

8 Adaptation and the Aquatic Ape

Alan Turner

SUMMARY

The arguments for an aquatic ape phase during the course of human evolution represent a 'Just So Story' approach to explaining evolutionary development, and show little awareness of the current debate in evolutionary biology about the significance of apparent adaptations. The fact that aspects of our anatomy and physiology permit us to operate in water offers no insight into our recent evolutionary history, unless couched in the questionable terms of the adaptationist programme whereby origin is simply interpreted in terms of current utility. While the details remain to be established, the hominids appear to have evolved in the same manner as other members of the African terrestrial mammalian biota, and our aquatic 'adaptations' are adequately explained as part of our generalised mammalian ability to cope with a range of circumstances.

INTRODUCTION

The argument put forward by Morgan (1972, 1984, 1985, 1986; Morgan and Verhaegen, 1986) in favour of the Aquatic Ape Theory of human origins is presented as a fresh approach, unbiased by prior assumptions of the kind to be found in conventional writings on the subject of human evolution. But the sequence of Morgan's writings shows that the aquatic phase is itself taken as an established fact, with new fossil evidence simply fitted into the overall scenario. The result is that any resolution of the question, 'Did we or did we not go through an aquatic phase?', is rendered impossible. We simply get bogged down in disputes about whether hair loss, sweating, tears, diving ability, subcutaneous fat in babies and a host of other attributes necessitate a period of life in the water at some point in our past.

It is clear that most if not all of these attributes do not militate *against* an aquatic lifestyle of some sort or other, but that is scarcely the same as saying that they indicate an aquatic ape forebear. The arguments presented by Morgan, and by Hardy (1960, 1977), exemplify what may be termed the 'Just So Story' or *ad hoc* approach to explaining evolutionary development – a method of scientific inquiry that is of dubious validity (Clutton-Brock and Harvey, 1979). We might as well argue that the fine balance controls and other characteristics that enable us to ride bicycles must have been developed during a bicycle-riding phase in the Pliocene – perhaps with tandems as reinforcing devices for ensuring monogamous coupling (although not necessarily in transit). If

bicycles are unknown from the Pliocene we obviously have to look harder for them, unblinkered by the academic status quo.

I do not intend to become involved in the discussion about specific details of morphology and physiology. Instead, I want to step back and examine the logic that underlies the development of the AAT, and then to examine some of the explanations offered by Morgan and Hardy as examples of the tenor of the argument.

THE ADAPTATIONIST SCENARIO

Evolution is the chief organising concept of modern-day biology, and a key aspect of that organisation is the notion of adaptation. But what is an adaptation? Williams (1966), Lewontin (1978), Gould and Lewontin (1979), Clutton-Brock and Harvey (1979) and Gould and Vrba (1982) have all pointed in recent years to the difficulties that beset the unwary who use the term in an unguarded manner. Gould and Vrba (p. 5) cite the early recognition of the problem by Darwin (1859, p. 197) and repeat his example: skull sutures in young animals may indeed assist parturition, but if their presence is due to the 'laws of growth' (in Darwin's words) then they cannot be termed adaptations towards ease of birth. But, to this day, the logic of Darwin's argument appears to have fallen on deaf ears among those who offer an adaptationist explanation for every trait of an organism in what Gould and Lewontin term the 'adaptationist programme'. As Gould and Vrba (p.13) express it, 'current utility carries no automatic implication about historical origin'. In making this point, the latter authors were particularly concerned to highlight flaws in the concept of preadaptations, but in the course of their discussion usefully illustrated the problems surrounding the entire issue of adaptation. In their view, only features promoting fitness *and built by selection for their current roles* may be termed adaptations, whereas the operation of other useful characters not built by selection for their current roles should be considered as effects. In the present context this may seem like rather rarefied academic hair-splitting, but it illustrates the essential point that one cannot simply assume that a given character was selected to perform a given function. And if one cannot make such an assumption, then it becomes very difficult indeed to use such characters to infer the precise course of evolutionary development and the detailed natural history of a species at given times.

