in a variety of contexts within various Christian denominations, the concept of hellfire. Hellfire is preached as a potential punishment for those who do not observe Christian principles in their daily lives, and submitted as a contrast to the reward of eternal life, which is granted to those who do behave as requested. There are thus two reasons for observing the Christian code; the hope of attaining ever lasting life in Heaven, and avoiding everlasting damnation in hell. Some may laugh at such notions, but the idea of hellfire has caused many people a great deal of psychological distress through the ages (Dawkins 1989:197). It also appears that fear of punishment in hell is a significant factor in the maintenance of belief in some Christian meme complexes, and that people may actually continue to observe religious concepts precisely for this

reason. Dawkins considers the possibility that priests may have actually designed the concept of hellfire to fulfil this role, but once again rejects the role of conscious factors in favour of the theme of intrinsic qualities of the hellfire concept, and its ability to *self* replicate, almost like genes capable of meiotic drive, where one gene at a particular locus is able to become represented at that locus in more than 50% (the usual number) of the gametes produced during meiosis (Dawkins 1989:235). The theme of memes being gene-like *replicators* rather than virus-like replicatees, spreading without any conscious intervention on the part of the minds they occupy, is quite explicit:

> "...the threat of hellfire...is a particularly nasty technique of persuasion...But it is highly effective. It might almost have been planned deliberately by a machiavellian priesthood trained in deep psychological indoctrination techniques. However, I doubt if the priests were that clever. Much more probably, unconscious memes have ensured their own survival by virtue of those same qualities of pseudo-ruthlessness that successful genes display. The idea of hellfire is, quite simply, self perpetuating, because of its own deep psychological impact. It has become linked with the god meme because the two reinforce each other, and assist each other's survival in the meme pool." (Dawkins 1989:198).

The next part of the religious meme complex discussed is the idea of "blind faith", and the significance of the tale of Doubting Thomas. This "faith" meme has the effect of protecting both itself and other memes through limiting the exposure of the religious meme complex to sensory verification. If the human individual whose mind a meme occupies were to look for evidence there would always be the danger that certain information would be found which refuted that meme. Nothing is more lethal to memes held purely on faith than a willingness to seriously consider the views of others, or to submit the idea to any sort of testing procedure. The "faith" meme promotes its social transmission from person to person through "the simple unconscious expedient of discouraging rational enquiry" (Dawkins 1989:198), as it even discourages rational enquiry into the phenomenon of "faith" itself. Once acquired, it is thus able to protect itself and the memes to which it is attached.

The final meme of the religious complex discussed involves the phenomenon of celibacy. The psychological appeal of this particular meme is

judged as being concerned with the fact that reproduction tends to distract energy away from the religious meme complex, toward the needs of the family (rather like the workaholic viral phenomenon of Chapter 9). Here Dawkins uses an explanatory structure which comes closest to the themes which are of special interest in the Cultural Virus Theory, as a certain amount of conscious intent on behalf of the priests is definitely implied:

> "The meme for celibacy is transmitted by priests to young boys who have not yet decided what they want to do with their lives. The medium of transmission is human influence of various kinds, the spoken and written word, personal example and so on. Suppose, for the sake of argument, it happened to be the case that marriage weakened the power of the priest to influence his flock, say because it occupied a large proportion of his time and attention. This has, indeed, been advanced as an official reason for the enforcement of celibacy among priests." (Dawkins 1989:199).

Dawkins proposes that such meme complexes evolve in much the same way that co-adapted gene complexes evolve in gene pools. Evolution in meme pools over a period of time comes to favour certain memes which are adapted to the environment of the memes. Part of the environment of each meme is all the other memes in the meme pool, and so the memes are selected on the grounds of their "fit" to other memes which have already been selected, memes which in turn are now more fit than before because of the presence of the new memes.

In sum, it can be seen that there is a tendency in the variety of illustrative examples employed by Dawkins for a focus upon situations where memes spread as a result of unconscious factors of human thought or action. In the next section, it is shown how the possibilities for memetic transmission can also encompass an immense range of deliberate and intentional acts of cultural transmission, and a wide range of other "intentional" aspects, within a virus framework, without violating any basic neo-Darwinian principles.

10.3 Virus Hypotheticals, People, and the Jungle of Human Sentiment

While the meme discourse has focused on examples of the kind outlined above, the virus discourse has contemplated the possibility of applying cultural parasite/symbiont principles to the most "conscious" and "intentional" aspects of individual psychology; in short, the aspects of human affairs which are normally considered to be most problematic for a parasitic interpretation. In order to illustrate this difference between the foci of interest in the two perspectives, I will now propose some possible alternative explanations of Dawkins's hypotheticals in which human intent is brought into greater prominence, not only in order to illustrate that it is possible to do so, but rather to indicate that such "motive driven" alternatives can be constructed in such a way as to be every bit as neo-Darwinian as the more unconscious examples which occur in the meme discourse. This exercise will highlight the significance of the virus position's preference for the term "replicatee" and "viral phenomenon" over the meme position's term "replicator". Memes are not *capable of physically assembling copies of themselves; and they are therefore* actually better considered as resembling viral phenomena, and not true self-replicators. To put it another way, it appears that in some sense memetic material occurs only in a form analogous to RNA, and not DNA. Despite an interesting and sophisticated discussion by Ball (1984:155-158) of the critique of the meme concept by Midgeley, Bonner, and Lumsden and Wilson, which points out that memes, unlike genes, are not independent replicators, he ultimately asserts equivalence on the grounds that genes are not wholly independent either. Despite the fact that he even mentions RNA viruses in this very section, he does not focus upon the fact that retroviruses are somewhat less of a true "replicator" and more of a "replicatee". I prefer to defend culture from this direction; I accept that genes are independent replicators, and that cultural phenomena are not. However, as already argued extensively in the last chapter, there are special kinds of parasites (ie. retroviruses) *which have no independent replicating material either.* The key to silencing the meme critics is, therefore, not to argue that *genes* are not independent replicators (as they are capable of replication outside the organism, such as in a test tube, without any other genetic material being present), but rather,

to argue that *retroviruses* are not independent replicators, as these require the presence of host DNA in their environment.

This is where the terminology of the virus perspective has advantages on the "independent replicator" terminology of the meme discourse. The analogy with actual viruses fits with remarkable precision. When microbial retroviruses parasitise cells, they hijack cellular machinery which has successfully survived due to its capacity to produce a range of "desirable" products, which generally serve the "purpose" of promoting organismic survival and reproduction. Let us then "personify" the cell for the purposes of the discussion. The cell can be imagined as "trying to achieve" something, which we could define as something not too dissimilar to its part in the overall purpose of the organ in which it resides (of course it is only blindly performing these tasks, but it is not stretching the metaphor too far to speak of many of these tasks as functions or goals). Liver cells are trying to achieve a set of goals which could be crudely summarised under the label "liverness", kidney cells are struggling to achieve "kidneyness", brain cells "brainness", and so on. Presumably, continuing the metaphor, a normal cell is still "trying" to achieve its original "task" or "function", even when it has come to contain a virus, and either failing or partially failing in its attempts to do so. It is logical to suppose that viruses have evolved to "cash in" on the various task related chemical assembly lines of the cell, diverting existing processes away from their intended objectives toward making viruses. Each adaptation could be expected to be highly specific to certain "goal structures" in the cell. This would be far more economical than trying to establish their own assembly lines from scratch. In other words, it is fair to suppose that a virus is actually adapting, first and foremost, to a chemical environment criss-crossed with all sorts of "intentional" and "teleological" processes, and that the presence of such ordered, intentional and even, in the case of cultural phenomena, emotional environmental features, are actually crucial to its survival.

Thus the words "virus", "viral", and "replicatee" not only suggest a lack of intrinsic agencies in the phenomena concerned (qualities which DNA

also lacks in any case, apart from its self-replicating capacity), but also suggest the possibility of, and even the necessity for, *considerable* agency in the *environment* of the said phenomena. The word virus thus implies a relatively inactive phenomenon selected for its capacity to passively benefit from a highly "active" environment full of all sorts of agencies. Rather than adapting in such a way as to develop its own structures of agency, it has adapted directly to the agency structures of other phenomena. A viral phenomenon is not a conscious, purposeful agent; but a viral phenomenon which possess qualities which incite the agency of other phenomena or organisms is more likely to be replicated than one which cannot.

Such "replicatee" premises lead to scenarios where human intention is featured (for example, see the discussion of the phenomenon of "workaholism" in Cullen 1992b and Section 9.2; and teaching institutions, Cullen 1990:65; and also below), rather than merely implied or even ignored as tends to be the case in the meme discourse. This can best be illustrated by exploring a number of Cultural Virus Theory permutations of Dawkins's meme examples, and showing the potential for all sorts of highly "intentional" alternatives to the basically unconscious scenarios he employs. The whole point of this section is to show that the virus concept in no way excludes the possibility that the psychological landscape to which ideas adapt might be conditioned by all sorts of genetically emergent but open ended "Darwinian algorithms" of the kind articulated by recent "Darwinian psychology" (Mithen 1989, 1993; Cosmides and Tooby 1987). Rather, such algorithms may provide highly specialised symbolic niches for which different culturally transmitted phenomena may compete.

The interesting implication which emerges from the exploration of a range of more "viral" alternatives to the Dawkins hypotheticals is that *no amount of human interference* can really violate the selective principles of cultural transmission. No matter how much "intent" we attribute to our hypothetical actors, the same principles ultimately remain. Far from denying the existence of people and their intentions, such phenomena become absolutely crucial, as some of the most important features of the psychological environments to which populations of viral phenomena adapt.

So if we follow the lead of the words "replicatee" or "viral phenomenon", the task is not to find the few cases of cultural transmission where deliberate human agency is either negligible or absent; the real problem is to attack the vast majority of deliberate transmissions, and to show that such cases actually exemplify, rather than threaten, a parasitic or symbiont perspective.

Let us begin with the phenomenon of the erroneous version of Burns's "Auld Land Syne". As described in greater detail above, the correct line of the refrain, "For auld lang syne", becomes the longer "For (the sake of) auld lang syne" in the mutant version of the song. The English phrase of "the sake of" has been added to the original idiomatic Scots expression "auld lang syne" which translates literally as "old long since", and means something like "days long ago". The Dawkins explanation has been discussed above, and the theme emphasised was that of the acoustic prominence of certain consonants such as "s" and "k", which render the erroneous version more audible than the original. The mutant meme is spread without any necessary knowledge of the participants, and the possibilities for any deliberate or manipulative forms of cultural transmission are not explored.

Yet when more "intentional" scenarios are explored within a viral perspective, any perceived threat to a neo-Darwinian interpretation can be quickly deconstructed. No amount of deliberate intervention on the part of the hosts of the "Auld Lang Syne" phenomenon - the people who are singing it - ever corrupts the fundamentally Darwinian nature of the underlying processes involved. For instance, it is conceivable that if an adult was trying to teach a child how to sing the song, they might deliberately teach the mutant version instead of the original, thinking that the addition of the English phrase "the sake of" would make the words more meaningful to the child. To most people, whether they be adults or children, the meaning of the words "auld lang syne" (old long since) would presumably be rather obscure. The "the sake of" addition makes "auld lang syne" appear to be the mysterious subject of an otherwise perfectly comprehensible sentence. But such deliberate intervention in the cultural transmission process on the part

of the adult does not challenge any aspect of the Virus framework. The adult is actually surmising about the relative fitness of the two versions in the "local environment" represented by the mind of the child, and then deliberately choosing to replicate one instead of the other. The model that the adult has of the particular child's aesthetics constitute a set of local psychological "conditions" which temporarily raise the fitness of the erroneous version in that part of the adult's mind. The desire of the adult to choose a version which the child will understand need not be viewed as an intentional subversion of memetic fitness; rather, the presence or absence of such desires in a given mind define and thereby change the local parameters of memetic fitness in the first place. Thus the fact that the post-processualist's "context" (Hodder 1992:14) affects the fitness of the viral phenomenon only indicates that the social context is an integral part of the environment which it inhabits. It does not indicate that something "other" than the environment has entered the equation.

Alternatively, let us suppose, that during an evening speech at an annual club dinner held near the time of New Year's Eve, an influential personality who was an expert on Burns's songs decided to speak about the two versions of Auld Lang Syne, and to explain which was the right version, and which was the wrong. Suppose that the speaker concerned considers that the "correct" version can only be defined in terms of the original score; and that he or she follows the somewhat conservative ethic that the original version is always the best. Despite the fact that the "majority" now consider the mutant form to be correct, our imaginary speaker has no qualms in informing anyone of their error. He or she is a self confident moral crusader for "accuracy" and "truth", and considers the mutant version of the song to be an insult to Burns. The audience might well be comprehensively convinced by such a person, and at the next New Year's Eve everyone might then sing the correct version in confident synchrony. The speaker feels a sense of satisfaction in being able to "rectify" what he or she felt to be a sad state of affairs, restoring the correct version to its rightful place, and condemning the erroneous version to the scrap heap. The two song versions are merely passive replicatees in this sequence of events, and, moreover, it is clear that

the speaker is deliberately and intentionally manipulating the circumstances of cultural transmission so as to favour one version over the other.

But no reversal of the "Darwinian order" has occurred. The influential after dinner speaker has not forced the "less fit" version to prevail at the expense of the "fitter" mutant. Rather, the relative fitness of the two phenomena has been altered by the sudden appearance and propagation of other memes in their local psychological environment. The speaker has achieved this through increasing the knowledge of the audience with respect to the two phenomena, and the mutant phenomenon begins to find it more and more difficult to compete with the "original" "Auld Lang Syne" in the enriched and more knowledgeable milieu in which it now finds itself. The speaker is in a position to alter the psychological environment of the ideas through the cultural transmission which occurs during the after dinner speech. Whether the speaker has a conscious intention to alter the knowledge of the listeners or not is irrelevant to the fact that the basic means of doing this is to alter the "environment" of the two phenomena in question, rather than the phenomena themselves.

