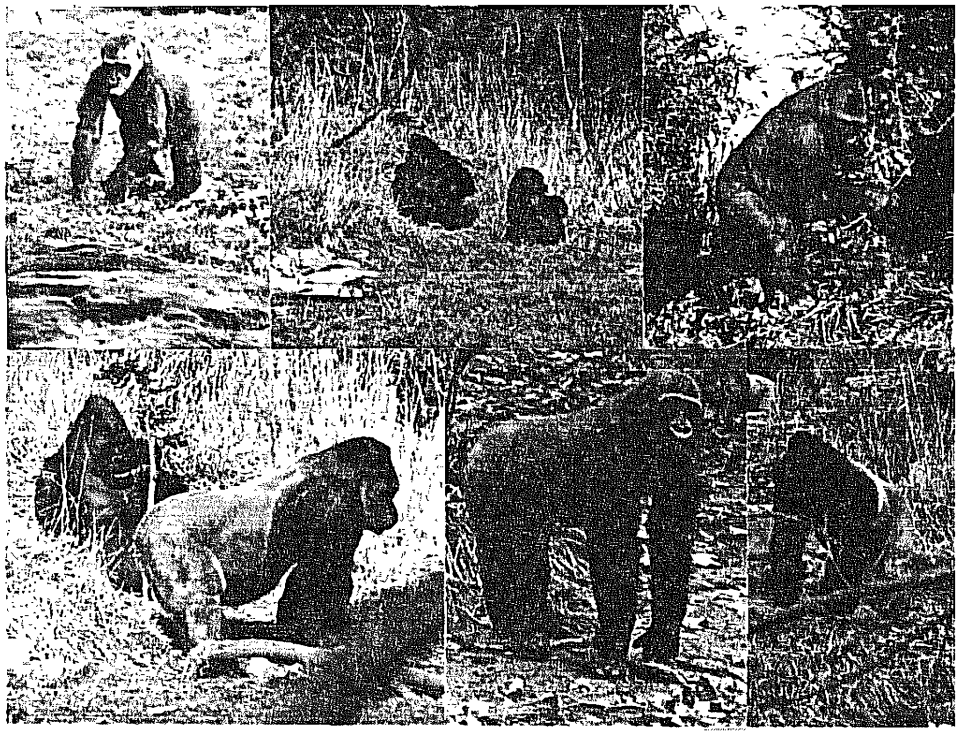
LIFE HISTORY STRATEGIES



HUMAN ETHOLOGY NEWSLETTER

JOAN S. LOCKARD, EDITOR VOLUME 4 UNIVERSITY OF WASHINGTON
MARCH 1983 ISSUE 9 SEATTLE WASHINGTON 98195

The photographs above were taken by Colleen R. Lasley, who is working with me and another student, Cynthia A. Goyal, on a long-term study of the social behaviors of captive western lowland gorillas at the Woodland Park Zoological Gardens in Seattle. We wish to thank the Zoo staff for their cooperation in this endeavor. The composite seemed a suitable flag for the upcoming workshop on Models of Life History Strategies to be held at UCLA May 21, 1983 (see page 3).

A CHANGE OF GUARDS

Elected to the Executive Board were: Nicholas Blurton-Jones, Susan Essock-Vitale, Alan Fogel and Albert Somit. They will serve two year terms, overlapping in 1983 with Michael McGuire, Esther Thelen, Ian Vine, and Ronald Weigel. Congratulations to our new Board Members and many thanks to Robert Adams, Gordon Burghardt, Wade Mackey and Gail Zivin who are stepping down.

Members assigned to committees are Nicholas Blurton-Jones, who we have asked to chair the National Meetings Committee while Ron Weigel will continue to head the International Meetings Committee. Susan Essock-Vitale has agreed to assume the Nominations from Esther Thelen who will edit the Human Ethology Abstracts. Albert Somit will take on the Membership Recruitment. Bob Adams will continue to handle the Recent Literature and Ian Vine and Bill McGrew the Book Review section. Alan Fogel will chair the Long Term Goals Committee when Michael McGuire's term expires next year.