Seen from this perspective, the AAT is a first-class example of the adaptationist programme taken to an extreme. For it interprets virtually every feature of human morphology, physiology and behaviour that does not positively indicate hydrophobia as the product of natural selection during an aquatic phase of hominid evolution. This procedure is, of course, unsound. No consideration is given to the role of incidental

effects in the development of characters. No effort is made to distinguish between any likely effects and any possible true adaptations. The entire presentation is one of dubious scientific value, offered under the flimsy pretext of examining the supposedly competing 'hypotheses' of neoteny and a strictly savannah origin.

As Morgan (1982; p. 22) admits, neoteny is a mechanism. As such it cannot be set in comparison with theories of human origins in a savannah environment; yet although it is really irrelevant to the argument, it is forced to function as straw-man in the presentation of the AAT. As for shortcomings in the argument for human origins on the African savannahs, it is not particularly difficult to criticise as inadequate in one way or another some of the published efforts to produce a synthesised interpretation of human evolution. Many such efforts appear in the secondary literature, often written by people with no greater claim to expertise in current evolutionary theory (and its major points of debate) than familiarity with popular notions of Darwinian natural selection, survival of the fittest and adaptation as an overall outcome. The correlation between the first and last of these three is usually taken to be extremely close. Within- and between-species changes are often inextricably confused, leading to the assumption that major adaptive changes must correlate with speciation events. A new hominid species must, in such an interpretation, have exhibited locomotor and behavioural traits, as well as a level of intelligence, different from anything that went before. On the other hand, more specialist investigations often concentrate on particular features of hominid skeletal anatomy and the inferences that may be drawn about relevant details of the soft tissues. Over-emphasis on the possible adaptive significance of certain skeletal features is almost predictable in such circumstances. To judge by the examples quoted by Gould and Lewontin (1979), few such specialists are likely to be immune to the lure of the adaptationist programme, or to be able to distinguish true reasons for origin from interpretations of current utility. And although they are concerned with evolution in a particular family, few such specialists are really evolutionary biologists engaged in debate about the theoretical underpinnings of the subject. Rather, they are users of evolutionary theory as they currently understand it, and as Gould and Lewontin, and Gould and Vrba (1982), have pointed out, opportunities for misunderstanding are rife.

But all such interpretations, by synthesisers or by specialists, correct in their details or otherwise, set the hominids within a generalised woodland-savannah mosaic of the kind clearly indicated for eastern Africa during the Plio-Pleistocene. This is not surprising, because that is what the environmental evidence points to for the areas in which all the East African hominid fossils have so far been found. It is not as though

the 'conventional' interpretations of hominid evolution are being forced into some inappropriate and wrong-headed palaeo-environmental mould. The Savannah Theory of human evolution does not stand in competition with one or other rival theories, as Morgan suggests. The pattern that we see and the evolutionary process that we infer takes place within a changing vegetational mosaic, in turn linked to environmental shifts (Vrba *et al.*, 1985). Abundant evidence is now emerging that macroevolutionary events in the African terrestrial biota are closely correlated with environmental changes, and the hominids do not appear to be unusual in this respect (Bonnefille, 1985; Delson, 1985; Vrba, 1985a, 1985b; Turner, 1985, 1986a, 1986b; Wesselman, 1985). That these environmental changes are likely to be a major causal factor in macroevolution has received strong support from recent developments in evolutionary theory (Vrba, 1985c, 1985d; Paterson, 1986), developments that stress the continued existence of a species while its normal habitat prevails. In other words, while we may quibble about details and adaptive scenarios, the terrestrial evolution of the hominids makes very good sense when assessed in conjunction with the palaeo-environmental evidence and the evolution of contemporaneous taxa, and is supported by a strong inference about the cause of speciation.

ON THE BEACH

What do Morgan and Hardy offer between them by way of a replacement for terrestrial origins of the hominids? The simple answer is a total explanation for every feature; but here I shall be necessarily selective. Three major points seem to lie at the heart of the AAT: first, the problem of our missing body hair; second, the implications of our apparent aquatic adaptations; and third, the significance of the fossil evidence.

