

Part II

REACTIONS TO THE AQUATIC APE THEORY: FOR AND AGAINST

6 The Refutation that Never Was: The Reception of the Aquatic Ape Theory, 1972–1987

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SUMMARY

Elaine Morgan's work has been rejected or ignored by scientific researchers in the human evolution field since the publication of her first book on the subject, *The Descent of Woman*, in 1972. This chapter considers the reasons for this rejection, which persisted in the absence of any adequate refutation of the Aquatic Ape Theory (AAT). It is argued that no refutation was forthcoming because there was a lack of consensus among the scientific workers themselves regarding the central issues of bipedalism and hairlessness. The rejection of Morgan's work is considered to be primarily due to her outsidership, a position exacerbated by the barriers which human evolution researchers had understandably erected in the face of the speculative, outlandish and amateur attentions which the topic, by its very nature, tends to attract. On examination, her work does not fall into this category. It conforms to current theories of speciation better than the savannah-origins model, and accounts for a number of diverse phenomena hitherto not seen as connected – which is also generally considered to be a major virtue in a scientific theory. Finally, it is argued that the behavioural and psychological aquatic affinities of modern humans are sufficiently marked to require an evolutionary explanation, at least as much as other behaviours well established on the evolutionary research agenda.

INTRODUCTION

Between 1972, when Elaine Morgan's reformulation of Sir Alister Hardy's Aquatic Ape Theory (AAT) first appeared in her best-selling book *The Descent of Woman*, and the convening of the Valkenburg meeting in 1987, the hypothesis received no serious academic attention. The references to it in the academic literature were at best patronising (e.g., Isaac, 1983) and at worst contemptuous (e.g., Gowlett, 1984). A review by Pfeiffer in *Time* magazine (1972a) equating *The Descent of Woman* with 'another largely fictional work' – Clifford Irving's discredited biography of Howard Hughes – came close to eliciting a libel suit from the publishers Stein & Day. The same reviewer, writing a week later in the *New York Times Book Review* (1972b), noted 'Pseudoscience marches on'. Non-academic reviewers, by contrast, found the work logical, exciting, provocative and liberating. The ensuing silence was deafening. In spite of repeated pressure, the academic establishment failed to state publicly in clear terms the grounds on which the AAT was unsustainable, while assuring everybody that it was. A letter from C.O.

Lovejoy (1979) to one of Elaine Morgan's most indefatigable lay advocates, Chuck Milliken, typifies the posture adopted: 'the preponderance of evidence is so overwhelmingly contrary to the "aquatic theory" that no one found it necessary to consider a published reply'.^{*} This is unconvincing; faced with widespread popular acceptance of such an allegedly false theory, scientists have a public responsibility to refute it as firmly as possible – a responsibility they usually show no reluctance in discharging, especially given the financial rewards of doing so via the US media!

Following the publication in 1982 of Morgan's *The Aquatic Ape*, the academics continued, in the main, to turn a blind eye to the theory. Lowenstein (1984) published a scornful attack in the journal *Oceans*, and in 1986 co-authored with A. Zihlman a two-page restatement of this in *BBC Wildlife*, the editors of the latter refusing Elaine Morgan the right to a detailed reply. The Lowenstein and Zihlman posture is typified by their statement that 'the only evidence for an aquatic phase in human evolution is a television script patched together from a scrapbook of unrelated anatomical and physiological snippets'. In their few public statements on the issue since 1972, academics have persistently assured the general reader that the reasons for hairlessness and bipedalism are well understood. They have never said what they are.

LACK OF CONSENSUS AMONG THE EXPERTS

In this chapter I will argue that, regardless of the ultimate evaluation of the AAT, its treatment by the academic establishment during the period 1972–86 was patently inadequate and needs explanation. A major factor in the situation in 1972 was that the academic debate on human evolution was itself in a crucial phase. Human evolution research had had a fairly disastrous first century, in which everything that could have gone wrong did; from forgeries like Piltdown Man to accidents like the loss of the Peking Man and Mikulov Castle material, from erroneous theoretical