Let us envisage a final permutation of the "Auld Land Syne" phenomenon where an attempt is made to deliberately influence the course of cultural transmission, only in the opposite direction. Suppose that in the audience of the above speaker, there is someone else who is also called upon for the odd after dinner speech themselves, and that this person views the political persuasion of the current speaker to be too far to the "right". In fact, let us imagine that this listener finds the whole idea that the original version is the "best" and the most "correct" to be arrogant and offensive, and an insult to his or her egalitarian principles. He or she is a linguistics graduate, and an editor of a respectable dictionary, and feels that it is not antiquity but usage which defines what is "correct". If the mutant version now holds sway, then that is the way that "Auld Lang Syne" is now sung, and as far as our second speaker is concerned, that is that. In order to redress this injustice, the second speaker resolves to expound the principle of "majority rules" and the virtues of what really ought to be called the "new"

rather than the "erroneous" Auld Lang Syne, at their next guest speaking appointment.

But once again the Virus principles would not be subverted, even if our second speaker was able to fulfil his or her aim. Even though the mutant "Auld Lang Syne" has only spread as a result of the intentional and highly directed "agency" of the leftist guest speaker, it has spread none the less. The fact that it has been a wholly passive "replicatee" does not present any major barrier to an explanation which employs a neo-Darwinian perspective. The mutant version appeals to its host precisely because it is different to the original, and these differences make it seem more appealing to someone whose morality resembles that of the second speaker. It has been spread as a direct result of such differences. That the successful spreading of the mutant version required, in this case, the presence of a highly motivated person with leftist political leanings, does not present a challenge to neo-Darwinian principles. A given viral phenomenon's requirement for the presence of particular kinds of people with a social or political agenda in its milieu is no different to a cow's need for grass, or a lion's need for wildebeest.

Thus the viral replicatee concept frees the cultural parasite perspective from any dependence on or preference for "unconscious", "insidious" or "meme driven" scenarios. Human intentions and individual identities of all sorts can be explicitly incorporated into the perspective as crucial environmental phenomena, without which few instances of cultural transmission could be understood or satisfactorily explained. When "people" are relegated to the environment in this way, they are not being dismissed or discarded, but quite the opposite. Once the ecology of an artefact or idea, its Darwinian environment, is constituted not from the landscape at large but the social and symbolic milieu, the concept of cultural fitness becomes highly contextual. No longer are we speaking of the "survival of the fittest" (or rather, "fitter") in the rainforest or desert, but "survival of the meaningful" in a jungle of human sentiment. This kind of Darwinian construction can easily accommodate concepts like postmodernist contextualism (Engelstad 1991, di Leonardo 1993, Hodder 1992, Miller 1993, Shanks and Tilley 1987a, 1987b) or Geertz's thick

description (Geertz 1973). As others such as Maschner and Mithen (1993) have remarked, post-processual and cultural selectionist approaches have much in common.

People are thus far from dismissed in a virus approach. The (very human, and very cultural) environment and the individual phenomenon are of equal importance in Cultural Virus Theory, as indeed they are, for that matter, in neo-Darwinian theory in general. Indeed, it is usually the case that any one phenomenon will not only be selected by other phenomena (play the part of a selectee), but also exert selective pressures on other phenomena (play the part of a selector).

But the virus concept also suggests a starting point for a comprehensive explanation of another area of cultural systems which appear to challenge the parasitic perspective. This is the pervasive occurrence of ideas, behaviours, and artefacts which are not apparently used for cultural transmission, and which appear to have zero fitness in the cultural sense. I believe that large numbers of this kind of phenomenon can be explained through evoking cultural equivalents of the general neo-Darwinian concept of genetically selfish (which would become "memetically selfish") altruism. Complex altruism and their attendant social structures would presumably not be possible in microbial viruses. Yet, as will be discussed in the next section, the viral phenomena of culture are able to "borrow" the complicated social structures of human communities in such a way as to achieve "societies" based on memetically selfish altruism.

10.4 Eusocial Cultural Phenomena

While the main explanatory theme of Dawkins's meme examples involved the unconscious effect of certain memes on their hosts, the above section showed how the "Auld Lang Syne" meme hypothetical could form the basis of a whole range of more "viral" permutations where deliberate strategies of

cultural transmission on the part of individual people were involved. The aim of the section was to show how the notion of the passive "replicatee" status of cultural phenomena, explored within the virus perspective, can supply a whole range of scenarios of deliberate human intervention which are no less neo-Darwinian than the more unconscious factors emphasised in the meme examples.

Dawkins's religious meme complex of God, immortality, celibacy, hellfire and faith was also discussed a little earlier, and once again it was seen that explanatory factors which involved deliberate machiavellian or benevolent factors were avoided wherever possible. For example, Dawkins emphasised that the idea of hellfire "ensured its own survival", and that it was "self-perpetuating, because of its own deep psychological impact" (Dawkins 1989:198). Dawkins's discussion of the unconscious factors involved in the cultural fitness of memes was interesting, but it is important to note again that a virus perspective does not require "unconscious self perpetuation" in order to work as an explanatory framework. Rather, as already discussed with reference to "Auld Lang Syne", a framework based on the virus concept often implies and at times even requires the presence of individual people who are struggling to carry out all sorts of conscious and unconscious goals, regardless of whether these goals are perceived as "rational" or "irrational" by an observer.

The theme of personal intentions and goal oriented behaviour, made explicit in the virus perspective, will now be explored with reference to Dawkins's religious meme complex, in the hope of illustrating once again how these factors can be integrated under the neo-Darwinian umbrella using a virus perspective.

Let us suppose, for example, that Dawkins's "self-perpetuation" explanation of the hellfire concept proved at some later date not to be true, and that a general policy of machiavellian intervention on the part of the priests who spread the hellfire concept began to look more and more likely. The real strength of the virus "replicatee" concept is that if such were the

case, the perspective could expand to accommodate the new facts, with the "machiavellian intentions" of the priests being classified as a suite of local psychological phenomena acting as selective criteria for the hellfire phenomenon, which the priests then spread.

Advancing a fully fledged and more obviously "viral" alternative to the Dawkins's meme complex hypothetical will require, however, the introduction of the concept of "cultural eusociality" (Cullen 1990:65). This is where the complicated interplay of both intentional and unintentional human behaviour with assemblages of viral phenomena produces a cultural equivalent of the phenomenon of eusociality in organisms (Dawkins 1989:174). Most biological examples of this phenomenon occur in social insects such as the hymenoptera (ants and bees), although quite recently (see Brett 1986, in Dawkins 1989) a case of this kind of social structure was found amongst *Hydrocephalus glaber*, the naked mole rat of Kenya. Such societies are usually founded by a single male/female couple, or by a fertilised female, which then begins to produce offspring. But the offspring produced do not develop into replicas of their parents; instead, they grow into sterile individuals (such as "workers", "soldiers" and "nurses") which work exclusively toward the general well being of the close kin community. Such individuals increase the resources and facilities available to their parent (the queen) who only then begins to produce a percentage of fertile offspring (young queens and drones) which go off to found other hives. This is perfectly in keeping with neo-Darwinian principles, as in helping their mother to produce sisters and brothers the non-reproductive workers reproduce their genes as effectively, or even more effectively than the queen herself (Dawkins 1989:175). The behaviour of workers is thus perfectly altruistic as far as each individual is concerned, but perfectly selfish in terms of genetic survival.

When I first began to develop the virus perspective in the early 1980s other academics would often point out a range of challenging examples where ideas, artefacts or behaviour patterns were being used to human benefit, without any apparent cultural transmission of the phenomenon occurring as a result, apparently refuting the virus hypothesis. Such phenomena are

extremely numerous, and examples would include cassette tapes or records which are played and played without ever being reproduced in the process, the horse shoe which is used until worn out, and then discarded, the exploitation of secret knowledge by an individual who never tells anyone else, and many others.

It was quite possible that many such phenomena were simply in the process of becoming extinct. But it occurred to me that these phenomena, while not functioning as reproductives in their own right, might actually be increasing the fitness of their "cultural relatives" through their existence. This would explain why, for example, a stables might discard every horse shoe they ever used for centuries, and never make any of their own, while at the very same time horse shoes in general could be surviving very well. There might be a whole range non-reproductive "worker" cultural phenomena, distributed throughout human communities, which through their functional or aesthetic appeal increase the reproductive success of the "master" copy from which they were originally made, while in the meantime these "worker artefacts" convey a general impression of perfect "altruism" (in the Darwinian sense).

Complicated networks of social interaction and stated and unstated goals set up a rich and complicated psychological environment, a "jungle of human sentiment" (Cullen 1993a), in which equally complicated patterns of viral exploitation can emerge. If single structures can survive through their capacity to exploit this environment, then there is no reason why a whole complex of phenomena - which generates not one phenotypic expression, but a range of different expressions - cannot also be successful.

This is one particular set of biocultural analogies in Cultural Virus Theory which appear to have no equivalent in the Meme discourse so far. A related element which also seems to be absent in the meme discourse is the development of cultural virus equivalents of the general notion of genetically "selfish" altruism, and kin selection, of which the eusocial phenomenon is a special case. As Dawkins points out in his principal research area, genetic

process, all parental investments in offspring represent a ubiquitous kind of *genetic* kin selection (Dawkins 1989:108). Perhaps "selfish" altruism is equally ubiquitous in *cultural* systems. But to what existing psychological factors could populations of cultural phenomena adapt in this way? The beginnings of the answer could lie in the capacity of viral phenomena to hijack systems of vague, inchoate human instincts which originally evolved due to their capacity to produce genetically selfish altruistic behaviour. Say, for example, there was a human tendency to invest more energy in helping those which they perceived to be similar to themselves. It is easy to imagine the genetic advantages which might result from such behaviour; this tendency might produce behaviour which while detracting from the reproductive opportunities of the actor, might in fact increase the number of reproductive opportunities available to the individual's genetic kin. This would only be effective under social conditions where a similar person would tend *to be* a relative. When the individual is removed from the close kin community, the possibility for cultural exploitation of these tendencies emerges. If an idea can entwine itself with an individual's sense of self to the point at which that person considers that idea to be a fundamental part of their identity, it might also lead that person to define similar others (potential kin) on the basis of their acceptance of that idea.

However at this point care must be taken to coherently translate concepts of altruism into purely cultural analogues, and to avoid becoming bogged down in some form of biocultural confusion. *Culturally* selfish altruism must involve the sacrifice by a cultural phenomenon of the opportunity to culturally reproduce, not the sacrifice by a human individual of an opportunity to genetically reproduce. We are not looking for human martyrs who sacrifice life or reproduction for cultural advancement, but for cultural structures which remain silent or untransmitted in order that other copies of that structure are transmitted.

So, for example, one might envisage a teaching establishment such as a school or university as a vast hive of culturally transmitted viral behaviours and beliefs. The vast majority of copies of a department's concepts reside in

alumni which leave the institution and never become directly involved with teaching. Their copies remain as silent or "sterile" phenomena which "work", or aid a given individual and his or her family to live off the proceeds of a particular profession, rather than acting as a template for cultural transmission. The principal way in which they can affect the next generation of copies of their particular enculturated state is by demonstrating its value to other people who are then attracted to the master copy back at the original institution, the "queen" neuronal structure, which is exclusively "taught from" rather than used. The existence of a "queen" viral phenomenon is made possible through the attraction of people who identify closely with the idea, who then "tend" or "farm" the concept in the workplace. Music recordings constitute another example; millions of recordings, most of which will never become primary reproducing templates themselves, are produced by a recording company. These constitute a vast assemblage of sterile "worker" viral phenomena which indirectly affect the likelihood of continued preservation of a master copy back at the company, and ultimately divert energy back to the "queen of queens" residing in the mind of the original musician. Some recordings have actual sterility features, such as the anti-recording devices built into cassettes (or, for that matter, computer software). If record sales are really successful, they will enable the survival of an elite lineage of "queen" recordings, each reproduced from the one before, and not from the damaged copies which have been continually played. The more successful the worker recordings, the more resources available to the original musician to produce recordings of new songs. Furthermore, if all master copies are lost, then a new master copy may emerge from the rank and file, competing with other copies *viz a viz* prevailing enculturated definitions of "authenticity". Alternatively, the recordings might well have reproduced themselves quite successfully into the minds of the people who played them. This does not change the fact that the cassette or record itself is not employed in the making of new cassettes or records, and is therefore a genuinely "sterile" cultural phenomenon.

But the "eusocial virus" idea might also provide an alternative scenario to Dawkins's religious meme complex hypothetical. I stress that it is not my

intention to carry out any sort of in depth scholarly analysis of the history of patterns of cultural transmission in the Christian church. The discussion which now follows is purely hypothetical, intuitive, and it is employed for heuristic purposes only. All of the following assertions would have to be verified with sociological or ethnographic research before they could be considered reliable representations of actual cultural systems.

Yet it is conceivable that the viral complex of God, hellfire, immortality, celibacy and faith might not manifest itself as a series of identical phenomena which function in the same way in every mind they occupy. Instead, like bees, ants, and mole rats, religious concepts may occur in a whole range of reproductive and non-reproductive forms, which persist in history due to their tendency to facilitate the cultural transmission of the reproductive forms. Their existence might work altruistically toward the good of the Church, and toward the general well being of the people who maintain them, increasing the wealth and resources available to the reproductive viral phenomena residing in the minds of priests.

So one of the main distinctions in the eusocial order of the viral phenomena of religions would presumably be that between the ideas in the minds of priests and the ideas in the minds of the congregation. There is the central line of cultural descent from priest to acolyte, and the knowledge transmitted down this line consists of a vast viral assemblage of interrelated ideas which takes years to successfully reproduce to a satisfactory extent. Replication of this sort would consist of the kind of learning normally involved in obtaining a theology degree. This main line of descent could be called the "queen" viral assemblage, and if this were all that existed, and priests never preached to anyone but future priests, then its strategy of survival would not be fundamentally different to that of smaller ideas such as "Auld Lang Syne". As in the case of "Auld Lang Syne", the cultural fitness of concepts such as "hellfire" and various ideas about the nature of "God" could well involve more than the various unconscious and unintentional aspects of psychological appeal explored by Dawkins. The theologians in any teaching body might be involved in all sorts of personal conflicts and collaborations, and these social struggles

would affect which ideas they spend the most time teaching and how. One theologian might advocate one idea purely because an arch rival advocates another, and vice versa. Moreover, some theologians may even be aware of the subjective nature of their teaching focus. In any case, factors such as these will only affect which ideas establish themselves in the main line of cultural descent from priest to priest.

