

On the Group System of Man and Paedomorphosis

by MILFORD WOLPOFF

Cleveland, Ohio, U.S.A. 19 II 71
Crombie's contentions (CA 12:147-69) are utterly devoid of any basis in fact or theory. He denies evolutionary advantages to such hominid features as the position of the foramen magnum and the flexure of the cranium, the retarded closure of the cranial sutures, the reduced size of the dentition, the position of the big toe, and so on (p. 163).

He continually suggests that intense selection is accompanied by *increased* variability (pp. 163-64), linking factors such as loosened sexual behavior and the increased incidence of diseases affecting polygenic traits to increased intensity of selection. Indeed, the very facts presumably requiring explanation are questionable. If the prehomnids were like chimpanzees, has there really been a loosening of sexual behavior and aggressive motivation from "rigid, detailed behavior patterns"?

Neoteny is not a driving or orienting evolutionary force, but rather a description of certain directions of change. Neoteny changes, in this sense, may have no relation whatsoever to ontogeny. After all, any morphological change makes a given characteristic either more like or less like the fetus. The primary orienting factor of evolutionary change is selection. Selection may render advantageous changes that appear neoteny and allow their effect by simply prolonging or halting the developmental process. To reverse the order puts the cart before the horse.

Reply

by D. L. CROMBIE

Birmingham, England. 13 IV 71
I find it difficult to take any comment seriously which purports to be scientific and which begins: "Crombie's contentions [with regard to paedomorphosis] are utterly devoid of any basis in fact or theory." I am not sure whether *all* my contentions are covered by this umbrella or not and must content myself by answering the specific points which Wolpoff raises.

Firstly, I did not deny

evolutionary advantages to such hominid features as the position of the foramen magnum and the flexure of the cranium, the retarded closure of the cranial sutures, the reduced size of the dentition, the position of the big toe, and so on.

I did say (p. 163) that "the direct evolutionary advantages of these fea-

tures, other than brain size, seem minimal," but I also went on to say (p. 164),

Nevertheless, the modification of skull structure, and particularly the central position of the foramen magnum, was essential if the utmost advantage was to result from bipedalism, since this allowed the head to be easily balanced in the upright position. This modification may well have been the first set of circumstances which produced increasing pressure towards paedomorphosis.

Secondly, if Wolpoff is under the impression that I

continually suggest that intense selection is accompanied by *increased* variability (pp. 163-64), linking factors such as loosened sexual behavior and the increased incidence of diseases affecting polygenic traits to increased intensity of selection,

then I fear he has misunderstood my main thesis, which is summarised in the last paragraph of the paper:

In this sense, then, the demands of their primitive group system, initially imposed on individual men via language and knowledge (which was at first only implicit in the structure of language but is now an evolutionary system in its own right), first influenced the behaviour of prehomnids and subsequently drastically transformed their genetic structure via the mechanism of paedomorphosis. This had secondary effects which may be expressed in certain disease processes, including the allergies, cardio-arterial disease and hypertension, diabetes, and severe anxiety states and other mental disorders. Paedomorphosis is the essential link at the interface between the structure of man and his unique behaviour.

Any increased variability results indirectly from the choice of neoteny as the mechanism by which ever increasing behavioural plasticity could be elicited by the external selection processes.

Thirdly, for the facts about the process of neoteny itself, I quoted De Beer (1958). For a review of the facts on which the relationship of certain disease processes in humans may be linked with neotenic processes, I quoted Harper (1962). The evidence for the way in which the motivational basis of man's behavioural structure can support an enormous range of different patterns of social structuring, each with its own detailed ad hoc structure of behaviour patterns, while the equivalent variety for any primate is very restricted, is surely incontrovertible. For the former, almost any inter- or cross-cultural study in social anthropology provides evidence. I quoted Roheim (1950). For the latter, one need only refer to Chance and Jolly (1970).

Fourthly, neoteny is a "driving force" to the extent that: neoteny is one "direction of change" available to organisms, which often assures the consistent production of phenotypes which at least are viable at birth; neoteny is a "package deal" in which selection for one attribute, in this case behavioural plasticity, willy nilly involves the concomitant appearance of other, non-adaptive or even maladaptive, attributes.

I have re-read the section on pp. 163-64 that Wolpoff criticises and would agree that the words "Apart from selecting behaviour" (p. 163, head of col. 2) would more appropriately have read "Apart from allowing the selection of behaviour. . . ." At all other parts of the argument, the concept of neoteny as a "driving force" is always in the context of its prior selection by external selection processes. Whether one uses the term "driving force" for an occurrence which produces an inevitable effect in some other part of the same system or not seems to me to be a matter of personal choice.