The need to invoke an aquatic development phase in explanation for our absent body hair stems from a perceived evolutionary paradox: we have lost our hair but apparently gained subcutaneous fat. As viewed by Morgan and Hardy, this seems to make no sense in a savannah-living animal; yet the real problem lies in the perspective, and in the assumption that each feature can be tied to a specific, adaptive response. We are told by Morgan that presumed hair loss in humans may have resulted from selection pressure for faster swimming. The evidence is merely a claim by Hardy (1977) that members of the Sydney University swimming team saved one second in a one-hundred-yard swim by shaving off all their body hair. Hardy suggested that groups of our ancestors might have been better able to escape a shark attack by being less hairy. But while a fast-swimming species might benefit from a little fine-tuning of streamlining,

are we really to believe that a swimming primate in a life-or-death contest with a superb swimmer such as a shark would achieve much by the loss of body hair?

Hardy followed his suggestion about body hair with a claim that the streamlined shape of the human body is further evidence for an aquatic past, a claim with which Morgan appears to agree. But both authors completely overlook the fact that our present-day body shape can have little to do with the matter if the intense selection for a streamlined body shape took place during the Pliocene, when hominid size, limb proportions and body shape were somewhat different and remained different. One might also note that the proboscis monkey, favoured by Morgan (1982) as an example of a primate with a generalised swimming ability, shows few obvious signs of the supposed characters strongly selected for in our own lineage. In other words, it can, like ourselves, get by very well in the water with its existing generalised abilities.

Hardy's article concluded with a suggestion concerning a 'recent' discovery (the diving reflex — see Patrick, this volume, chapter 14) — one which, in Hardy's words 'is almost as conclusive as the fossil evidence, I believe'. His suggestion concerned putting members of all known ape species in a bath with their faces submerged, to see if they, like other diving species and humans, exhibit a diving reflex. The presence of the reflex was to be determined by electrocardiographically monitored changes in the circulation of the blood. Morgan (1982) draws attention to the practical difficulties involved in such an experiment, but reiterates Hardy's belief in the importance of the phenomenon for the AAT. But the diving reflex, including both breath-holding and bradycardia, seems to be a much more generalised mammalian response than Morgan considers it to be (Zapol, 1987), and really offers little support for ideas about selection pressures during our relatively recent past.

It is quite legitimate to investigate the physiology of an animal that clearly has a major capacity for aquatic activity, to examine the features that permit it to dive deeply or to swim quickly, and to talk in terms of adaptations to such a mode of life (provided, of course, that one bears in mind the necessary caveats about historical origin in relation to current utility discussed above). But it is quite another thing to take human characteristics that *permit* aquatic activity and to construct from them an otherwise wholly unsupported evolutionary scenario of the kind presented by Morgan. The Weddell seal, *Leptonychotes weddelli*, for example, is obviously well adapted to life in deep, cold seas, and shows a remarkable range of physiological and anatomical refinements to this end (Zapol, 1987). Human 'adaptations' to aquatic life are by comparison rather feeble, among the range in animals able to cope in water rather than in those that have to exist in it permanently or for much of their

time. Viewing human abilities in terms of intense selection pressures during an aquatic phase of development therefore appears a rather extreme explanation for what are no more than generalised mammalian abilities. To raise this objection is not to render human characteristics 'explanation-proof', as asserted by Morgan (1984, p. 12). The intention is, rather, to avoid raising what is no more than an *ad hoc*, Just So Story 'explanation' to the status of a 'proof', in the manner of the AAT.

The issue of the fossil evidence, or rather its absence, appears at a number of places in the argument. Hardy (1977) suggested that attention should be concentrated in those areas of the world where fossil proof of his theory might be forthcoming. It is hard to envisage precisely what form such proof might take, however, since simply finding hominid fossils in deposits close to the sea would hardly inform us about intense marine activities of the kind implied by the theory. In contrast, Morgan (1984, p. 12) attacks what she characterises as the 'we must wait for more evidence' school of palaeontological response to the AAT, claiming that such a stance 'tacitly assumes that fossil evidence is in some way intrinsically more valid than other kinds of evidence', and that 'many scientists are disputing this proposition'. In support of that latter point she cites a statement by Colin Patterson to the effect that fossils rarely overturn theories of relationships based on recent organisms. That citation reveals a degree of unfamiliarity with biological debate. Patterson was referring to systematics and to the question of the part to be played by fossil data in reaching decisions during cladistic analyses of relationships. His statement has nothing to do with the utility of fossil data in inferring past activities of a taxon, and Morgan has simply taken it out of context.