But there is also the cultural transmission from priest to congregation to consider. This occurs during church services, particularly during sermons, and the more intense one to one interaction which occurs during situations such as confession, or a priest's "rounds" of personal visits to administer pastoral care of various sorts. It is in these situations that the cultural reproduction involved becomes less than straight forward. The priest is teaching concepts to people who are unlikely to become involved in teaching novices themselves. There is also likely to be a significant gap between the theological knowledge of the priest and that of the listeners. Nor are the listeners likely to be able to preach to each other with confident authority, as, generally speaking, someone is more likely to believe what a priest says about aspects of the religion than they would a layman (other things being equal, of course). Moreover, it is not just a matter of a layman finding someone to listen to them; many of the doctrines of the church are so complicated that it is almost impossible to expound them effectively without some form of intensive training. Such ideas could never be spread beyond the cloisters, and copies of these ideas that exist outside the theological milieu are therefore effectively sterile.

Yet members of the congregation may listen very carefully to a priest expounding these ideas, and come to feel that they have a reasonable understanding of a whole range of concepts which are integral to the religion they follow. Nonetheless, their understanding may be highly individualised and not easily expressible in words. Some regular church goers may not be as articulate as the highly educated theologian they listen to on a Sunday. The average layman can speak for hours about all sorts of religious concepts, but a well trained theologian who listens to these expressions can generally

recognise a range of concepts which have been substantially individualised, altered, simplified or left out altogether. The contrast in depth of understanding must have been particularly pronounced during the early periods of missionary activity in the British colonies in America, Africa, and Australia, when large numbers of indigenous people became "Christians" virtually overnight. In most cases these rapid conversions must have produced individuals whose basic psychological make up consisted of the indigenous religion with a superficial overlay of Christian notions. The personalities of these "Christians", and their religious ideas, must have remained substantially different to that of their cultural conquerors. Moreover, it is quite possible that some indigenous individuals simply continued to worship their old religion in a new terminology, and that the few truly English religious concepts that they did identify with were not easily communicable, and therefore culturally or memetically sterile.

Indeed, while both priest and different members of a congregation use common words such as "God", "faith" and "hellfire", the concepts behind the words which denote religious concepts can be quite varied, even in a congregation which speaks English as a first language. One example of this is afforded by an article of Catholic faith known as the "Immaculate Conception". Almost any person who considers themselves to be Catholic, whether priest or layman, will adamantly insist that they know whose conception this doctrine refers to. However, most Catholic laymen, and even some priests, mistakenly believe that the Immaculate Conception is all about how Christ was conceived in the womb of the Virgin Mary. Yet this is not the case. As almost any priest well versed in the doctrines of the Catholic church will testify, the Immaculate Conception is actually an account of how Mary herself was conceived in the womb of her own mother in a manner which rendered her free of original sin, *preparing* her for the later events associated with the conception of Christ, but in no way *part of* this second and later conception. But could the inaccurate, and even nonsensical derivatives of this complicated doctrine, which are unlikely to make it to the next generation of priests, and which exist in the congregation, possibly work toward the replicative success of the correct doctrines in the mainline of theological descent? Could some of the

replicative success of the "Immaculate Conception" doctrine, as part of the general complex of the Catholic faith, derive in part from the personal appeal of its erroneous versions? Misappropriations of the words "immaculate conception", so that they are thought to apply to the conception of Christ in Mary's womb, might, for example, enhance the general metaphorical power of the personality of Christ in the general congregation. Exploring the possibility further, however, would presumably require actual sociological or ethnographic fieldwork.

Other elements of a religious cultural complex might also occur in a range of forms, some more suited to cultural reproduction, and others more suited to the indirect facilitation of that reproduction. A priest might have a thorough knowledge of the many different meanings that words like "faith", "celibacy", "hellfire", "immortality" and "God" may take. But he or she may not preach these concepts in precisely the same form that they were laid out in theological lectures. Having faith in an idea, for example, in the opinion of the priest, may not represent a complete absence of doubt, or even any absence of the ability to challenge that idea. But the more extreme ideal of absolute faith may nonetheless be preached as something for a congregation to aspire to.

Conversely, other concepts may occur in their most extreme form in the minds of priests. When celibacy occurs in a religion, it is normally only advocated for a small minority such as the priesthood itself, although it may also be advocated for other groups, such as unmarried women. But related and less comprehensive notions of various kinds of sexual repression, such as rules against sex in certain positions, with someone of the same sex, or with someone other than one's primary partner, commonly occur in religions with celibate priests.

Similarly, if a number of people who consider themselves to be Christians are asked to define who or what they consider the word "God" to signify, a wide range of responses is often received. Many would say that their particular idea of "God" was influenced by or inspired by the teachings

of some priest or theologian, so there is no doubt that each version is usually learnt from some source. But one of the most common complaints is the idea itself is intensely personal, and cannot be adequately communicated to anyone. Ideas which cannot be communicated have, by definition, zero cultural fitness, and so notions of "God" which are too personal to be expressed must have some other means of survival other than direct transmission. This is in contrast to theological notions of "God" being transmitted from priest to acolyte within an academic institution. Like the complicated and counterintuitively named concept of the Immaculate Conception, a complex definition of the nature of God can be learnt far more coherently by an acolyte, who is privy to many hours of religious instruction and supervised study, than it can by a member of the congregation. Yet, whether they can express it or not, most congregationists believe they "know" what "God" is, that they "know" what the "Immaculate Conception" is, and so on. In any case, members of a congregation may only be required to say that they "believe in God", and not to give a personal description of Him (or Her). Such non-specific commitments allow much scope for the aesthetic "customisation" of non-reproductive (i.e. non-priest) forms of a given idea.

Obviously, then, it would appear that ideas occur in religious communities in both reproductive and non-reproductive forms. The question is how to interpret the non-reproductive forms, and I have been implying that the fragmented, simplified or distorted nature of the congregation's versions of certain religious concepts may actually work to the reproductive advantage of the "master" or "queen" copies in the minds of priests. There is space enough to consider the possibility of eusocial cultural altruism with reference to one of the above ideas, that of celibacy, in a little more detail. Firstly let us define the notion of total celibacy, or at least total repression of both heterosexual and homosexual yearnings, to be the "queen" or "master" phenomenon. It is easy to imagine, as already discussed with reference to Dawkins, where this viral phenomenon derives most of its cultural fitness *from: the prevention of the normal outlet of one of the basic appetitive drives.* It is able to use this drive as a vast supply of surplus energy which is diversted into the cultural transmission of both the idea itself, and that of

other elements in a religious ideology. The ideal of celibacy is the basic line of descent, as it is what is taught in our hypothetical institution to each new acolyte.

Let us imagine, then, that our acolyte, once ordained, goes out and is able to rigidly adhere to a celibate lifestyle. Imagine also that our young priest gives sermons which advocate celibacy, in no uncertain terms, for all unmarried members of his or her congregation. It is unlikely that all of the congregation will be able to live up to the ideal of the sermon, but it is equally likely that most of them will try to hide the fact. Some young people may try to restrict their activity to "heavy petting". Others may come to other private solutions. In hiding their personal solutions, each person creates a culturally sterile derivative of the priest's ideal; a distorted, compromised, or corrupted version of the original which is excluded from primary "reproductive" location of the priest's consciousness by the original idea itself.

In this way each of the descendant ideas is restricted to a worker role in the genealogy of the religion. Being derived from the "celibacy" model, repression is likely to remain the theme of the ideas about sexual activity in the congregation. Each digression from the ideal is likely to create emotional energy in two forms. Firstly, there would presumably be a measure of guilt involved in not measuring up to the ideal of complete celibacy. Secondly, this guilt is likely to redouble the individual's efforts to repress other aspects of their sexuality which remain unexpressed, presumably on the implicit grounds that "partial" celibacy is better than none at all. The two forms of energy are thus guilt for those desires which are illicitly satisfied, and the sublimated desire of those which remain repressed. The viral assemblage wins both ways: energy lost in the satisfaction of desire is regained in guilt, while successful repressions of aspects of sexual desire produce energy which may then be harvested by the church in the same way that the repressed desire of the celibate priest may be. As long as the resulting guilt and repressed desire is discharged back in the direction of the church, whether on the Sunday collection plate or in more extreme forms such as missionary activity, the

master phenomenon of celibacy is able to harvest the "work" of its silent cultural offspring in the congregation.

The possibilities for the viral perspective on culture are so vast that the full implications could not be discussed in a study twice as long as this one. As in the case of single viral phenomena such as Auld Lang Syne, intention need not be excluded from eusocial viral assemblages either. No amount of deliberate intervention in the cultural transmission process need pose any threat to a neo-Darwinian interpretation. The desire to intervene, and the presence of one or more goals which lead to such desire in an individual's mind, can merely be counted as additional (and very interesting) elements of the psychocultural milieu which affects the fitness of a viral assemblage. Indeed, the strategic introduction of a cultural phenomenon into another culture could even be considered a kind of cultural "biological warfare", where a pernicious idea or artefact (such as alcohol, or nicoteine) is introduced in the hope of hindering the functioning of a community. Even more "benevolent" introductions, such as those involved in missionary activity, could sometimes be included under the this general umbrella. There are other genetic processes which could yield useful cultural analogies. Delius (1989:69, 1991:95), for example, has even discussed cultural equivalents of sexual recombination, although there is insufficient space to go into this interesting direction here. A particularly interesting biocultural analogy is the one developing between viral spores and various forms of stored cultural information (Ball 1984:153; Delius 1989:54). Moreover, there are many other sub-directions which have been left unexplored.

10.5 The Importance of Being Pernicious

Most of the hypothetical examples considered above do not specifically involve artefacts or the archaeological record. In order to partially redress this shortfall, let us think in terms of attempting to explain an idealised transition from one artefact type to another in the archaeological record. The

artefact hypothetical will involve six different structures, each competing with the rest for continued existence in a band of *Homo erectus*. Let us imagine that a band of some fifty *Homo erectus* individuals inhabits a particular area, and that variation exists within the band with respect to a number of factors.

Firstly, let us imagine that two basic kinds of thumbs are present in the *erectus* population, and that one of these thumb types is a little more mobile than the other. Relatively speaking, then, within the population, we have a beneficial thumb type and a pernicious thumb type.

Secondly, let us imagine that two basic kinds of flea can be found on the bodies of our *erectus* individuals: one which spreads a serious but not deadly disease, and can jump a long way; and one which if eaten by the bearer or the bearer's grooming companion, provides a valuable source of B-group vitamins, but can only jump a short way. In other words, we have a pernicious but contagious parasite, and a beneficial but noncontagious parasite.

Thirdly, let us imagine, for the sake of argument, that there are two basic kinds of scraper in use in our *erectus* community: a Mousterian side scraper, and an Acheulian hand axe (the hand axe being cast in the functional role as a "scraper" by definition for the sake of simplicity) along with the neuronal structures associated with the knowledge of how to manufacture them. Let us also imagine that in the peer group of our imaginary hominid, side scrapers are in, and Acheulian hand axes are out. Most *erectus* individuals find that side scrapers much more aesthetically appealing than the old fashioned hand axes. However, hand axes still perform the scraping tasks involved in *erectus* leatherwork a little more effectively than the new scrapers (nonetheless, the scrapers function quite adequately). So if the side scraper is a pernicious but popular cultural phenomenon, the hand axe is beneficial but banal. There are therefore two traits, two parasites, and two artefacts; one of each pair is relatively pernicious, while the other is relatively beneficial.

The Cultural Virus Theory is based on the general proposition that cultural process is neo-Darwinian in every particular. A corollary of this premise is that memetic mutation is logistically or mathematically homologous to genetic mutation in every conceivable sense. Each new viral phenomenon, therefore, must be derived from some previous set of memes or concepts, some previous viral phenomenon. For the purpose of simplicity, then, let us imagine that the side scrapers in this hypothetical were derived via a quasi-random memetic mutation from an Acheulian industry of some sort. The word "quasi-random" is used to denote the fact that although memetic mutation is locally random, in a broader sense it is subject to profound heritage constraint. This is due to the fact that the first conception of the new technique which in turn lead to the manufacture of the first side scraper occurred *as a derivative of some old technique* that the particular erectus individual concerned had previously learnt. Heritage constraint is embodied by the fact that the side scraper displays a whole range of similarities to the hand axe, when compared to the full range of possibilities of artefacts. It is made of the same raw material, using some of the same techniques, it is of similar complexity, and the two artefacts are much the same size when compared to the largest and smallest possible artefacts - such as large buildings, or artificial molecules.

Moreover, while the side scraper may be marginally more appealing, and the hand axe (as defined) marginally more functional as a scraper, neither is likely to be perfectly functional or perfectly appealing. The knapper who makes these tools is not able to transcend the constraints of his or her own manufacturing notions (except by the relatively small conceptual leaps of memetic mutation) to produce the perfect solution to prevailing erectus concepts of either efficacy or aesthetic appeal. This principle can be crudely illustrated with a little thought experiment; by imagining the artificial introduction of a swiss army knife into the *erectus* band. It is easy to imagine that such an artefact (especially with an appropriate demonstration of its scraping and butchering possibilities) could beat the old scrapers on both functional *and aesthetic* grounds. Yet it is highly unlikely that the principles of metallurgy and forgery involved in replicating the army knife could be

stumbled upon via memetic mutation from the Acheulian tradition. Like the Coca Cola bottle in the Kalahari Bushmen band of the film *The Gods Must be Crazy*, a swiss army knife in an *erectus* band is a "charismatic survival success" but a reproductive *cul de sac*. Thus the side scraper may be an aesthetic improvement (as defined *ipso facto*) on the hand axe, but the cultural heritage constraint implicit in the Acheulian industry ensures that it is also thematically similar to other artefacts in the assemblage from which it was derived.

