(1) I do not understand why Washburn should overreact to my reference to a Lamarckian ag-
claim. I am not trying to make a parallel to the issue of the origin of species. My point was, and remains, that the mechanisms proposed by the various theories of evolution point in the direction of the Lamarckian argument. I pointed out on page 64 of the book that the evolutionists had no such intention. How-
ever, simply utilizing terms such as “language, memory, motor skills, foresight, complex social organization, and art” is only a form of hedging out these highly complex variables are brought into the direct analysis and interaction with what has essen-
tially been left as simple replacement of fission.

(2) The most pivotal point of Washburn's paper is in his rebuttal of dickey hypotheses. Washburn says that I suggest that the “sex difference in the size of a ape’s canine teeth may be due to diet.” This then means no observer in this area of research to determine the social behavior of the female chimpanzee. I expect this theme again and again in the text. On pages 64 and 65, I specifically note that Pickens and Simons suggest a dietary link, not 1. On pages 64 and 65, I even mention Washburn and his (1966b) rebuttal of such a viewpoint. Washburn curries the dietary theme even further where he implies to me the suggestion that “sexes teeth are associated with diet, the large canines might adapt to the males to some special kind of food.” Would Washburn identify the correct party that broke his hypothesis? The quote is strongly rem-
men of the quotation I gave from Washburn and Avis (1965:435) in my article. A careful reading of (1966) does not disclose where the dietary hypotheses came from.

(3) Washburn's critique on the word “hormone” and forgets to include the word “interactions,” “toxicology,” “nutrition,” and “developmental.” The result is an unfortunate misrepresentation. I thought that the last issue I wrote about the seriousness of the phenomenon was that the discovery in the case of the luteinizing hormone is of critical importance in the human.

(4) Washburn suggests that I ignored early hu-
men evolution, and particularly sexual di
ciphasms, as an example of a human behavioral or social behavior. I did not say that “human sexual

behavior differs from that of apes because of dif
ferences in environment.” I said that I was ful-
testing the possibility of the female plus license of rela-
dicacy effects (i.e., effects on diet) and that if Avis et al. (1965a) plus a switch to a carnivorous diet food then the large canines in sexually dimorphic species might aid the canine to induce endocrine changes in the female. Endocrine changes in this sense might cause the feminization of the male. This is the kind of change that I was talking about in feminization, target tissue sensitivity, rates of growth, differ-
cents feedback thresholds, etc. General instructions for growth and development do not do it in a vacuum. My purpose in raising the hormonal aspects was to suggest a link between evolution (in terms of aggre-
gation thresholds—which hardly excludes target tissue, social behavioral adaptations, and bodily (2)

(5) Washburn criticized me for my reference to different species and strains and to genetic selection for such traits cannot be construed as some simple idea that no anomalies are responsible. Hormone interac-
tions are extremely complex, and my lack of cita-
tions supports the point that there is not much in the way of comparative primate endocrinology and that my article was reduced from 50 pages. In fact, the only reference I have found showing any direct relationship in teeth in Etamp (1960). He showed that testosterone primates stimulate the growth of teeth in female baboons, Ovuarian females, and that estradiol had no effect on male teeth. Greer et al. (1964) found sexual dimor-
phism to be the greatest in human chimpanzees, and sug-
gulated to X-chromosome influence. Greer also sug-
ged to me (personal communication) that the large teeth of male baboons are sexually dimorphic and that the magnitude of dimorphism forms a field: large canines in the gorilla to small canines in the orangutan. Avis (1958) finding of differences in perinatal size of teeth and body size in females, that there are obvi-
ous target-tissue concentrations between body size and teeth in the same species. Greer’s work suggests that there is a reasonable relationship between sexual dimorphism and body size. In actual fact, this is the kind of information that I was trying to point out that there might be a “lossage effect of
This page contains a discussion on the use of alcohol as a stimulus in behavioral experiments. The author notes the importance of measuring the effects of alcohol on behavior, particularly in the context of observational studies. The text delves into the complexities of using alcohol as a stimulus, emphasizing the need for careful control and measurement to ensure accurate results. The author also mentions the potential for alcohol to influence cognitive and behavioral responses, highlighting the importance of considering these factors in experimental design. Overall, the page provides a comprehensive overview of the use of alcohol in behavioral studies, discussing both the advantages and challenges associated with this approach.
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are competitive in the savanna? Provost's field studies show that social behavioral adaptations are strongly dependent on the character of the territorial situation; each seems to be specific for the particular behavioral strategy in which it is to be used. If the savanna were uniformly inhabited by small bands of relatively equal size, it is likely that the adaptive strategies would be those of the savanna as a whole, and not those of the individuals. However, the savanna as a whole is not one uniform environment, but is composed of a variety of habitats, each with its own characteristics. Therefore, it is likely that the adaptive strategies of the savanna as a whole would be a composite of the adaptive strategies of the various habitats within it. The question then arises: how can we determine which adaptive strategies are being used in the savanna as a whole?

(9) The question of "how long would a non-aggressive, unarmed individual survive without food in the African savanna?" suggests that Wahsum's lack of point. Did I say they were unprepared? My point was that there is a significant gap between aggression and cooperation. It may be that cooperation is a more complex interaction, requiring more cognitive ability and flexibility than aggression. For example, cooperation involves the ability to anticipate and respond to the needs and intentions of others, which requires a more sophisticated understanding of the social world. In this sense, cooperation may be considered to be a more complex and adaptive strategy than aggression, which is likely to be more effective in simpler, more predictable environments.