DUES LAMENT

As of this printing, over one-hundred members have yet to pay their 1983 dues. This is positively the last issue to be mailed to you if your dues are unpaid (check your address label - a 1982 date indicates money still owing). Take the time and please send in your check along with the form at the end of the newsletter. Keep our Society strong!

DIRECTORY GRIEVANCES

In response to subdividing the 1982 Directory of Members by geographical location (i.e., "Domestic", "Canadian", and "International"), we thought it best to simplify things by following the method used by the U.S. Post Office. In other

words, the newsletters are mailed out according to geographic location and that is the same manner in which our filing system is organized. No offense was intended in choosing the word "international"; in fact, the term, "foreign" (used by the U.S. Post Office) seemed more egocentric.

A few members who had not paid their 1982-83 dues until late November or December were omitted from the Directory; please accept our apologies and be assured your name will appear in the next directory printed.

WORKSHOP LAUNCH

"The Application of Models of Life History Strategies to the Study of Human Development"

Date: Saturday, May 21, 1983

Location: University of California, Los Angeles

Organizers: Ronald M. Weigel and Nicholas G. Blurton Jones

The goal of this workshop is to discuss the application of evolutionary theories of life history strategies to the study of human development. Emphasis will be placed on developing models and methodologies for research. Issues to be addressed will include the following:

(1) Theoretical models of human life history strategies
(2) Age-related human reproductive strategies
(3) Childhood precursors of adult reproductive behavior
(4) Ecological factors affecting human reproductive strategies
(5) The interchange between human life history analysis and life-span developmental psychology.

For more detail, see Ron Weigel's mini-communication in the December 1982 HEN.

The format of the workshop will resemble the following. We expect to have 5 or 6 speakers who will present background material for research in an area of human life history analysis. This can include a presentation of previously collected data (either by the speaker or from the literature), a discussion of theoretical issues, or evaluation of available methodologies. The speaker and workshop participants will then discuss the material presented, with a goal of developing research methodologies for investigating specific issues in human life history analysis. Depending upon the number of speakers, we hope to allow up to one hour per speaker. We may also have room on the program for shorter presentations of data and/or models only.

Priority in the program will be given to discussion oriented presentations that deal with specific research questions in human life history analysis. Theoretical models and previous research on nonhuman species, that have direct implications for research on humans, will also be considered. Individuals desiring to make a presentation should submit an abstract of one page or less to the organizers (address given below) by April 29.

Due to the decrease in the effectiveness of a discussion oriented format with increased group size, it will be necessary to limit the number of workshop participants to 30 people. If you would like to attend the workshop and we are not already familiar with your research, please submit a copy of your curriculum vita along with the registration form. All registration materials should be received by April 29.

The workshop will last all day, Saturday, May 21, with morning and afternoon presentations/discussions, and a dinner/reception in the evening. There will be no registration fee for the morning and afternoon sessions. However, there will be a charge of not more than $15 for those individuals who attend the dinner/reception, to be collected at the dinner. (Speakers will be exempt from this fee.)

If you have any questions concerning the workshop, call us at (213) 825-8315, or write to:

Drs. Nicholas G. Blurton Jones & Ronald Weigel
Department of Education
University of California, Los Angeles
Los Angeles, CA 90024.

SEE WORKSHOP REGISTRATION FORM ATTACHED

NOT ALONE

Appreciation is extended to Ms. Vicky Coleman, whose editorial assistance has made my task as Editor immeasurably easier. My gratitude is also expressed to the handful of members without whose contributions, the Newsletter would not be possible. A case in point are the unfailing book reviews by Bill McGrew and Ian Vine, Recent Literature by Bob Adams, and the repeated input of colleagues such as Clara Jones and Ron Weigle. There are also many others whose encouragement and help have really made the difference. Thank you all!