Now let us attempt to develop some kind of general picture of the different survival challenges which face the six different structures in the *erectus* community. Firstly, the thumbs. Hominid thumbs share their genealogy with all the other traits of *Homo erectus*. There is only ever one place that the traits of a particular *erectus* body can hope to escape the death of their host, and that is in the bodies of his or her descendants. Thus thumb genealogy is, strictly speaking, *erectus* genealogy. Thumbs are part of a single organism, and there can be no question that what is good for either of the thumbs is synonymous with what is good or adaptive for the rest of the hominid.

Conversely, what is pernicious for the thumbs is likely to be pernicious to *Homo erectus*. The only way a thumb can enhance its chances of reproductive success is by contributing to the reproductive success of the body of which it is part (or at the very least through a nepotistic relationship with other bodies related to that body). An immobile thumb cannot adopt an exploitive or parasitic relationship to its hominid host, as it cannot "leap across" the genealogy of the *erectus* population and infect new or unrelated lineages. The only recourse open to the immobile sector of the thumb population is to "hope" that mobile thumbs prove to be as much of a disadvantage as an advantage. The pernicious immobile thumb may therefore be legitimately referred to as a "maladaptive trait", while the beneficial mobile thumb may be legitimately referred to as an "adaptive trait".

So much for the thumbs: now, I would like to turn attention to the fleas. The fleas, unlike our hypothetical thumbs, do not have synonymous genealogies with the *Homo erectus* individual, and they, therefore, need not necessarily throw in their lot with their host. Fleas are not automatically reproduced at human conception, and can only hope to colonise infant *Homo erectus* individuals after birth. But fleas are able to colonise unrelated *Homo erectus* individuals, and so those fleas which carry disease, thereby being pernicious to their hosts, are not necessarily at a disadvantage. Being a pernicious thumb is definitely maladaptive; but being a pernicious parasite may, under certain circumstances, be highly adaptive, particularly if you can jump much further than your beneficial competitors.

Finally we come to the Acheulian hand axe, which has been defined as beneficial but boring, and the Mousterian side scraper, which has been defined as pernicious but pretty. Are they tied traits, like *erectus* thumbs? Or are they genealogically independent, like *erectus* fleas? It is really not difficult to see that the side scraper, along with the knowledge of its manufacture, is much more like the freewheeling flea than it is like a thumb. Like fleas, knowledge of the manufacture of hand axes and scrapers is not passed on during human meiosis. *Homo erectus* embryos don't know much about making stone tools. There is no "hand axeness" in hominid genes, in any but the most general of senses. The capacity for hand axes is genetically reproduced, but this capacity can be put to so many diverse ends that the reappearance of hand axes in the next generation could never be guaranteed by the genetic reproduction of hand axe making potential. There is always the chance that this capacity could be channelled into the manufacture of another tool, such as the side scraper. While it is thus more difficult for scrapers to gain access to their host's offspring than it is for a thumb, they can do something which no *Homo erectus* trait could ever do. They can spread directly to their host's brothers and sisters, aunts and uncles, cousins and nieces, mothers and fathers-in-law, even to the strangers just across the gorge. In this way, pernicious artefacts, like pernicious parasites, have a special escape route which maladaptive traits do not.

Now let us imagine that our hypothetical *erectus* band attends a long and arduous "central place" gathering. We can then continue the thought experiment, by imagining which structures are most likely to be able to take advantage of several weeks of sustained interaction between large numbers of people. When the gathering has dispersed it is quite possible that side scrapers have been a great success, and that every *erectus* body who is any *erectus* body appears to be using them. In fact, as long as at least one member in each band now knows how to make them, and thereby cater for the eager and fashion conscious market of *erectus* communities, it is quite possible that they will take over from hand axes as the most common scraping tool. This is, of course, provided that its functional disadvantages are not too obvious, and that the penny doesn't drop in too many *erectus* minds. If its inefficacy is perceived, the scraper is effectively polluting its own psychological environment with the depressing *erectus* experiences it causes. Provided, however, that its rate of social transmission exceeds the spread of *erectus* disillusion, ie. that the rate at which it finds new pastures exceeds the rate at which it destroys the old, it will successfully displace the hand axe. Because it has the potential for this strategy, the scraper cannot be called a maladaptive trait. It is instead an adapted parasite. Like the scrapers, the pernicious fleas could well have been quite successful, each band now having its new population of resident parasites. Although each new host that the fleas have found will be recovering from the disease they carry, the negative affect of this could potentially be offset by the huge number of new hosts infected.

Thus both pernicious parasites, both fleas and scrapers, would probably do very well at a central place gathering. But the distribution of immobile thumbs will always remain precisely where it was before the central place gathering was held; along strict lines of *erectus* descent. Immobile thumbs will always be restricted to the family of the original immobile thumb individual, and the future of the thumbs will always be clouded with the difficulties which they convey to their bearers.

So the pernicious scraper, by virtue of cultural transmission, has the opportunity for a whole new strategy of adaptation which would not be possible if it were merely a maladaptive trait, like the thumb. As long as it can continue to sustain a reasonable rate of social transmission, it can continue to survive. For the immobile thumb, on the other hand, there is little hope.

10.6 Some General Archaeological Implications: Memetic Mutation and Cultural Heritage Constraint

There is not enough space to expand on any of the other elements of CVT, or to provide an in depth virus account of a particular archaeological or historical sequence. That will have to left for a later study. But a few general statements about the empirical implications of CVT's account of cultural process can now be summarised. CVT implies a particular view of the archaeological record, and two points are prominent in this view. The first of these is the concept of heritage constraint, discussed in Chapter 8 with respect to Cultural Selectionism as a whole. Here heritage constraint is discussed with respect to the developing notion of memetic mutation within Cultural Virus Theory, where all new ideas are derived from thematically related ideas elsewhere in the primary repertoire (see also the "swiss army knife" discussion in the section above, and Chapter 9, Section 9.4).

Most of the above discussion focuses on one half of the cultural evolution story; selection, to the exclusion of the other basic neo-Darwinian process, mutation. "Cultures" have been presented as ecological assemblages of viral neuronal structures, distributed asymmetrically throughout the minds of a human community. Selection favours those ideas which, through their symbolic connection to other ideas, and/or through their "fit" to open ended human dispositions, appeal to human minds in that community. Many ideas enhance their fitness and reproductive success through their extended

phenotype of behaviour or artefacts which, through association with the original idea, add to its appeal.

Another element which is not be developed in this chapter (see Chapter 9, Section 9.4), is of course mutation or invention; those processes which produce the variation in populations for selection to then act upon. Selection cannot create variation, only delineate it; therefore it must be supplied with variation by mutation. What psychological processes produce mutations in culturally transmitted viral phenomena? As discussed in the penultimate section of the previous chapter, the complete answer to this question will eventually be found as archaeologists and anthropologists acquire a more and more detailed knowledge of human psychobiology. For the time being, the simple answer must go something like; the imperfect process of "remembering" constantly causes viral neuronal patterns to be partially forgotten, accidently extended, and mistakenly confused or recombined with other neuronal patterns. All new ideas then, are derived from other ideas; sometimes from the reassociation of two or more well understood other ideas, but they are probably more often derived from spontaneous reassociation within a miscellaneous "scrap heap" of nonsense, fragments of old ideas, long since forgotten, lying around in our unconscious. Either way, new ideas do not spring spontaneously out of nothing.

So an important empirical implication for archaeologists to note about CVT is that ultimately nothing *determines* the structure of cultural phenomena except perhaps previous cultural phenomena. Since all mutations or inventions are derived from existing forms, all new forms share the same heritage or historical constraints of their parents. The primary constraint, then, in a Darwinian system, is cultural heritage. No invention, however different it may appear to be, ever completely transcends the structural limitations imposed upon it by its history. There will always be particular discoveries which can only be 'accessed' via mutation from a particular cultural status quo; and conversely there will always be a range of structural forms which can never be 'accessed' via random mutation from that point. CVT's conception of archaeological variability is one of a ubiquitous pattern of indefinitely

increasing local specificity, with periodic cultural 'whitewashing' occurring when a particularly successful structure spreads throughout a region and displaces all the rest. Generally, in a CVT system form reflects phylogeny; cultural history is the fundamental cause of the cultural present. If anything *does determine cultural phenomena, it is previous cultural phenomena; all other influences are selective.*

The second implication to note is that the environment to which populations of artefacts adapt is not the one to which hominid bones adapt. Artefacts, and their corresponding viral neuronal patterns, do not adapt to the large scale environment of, say, the savannah, the steppe, or the metropolis. Their world exists inside human brains; their palpable existence as objects is largely irrelevant to their survival except, of course, to the extent that as objects they can reenter the psychic world of the human mind via the peripheral nervous system. Their world is a place where human fantasy and desire, likes and dislikes, plans and suspicions, fears and hopes, hates and loves are all as influential and concrete as the droughts and carnivores which affect hominid survival in the world at large. The "survival of the fitter" (or, as is more specifically neo-Darwinian in many cases, yet often forgotten: the "survival of the more altruistic") battles in cultural phenomena take place not in the rainforest or savannah, but in a jungle of human sentiment. But as these forces only select from within the variation supplied by psychological mutations, they can only favour forms which the population is capable of producing in the first place. Selective pressure does not enable a population of concepts to transcend its own cultural heritage, no matter how desperate the bearers of those concepts may have become.

10.7 The Viral Domesticate: A Brief Reinterpretation Of Foley's Hominid/Artefact Correlation

As stated in Chapter 8, the pervasive pattern of cultural heritage constraint is no new idea. As a result, the "preliminary test" half of this

study involves as much ostension (pointing in order to draw attention to a pattern which already exists) as it does research (the creation of new patterns). By this I mean that often the easiest way to illustrate the ubiquity of heritage constraint in cultural data is simply to "point" to any and all of the data produced already by archaeological research. The pattern is implicit in practically any archaeological report in existence. There is no special methodology required to make the cultural heritage constraints of regional archaeological sequences apparent, and as a consequence the pattern appears throughout archaeology. The function of the analyses of Chapters 6-8 was not to introduce the "cultural heritage constraint" pattern for the first time, but to show how pervasively this pattern occurs on a global scale, and also to show its equally pervasive occurrence in a "known" selective system governed by human genetic inheritance. Moreover, to say that anthropologists have known of the existence of cultural descent is not to say that it has been frequently used (Durham 1990:190). Although some readers may ask whether genealogical heritage constraint could not be explained equally well by a non-selective theoretical framework, so far those who have made such suggestions have been unable to name a theory which predicts heritage constraint without evoking a selective metaphysic of some description.

But while archaeologists have always been aware of cultural heritage constraint at the empirical and methodological levels of the discipline, the pattern has not been interpreted as having any particular implications for the general evolutionary frameworks used at more general levels. On the other hand, biologically oriented anthropologists, and sociobiologists in particular, have made quite extensive use of the Darwinian patterns in artefacts and ethnography for heuristic demonstrations of the validity of their own approach. But the "artefacts as viral phenomena" approach advocated here makes possible a vastly different interpretation of the very same patterns. The distinctive selective patterns to be found in artefacts, and their corresponding tendency to covary with similar selective patterns in human bones, cannot be evoked in support of Sociobiology. The bones of domesticates and commensal animals also covary with human bones; yet it would be preposterous to claim that the morphology of dogs and mice was determined

by human genes (except in a Dawkins's "extended phenotype" sense, which is presumably a relatively insignificant factor). Domesticates and commensals are better considered to be adjacent hereditary systems, with their own intrinsic selective logistics. While genes do not determine language, they may have an indirect influence on which language an individual is exposed to in early childhood, by virtue of the simple fact that language is normally learnt from parents (Cavalli-Sforza 1991:77). Similarly, genetic makeup may also come to correlate with which artefacts an individual is exposed to, and which domesticates they come to be responsible for. This is a correlation of an altogether different kind, yet one which deceptively similar at first glance.

Cultural Selectionism is in a position to "slay the dragon" and "get the treasure" all at once; to win both fame and fortune in the same moment. It should now be possible to refute Sociobiology, alienating it from its "riches" or emassed data of neo-Darwinian patterns, and appropriate that very same data in support of Cultural Selectionism. Rather than attempting to refute the empirical support for the perspective, the aim is to appropriate that empirical support for itself. Performing comprehensive tests of its own into the nature of cultural data may not be necessary; if sociobiology has established beyond reasonable doubt that there are Darwinian signatures to be found in the archaeological record, Cultural Selectionism need only make a more robust claim for those same patterns to advance its cause. The critique of culturally oriented Sociobiology offered in light of basic selectionist principles in Chapter 3 attempted to show that sociobiology's conception of the nature of cultural process itself cannot be considered properly Darwinian. Logically, therefore, they cannot validly continue to claim "explanatory rights" to the Darwinian patterns they have nonetheless proved to exist.

This ironic situation, where culturally oriented Sociobiology has refuted itself, and left Cultural Selectionism unrefuted, with the very patterns it intended to find, is well illustrated in Foley's synthetic study of hominid remains. The paper (Foley 1987) focused on the tendency of artefacts and hominid remains to covary in the early archaeological record. Clearly, in order to covary with the Darwinian patterns in human remains, artefacts have to

exhibit Darwinian patterns themselves. The assumption I would like to challenge here is that such patterns are imposed upon the archaeological record by the selective logistics involved in human genetic evolution. In other words, no notion of genetic determinism or even genetic constraint is required to make sense of these patterns. Cultural Virus Theory can make a direct claim to these patterns through its claim that artefact systems are in fact ordered by intrinsic Darwinian logistics of their own, which take place in fully developed human brains, and that they are not ordered by the other Darwinian processes which occur in hominid gene pools. Most importantly, a viral explanation of these patterns would not depend upon tenuous claims for a series of highly specific genetic constraints on hominid behaviour.