^{*} Responses of other leading figures to Milliken's inquiries include Mary Leakey (30 March 1980): 'I regard Mrs Morgan's theory as nothing more than the outcome of an over-fertile imagination without any possible bearing on the research on human evolution'; Robert Ardrey (5 November 1978): 'while Elaine Morgan's book is marvelous entertainment, it is not good science'. Stephen Jay Gould (14 November 1980) gives two reasons for rejecting the AAT, both of which were subsequently dealt with in Morgan (1982): (1) the evolution of strong legs is the reverse of what usually happens in aquatic mammals; (2) all the features cited are better explained by neoteny (however, as Morgan points out, this may be the mechanism, but it does not explain why the features had adaptive value – the ghost of 'orthogenesis' seems to be faintly hovering behind Gould's argument here). It should be noted, though, that all these attacks are directed at the case as presented in *The Descent of Woman* rather than in the more recent *The Aquatic Ape*.

presuppositions (brain-first and eastern origins models) to personality problems (Virchow, Dubois). Following the Second World War, with the discoveries in Olduvai and South Africa of further Australopithecines and *Homo habilis*, the situation had improved, and the possibility of settling down to 'normal science' seemed to be on the agenda at last. By 1972 something approaching a consensus was on the verge of emerging. But it was very fragile, and already under pressure from within the scientific community, most awkwardly from molecular biologists. The solid closing of ranks by human evolution researchers in the face of *The Descent of Woman* masked the fact that the solid, reliable, 'scientific' knowledge of the facts of human evolution, which so assuredly disproved the AAT, was a fiction, and that internal dissensus was mounting. In the event it transpired that most of the key features of the 1972 consensus – in so far as it existed – were to be rejected over the following decade. These included the beliefs that:

- (1) *Ramapithecus* was probably the earliest hominid.
- (2) The hominid and pongid lineages split approximately 15 million years ago.
- (3) Hunting was a major feature of early hominid lifestyle.
- (4) Bipedalism was directly associated with brain enlargement.
- (5) Bipedalism was directly linked to stone-artefact manufacture.
- (6) Hairlessness was sexual in function (facilitating display of secondary sexual characteristics).
- (7) Although the initial adaptive value of bipedalism was unknown, facilitation of hunting was a likely candidate.

On the vital questions of the timing of the origin of bipedalism and the similarity of australopithecine walking to that of modern humans, there was heated contention. Pilbeam (1972, p. 71) can only vaguely talk about 'the Middle to Late Pliocene', and has doubts about the efficiency of australopithecine gait; he notes (Pilbeam, 1970) that the issue is puzzling and that 'The combination of circumstances which led to the emergence of hominid bipedalism two million or more years ago was clearly unique' (p. 95). There was in fact a major controversy between those who saw the *Australopithecinae* as very human-like and those who saw them as very different, even having evolved bipedalism independently (Napier, cited in Pilbeam, 1970, p. 142). (See Zuckerman, 1973, for key papers in this dispute centring on the interpretation of the Sts-5 pelvis.)

Every single one of the above beliefs is now either rejected, or adhered to by only a small minority of scientific authorities. Given that the AAT focused particularly on hairlessness and bipedalism, it is clear that the establishment position on these matters in 1972 was entirely devoid of the reliability that it claimed. Since its own theories on these issues were

either false or matters of controversy within the discipline, it was actually in no position to refute alternative ones definitively.

SAVANNAH ORIGINS – THEORY OR AXIOM?

There is only one tenet from the 1972 consensus that is still preserved – namely, the savannah-origins theory. However, on closer examination the status of this proves to be less secure than it initially appears to be. The notion that hominids originated on the savannah is less a theory than an axiom. It was never formally arrived at by a process of rigorous empirical evaluation of its merits against other ‘theories’. Rather, it slowly established itself as an axiomatic assumption on the grounds that, since our nearest relatives are forest dwellers and the earliest known fossil hominids were savannah dwellers, the crucial factor in hominid-pongid divergence was a move of hominids from forest to savannah. This is reasonable yet hardly watertight, since it leaves open the question of whether such a transition alone is sufficient to account for the manifest morphological and physiological differences between two such genetically similar groups. This question has rarely been explicitly addressed by mainstream human evolution theorists, who, treating the savannah origins account as axiomatic, frame their explanations of such differences within its parameters.