FORUM DEJA VU

Rock Throwing As A Determining Event in Human Development

Virginia Bruce
Los Angeles, CA

Abstract. This is a commentary on a recent article by Calvin (1982) dealing with the development of sequencing. He suggested that feedback, lateralization, thinking, conceptualization, and the left hemisphere localization of all these are the result of right handed throwing to kill small animals at a distance. In contrast, the initiation of brain enlargement is hypothesized here as being the result of right handed-right eyed activities and the demands made upon the left hemisphere by the asymmetric stimuli that were impressed upon it. It is proposed that the initial enlargement occurred long before the subsequent enlargement due to tools and language.

The idea that early hominids threw rocks for the purpose of killing small animals is not new (Darlington, 1875, Hall, 1863, Hamilton, 1973). However, all the implications of that activity have not been investigated. In a recent article, Calvin (1982), has taken up the matter of rapid motor sequencing and made a number of interesting suggestions as to its role in the development of many human functions. However, he seems to be guilty of a common logical error of what might be called "top down reasoning!" That is, looking back down the phylogenetic tree and finding some important factor which obviously occurred somewhere in our evolution, and then proceeding to build all subsequent developments upon that. Cases in point are tool making, carrying and, more recently, the idea of extractive foraging (Parker and Gibson, 1979).

Everything subsequent to these events, inventions or behavioral innovations and based upon them, appear to make very good sense but there is always the question of their antecedents. What came before that made these inventions or behaviors possible and what circumstances would have provided the challenge that would have brought them into existence? Small genetic changes may come about due to some accident of nature but any important change in the direction of an animal's evolution may have genetic change at is root, but it is also dependent on the environment and life style to determine whether it contributes to the animal's fitness and thus to its perpetuation in subsequent generations. Thus, it is of prime importance to understand what went before some new development that would have laid the ground work for it (i.e., preadaptations). It is not enough just to say that "the making of tools is what brought about man's enlarged brain and

subsequent developments" or "the invention of trays or bags or containers to bring home collected foods was essential to man's development" or "learning to extract foods which are encased, buried or embedded, caused man's brain to enlarge, to become lateralized and set the stage for modern development." Likewise, Calvin's suggestion that it was the development of a rapid sequencer that led to modern man's brain and its functions is too simple. There had to be something before that allowed or promoted this development. We shall consider the possibilities a bit later on.

A further confusion is introduced by the current tendency to cite what has been observed in monkeys, chimpanzees, or baboons as some sort of evidence of what went on in our own past. While these observations are very interesting and sometimes suggestive of new ways of looking at various problems, to take them as evidence of our own developmental path seems unwarranted. After all, present day primates have had as many millions of years to evolve in their own directions from our supposed common ancestor as we have. It would certainly be expected that they would have "learned" considerable during that time and evolved in their own particular directions. For example, we early became bipedal and they did not. This fact alone opened some doors to evolutionary possibilities and closed others. Our hominid ancestors ventured out onto the savannahs, chimpanzees stayed in the forest, while baboons have come to inhabit both kinds of terrain and the woodland in between. So, applying directly what we see today's primates do to our own early development does not seem appropriate.

Calvin, for example, cites the fact that chimps have been seen to throw rocks at the skull of a dead monkey in order to break it open and make the brain tissue available. From that, he theorizes that some hominids, "did not wait until after the main meal to attack the cranial vault with a rock -- and hence discovered a useful way of permanently disabling their prey while standing outside the range of their wounded prey's teeth: throw first, eat later." And, "From braining a downed prey, to throwing the stone to down the prey in the first place, is a simple invention sequence exposed to natural selection." He seems to have the developmental cart before the developmental horse. Surely a previously largely vegetarian primate is not going to suddenly be attacking an animal big enough to have dangerous teeth. It seems much more probable that things went in the opposite order from what he suggests, namely that hominids learned to throw, hit, and kill small animals first; and only after they were dead, did they smash open the skull to get at the tasty brain.