It is quite clear that Foley rejects any notion of cultural phenomena having genealogical independence, as a class of viral phenomena analogous to very simple organisms. In fact, he blames the "artefacts as organisms" approach for bringing evolutionary theory a bad name. But as already discussed in the early part of Chapter 9, there is very little to prevent archaeologists treating artefacts precisely in this way. The only mistake would be to treat artefacts as self reproducing organisms, and this can be avoided by using the concept of a viral phenomenon, as this concept implies a dependency of the said phenomena on human action for their replicative success. Foley goes far beyond this criticism however. Any notion of artefact production systems which grants some independence to cultural process, is seen to be guilty of separating culture from biology altogether (as distinct from merely separating culture from genetic inheritance). The possibility that culturally transmitted behaviour *is* properly distinguished from the biological process of genetic evolution, but only as another "nested" subset of the general category "biology" (ie. as a special case encompassed by a larger category, like the category "square" is encompassed by the category "rectangle") is not given consideration. As far as being "biological" is concerned, in this fallacious view, it would appear to be a case of "genes or nothing". Culture is not allowed to have a material and thoroughly biological reality all of its own, independently of the aspects of the organism which are organised around genetic reproduction. Thus Foley writes:

> "Perhaps the central problem in the unification of palaeoanthropology as a single discipline lies in the way "biology" and "culture" have been dichotomized. On the one hand the evolution of human anatomical form is seen as the product of natural selection, much as that of any other species: archaeology, however, dealing as it does with behaviour, has been considered to be beyond the scope of natural selection, and change has been accounted for through other, usually cultural, mechanisms." (Foley 1987:380).

But while Postmodernism (which I would assume to be Foley's most recent implicit target) may indeed claim some fundamental separation of culture from biology, the newly defined account of culture in terms of viral phenomena only claims separation of cultural process from genetic process. Separating culture from biology, thereby transforming it into some sort of "supernatural phenomenon", becomes completely unnecessary. Cultural Virus Theory can thus provide a unique third alternative to conventional Darwinian Anthropology and Postmodernism; one that is simultaneously as Darwinian as the first and as purely cultural as the latter. Foley is absolutely correct about the existence of these patterns; but there is no need to appeal to the neo-Darwinian processes of hominid gene pools to explain them; a neo-Darwinian conception of the intrinsic logistics of cultural process will suffice.

In other words, Cultural Virus theory offers a completely different explanation for the "stone/bone" correlations of the archaeological record. Sociobiologists may predict a virtually complete correlation of the two, and then, when finding a somewhat imperfect yet positive correlation, consider their view to be verified. For Sociobiology, the temperamental nature of the stone/bone correlations is something to be explained away. For Cultural Selectionism, however, a temperamental correlation is predicted in the first place, as any total correlation is prevented by the cultural or genetic substitutions (Cavalli-Sforza 1991), which are themselves a function of the genealogical independence of genes and culture.

Armed with a cultural selectionist or viral perspective, archaeologists wishing to avoid any notions of genetic constraint on behaviour no longer need to fear the ubiquitous heritage constraint patterns found in archaeology, as such patterns can now be viewed as a result of the intrinsic neo-Darwinian

logistics of human thought, and not as the result of neo-Darwinian process in human gene pools. At the risk of confounding the issue with another metaphor, Cultural Virus Theory could perhaps be considered the "anti-sociobiology vaccine" that modern anthropology - post-modernists included - have inadvertently been "waiting for". Its a safe "dose of biology" which "immunises" the cultural anthropologist against the more serious condition involving the discussion of cultural diversity in terms of genetic constraint, on the grounds of the stone/bone correlations which Foley has elucidated. Cultural Virus concepts allow Foley's correlations to be explained via an alternative scenario, as a result of the fact that cultural and genetic evolution are equally Darwinian, but not as a result of the causal influence of hominid genomes on cultural systems. The theory conveys to cultural anthropology recourse to a perfectly Darwinian biology all of its own: it grounds cultural studies in the extra-genetic biology of the human nervous system, while simultaneously securing the independence of cultural phenomena from human genetic process.

10.8 Final Summary of Basic Principles of Cultural Virus Theory

It is important to distinguish the term "viral phenomena" from the established scientific term "virus". The latter has a long history of consistent association with microbial viruses, the smallest and simplest of all kinds of organisms which consist of nucleic acids enclosed in a protein envelope, and sometimes a few additional proteins which facilitate replication (Ewald 1994:222). More recently, the word "computer virus" has been applied to computer programs which, once created by human agency, are able to interact with functioning computers in such a way as to propagate themselves indefinitely. Cultural Virus Theory does not entail an assertion to the effect that cultural phenomena are either of the above, i.e. that they are "cultural viruses" in any literal sense, although some commentators have been left with this impression (Jones 1993; Knight 1993; Zubrow 1993). The word "virus" is included in the title of Cultural Virus Theory only as a convenient shorthand

for the concept of viral phenomena. For this reason, Viral Phenomena Theory is perhaps a more descriptive label, although this would then entail the inconvenience of abandoning any reference to cultural phenomena.

In contrast to the word "virus", the notion of "viral phenomena" was created as a larger and more inclusive generic category. The category includes all phenomena which lie near to the boundary between organic and inorganic fields of enquiry, but which display some of the qualities of "life". A viral phenomenon could be summed up as a "sub-organismic living or quasi-living thing". It is also a non-phylogenetic category, i.e. a set which is not constituted of members who share a common origin, as discussed elsewhere (Cullen 1993b). Computer viruses, microbial viruses and cultural phenomena are not united by common ancestry, but by logistic or physical qualities, such as their sub-organismic or near sub-organismic status, and a number of other aspects which are expanded upon below.

It is also crucial to emphasize that a viral phenomenon may be perfectly symbiotic and therefore beneficial to its host; there is no necessary connotation of negativity.

Within Cultural Virus Theory the notion of "viral phenomena" may be defined more precisely. Summarised very briefly, a *cultural* viral phenomenon may span three levels of material existence: (a) The level of (culturally imparted) neuronal configuration within the brain, including memory and knowledge; (b) The level of decision making, thought, action and ritual; and (c) The level of artefact and extrasomatic signature. Ideas, actions and rituals do not necessarily exist in the third level, while artefacts necessarily span all three. Many thousands of such phenomena, when coupled with the genetically emergent dimension of the human body (which could be described as a partially disembodied naked ape; i.e. acultural and therefore not fully embodied) make up each human individual and its consciousness. Human thought may then be envisaged as an ongoing cascade of selective events imposed upon any one subset of the brain's cultural phenomena, by another cultural subset and/or the genetically emergent dimension of the brain.

The condensed summary offered above can be expanded into a series of models and principles, and they are as follows:

1. **Genealogical Independence.** This is the definitive quality which distinguishes a viral phenomenon from a trait or organ. No matter how integrated or indistinguishable a cultural phenomenon may be from the rest of a human individual, it is, by definition, distinct from the genetically emergent dimension of the host in so far as it is not passed on through host reproduction. It is also distinct from other cultural phenomena which jointly constitute that human individual, to the extent that they, too, may be learned (reproduced) at different times and via different media. This principle of CVT is a cultural translation of genetic principles advanced as a means of distinguishing between host and pathogen, and the virulence of such pathogens, in parasite ecology (e.g. Dawkins 1982, 1989:234-67; Ewald 1994:35; Mitchison 1993). Yet while genetic parasites which reproduce in unison with the host, and are passed on in sperm and egg cells, can no longer be distinguished from other host traits - such as some cellular organelles (Margulis 1981) - a cultural phenomenon can never, by definition, achieve such a degree of integration, and therefore always retains the potential for genealogical independence. Knowledge of how to make a megalithic tomb, or to perform a funerary rite, for example, must always be reproduced extragenetically, and must therefore retain a potential capacity to be passed on to individuals who are genetically unrelated to the megalith maker.

2. **Continuous Spectrum of Adaptive Strategy.** Genealogical independence releases cultural phenomena from the yoke of "man's extrasomatic means of adaptation", a role championed by Leslie White and processualists such as Binford in 1960s (Renfrew 1994:3). A true trait or organ is selected to the extent to which it collaborates with its host, as it has no alternate means of reproductive success. Yet, as viral phenomena, knowledge of initiation rites, megaliths and microliths enjoy a full spectrum of potential adaptive strategies. Like other pathogens or

symbiotes, their effect upon their maker's genetic success can range from dramatic enhancement (where a megalith maker might have a large family) to devastating inhibition (where a megalith maker might become a celibate devotee of a megalith cult). Like Principle 1, this principle is also inspired by aspects of parasite ecology discussed by Dawkins (1982, 1989), Ewald (1994) and Mitchison (1993).

3. **Dependence and Integration.** As viral phenomena have only a very simple body of their own, they are dependent upon aspects of the host body in all sorts of ways. For instance, the knowledge of how to manufacture a small tool industry could not possibly produce an actual microlith if it were not linked up to (and indeed part of) a human body, which in this case provides a service analogous to that provided by a computer, printer and human user in printing a hard copy of a document. Such dependence inevitably demands high levels of integration between the genetically emergent and culturally emergent dimensions of the human body. Like a RNA retrovirus which has reverse transcribed into the DNA of its host (Greene 1993:69; Kuby 1992:463), the knowledge of how to make a microlith, or an understanding of a religious concept, may be physically intertwined and essentially indistinguishable from the genetically emergent dimension of the microlith maker. However, this intimacy is never complete (as may sometimes be in the case of a DNA-based parasite becoming a trait or organelle as explored by Margulis 1981) due to the implications of Principle 1.

4. **Limited Intrinsic Agency.** Dependence on the external agencies provided by the host body implies the absence of, and indeed a reduced need for, intrinsic agency; they must rely on the agencies which exist in the bodies in which they find themselves. Viral phenomena have no nervous or hormonal systems; no integrated networks of internal communication; they cannot think or feel. While fully organismic parasites have elaborate mechanisms for concealing themselves within host bodies (Paul 1993), their bodies remain physically distinct from that of their host. Microbial viruses, as described in Principle 3, move beyond this point,

such that cellular boundaries are transgressed, although it is still possible to argue that they are "living" (Slap 1991). Cultural phenomena have even less intrinsic agency, such that they are sometimes better imagined as an aspect or dimension of the host/guest composite of which they are part. It becomes difficult to determine where the knowledge of how to make a megalithic tomb ends, and the genetically emergent structures and other cultural phenomena of a megalith maker begin. But this intimate and fuzzy interface between host and viral phenomenon increases opportunities for the latter to be taken up in the actions of the host, possibly in such a way as to lead to its successful reproduction.

5. **Dependence During Replication Itself.** The lack of intrinsic agency is so profound in cultural phenomena that they are not true self replicators, unlike nucleic acid based phenomena. Closest to cultural phenomena in this respect are RNA-retroviruses, which contain no DNA, but carry a protein (reverse transcriptase) which is capable of initiating reverse transcription back to self-replicating DNA once inside a host cell (Greene 1993; Ewald 1994:222). But cultural phenomena are more dependent still, given that they require the mediation of human agents in the act of replication itself (Cullen 1993b:199). While megalithic tombs and microlithic assemblages may contain a variety of clues as to how they might be replicated, these clues can be read far more effectively by those who already know how to manufacture such phenomena, and to whom the hereditary material of the megalith has already been passed. They do not contain structures which are capable of forcing a human individual to begin megalith manufacture in the face of complete ignorance as to how this might be achieved.

6. **The Three Tiered Bodies of Cultural Viral Phenomena.** Cultural phenomena have three levels of physical existence; a level of hereditary material, and two levels of phenotypic product, namely the level of action, and the level of artefact. A viral phenomenon is not an object which can be clearly separated from its host's body and actions (except during

reproduction), as each is in fact part of the body or "self" to which it belongs (Cullen 1992). Rather, its body is distributed throughout the three levels, as an interactive dimension or aspect of each:-

(6A) Hereditary Material. The first level is that of the idea, memorised action sequence, or knowledge about techniques of manufacture. This is the set of physical alterations to a nervous system which occur during the socially mediated learning involved in cultural reproduction, and consists of a set of strengthened synaptic connections within the brain (Delius 1989, 1991). Here Cultural Virus Theory can be connected to Edelman's vision of how the brain develops and functions (Edelman 1989, 1992; Levine 1988), although it is by no means dependent upon this particular model of the brain. When an apprentice flint knapper is being taught by a master, those neuronal groups which best symbolise and facilitate the remembering of microlith manufacturing knowledge are selected (amplified) from amongst a diverse array of neuronal groups, each with its own distinctive set of interconnections. This "primary repertoire" of diverse neuronal groups (Edelman 1989) was generated during brain development, and contains a spectrum of symbolling possibilities so vast that those neuronal groups capable of embodying the knowledge of microlith production could remain indefinitely unselected (latent and undiscovered) in the absence of the socially mediated instruction of a master flint knapper. In this sense, the presence of one set of amplified "microlith manufacturing neuronal groups" or microlith "genes" in a human community is necessary to bring about the existence or selection of another. Once realised, the amplified neuronal groups in the brain of the apprentice microlith maker constitute a new copy of microlith hereditary material. Within Cultural Virus Theory, amplified neuronal groups are also the primary hereditary material of other kinds of cultural phenomena, such as concepts and rituals (elaborated in Chapter 9, Sections 9.3 and 9.4).

(6B) Mutation in Hereditary Material. Since the developmental processes which lead to the formation of the primary repertoire are indeterminate, no two neuronal groups are identical in their connectivity, nor does any one group achieve a perfect "match" with the knowledge it embodies. Moreover, due to the Edelman principle of "degeneracy", neuronal groups with very different internal structure may match an external stimulus equally well, while the same neuronal structures may respond to quite different stimuli (Edelman 1989:57). The process of amplification produces further diversity in the connectivity of other neuronal groups which are not undergoing selection at that moment (Edelman 1989:319). Spontaneous and unpredictable diversity ought therefore to characterise the cultural reproduction of knowledge, as diverse populations of neuronal groups are awakened during the learning processes associated with artefact manufacture. Such processes constitute a mechanism for "random" mutation in the hereditary material of cultural phenomena (elaborated in Chapter 9).