This has resulted in a proliferation of feature-specific explanations – as if we need one for bipedalism, another for hairlessness, another for fattiness, a fourth for vocal channel dominance, a fifth for ventro-ventral copulation and so on – the overall ensemble of distinctively human features being the result of a happy coincidence of a variety of adaptive responses to numerous separate selection pressures. With the exception of Wheeler’s recent formulations (see chapter 13, this volume), evolutionary theorists have rarely attempted to offer an integrated explanation of how the forest–savannah transition could have had such dramatic sequelae for hominids, given that it is a move which numerous mammals have made without evolving any hominid traits. What is needed, then, is a serious appraisal of the explanatory adequacy of the savannah-origins model, an appraisal which takes on board the whole suite of hominid features incorporated in the AAT in addition to the ‘big three’ of bipedalism, hairlessness and subcutaneous fat. The academic establishment was thus guilty of misrepresenting an axiom as an empirically based theory.

HOW RELEVANT ARE FOSSILS?

A third respect in which the palaeontological establishment in particular has overstated the firmness of its knowledge claims is the assumption that

the kinds of aquatic adaptation proposed by Sir Alister Hardy and Elaine Morgan are amenable to refutation on the basis of fossils alone. If hominid pelvic reorganisation is not, *per se*, evidence of an aquatic adaptation but is equally construable in terms of, for instance, vertical climbing, we are left with serious problems in identifying a skeletal trait which would be acceptable as unambiguously aquatic in its implications. The majority of the physiological traits with which the AAT is concerned are soft-tissue ones. This brings us to the regularly played-down problem of the identifiability of species from skeletal material alone; many extant mammalian species (such as lion and tiger) are known to be virtually undifferentiable at the skeletal level. We are bound to ask, then, how many proto-hominid subspecies were at large in East Africa between, say, 8 and 4 million years ago. What proportion of them have we so far identified? The recently discovered OH-62 (Johanson *et al.*, 1987), for example, is causing considerable problems, as it has relatively shorter legs than *Australopithecus afarensis* but has a habiline skull. So must we revise our image of *Homo habilis*, or suppose this to be a more arboreal subspecies – '*H. habilis sylvestris*' perhaps? This, one should note, could let the 'terrestrialists' off the hook even if an unambiguously aquatic hominid fossil were found – it would be perceived as a subspecies unrelated to the main hominid line.

The presence of early hominid fossils in one area (for instance, Java) has no *a priori* bearing on the probability of finding them in another area (for instance, South Africa), and hence the AAT is untouched by the discoveries to date – since no human palaeontological work has actually been done in the regions, such as the Danakil area, identified by the AAT proponents as crucial for their theory (LaLumiere, chapter 3, this volume). If fossils are to have any relevance to this debate at all, then both sides must state clearly what kinds of feature they would consider to be either characteristic of, or fundamentally incompatible with, the theses they are proposing. Since the relevance of fossils to the issue is in a sense far more central to the palaeontological establishment's case than to Elaine Morgan's – which is primarily formulated in terms of quite different kinds of data – the onus in this respect falls on them, perhaps, rather more than on the aquaticists.

Finally, it must be said that it is far from clear why the AAT is considered as intrinsically far-fetched, implausible or outlandish. Marsh-dwelling and wading primates are well known (including extinct ones like *Oreopithecus* (Azzaroli *et al.*, 1986)), and most classes of mammals have at least one aquatic member. As a scenario the AAT would seem to be perfectly compatible with current evolutionary theory; speciation is generally portrayed as involving the isolation of a small sub-population and its exposure to novel selection pressures (e.g., Gould and Eldredge, 1977). Such conditions typically occur when populations become

isolated, as they might be on islands. The ATT thus fits this approach far better than the savannah-origins model, in which it is far from obvious how the ancestral population could have become isolated or subjected to the intensity of novel selection pressures required to bring about such a massive divergence from the pongids (though see Malone, 1987). One criterion frequently proposed as a characteristic of a 'good' theory is that it economically accounts for and unifies a range of phenomena that have previously not been seen as related; the AAT seems to do precisely this. To refer to it as consisting of 'unrelated anatomical and physiological snippets', as Lowenstein and Zihlman (1986) do, is simply to beg the question.