As to the question of whether fossil evidence is intrinsically more valid than other kinds, the answer depends on what it is one wants to know about. It would certainly be difficult to learn very much about Plio-Pleistocene hominids without the fossil evidence. And in the same paper that questions the supremacy of the fossils, Morgan still seems to find in the most recent finds considerable support for her ideas. For example, we read (Morgan, 1984, p. 13) that the bipedality of our putative ancestor *Australopithecus afarensis* may have been the result of an aquatically induced preadaptation towards bipedal locomotion. Fortunately for Aquatic Ape Theory supporters, the aquatic phase took place during the undocumented gap in the fossil record before 4 million years ago, presumably at the same time that they believe we were losing our hair in order to be able to swim faster than sharks. But if the selection pressure for swimming speed was so intense, then why, one might ask, did the process end up with hominids possessing a skeleton like Lucy's that lacks evident adaptations towards high-speed swimming? Morgan (1982, p. 78) offers the observation that 'no other aquatic mammal is descended from a

primate. This would affect, for one thing, his style of swimming', and goes on to argue that apes can adopt a frog-like swimming action. That may be true, and *A. afarensis*, even with its relatively short legs (Susman, Stern and Jungers, 1985), may also have been quite adept at such a stroke, but that argument does little to clarify the need for hair loss in an animal moving through the water in such a manner. At this point the AAT starts to become entangled in its own adaptationist web: the only solution would be to discard elements of the adaptationist explanation – but which ones? If one adopts a rigorously adaptationist stance then how does one select from among the multitude of phenomena to be accorded such an explanation when the explanations begin to conflict?

The fact that we have a dearth of fossil evidence prior to 4 million years ago does not really mean that all options are open for guessing what happened in the late Miocene and early Pliocene. It serves no scientific purpose to set out hypotheses of 'preadaptations' for bipedality based on Just So Stories of an aquatic lifestyle. If one is forever going to argue about 'missing evidence' that might change our views, evidence that will be found if only we look in the right place unblinkered by conventional wisdom, then one might as well go the whole way and put everything down to unknown causes. Mio-Pliocene outings to the seaside by hominids are not ruled out, but they need hardly be invoked as causal mechanisms.

CONCLUSION

Ideas about evolution, both in overview and in relation to specific taxa and events, are constantly subjected to criticism from within the biological sciences. I have discussed here some of the criticisms levelled at the adaptationist programme in particular, as well as pointing to the shortcomings of many efforts to explain the evolution of the hominids. What is apparent from these in-house disputes, however, is the need for an awareness of the basic issues involved, the theoretical underpinnings of the subject. Evolution is not in dispute, but some of the details are being revised, and many working in the field are not fully aware of the details of the debate. In criticising from outside, one can be right (to criticise) for the wrong reasons if one is not aware of the larger problems – a trap that always awaits the non-specialist commentator in any field. Subjects like evolution in general, and human evolution in particular, seem to invite non-specialist attention (at least in the form of criticism) rather more than, say, physics and its more esoteric aspects like quantum mechanics. Perhaps it is because much of physics is largely impenetrable without the mathematical tools, whereas evolution gives more the appearance of an academic free-for-all indulged in by everybody from Creationists upwards. This is not a dismissal of critics (Creationists

excepted), but it is an attempt to encourage an appropriate degree of caution on the subject. There is more to understanding evolution than a belief in natural selection and the construction of adaptationist scenarios, and if specialists can be shown to have fallen into that trap then the non-specialist may need to exercise great caution.