(6C) Action Based Phenotype. The second level of the body of a cultural phenomenon, and the first level of its phenotype, is that of thought, action and ritual. Actions and rituals are not solely produced by the hereditary material of viral phenomena; rather, each action or ritual is a cocktail of genetic and cultural influences, as discussed in Principle 3. The second level of a viral phenomenon is, therefore, not any one action in its entirety, but an aspect or dimension of an action; an aspect of a process or event.

(6D) Artefact Based Phenotype. The third level of physical existence is that of the artefact, which would include all forms of physical signature on any human body or any other extrasomatic object. This category would not only include all objects or surfaces made or altered through human agency, but also effects upon materials derived from other organisms, whether dead (as in the case of "ecofacts" such as bone points or Inuit sleds made from antlers and frozen fish wrapped in animal skins) or living tissue (as in the case of tattoos, body piercing,

and deliberate scarring, castration of domesticated animals). Perceptions of artefacts in the brains of individuals are the most distant part of the extended phenotype (term from Dawkins (1982) to describe substances excreted by parasites to influence the body of the host) of a cultural phenomenon (a 4th structural level?). The artefact is the altered dimension of the resulting object, not the whole object. When the hereditary material of the artefact is actually reproduced, rather than used to produce an artefact, the altered neuronal structures in the brain of the apprentice megalith or microlith maker are themselves a special case of artefact, in which a full cycle of reproduction is made complete.

7. **Capacity for Physical Separation of Genotype and Phenotype.** One novel implication of the distributed qualities of a viral phenomenon is that its hereditary material and the behavioural and artefact products of that material may become separated in time and space. A potter, for example, may produce pots which are traded over a wide distance, such that the pots are used in a context where no knowledge exists of how to make them (Cullen In Press A). This spatial separation between artefact and hereditary material occurs on a smaller scale from the moment the pot is complete, as the two are only ever joined by the relatively fleeting act of making. Such qualities find there nearest living equivalent in the reproduction of microbial viruses. During reproduction, a virion attaches itself to the membrane of a host cell (Greene 1993) and injects its hereditary material into the host cell, thereby separating the two parts of its body in a manner which does not occur during the cell division of "higher" organisms.

8. **Intrinsic (i.e. Extragenetic) Neo-Darwinian Logistics.** Viral phenomena are subject to phylogenetically constrained "random" mutation (Principle 6), a process which gives rise to novel forms and which thereby increases the diversity of a population of such phenomena; but due to the dependence of Principle 3, such "mutations" emerge through involvement in the activities of the host organism. Viral phenomena are also subject

to selective processes whereby some forms are propagated more than others. These processes run separately to a similar set of neo-Darwinian logistics which are at work in the population of host organisms with which the viral population coexist, and are therefore not determined by them. A viral phenomenon is at once both intrinsically neo-Darwinian and yet entirely extrinsic to host genetics. Cultural phenomena have, therefore, a genealogy, selective history and phylogenetic inertia which is all their own, as do microbial parasites (Fenner *et al* 1974:1-33; Postgate 1989; Sonea 1988). But that genealogy is not synonymous with the phylogenetic history of their hosts, and is therefore to be approached on its own terms. Culturally speaking, each microlithic or megalithic tradition has its own genealogy and adaptive history, and belongs to a family of similarly manufactured artefact assemblages which share a common cultural origin. Artefact history and genealogy need bear no systematic relationship to the *genealogy* of the genetically emergent dimension of its human makers, nor to the genealogy of any other cultural phenomenon constituting the consciousness of that individual or its community. Due to Principle 2, however, systematic relationships abound along every other dimension. For example, as we follow an artefact down the course of its own cultural genealogy, all sorts of systematic relationships will emerge between it and various cultural and genetically emergent contexts, but not necessarily with the *genealogies* of those phenomena.

9. **The Darwinian Schizophrenic.** Human individuals remain of crucial importance in Cultural Virus Theory, despite the conclusions of some archaeologists (Mithen 1993b). However, they are conceived somewhat differently to the individuals of the sociobiological model which Mithen would advocate; as somewhat more confused, and as subject to unresolvable internal conflicts in their motivation. The human individual is conceived as an ongoing theatre of Darwinian schizophrenia, consisting of one genetically emergent body, and many thousands of culturally emergent alterations to that body. As such, human individuals are no longer individuals in strict Darwinian sense, but one genetically

emergent individual and a multitude of cultural individuals; a small ecological assemblage. The genetically emergent dimension struggles to integrate and control "itself", but must enlist the help of cultural phenomena in its struggles to resist others, for without the incorporation of *some* cultural phenomena there would be no (complete and fully defined) "self" to protect in the first place. The "self" which results is therefore not fixed, but contingent upon the cultural context in which it is formed. Populations of small ecological assemblages are not true Darwinian populations, for the same reasons that one ecological assemblage is a not a true Darwinian individual. Thus within Cultural Virus Theory human populations undergo a corresponding promotion to a higher ecological level, in a similar manner to the promotion of the individuals of which they are composed. They become one genetic population and a multitude of cultural populations, a seamlessly interpenetrating ecological assemblage. Such groups are as contingent and historically situated as the individuals from which they are constituted.

These principles are about what ideas, actions and artefacts *are* in Cultural Virus Theory, and how they jointly make up the cultural dimension of a human individual. As such, these statements do not constitute an explanation in and of themselves; rather, they are a set of general elements which would be manipulated in the course of building up an explanation of a particular sequence.

The Cultural Virus Theory is thus first and foremost a conception of socially transmittable psychological phenomena as human parasites or symbionts, and not as human traits. Correspondingly, the individuals of cultural process are not individual animals, but "seldom divided" socially reproduced viral phenomena, each consisting of one or more memes (or ideas) and their thought, behavioural and artefact products. A "population" in CVT is therefore a population of individual viral phenomena, not a population of enculturated animals. A viral population can therefore exist within a single brain, several brains, or any subgroup of the brains of a human population –

it need not be sympatric with the genetic population. New viral phenomena emerge from spontaneous reassociation amongst fragments of old ideas. All inventions, then, are derivative, and never completely transcend the cultural heritage from which they come. And since the fitness or sustainable appeal of an idea depends upon the prevailing cultural milieu, further heritage constraint is imposed upon the viral population by the context in which it occurs.

Although it may be difficult to accept that cultural phenomena are individuals in and of themselves, and not traits, their undeniable genealogical independence can lead to no other conclusion. We should not retreat from a strange idea purely because of that strangeness. Nor should the difficulty (or even impossibility) of reconstructing the psychological environment which any given artefact technology inhabited in the past deter us from recognising that that environment *was* an essentially psychological environment. As Hodder has so cogently argued, it is virtually impossible to imagine an explanatory Archaeology which does go some way toward interpreting what is in "their" heads (Hodder 1992:17-18), and such challenges would certainly be part of a virus approach to the archaeological record. The specifics of grand scale ecological environments of animals from the palaeontological record are almost as difficult to reconstruct, yet Palaeontology has nonetheless wholeheartedly accepted a range of neo-Darwinian principles. That the archaeological record may be a transcript of artefacts selected on the basis of partially unrecoverable "psychocultural" selective criteria, ie., that artefacts are adapted to a purely cognitive environment, may seem unsatisfactory to some, yet, in this regard, Archaeology would be no worse off than Palaeontology. It may be difficult to reconstruct the context of a given artefact in the archaeological record in order to understand why it sustained local appeal, but it is also extremely difficult to reconstruct the local ecology of a given skeleton in palaeontology. The essentially symbolic nature of the psychological environment to which ideas and artefacts must adapt obliges the archaeologist to make some attempt to address context. Artefact form is best understood when reference is made to the jungle of human sentiment from which it came.

REFERENCES

Alexander, R.D. 1979. *Darwinism and Human Affairs*. Seattle: University of Washington Press.

Alexander, R.D. 1988. Evolutionary approaches to human behaviour: what does the future hold? In Betzig, L.L., Borgerhoff Mulder, M., and Turke, P. (eds). *Human Reproductive Behaviour: a Darwinian Perspective*. Cambridge: Cambridge University Press. Pp. 317–325.

Alland, A. Jr. 1975. Adaptation. *Annual Review of Anthropology* 4:59–73.

Ball, J.A. 1984. Memes as replicators. *Ethology and Sociobiology* 5:145–161.

Bargatsky, T. 1984. Culture, environment, and the ills of adaptationism. *Current Anthropology* 25:399–41.

Barnett, H.G. 1953. *Innovation: the Basis of Cultural Change*. New York: McGraw-Hill.

Bartlett, F.G. 1923. *Psychology and Primitive Culture*. New York: MacMillan.

Bateson, G. 1958. *Naven*. Palo Alto, California: Stanford University Press.

Bateson, G. 1973. *Steps Toward an Ecology of Mind*. Frogmore, St Albans: Granada Publishing Limited.

Bateson, G. 1980. *Mind and Nature*. Glasgow: Fontana/Collins.

Betzig, L.L. 1986. *Despotism and Differential Reproduction: a Darwinian View of History*. New York: Aldine Publishing Company.

Betzig, L.L., Borgerhoff Mulder, M. and Turke, P. 1988. (eds) *Human Reproductive Behaviour: A Darwinian Perspective*. Cambridge: Cambridge University Press.

Binford, L.R. 1981. Behavioural archaeology and the "Pompeii premise". *Journal of Anthropological Research* 37:195–207.

Binford, L.R. 1983. *In Pursuit of the Past: Decoding the Archaeological Record*. New York: Thames and Hudson.

Blum, H.F. 1963. On the origin and evolution of human culture *American Scientist* 5:32–47.

Blute, M. 1979. Sociocultural evolutionism: an untried theory. *Behavioural Science* 24:46–6.

Bottomore, T.B. and Rubel, M. (eds) 1963. *Karl Marx: Selected Writings in Sociology and Social Philosophy*. Penguin: London.

Boyd, R. and Richerson, P.J. 1982. Cultural transmission and the evolution of co-operative behaviour. *Human Ecology* 10:325–351.

Boyd, R. and Richerson, P.J. 1985. *Culture and the Evolutionary Process*. Chicago: University of Chicago Press.

Braun, D.P. 1990. Selection and evolution in nonhierarchical organization. In *The Evolution of Political Systems*. ed. S. Upham. Cambridge: Cambridge University Press. Pp. 63–86.

Bray, W. 1973. The biological basis of culture. In *The Explanation of Culture Change: Models in Prehistory*. ed. C. Renfrew. London: Duckworth. Pp. 73–92.

Cairns-Smith, A.G. 1982. *Genetic Takeover*. Cambridge: Cambridge University Press.

Campbell, D.T. 1965. Variation and selective retention in sociocultural evolution. In *Social Change in Undeveloped Areas: A Reinterpretation of Evolutionary Theory*. eds. R.W. Mack, G.I. Blankstein and H.R. Barringer. Cambridge: Schenkman. Pp. 19–48.

Carneiro, R.L. 1985. Comment on "Darwinian selection and symbolic variation". *Current Anthropology* 26:77-78.

Cavalli-Sforza, L.L. 1991. Genes, People and Languages. *Scientific American* 265:72-78.

Cavalli-Sforza, L.L. and Feldman, M.W. 1981. *Cultural Transmission and Evolution: a Quantitative Approach.* Princeton: Princeton University Press.

Cavalli-Sforza, L.L., Piazza, A., Menozzi, P., and Mountain, J.L. 1988. Reconstruction of human evolution: bringing together genetic, archaeological and linguistic data. *Proceedings of the National Academy of Sciences* 85:6002-6006.

Chagnon, N.A. 1982. Sociodemographic attributes of nepotism in tribal populations: man the rule breaker. In *Current Problems in Sociobiology.* eds. King's College Sociobiology Group. London: Cambridge University Press. Pp. 291-318.

Chen, K.H., Cavalli-Sforza, L.L. and Feldman, M.W. 1982. A study of cultural transmission in Taiwan. *Human Ecology* 10:365-382.

Childe, V.G. 1951. *Social Evolution.* New York: Henry Schuman.

Clarke, D.L. 1968. *Analytical Archaeology.* Second Edition, revised by Robert Chapman. London: Methuen.

Clegg, J.K. 1978a. *Mathesis Words, Mathesis Pictures.* Unpublished MA thesis, Department of Anthropology, University of Sydney.

Clegg, J.K. 1978b. The meaning of "schematisation". In *Form in Indigenous Art.* ed. P.J. Ucko. Canberra: Australian Institute of Aboriginal Studies. Pp. 21-27.

Clegg, J.K. 1978c. A method of resolving problems which arise from Style in art. In *Form in Indigenous Art.* ed. P.J. Ucko. Canberra: Australian Institute of Aboriginal Studies. Pp. 260-276.

Clegg, J.K. 1978d. A Saussurian model of prehistoric art. *The Artefact* 2:151-160.

Clegg, J.K. 1979. Science, Theory and Australian prehistoric art. *Mankind* 12:42-51.

Clegg, J.K. 1985. Reply to D. Groenfeldt. *Rock Art Research* 2:35-45.

Cloak, F.T. 1975. Is a cultural ethology possible? *Human Ecology* 3:161-183.

Cohen, R. 1981. Evolutionary epistemology and human values. *Current Anthropology* 22:201-218.

Cooper, E. 1985. Comment on "Darwinian selection and symbolic variation". *Current Anthropology* 26:78.

Corning, P.A. 1987. Evolution and political control: a synopsis of a general theory of politics. In *Evolutionary Theory in Social Science.* eds. M. Schmidt and F.M. Wuketits. Pp. 127-170.

Crick, F. 1994. *The Astonishing Hypothesis: The Scientific Search for the Soul.* London: Simon and Schuster.