Man did kill large animals as the findings of elephant and hippopotamus bones in association with hominid bones indicates. But he surely did not start out his hunting career attacking animals of this size. He must have learned and perfected his hunting skills for thousands of years by preying

on small mammals the size of, say, rabbits. But even predation of this dimension has its special initiating circumstances. It has been asserted often that hominids began their meat eating by scavenging the kills of the larger predators. If they did, there would surely be times, as there are today, when the principal prey, the wildebeasts, zebras, gazelles, antelope, etc. migrate from Kenya to Tanzania at one time of the year and reverse themselves six months later. Those species preying on these animals would certainly not stay where they were, hungrily awaiting the return of the prey species six months later. Rather, they would probably migrate right along with the prey. But hominids would be unable to travel that far and that fast because the females would have to carry the young and could not feasibly do so over long distances. So, what did they do for meat in the meanwhile?

Whether hominids started their meat eating by scavenging or whether they just worked their way up to it from eating what animal protein they could catch such as insects, grubs, slow lizards, young non-flying birds, eggs, etc., is not critical. What is important is that having learned to eat meat, enjoy it and consider it part of their diet, the migration of prey and predator alike would have left them without their usual meat supply. It might have taken generations for some individual to get the idea that if pieces of wildebeast were good to eat then perhaps rabbits or similar small animals would be too, and that they would not have to go without meat for six months awaiting the return of prey and predator. I am informed that rabbits did not enter Africa until much later, but nevertheless I will use them as a familiar animal for my example in the following. Of course, any other animal of equivalent size such as the Hyrax would have served the same purpose.

The next problem would be to catch a potential prey. Doubtless numerous attempts were made by many individuals over many years before it became common knowledge that rabbits will not wait for you to get them. So what else could you do? At that stage, there were no weapons except those that lay upon the land, namely rocks. It does not strain probability to presume that animals with grasping hands, developed by their long residence in the forest (I am assuming that, by this time, hominids were living in the woodlands and occasionally venturing out onto the savannah) would have handled and played with stream rocks and pebbles, tossing them into the water for the splash, possibly even throwing them back and forth in play. They could easily hold them and let them fly off in a toss or a throw. This pre-familiarity with stones and moving them through the air would have been a prerequisite to the first attempt to kill a rabbit (or any other animal) by throwing a stone at it. The first point of aim would be where the rabbit sat, of course, but it would not be successful because the rabbit would see it coming, run off about 20 feet and, in effect, laugh at the clumsy hominid. It again probably took many individuals over many tries before the

impossibility of hitting the rabbit where it sat "dawned" on them. But some one of them would surely eventually try throwing the rock ahead of the animal having already seen it start running or inducing it to do so by pretending to throw. For illustrations sake, let us say that he tried throwing the rock about 5 feet, ahead of where the rabbit had been sitting, in an attempt to hit it on the run. Of course, that did not work either, but it would take a lot of tries to realize it. So eventually, someone would try it at 10 feet, and, again, without success. Fifteen feet was tried and, again, failure. But, in due course, someone would throw it about 20 feet ahead of where the rabbit had been sitting and managed to hit it. Success! --- And dinner! (I have used 5, 10, 15, and 20 feet distances just as examples, of course, any other trial distances would have served as well.) But right here we have the first example of what is suggested to be the greatest single shaping event in man's development after bipedalism, which was, of course, a necessary prerequisite since otherwise the hands would not have been free for throwing.