THE REJECTION OF ELAINE MORGAN'S IDEAS

In the light of the above, the academic response to the AAT requires further diagnosis. We have seen that its appearance in 1972 came at a particularly delicate time for mainstream theorising, and the ensuing silence can only legitimately be construed as arising from a genuine embarrassment at being unable convincingly to refute it. It was as if the scientific establishment was saying to itself, 'It cannot be true because if it was one of us would have thought of it first'. They all seemed to be awaiting the knock-down refutation from one of their colleagues, only to discover that none of them could actually provide one. Since human evolution research was in a state of internal turmoil throughout the late 1970s and early 1980s, any extensive attempted refutation would in any case have been rejected by a substantial body of the writer's fellow scientists, for it could not fail to commit the writer on matters which were still bones of contention. But why the need to reject Morgan's work quite so categorically?

There are two suggestions one could make as to why the theory met with this peculiar combination of vehement rejection and absence of coherent refutation. The first would be to blame it on Elaine Morgan's feminist identification. Undoubtedly this was a major factor in the initial reception of *The Descent of Woman*, but the *Zeitgeist* was on her side here, and few contemporary human evolution theorists have failed to make concessions to the feminist perspective, accepting that females as much as males have played a role in human evolution. Furthermore, there are now a number of eminent women in the area, in addition to Mary Leakey: for example, Dean Falk, Sarah Blaffer Hrdy, Jane Lancaster, Sue Parker, Brigitte Senut, Pat Shipman, Elizabeth Vrba and, indeed, Adrienne Zihlman. Nancy Tanner's *On Becoming Human* (1981) is explicitly feminist in orientation, but received a hearty endorsement from F. Clark Howell and the cooperation of people like John Pfeiffer and

Don Johanson. It includes the definitely *outré* assumption that Pliocene hominids constituted a single interbreeding gene-pool. Nevertheless, no wrath descended on her head on this account (though not all reviewers were very impressed: e.g., Wind, 1983).

The second line would be to focus on Elaine Morgan's 'outsider' status (Tanner, by contrast, is a Professor of Anthropology at the University of California, Santa Cruz). This was frequently stressed in reviews of *The Descent of Woman*, and has remained her most serious handicap. Modern sciences, particularly the hard sciences such as physics, chemistry and astronomy, are generally so specialised that an outsider has little hope of making a serious contribution to them. At the opposite end of the spectrum is psychology, my own discipline, where perennial mayhem reigns and theorists can, given sufficient fervour and energy, establish some kind of following within the discipline, however bizarre their position. Human evolution studies are somewhere between. It is a topic which has frequently attracted speculation by amateurs, von Däniken (1971) and Oscar Kiss Maerth (1973) being recent examples. Although the data on human evolution are in some respects 'hard', the topic itself is psychologically highly salient, since accounts of origins inevitably serve as cultural creation myths (see Richards, 1987, chapter 1).

Given the internal problems of the disciplines concerned, it is not surprising that human evolution theorists have sought to erect a particularly impermeable boundary between their 'science' and speculative amateur nonsense. Their initial, and broadly sensible, response to outsiders has been to ignore them for fear of encouraging them and getting embroiled in time-wasting public controversy (a tactic not always successful when dealing with powerful ideologically rooted opponents such as Creationists). The same routine was triggered by the publication of Elaine Morgan's first book on the topic, and those most adamantly opposed to her appear to be continuing to try to pigeon-hole her position in this way. The journal *New Scientist*, having treated the AAT sympathetically for some years, now seems to have fallen into line. Nevertheless it is clear, especially since *The Aquatic Ape* (1982), that this policy of intellectual isolation is inadequate and badly judged. Informally one knows that some authorities are covertly sympathetic, while the theory itself does not belong in the realms of bizarre speculation. To many academics in other disciplines who have read Elaine Morgan's work its rejection appears to require far more detailed arguments than it has so far received. Vague accusations that it is too speculative or 'unscientific' carry little weight among those of us aware how high the speculative content of human evolution theorising is in general (the 1972 'consensus' turned out to be virtually entirely so!) and that what counts as 'scientific' is far from clear in the wake of post-Kuhnian philosophy of science.