Crook, J.H. and Crook, S.J. 1988. Tibetan polyandry: problems of adaptation and fitness. In *Human Reproductive Behaviour: A Darwinian Perspective.* eds. L.L. Betzig, M. Borgerhoff Mulder, and P. Turke. Cambridge: Cambridge University Press. Pp. 97-114.

Cosmides, L. and Tooby, J., 1987. From evolution to behaviour: evolutionary psychology as the missing link. In *The Latest on the Best: Essays on Evolution and Optimality.* ed. J. Dupre. Cambridge, Mass.: MIT Press. Pp. 277-306.

Cullen, B.R.S. 1986. *Lamarckism and Recent Cultural Prehistory.* Unpublished BA Honours Thesis, Prehistoric and Historical Archaeology, University of Sydney.

Cullen, B.R.S. 1990. Darwinian views of history: Betzig's virile psychopath versus the cultural virus. *Crosscurrents* 4:61-68.

Cullen, B.R.S. 1991. Comment on "Biology and behaviour in human evolution". *Cambridge Archaeological Journal* 1:217-219.

Cullen, B.R.S. 1992a. Cultural viruses and Darwin's fools. *The Independent* 9th May 1992:33.

Cullen, B.R.S. 1992b. *The Cultural Virus* (15 minute summary of the Cultural Virus perspective). First broadcast June 1992, on the radio programme *Ockham's Razor*. Tape available at The Science Show, ABC Radio National, GPO Box 9994, Sydney 2001, Australia.

Cullen, B.R.S. 1993a. (In Press). Cultural Virus Theory. In *Darwinian Archaeologies*. eds. H. Maschner and S. Mithen. New York: Plenum Press. Pp. 1-12.

Cullen, B.R.S. 1993b. The Darwinian resurgence and the Cultural Virus critique. *Cambridge Archaeological Journal* 3:179-202.

Cullen, J.M. 1972. Some principles of animal communication. In *Non-verbal Communication*. ed. R.A. Hinde. Cambridge: Cambridge University Press. Pp. 101-122.

Csanyi, V. 1989. *Evolutionary Systems and Society: A General Theory of Life, Mind, and Culture*. Durham, London: Duke University Press.

Daly, M. 1982. Some caveats about cultural transmission models. *Human Ecology* 10:401-408.

Darwin, C.R. 1859 (1872). *The Origin of Species by Means of Natural Selection*. London: John Murray.

Darwin, C.R. 1871. *The Descent of Man and Selection in Relation to Sex*. Volume I. London: John Murray.

Darwin, C.R. 1889. *A Naturalist's Voyage: Journal of Researches into the Natural History and Geology of the Countries Visited During the Voyage of the H.M.S. "Beagle" Round the World*. London: John Murray.

Darwin, F. (ed) 1888. *The Life and Letters of Charles Darwin*. Vol 2. London: John Murray.

Dawkins, R. 1976. *The Selfish Gene*. London, Oxford: W.H. Freeman.

Dawkins, R. 1982. *The Extended Phenotype*. Oxford: W.H. Freeman.

Dawkins, R. 1986. *The Blind Watchmaker*. London: Longman.

Dawkins, R. 1989. *The Selfish Gene: New Edition*. Oxford: Oxford University Press.

Degler, C.N. 1990. *In Search of Human Nature: the Decline and Revival of Darwinism in American Social Thought*. New York: Oxford University Press.

Deleuze, G. and Guattari, F. 1992(1987). *A Thousand Plateaus: Capitalism and Schizophrenia*. London: Athlone Press.

Delius, J.D. 1989. Of mind memes and brain bugs: a natural history of culture. In *The Nature of Culture*. ed. W.A. Koch. Bochum: Studienverlag Brockmeyer. Pp. 26-79.

Delius, J.D. 1991. The nature of culture. In *The Tinbergen Legacy*. eds. M.S. Dawkins, T.R. Halliday, and R. Dawkins. London: Chapman and Hall. Pp. 75-99.

Depew, D.J. and Weber, B.H. 1988. Consequences of non-equilibrium thermodynamics for the Darwinian tradition. In *Entropy, Information and Evolution: New Perspectives on Physical and Biological Evolution*. eds. B.H. Weber, D.J. Depew, and J.T. Smith. Cambridge, Mass.: Bradford Books, MIT Press. Pp. 24-37.

Desmond, A. and Moore, J. 1992. *Darwin*. London: Penguin Books.

di Leonardo, M. 1993. What a difference political economy makes: feminist anthropology in the postmodern era. *Anthropological Quarterly* 66:76–80.

Dickemann, M. 1981. Paternal confidence and dowry competition: a biocultural analysis of the Purdah. In *Natural Selection and Social Behaviour: Recent Research and New Theory*. eds. R.D. Alexander and D.W. Tinkle. New York: Chiron Press. Pp. 417–438.

Dunbar, R. 1988. Darwinising man: a commentary. In *Human Reproductive Behaviour: a Darwinian Perspective*. eds. L.L. Betzig, M. Borgerhoff Mulder and P. Turke. Cambridge: Cambridge University Press. Pp. 161–169.

Dunnell, R.C. 1978. Style and function: a fundamental dichotomy. *American Antiquity* 43:192–202.

Dunnell, R.C. 1980. Evolutionary theory and archaeology. In *Advances in Archaeological Method and Theory*. ed. M.B. Schiffer. Orlando: Academic Press. Volume 3, Pp. 38–99.

Dunnell, R.C. 1985. Comment on 'Darwinian selection and symbolic variation'. *Current Anthropology* 26:78.

Dunnell, R.C. and Wenke, R.J. 1980. Some comments on "The Decline and Rise of Mesopotamian Civilisation" *American Antiquity* 45:605–609.

Durham, W.H. 1976. The adaptive significance of cultural behaviour. *Human Ecology* 4:89–121.

Durham, W.H. 1978. Towards a coevolutionary theory of human biology and behaviour. In *The Sociobiology Debate*. ed. A.L. Caplan. New York: Harper and Row. Pp. 428–448.

Durham, W.H. 1982. Interactions of genetic and cultural evolution: models and examples. *Human Ecology* 10:289–299.

Durham, W.H. 1990. Advances in evolutionary culture theory. *Annual Review of Anthropology* 19:187–210.

Durham, W.H. 1991. *Co-evolution: Genes, Culture, and Human Diversity*. Stanford: Stanford University Press.

Durkheim, E. 1893. (1933). *The Division of Labour in Society*. New York: The Free Press.

Durkheim, E. 1895. (1966). *The Rules of Sociological Method*. New York: The Free Press.

Edelman, G.M. 1978. Group selection and phasic re-entrant signalling: a theory of higher brain function. In *The Mindful Brain: Organization and the Group-selective Theory of Higher Brain Function*. eds. G.M. Edelman and V.B. Mountable. Cambridge, Mass.: MIT press.

Edelman, G.M. 1989. *Neural Darwinism: the Theory of Neuronal Group Selection*. Oxford: Oxford University Press.

Edelman, G.M. 1992. *Bright Air, Brilliant Fire*. London: Allen Lane, The Penguin Press.

Eldredge, N. and Gould, S.J. 1972. Punctuated equilibrium: an alternative to phyletic gradualism. In *Models in Palaeobiology*. ed. T.J.M. Schopf. San Francisco: Cooper and company. Pp. 82–115.

Ellen, R.F. 1985. Comment on "Darwinian selection and symbolic variation". *Current Anthropology* 26:79.

Engels, F. 1884. *The Origins of the Family, Private Property, and the State*. Moscow: Progress Publishers.

Engelstad, E. 1991. Images of power and contradiction: feminist theory and post-processual archaeology. *Antiquity* 65:502–514.

Ewald, P.W. 1994. *Evolution of Infectious Disease*. Oxford: Oxford University Press.

Fagan, B.M. 1980. *People of the Earth: An Introduction To World Prehistory.* Third Edition. Boston, Toronto: Little, Brown and company.

Fenner, F., McAuslan, B.R., Mims, C.A., Sambrook, J., and White, D.O. 1974. *The Biology of Animal Viruses.* 2nd Edition. London: Academic Press.

Feyerabend, P. 1975. *Against Method: Outline of an Anarchistic Theory of Knowledge.* Norfolk: Thetford Press.

Flannery, K., Reynolds, R.G., and Marcus, J. 1989. *The Flocks of the Wamani: A Study of Llama Herders on the Punas of Ayapucho, Peru.* New York: Academic Press.

Fletcher, R.J. 1977. Settlement studies: micro and semi-micro. In *Spatial Archaeology.* ed. D.L. Clarke. Cambridge: Academic Press. Pp. 47–162.

Fletcher, R.J. 1981a. People and space. In *Patterns of the Past.* eds. I. Hodder, G. Isaac, and N. Hammond. Cambridge: Cambridge University Press. Pp. 97–128.

Fletcher, R.J. 1981b. Space and community behaviour. In *Universals of Human Thought.* eds. B. Lloyd and J. Gay Cambridge: Cambridge University Press. Pp. 71–110.

Fletcher, R.J. 1988. *The Evolution of Human Behaviour.* Australian Natural History, supplement no. 2, 1988.

Fletcher, R.J. 1989. The messages of material behaviour: a preliminary discussion of non-verbal meaning. In *The Meanings of Things: Material Culture and Symbolic Expression.* ed. I. Hodder. London: Unwin Hyman. Pp. 33–40.

Fletcher, R.J. 1992. Time perspectivism, *Annales*, and the potential of archaeology. In *Archaeology*, Annales, *and Ethnohistory.* ed. A.B. Knapp. Cambridge: Cambridge University Press. Pp. 35–49.

Flinn, M.V. and Alexander, R.D. 1982. Culture theory: the developing synthesis from biology. *Human Ecology* 10:383–397.

Foley, R. 1985. Optimality theory in Anthropology. *Man* 20:222–243.

Foley, R. 1987. Hominid species and stone tool assemblages: how are they related? *Antiquity* 61:380–392.

Forde, C.D. 1934. *Habitat, Economy and Society.* London: Methuen.

Freeman, D. 1974. The evolutionary theories of Charles Darwin and Herbert Spencer. Current Anthropology 15:211–221.

Freeman, D.K. 1983. *Margaret Mead and Samoa: the Making and Unmaking of an Anthropological Myth.* Cambridge, Mass.: Harvard University Press.

Freud, S. 1962. *Two Short Accounts of Psychoanalysis.* Translated and edited by J. Strachey. London: Penguin.

Geertz, C. 1973. *The Interpretation of Cultures.* New York: Basic Books.

Gerard, R.W., Kluckhohn, C. and Rapoport, A. 1956. Biological and cultural evolution: some analogies and explorations. *Behavioural Science* 1:6–34.

Ghiselin, M.T. 1969. *The Triumph of Darwinian Method.* Berkeley and Los Angeles: University of California Press.

Ghiselin, M.T. 1974. A radical solution to the species problem. *Systematic Zoology* 23:536–544.

Ghiselin, M.T. 1987. Species concepts, individuality, and objectivity. *Biology and Philosophy* 2:127–144.

Gould, S.J. 1980. *The Panda's Thumb: More Reflections in Natural History.* Harmondsworth: Penguin Books.

Gould, S.J. 1983. The hardening of the modern synthesis. In *Dimensions of Darwinism.* ed. M. Grene. Cambridge: Cambridge University Press. Pp. 71–93.

Greene, W.C. 1993. Aids and the immune system. Scientific American 269:66–73.

Gullick, C.J.M.R. 1985. Comment on "Darwinian selection and symbolic variation". *Current Anthropology* 26:81.

Handwerker, W.P. 1989. The origins and evolution of culture. *American Anthropologist* 91:313-327.

Harris, M. 1968. *The Rise of Anthropological Theory*. New York: T.W. Cromwell.

Harris, M. 1974. Comment on "The evolutionary theories of Charles Darwin and Herbert Spencer". *Current Anthropology* 15:222-224.

Harre, R. 1979. *Social Being*. Oxford: Basil Blackwell.

Hendy, M.D. and Penny, D. 1986. How the theory of evolution could be disproved but isn't. In *The Fascination of Statistics*. eds. R.J. Brook (et al). New York: Marcel Dekker, Inc. Pp. 173-182.

Heylighen, F. 1992a. "Selfish" memes and the evolution of co-operation. *Journal of Ideas* 2:77-84.

Heylighen, F. 1992b. Non-rational cognitive processes as changes of distinctions. In *New Perspectives on Cybernetics, Self-Organisation, Autonomy and Connectionism*. ed. R. Trappl. Singapore: World Science. Pp. 3-10.

Ho, M. and Saunders, P.T. 1982. The epigenetic approach to the evolution of organisms with notes on its relevance to social and cultural evolution. In *Learning, Development and Culture*. ed. H.C. Plotkin. Chichester: John Wiley and Sons.

Hodder, I. 1982. *Symbolic and Structural Archaeology*. Cambridge: Cambridge University Press.

Hodder, I. 1985. Postprocessual archaeology. In *Advances in Archaeological Method and Theory*. ed. M.B. Schiffer. Orlando: Academic Press. Volume 8, Pp. 1-23.

Hodder, I. 1989. Poststructuralism, Postmodernism, and Postprocessual archaeology. In *The Meanings of Things*. ed. I.Hodder. London: Unwin Hyman.

Hodder, I. 1992. *Theory and Practise in Archaeology*. London: Routledge.

Hole, I. 1973. Questions of theory in the explanation of culture change in Prehistory. In *The Explanation of Culture Change: Models in Prehistory*. ed. C. Renfrew. London: Duckworth. Pp. 19-31.

Horgan, J. 1994. Can science explain consciousness? *Scientific American* July 1994:72-78.

Howells, W.W. 1973. *Cranial Variation in Man: a Study by Multivariate Analysis*. Cambridge, Mass.: Peabody Museum of Archaeology.

Hugill, P.J. and Dickson, D.B. 1989. eds. The Transfer and Transformation of Ideas and Material Culture. Texas: A & M University Press.

Hull, D. 1976. Are species really individuals? *Systematic Zoology* 25:174-191.