The reason this was such a significant event is that it taught man to think. Immediately there are two things that must be dealt with after that statement. First, there is the definition of thought and, second, a comment on the mental processes of all other species at that time and since. The following is offered as a working definition of "thinking" that is not too sophisticated or philosophical but adequate for explaining its beginnings. "Thinking is the mental manipulation of a series of previously recorded experiences." That is what we do whenever we say, "I'll think about it" or "What do you think about that?" etc. We search our past experiences (previously recorded) for a situation having something in common with the current situation, we compare the two (or more) and find out what is common to the two of them (or different). From this manipulative process, we draw some conclusions about the current problem before us. It may be a rather trivial matter in which case the words "conclusion" and "decision" are appropriate. But if it is of some importance and particularly if it is new, a more appropriate idea would be that we form a "concept" -- a new synthesis based on our recent experience. That is precisely what our hypothetical hominid ancestor would have done shortly after having successfully killed the rabbit. He could "manipulate" in his mind the five facts that he had to work with, namely the throwing at 0, 5, 10, 15 and 20 feet, with an increasing probability of success the greater the distance chosen.

This mental manipulation could have led to the concept of awareness of time. As there was no language at that point, he would not have "had a word for", but an awareness of something elapsing while the rabbit was running and the rock was flying through the air he could have had. Gradually, he would learn that the trick of killing rabbits with rocks was to estimate how much of this "time stuff" would be used up by the rabbit in getting to the 20 ft. point and then adjusting the force of

his throw and its angle so that it would use up exactly the same amount. If he did so correctly, the rabbit and the rock would arrive at the same place at the same time and he would have a dead rabbit for dinner. Now, such an explanation seems at once both too simplistic and, as it is stated, too complex. In reality, it is neither because what the hominid was learning in this type of experience was something that every other predator had known instinctively since its earliest hunting experiences -- this goes for fish, birds, reptiles or mammals, i.e., any animal that has had to capture or kill a moving target. I refer, of course, to feedback. Up to the point of rock throwing to obtain food, our ancestors were not predators in the same sense, for example, as a leopard, an eagle or a shark. While they would have caught and eaten various slow moving creatures, their main diet was stationary, meaning vegetable, so their predecessors had no occasion to learn much about feedback. But in the situation just described, they did and since other animals understand and employ the principle, it is not asking too much of our hominid ancestors to have learned it too.

The second thing to address is the matter of "thinking" as it applies to humans and to other species. Since human mental processes are both qualitatively and quantitatively different from other animals even though they both function by way of neurons, synapses, chemical transmitters, etc., it seems desirable to have some word that applies to what goes on in human brains as distinct from other species. "Thinking" seems to fill the bill. That is not to either deny or denigrate the c mental processes of other species some of which are quite suprising. But "thinking" is a word that should be reserved for human cognitive processes and, as a matter of fact, to those of the left hemisphere specifically.

If the above scenario is correct, it will be clear that hunting of small animals preceeded hunting of large ones and that it was not, as Calvin has suggested, a progression of skull bashing to killing at a distance but the other way around. Very probably, the animals first killed by thrown rocks had such small heads that it would either have been no great problem to break them open or the amount of brain tissue to be had was not worth the trouble.

The next interesting question about rock throwing would be "with which hand did the first successful hunter throw?" The fact that 90% of the people in all societies that we know anything about are right handed -- that boring tools from Clacton, England were scratched clockwise, meaning that they were used right handedly, 200,000 years ago (Keeley, 1977) that the baboon skulls found by Dart (1949) and dating from Australopithicine times most commonly showed depressed fractures of the left temple region indicating that they had been killed by a right handed blow from the front -- all point to our species having been right handed from "way back when."

All this provides a great temptation to suggest that the first successful hunter threw the rock with his right hand. But many would object, saying that, with two hands, he had a 50-50 chance of using either hand which of course is true. However, animals are very prone to imitate, particularly primates. The example of the Japanese monkeys that live on an island and have learned to wash yams before eating and to separate wheat of rice grains from sand by flotation in the water, are cases in point. One particular individual began both of those procedures and within a very short time, all other monkeys on the island were doing it. So, the suggestion must be made that if indeed there was an equal chance of using either hand, that the first one to do it successfully did use the right hand and all the rest copied him. After all, the other males in the group were just as desirous of fresh meat and all had doubtless tried rock throwing without success many times, so when one among them was successful they would observe him closely to see how he did it and then do exactly the same thing in the same way.