THE NEED FOR HUMAN AQUATICISM TO BE ON THE EVOLUTIONARY AGENDA

The final curious feature about the reception of the AAT is not only that it was rejected but that the issues it raised were not even placed on the agenda as topics requiring examination from the evolutionary perspective. As a psychologist looking at human evolution, I believe that the level of affinity with water of modern *Homo sapiens sapiens* asks for some kind of evolutionary attention, regardless of the fate of Elaine Morgan's particular theory. Leaving aside the physiological features as such, what of the behavioural ones? We are the only land mammal, as far as I am aware (other than elephants, for whose aquatic ancestry Elaine Morgan also makes out a case in Appendix 2 of *The Aquatic Ape*), that uses water for grooming, putting great energy into the associated technology of bathrooms, showers, saunas and the like! Any extraterrestrial ethologist watching us would surely be struck by our tendencies to migrate to beaches during the summer, to build swimming-pools, to adorn our dwellings with aquaria and images of lakes and seascapes, and to locate our settlements primarily in association with rivers and lakes or by the sea. Add to all this the not inconsiderable swimming and diving skills possessed by many species members, and the major role of both marine and freshwater foods in many of our diets. Although human boat-making skills were probably developed only within the last forty millennia, they again testify to our ability to adapt rapidly to marine and lacustrine environments (see Johnstone, 1980, for an account of the evolution of sea-craft).

Now it may be objected that one is here dealing with facultative rather than obligatory behaviours. This is true, but the notion of a distinction between the two is problematical in relation to human behaviour in general. Even our bipedalism has a substantial learned component (Devine, 1985), heterosexuality is not universal, and neither is any particular domestic or social structure. In short, it is hard to identify any human behaviour which is strictly obligatory. This has not prevented researchers and theorists seeking evolutionary origins for much of it, such as Lovejoy (1981) on monogamy, Lorenz (1966) on our alleged aggression, or Thornhill and Wilmsen (1983) on rape. Exception would not be taken to the identification of extant behavioural legacies of arboreality. We are, furthermore, fascinated by the drinking of liquids of all kinds, and a high proportion of our food intake can be in liquid form (for example, the Kenyan Masai traditionally subsisted largely on milk and blood from their cattle). This seems to go way beyond what would be necessary to meet the physiological needs for water which we share with all other living creatures. We also enjoy wallowing, gaze meditatively at breaking waves, and incorporate ritual immersion and

washing into our religions. Although perhaps not too much should be made of some of these inclinations – we do also gaze at trees, mountains and even savannahs – my general feeling is that the only phenomenon exerting a comparable overall effect is fire (for which a quite different analysis would be required). The AAT does not, for most of its advocates anyway, pretend to apply to events much beyond 5 million years ago. It is not surprising, then, that some current features of human physiology are maladaptive for aquatic behaviour, having evolved since the hypothesised aquatic phase. The argument is that there is an identifiable stratum of our current behaviour and physiology that is best explained as a legacy from a distant aquatic phase, and that the ‘scrapbook of unrelated anatomical and physiological snippets’, to which we may add some behavioural ones, adds up to a suite of typically aquatic features. My present point, however, is only that current levels of human aquaticism appear, on the face of it, to deserve at least as much evolutionary consideration as some of the behavioural traits which have been so favoured, such as monogamy, aggression, rape and non-verbal communication (e.g., Eibl-Eibesfeldt, 1973).

It must be noted that an emerging difficulty with this whole area is that there is a lack of detailed information available regarding the ontogenetic plasticity of certain human physiological features of relevance to the AAT (such as subcutaneous fat levels and distribution); how variable *is* the phenotypic expression of human genotypes under different environmental conditions? (For broader theoretical discussions of the genes/environment issue, see Lewontin, 1982, 1983.)