ACKNOWLEDGEMENTS

I am grateful to C.A. Gordon and A.C. Kemp for discussion of many of the points raised here, and for comments on an earlier version of the manuscript.

REFERENCES

- Bonnefille, R., 1985, Evolution of the continental vegetation: the palaeobotanical record from East Africa. *South African Journal of Science*, **81**, 267–70.
- Clutton-Brock, T.H. and Harvey, P.H., 1979, Comparison and adaptation. *Proceedings of the Royal Society of London*, **B205**, 547–65.
- Darwin, C., 1859, *On the Origin of Species* (London: John Murray).
- Delson, E., 1985, Neogene African catarrhine primates: climatic influence on evolutionary patterns. *South African Journal of Science*, **81**, 273–4.
- Gould, S.J. and Lewontin, R.C., 1979, The spandrels of San Marco and the Panglossian paradigm. *Proceedings of the Royal Society of London*, **B205**, 581–98.
- Gould, S.J. and Vrba, E.S., 1982, Exaptation—a missing term in the science of form. *Paleobiology*, **8**, 4–15.
- Hardy, A., 1960, Was man more aquatic in the past? *New Scientist*, **7**, 642–5.
- Hardy, A., 1977, Was there a *Homo aquaticus*? *Zenith*, **15**(1), 4–6.
- Lewontin, R.C., 1978, Adaptation. *Scientific American*, **239**(3), 156–69.
- Morgan, E., 1972, *The Descent of Woman* (London: Souvenir Press).
- Morgan, E., 1982, *The Aquatic Ape* (London: Souvenir Press).
- Morgan, E., 1984, The aquatic hypothesis. *New Scientist*, **102**, 11–13.
- Morgan, E., 1985, Sweaty old man and the sea. *New Scientist*, **105**, 27–8.
- Morgan, E., 1986, Lucy's child. *New Scientist*, **112**, 13–15.
- Morgan, E. and Verhaegen, M., 1986, In the beginning was the water. *New Scientist*, **109**, 62–3.
- Paterson, H.E.H., 1986, Environment and species. *South African Journal of Science*, **82**, 62–5.
- Susman, R.L., Stern, J.T. and Jungers, W.L., 1985, Locomotor adaptations in Hadar hominids. In *Ancestors: The Hard Evidence*, ed. E. Delson (New York: Liss), 184–92.
- Turner, A., 1985, Extinction, speciation and dispersal in African larger carnivores, from the Late Miocene to Recent. *South African Journal of Science*, **81**, 256–7.
- Turner, A., 1986a, Some features of African larger carnivore historical biogeography. *Palaeoecology of Africa*, **17**, 237–44.
- Turner, A., 1986b, Species, speciation and human evolution. *Human Evolution*, **1**, 419–30.
- Vrba, E.S., 1985a, African bovidae: evolutionary events since the Miocene. *South African Journal of Science*, **81**, 263–6.
- Vrba, E.S., 1985b, Ecological and adaptive changes associated with early hominid evolution. In *Ancestors: the Hard Evidence*, ed. E. Delson (New York: Liss), 63–71.
- Vrba, E.S., 1985c, Environment and evolution: alternative causes of the temporal distribution of evolutionary events. *South African Journal of Science*, **81**, 229–36.

- Vrba, E.S., 1985d, Introductory comments on species and speciation. In *Species and Speciation*, ed. E.S. Vrba (Pretoria: Transvaal Museum Monograph no. 4, ix-xviii).
- Vrba, E.S., Burckle, L.H., Denton, G.H. and Partridge, T.C. (eds.) 1985, Palaeoclimate and Evolution I. *South African Journal of Science*, **81**, 224-75.
- Wesselman, H.B., 1985, Fossil micromammals as indicators of climatic change about 2.4 myr ago in the Omo Valley, Ethiopia. *South African Journal of Science*, **81**, 260-1.
- Williams, G.C., 1966, *Adaptation and Natural Selection* (Princeton, New Jersey: Princeton University Press).
- Zapol, W.M., 1987, Diving adaptations of the Weddell seal. *Scientific American*, **256**(6), 80-5.