Finally, no one would deny that the "primary orienting factor of evolutionary change is selection," and I say so explicitly in the first part of the paper. However, I do not believe that this is an "all or none" matter. The external environment, via the mechanism of natural selection, sets the prior problem for any system capable of adapting, but the possible responses are in turn constrained by what Whyte (1965) has called the internal coordinative conditions of the system. The most important of these internal co-ordinative conditions arises from the fact that all biological systems are based on organism phenotypes with a complex ontogeny, and these must be at least viable at birth. You cannot have an evolutionary situation without external selection processes, but the content of these selection processes is determined by the internal coordinative conditions of the adapting systems. Without the prior existence of these internal co-ordinative conditions, and the systems in which they occur, selection processes are meaningless. To the extent that the range of possible viable variant phenotype production is always severely restricted for any complex adapting system, the range of the content of the "natural selection" processes in the future will also be restricted. There were no selection processes until the prior evolution of a living system capable not only of reproducing, but of producing viable variants of itself. Thereafter evolution has been a step-like process of viable variants better adapted to current

selection processes but with new internal co-ordinative conditions; extended selection processes; further viable variants; and so on. Which you call the horse and which the cart depends on where you come in. I prefer to come in at the beginning.

References Cited

CHANCE, M. R. A., and C. J. JOLLY. 1970. *Subhuman primates and their social life*. Englewood Cliffs: Prentice-Hall.

DE BEER, G. 1958. *Embryos and ancestors*. London: Oxford University Press.

HARPER, R. M. J. 1962. *Evolution and illness*. Edinburgh: Livingstone.

ROHEIM, G. 1950. *Psycho analysis and anthropology: Culture, personality and the unconscious*. New York: International University Press.

WHYTE, L. L. 1965. *Internal factors in evolution*. London: Tavistock.

Historical Notes

The Great Chernichewski

by ULF HANNERZ

Stockholm, Sweden. 24 v 71

While browsing among the shelves of an antiquarian bookseller in London recently I came across a slim volume entitled *Anthropological Report on a London Suburb* which may interest anthropologists with a taste for the curiosities of their discipline. Published by Grayson and Grayson of London in 1935, it is supposedly edited by Charles Duff from the notes of "Professor Vladimir Chernichewski, The Eminent Scientist." According to Duff's preface (p. 9), the notes were handed to him "by Professor Chernichewski immediately before his departure for Cambridge to make final preparations for his famous forthcoming expedition to the Island of Capri." Duff goes on (p. 10):

Chernichewski used to say to me: "Men of science are often less honest and less accurate than writers of unabashed fiction. I believe nothing until I have investigated for myself!" The result of this fine freedom of

mind and healthy distrust of his fellow scientists is that, in matters anthropological, to quote the authority of Chernichewski is the same as it would be in matters theological to quote the authority of His Holiness the Pope.

Given the name of this renowned anthropologist and the year of publication, the volume is obviously partially intended as a parody of Malinowski. Charles Duff, then, is apparently himself the author—there is a list of his other works, including *A Handbook on Hanging* and *The Truth About Columbus*, at the back of the book, and excerpts from reviews of these works indicate that he specialized in satire. Those who are knowledgeable about the circle around Malinowski in the middle '30s could perhaps tell us whether there was any personal contact between him and Duff or any reaction to the book on the part of Malinowski or anybody else.

Anthropological Report on a London Suburb may also be seen, not too

seriously, as one of the earliest contributions to urban anthropology. Chernichewski's introduction (p. 12) provides an excellent statement of the rationale for anthropological studies in modern communities:

I would emphasize that the science of anthropology is not only concerned with the naked savage, but with the man or woman in plus fours or evening dress. To the true man of science it matters little whether he is dealing with suburb or jungle, modern jazz dancing or savage sex orgy, forest magic or the anthropomorphic deism of a suburban greengrocer, the cures and charms of the Bantu medicine-man or the work of a Fellow of the Royal College of Physicians. The difference between ourselves and savages is often more apparent than real; plus fours may conceal a brute, and a coat of paint may cover a tender heart.

As for the general description of life in the suburb of Hamperleywood, it might not appeal very strongly to the sense of irony of today's reader—partially because he is distant from the object, but perhaps also because the idiom of social satire has changed since 1935. Yet it strikes me as curious never to have come across any reference to *Anthropological Report on a London Suburb* in the anthropological literature.