I would like to end by stressing the theoretical importance of the case of the recently extinct Agaiumbu tribe of New Guinea, to which I drew attention in Richards (1987). This well documented lake-dwelling tribe exhibited clear physiological adaptations to their primarily aquatic environment, particularly in the musculature of their lower limbs. Walking on dry land was painful and awkward for them (Monckton, 1920). They had presumably only been living in this environment for at most a few millennia, and possibly for a far shorter period. As wives were taken from neighbouring land tribes, they were not entirely genetically isolated. (Darlington (1969) overlooked this in his passing mention of them.) The ‘aquatic’ adaptations of the Agaiumbu either resulted from some change at the genetic level or represent a physiological response to their peculiar maturational circumstances. If the former, they testify to the rapidity with which humans can evolve in the aquatic direction; if the latter, they testify to the human genetic potential for water-adaptive phenotypic expression, given the right environment. The latter seems on balance more plausible, given that they were not genetically isolated. (See also Schagatay, this volume, chapter 15).

CONCLUSION

1 The reaction of traditional human evolution researchers to the 1972 statement of the AAT involved adopting a false posture of scientific certainty and within-discipline consensus. Yet, in so far as there was a consensus at that time, its central hypotheses on the critical issues of the evolutionary significance of bipedality and hairlessness have all been subsequently abandoned, dissensus remaining the norm ever since.

2 The presentation of the savannah-origins model as a 'theory' or well established fact was misleading, since it actually functioned as an axiom, an *a priori* assumption setting the parameters within which theory-construction proper was conducted. This resulted in a proliferation of feature-specific explanations.

3 The direct relevance of fossil evidence for the AAT is unclear, since most of the features it seeks to explain are soft-tissue ones, while pelvic changes are indecisive as they can result from vertical climbing as well as swimming. The respective merits of the AAT and the savannah-origins model cannot be fully evaluated as long as the search for hominid fossils is restricted to areas predicted by the latter.* The number of proto-hominid subspecies flourishing in the late Miocene cannot be deduced from the present fossil evidence and may be difficult to establish, even in principle, given the difficulty of differentiating closely related species on skeletal anatomy alone.

4 The perception that the AAT is outlandish is unwarranted, since there is nothing intrinsically unusual about a mammalian lineage producing an aquatic species. Several extant primate species are marsh-dwelling, sea-wading and/or have aquatic behaviour in their repertoires (see Ellis, this volume, chapter 4). The speciation scenario proposed by the aquaticists is also in some respects more in line with current speciation theories than the savannah-origins model.

5 The reaction of the scientific establishment to the AAT can be understood better in terms of the perceived 'outsidership' of Elaine Morgan than in terms of its feminist component or scientific short-

* It would be an extraordinary find indeed which could unambiguously falsify either the AAT or the savannah-origins model. Nevertheless, we do seem to have a situation where the AAT proponents are making a specific prediction that, given favourable geological conditions, hominid fossils will be found in contexts where proponents of the savannah-origins model do not expect to find them. Should they be proved right, then the probability of the AAT being correct would at any rate be enhanced. But since, *a priori*, the probability of finding such fossils is considered so low (by virtue of the dominance of the savannah-origins model), the likelihood of raising the resources to test the AAT prediction is minimal. Although out of place here, it would be an interesting intellectual exercise to analyse the implications of every possible outcome of such a fossil hunt (no fossils because of absence of fossil beds, hominid fossils without aquatic features, hominid fossils with aquatic features, fossil beds but no hominids, stone tools but no hominid fossils and so on) for the respective probabilities of the two positions.

comings. It was miscategorised as belonging to a genre of amateur, speculative theorising, to which the subject of human evolution remains perennially prone. This reaction was understandable, but mistaken. It was probably exacerbated by the fact that nobody in the field could formulate a comprehensive refutation during a phase in which, even within the scientific establishment, there was controversy (often quite heated) on a number of relevant issues.

6 Finally, it was argued that current levels of human aquaticism are as much a legitimate topic for the evolutionary theorist's agenda as, for example, monogamy, aggression and rape, which are already on it. The distinction between obligatory and facultative is unhelpful when considering human behaviour, and there are surely more swimmers in the world than there are rapists.

ACKNOWLEDGEMENTS

The author is grateful to Elaine Morgan for access to her files on the reception of *The Descent of Woman* and to Chuck Milliken for photocopies of, and permission to quote from, correspondence.

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