Ingold, T. 1986. *Evolution and Social Life*. Cambridge: Cambridge University Press.

Ingold, T. 1990. An anthropologist looks at Biology. *Man* 25:208-229.

Ingold, T. 1991. Comment on "Biology and behaviour in human evolution". *Cambridge Archaeological Journal* 1:219-221.

Irons, W. 1979. Cultural and biological success. In *Evolutionary Biology and Human Social Behaviour: An Anthropological Perspective*. eds. N.A. Chagnon and W. Irons. North Scituate, MA: Duxbury Press. Pp. 257-272.

Irons, W. 1988. Parental behaviour in humans. In *Human Reproductive Behaviour: a Darwinian Perspective*. eds. L.L. Betzig, M. Borgerhoff Mulder, and P. Turke. Cambridge: Cambridge University Press. Pp. 307-314.

Jones, M. 1993. Comment on "The Darwinian resurgence and the cultural virus critique". *Cambridge Archaeological Journal* 3:197-198.

James, W. 1896. *The Principles of Psychology, Vol 1.* London: Methuen.
Kamminga, J. and Wright, R.V.S. 1988. The Upper Cave at Zhoukoudian and the origins of the Mongoloids. *Journal of Human Evolution* 17:739-767.
Kimura, M. 1983. *The Neutral Theory of Molecular Evolution.* Cambridge: Cambridge University Press.
Kimura, M. 1985. The neutral theory of molecular evolution. *New Scientist* 11:41-46.
Kirch, P.V. 1980. The archaeological study of adaptation: theoretical and methodological issues. In *Advances in Archaeological Method and Theory.* ed. M.B. Schiffer. Orlando: Academic Press. Volume 3, Pp. 101-156.
Knight, C. 1993. Comment on "The Darwinian resurgence and the cultural virus critique". *Cambridge Archaeological Journal* 3:191-194.
Koerper, A.L. and Stickel, E.G. 1980. Cultural drift: a primary process of cultural change. *Journal of Anthropological Archaeology* 36:463-469.
Kroeber, A.L. 1948. *Anthropology.* New York: Harcourt and Brace.
Kuby, J. 1992. *Immunology.* New York: W.H. Freeman and Company.
Lamarck, J. B. Monet de, 1809 (1914). *Zoological Philosophy: an Exposition With Regard to the Natural History of Animals.* Translated by, and with an introduction by Hugh Elliot. London: MacMillan.
Lee, P.C. 1991. Biology and behaviour in human evolution. *Cambridge Archaeological Journal* 1:207-226.
Lenski, G. 1970. *Human Societies: a Macrolevel Introduction to Sociobiology.* New York: McGraw Hill.
Leonard, R.P. and Jones, G.T. 1987. Elements of an inclusive evolutionary model for archaeology. *Journal of Anthropological Archaeology* 6:199-219.
Levine, D.S. 1988. Survival of the synapses. *The Sciences* 28:46-52.
Levi-Strauss, C. 1963. *Structural Anthropology.* Translated by C. Jacobson and B.G. Schoepf. New York: Basic Books.
Lewontin, R., Rose, S., and Kamin, L.J. 1984. *Not in Our Genes: Biology, Ideology, and Human Nature.* New York: Pantheon Books.
Linden, E. 1974. *Apes, Men, and Language.* New York: Penguin Books.
Lourandos, H. 1983. Intensification: a late Pleistocene-Holocene archaeological sequence from Southwestern Victoria. *Archaeology in Oceania* 18:81-94.
Lorenz, K. 1963. *On Aggression.* Translated by Marjorie Latzke. London: Methuen.
Lumsden, C.J. and Wilson, E.O. 1981. *Genes, Mind, and Culture: the Co-evolutionary Process.* Cambridge, Mass.: Harvard University Press.
Margulis, L. 1981. *Symbiosis in Cell Evolution.* San Francisco: W.H. Freeman.
Marx, K. 1867. (1967). *Capital.* 3 Volumes. New York: International Publishers.
Maschner, H. and Mithen, S. 1993. eds. *Darwinian Archaeologies.* New York: Plenum Press.
Mayr, E. 1976. Typological versus population thinking. In *Evolution and the Diversity of Life.* ed. E. Mayr. Cambridge, Mass.: Harvard University Press. Pp. 26-29.
Mayr, E. 1982. *The Growth of Biological Thought.* Cambridge, Mass.: Harvard University Press.
Miller, J. 1993. *The Passion of Michel Foucault.* London: Harper/Collins.
Mitchison, A. 1993. Will we survive? *Scientific American* 269:102-108.
Mithen, S. 1989. Evolutionary theory and post-processual archaeology. *Antiquity* 63:483-494.
Mithen, S. 1993a. Pictures in the mind. *The Times Higher Education Supplement* July 9, 1993:17.
Mithen, S. 1993b. Comment on "The Darwinian resurgence and the cultural virus critique". Cambridge Archaeological Journal 3:194-195.

Monod, J.L. 1974. On the molecular theory of evolution. In *Problems of Scientific Revolution.* ed. R. Harre. Oxford: Clarendon Press. Pp. 11-24.

Morgan, L.H. 1877. *Ancient Society.* Chicago: Kerr Press.

Moritz, E. 1990. Memetic science: I - general introduction. *Journal of Ideas* 1:1-23.

Murdock, G.P. 1968. *Ethnographic Atlas.* Published as a series in the journal *Ethnology*:1968.

Murray, T.A. 1987. *Remembrance of Things Present: Appeals to Authority in the History and Philosophy of Archaeology.* PhD Thesis. Department of Anthropology, University of Sydney, Australia.

Neff, H. 1992. Ceramics and evolution. In *Archaeological Method and Theory.* Vol. 4. ed. M.B. Schiffer. Tucson: University of Arizona Press.

Nickerson, G.S. 1985. Comment on "Darwinian selection and symbolic variation". *Current Anthropology* 26:83.

O'Brien, M.J. and Holland, T.D. 1990. Variation, selection, and the archaeological record. In *Archaeological Method and Theory.* ed. M.B. Schiffer. Tucson: University of Arizona Press. Vol 2, Pp. 177-197.

O'Brien, M.J. and Holland, T.D. 1992. The role of adaptation in archaeological explanation. *American Antiquity* 57:36-59.

Paul, W.E. 1993. Infectious diseases and the immune system. *Scientific American* 269:56-65.

Peters, S. 1980. Comment on "The Decline and Rise of Mesopotamian Civilisation". *American Antiquity* 45:610-614.

Platnick, N.I. 1979. Philosophy and the transformation of cladistics. *Systematic Zoology* 28:537-546.

Poirier, F.E. 1981. *Fossil Evidence: The Human Evolutionary Journey.* Third Edition. Toronto, London: The C.V. Mosby Company.

Popper, K. 1974. The rationality of scientific revolutions. In *Problems of Scientific Revolution.* ed. R. Harre. Oxford: Clarendon Press. Pp. 72-101.

Postgate, J. 1989. Microbial happy families. *New Scientist* 21 January 1989:40-44.

Pulliam, H.R. 1982. A social learning model of conflict and co-operation in human societies. *Human Ecology* 10:353-363.

Pulliam, H.R. 1987. *Foraging Behaviour.* New York: Plenum Press.

Pulliam, H.R. and Dunford, C. 1980. *Programmed to Learn: an Essay on the Evolution of Culture.* New York: Columbia University Press.

Renfrew, C., and Bahn, P. 1991. *Archaeology: Theories, Methods, and Practise.* London: Thames and Hudson.

Renfrew, C. 1994. Towards a cognitive archaeology. In *The Ancient Mind.* Eds C. Renfrew and E. Zubrow. Cambridge: Cambridge University Press. Pp. 3-12.

Richerson, P.J. and Boyd, R. 1978. A dual inheritance model of the human evolutionary process. 1. Basic postulates and a simple model. *Journal of Social and Biological Structures* 1:127-154.

Richerson, P.J. and Boyd, R. 1985. Comment on "Darwinian selection and symbolic variation". *Current Anthropology* 26:83.

Rindos, D. 1980. Symbiosis, instability, and the origin and spread of agricultural systems: a new model. *Current Anthropology* 21:751-772.

Rindos, D. 1984. *The Origins of Agriculture: An Evolutionary Perspective.* Orlando: Academic Press.

Rindos, D. 1985. Darwinian selection, symbolic variation, and the evolution of culture. *Current Anthropology* 26:65-88.

Rindos, D. 1986a. The genetics of cultural anthropology: toward a genetic model of the origin of the capacity for culture. *Journal of Anthropological Archaeology* 5:1-38.

Rindos, D. 1986b. The evolution of the capacity for culture: sociobiology, structuralism and cultural selectionism. *Current Anthropology* 27:315-331.

Rindos, D. 1989. Undirected variation and the Darwinian explanation of cultural change. In *Archaeological Method and Theory*. ed. M.B. Schiffer. Volume 1, Pp. 1-48.

Rosenberg, M. 1990. The mother of invention: evolutionary theory, territoriality, and the origins of Agriculture. *American Anthropologist* 92:399-412.

Ross, C. 1987. Comment - A misuse of evolutionary models in archaeology: "The Evolution of Polynesian Chiefdoms". *Archaeology in Oceania* 22:37-38.

Sacks, O. 1993. Making up the mind. *The New York Review of Books* April 8 1993:42-49.

Sahlins, M.D. 1976. *The Use and Abuse of Biology: An Anthropological Critique of Sociobiology*. Ann Arbor: University of Michigan Press.

Sahlins, M.D. and Service, E.R. 1960. *Evolution and Culture*. Ann Arbor: University of Michigan Press.

Sartre, J.P. 1957. *Being and Nothingness*. Translated by Hazel Barnes. London: Methuen.

Schiffer, M.B. 1988. The structure of archaeological theory. *American Antiquity* 53:461-485.

Skinner, B.F. 1953. *Science and Human Behaviour*. New York: Macmillan.

Shanks, M. and Tilley, C. 1987a. Reconstructing Archaeology: Theory and Practise. Cambridge: Cambridge University Press.

Shanks, M. and Tilley, C. 1987b. *Social Theory and Archaeology*. Cambridge: Polity Press.

Shennan, S. 1989. Cultural transmission and cultural change. In *What's New? A Closer Look at the Process of Innovation*. eds. S.E. Van der Leeuw and R. Torrence. London: Unwin. Pp. 331-346.

Shennan, S. 1991. Tradition, rationality and cultural transmission. In *Processual and Postprocessual Archaeologies: Multiple Ways of Knowing the Past*. ed. Preucel, R.W. Center for Archaeological Investigations, Occasional Paper No. 10. By the Board of Trustees, Southern Illinois University. Pp. 197-208.

Slap, J.K. 1991. Virile viruses. *Australian Natural History* 23:668.

Sonea, S. 1988. The global organism. *The Sciences* 28:38-56.

Spencer, H. 1873. The Study of Sociology. No. XVI: Conclusion. *Contemporary Review* No. 22:663-677.

Sperber, D. 1985. Anthropology and psychology: towards an epidemiology of representations. *Man* 20:73-89.

Spuhler, J.N. 1984. *The Evolution of Apes and Humans: Genes, Molecules, Chromosomes, Anatomy and Behaviour*. The Douglas Ormonde Butler Memorial Lecture for 1984. Presented at the University of Queensland, Australia.

Stevenson, L. 1987. *Seven Theories of Human Nature*. Oxford: Oxford University Press.

Steward, J.H. 1955. *Theory of Culture Change*. Urbana: University of Illinois Press.

Symons, D. 1979. *The Evolution of Human Sexuality*. Oxford: Oxford University Press.

Thorndike, E.L. 1911. *Animal Intelligence*. New York: Hafner.

Turney, J. 1994. Matter over mind. The *Times Higher Education Supplement* May 27 1994:17.

Tylor, E.B. 1871. *Primitive Culture.* In 2 Volumes. New York: Harper and Row.

Wallace A.R. 1889. *Darwinism: an Exposition of the Theory of Natural Selection.* London: MacMillan.

Watson, R.A. and Watson, P.J. 1969. *Man and Nature: An Anthropological Essay in Human Ecology.* San Francisco: Harcourt, Brace and World Inc.

Wilson, E.O. 1971. *The Insect Societies.* Cambridge, Mass.: Harvard University Press.

Wilson, E.O. 1975. *Sociobiology: The New Synthesis.* Cambridge: Cambridge University Press.

Wittgenstein, L. 1953. *Philosophical Investigations.* English text of the Third Edition. New York: Macmillan.

White, D. 1989. Demonstration diskette for Maptab. *World Cultures Quorum.* (Leaflet) Vol. 1 No. 1.

White, L. 1949. *The Science of Culture.* Grove Press: New York.

White, L. 1959a. *The Evolution of Culture.* New York: McGraw Hill Publishers.

White, L. 1959b. The concept of evolution in Anthropology. In *Evolution and Anthropology: A Centennial Appraisal.* ed. B.J. Meggers. Washington: Anthropological Society of Washington. Pp. 106–124.

Wright, S. 1967. Comments. In *Mathematical Challenges in the Neo-Darwinian Interpretation of Evolution.* Wistar Institute Symposium, Monograph 5. Pp. 117–120.

Wright, R.V.S. 1989. *Doing Multivariate Archaeology and Prehistory: Handling Large Data Sets with MV-Arch.* Department of Anthropology, University of Sydney, Australia.

Yoffee, N. 1979. The decline and rise of Mesopotamian civilisation: an ethnoarchaeological perspective on the evolution of social complexity. *American Antiquity* 44:5–36.

Yoffee, N. 1980. Reply to Dunnell and Wenke: Do you see yonder cloud that looks like a camel? *American Antiquity* 45:615–619.

Yoffee, N. 1985. Perspectives on trends toward social complexity in prehistoric Australia and Papua New Guinea. *Archaeology in Oceania* 20:41–48.

Zubrow, E.B.W. 1993. Comment on "The Darwinian resurgence and the cultural virus critique". *Cambridge Archaeological Journal* 3:195–197.

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