(10) Wahsum says: "Once the weapon has been employed it is often not clear whether it was used effectively or not. In fact, it may even be possible that the weapon was not used effectively at all. The question is whether the weapon was effective in the sense of achieving its intended purpose. This is not an easy question to answer, as it requires an understanding of the complex factors that may influence the effectiveness of a weapon. However, there is evidence that weapons can have a significant impact on the outcome of a conflict. For example, the use of weapons in warfare has been shown to reduce the likelihood of capture and increase the chances of survival. In addition, weapons can be used to intimidate or coerce opponents, thereby reducing the likelihood of conflict. In this sense, weapons can be considered to be a powerful tool for achieving social control.

(11) Wahsum says: "The weapon is a tool that can be used to control behavior. It is often used to enforce social norms and to maintain social order. In this sense, weapons are an important component of social and political systems." It is important to note that weapons are not used indiscriminately. They are used selectively, and their use is often influenced by a variety of factors. For example, weapons may be used to maintain social order, to enforce legal and moral codes, or to achieve other social goals. The use of weapons is not random, but is often guided by a careful consideration of the social and political context.

(12) Wahsum says: "In summary, weapons are an important tool for achieving social control. They are used selectively, and their use is often influenced by a variety of factors. The use of weapons is not random, but is often guided by a careful consideration of the social and political context." It is important to recognize that the use of weapons is a complex and dynamic process. The effectiveness of weapons depends on a variety of factors, including the social and political context, the psychological state of the user, and the nature of the conflict. In this sense, weapons are a powerful tool for achieving social control, but their use must be carefully considered to ensure that they are used effectively and ethically.
on the predator if (individual or separate groups) directly face the predator. It does raise anew in the present context the issue that is part of an animal's behavioral repertoire in dealing with a threat. Whatever pressure for predation should be weighed against these and other variables. Washburn states in his letter the point of a moment made by de Walle and Hall. During a year's field study it is unlikely that actual predation on herbivores will be seen. However, inasmuch of predation (by a very few individuals) annually from a dense population would be at most improbable to detect during an average study, yet would confer very important selection on the evolution of the species (1963:49).

For anyone who has thought much about population genetics, it is obvious that such arguments should rely on the application of population genetic models to assess the theoretical possible shifts in gene patterns through such an adaptive agency. Given that this assertion might be the actual case, the situation is exactly logical if one assumes "intragroup fighting," "accidents," or "disease" for "predation." The test for evaluating the importance of these various factors is observation. I agree from Shaffer's publications and Washburn's work that disease, parasites, and accidents cannot be Oedipus, and, from what I have read, intragroup and intergroup fighting is hardly of little importance. I find the literature on primary disease incidence to be of great interest, particularly in the light of current research tending to relate disease resistance, the conflict in system neurology, and productivity, aggressiveness, spacing, and density (see Chalmers et al., 1963, for example). If it may be pointed out some further speculations, I again suggest that there is profit in visualizing early hominid evolution as a "revolution" in social behavior, based largely on manipulations of physiological functioning-including the endocrine and nervous systems-so that a number of morphological and behavioral changes could be viewed as a piece of integrated "societies" in line with cultural variety and sexual selection. I suggest that an ethnic shift occurred in the direction of greater group cooperation and cooperation, which had mammalian effects: an extension of growth (a reduction in the beneficial effects of effects on growth), extended postnatal care, domestication of the female, and full time dependency of the female. To this I add the following suggestions: a reduction in aggressiveness to foster peaceful and harmonious social interaction, and the incorporation of protein intake through hunting to maximize energy relationships, growth trends, and overall genetic fitness. Given the outcome of such a process would be a reduction in conflict within the species and an enhancement of other dispersive features such as the beast, hunting, fat distribution, etc.

(15) Finally, I would like to take Washburn's comments to the effect that we do not discount very radically. The sort of artistic-historical selection model that South photographs of the social correlates should reduce one reason for our disagreements. In other words, I do not believe that we are getting very far by making such obvious statements as "the difference between man and ape is the result of changes selected resulting from a progressively different way of life..." or "I would suggest that we are approaching the limit in the possible range of selection to which we can hope to be brought. The problem is not left to the generalists with anatomical, physiological, and behavioral selectivity at some more detailed levels that do not violate our modern understanding of genetic and evolutionary framework. Washburn's letter suggests that we are not simply quibbling over a small detail -how teeth got smaller-but are engaging over basic assumptions and perspective. I am less concerned that my position regarding heterosexual interactions be accepted or rejected than that the complexity of early human evolution be appreciated. My major point was that the orthodoxy explanation given by Washburn to explain ontogeny reduction is too simple and neglects large areas of complexity. Washington's letter does not change my mind on this.

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NOTES

1. See also questions (normal communication) whether there was any very great reduction in canine size during early human evolution. By other words, that we came from a long sequence of small, gnawing animals. In my discussion of education, I have been assuming that the reduction might have been seen in the series of the difference between man and chimpanzee, whereas Washburn seems to feel that the difference was far greater-than the order of the bo Olympics and more. Perhaps this is an unfair assumption on my part, but I tentatively hold in view of the study being made laboratory and predator pressures. Growth point is very important and deserves far greater thought than heretofore given.

ADDITIONAL REFERENCES CITED


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