But the other possibility seems more probable and that is that he intentionally used the right hand because the left hemisphere controls its actions. This will be objected to by some because they will say that right handed lateralization was not that far along at that point and that they would be correct, because it seems highly probable that lateralization, as we understand and see it now was the outgrowth of millenia of right handed activities which put strain on the left hemisphere to develop the mental skills to perform those activities efficiently. However, there is some reason to believe that even at that stage, there were some slight differences between the two hemispheres that would have given the left hemisphere the edge. As it were, Glick and Ross (1981) have shown that there are a number of assymetries in the motor performances even of rat brains. Morgan and Corballis (1978a,b) have proposed a developmental bias in favor of the left side running all up the phylogenetic tree. So there is some reason for expecting that the first important activities that had to be done one handedly would have been done preferentially by the right hand in all but a few cases. Very likely this assymmetry had begun to manifest itself long before throwing occurred. Thus sticks must have been used to knock down high fruit, to dig tubers out of the ground, to prod ground living animals out of their borrows, to threaten with or to beat small animals to death. If so, the right hand may well have had a running start in the competition for handedness and have been the preferred hand already.

Now clearly, the perfection of the behavior of the sighting, aiming and throwing involves both eye and arm and also involves, as Calvin notes, the exact coordination of a number of different muscles at just the right time to render the throw accurate and successful. Equally clearly, to accomplish this, an effective sequencer mechanism would have to develop and if the speculations about the handedness are

true, that sequencer would be expected to develop in the left hemisphere in close proximity to the centers controlling the activity, which is exactly where it is. Calvin says there would be "a tendency to use the hand and arm opposite to the better sequencer." Again that seems in the wrong order. Why would this "better sequencer" (presumably on the left side) have developed? What circumstances would have called it into being? And if it existed before handedness developed, why should it be on the left side? It is more reasonable and more parsimonious to see the sequencer developing as a result of the need for accurate throwing and coordination and that it developed on the left side because the right arm was the one used for that activity as dictated by a slightly biased left hemisphere.

Calvin goes on to say, "the easiest way to get more sequencer machinery in a concentrated center would be to simply enlarge the next-generation brain so that there was some extra uncommitted cortex surrounding the center which the sequencer could appropriate." He dwells at some length on the physics of throwing and aiming and on the principal of redundancy about which he is doubtless an expert, but to propose that brain tissue just suddenly grows bigger in one generation hardly seems likely. Moreover, the development of skill in aiming and throwing probably was spread over thousands of years not just a few generations. Likewise with the development of the sequencer.

Further on, he says, "the postulated genetic tendencies, bigger brain and lateralized sequencer could thus interact strongly with the cultural invention of throwing stones at prey." Perhaps Calvin's own sequencer was "out of sync" as the saying goes, because this statement strongly implies that the bigger and the lateral sequencer were already in place and working when the "cultural inventions" came along. This assumption is open to great question. There was no evidence offered other than a vague reference to "a novel combination of genes which led to relatively larger brains." The trouble with this is that no time period is mentioned so that (when taken) over the whole of man's evolution, the statement would be true but, in the time slot we are dealing with, there is no apparent stimuli leading to either a larger brain or lateralization of any great degree. Johanson's "Lucy" (1981), dated at 3.8 million years B.P., and the recently reported find by Clark and White (1982), of hominid bones dated to about 4 million years B.P., were both bipedal creatures but with small brains. An animal achieving bipedality thus freeing the hands for other creatures to follow would presumably have had ample time to do things that would have required increased brain tissue. The fact that both these creatures still had small brains would seem to indicate that although they were bipedal, they were still living in such a way that no stimuli for the brain's enlargement had yet taken place. Evolution at that stage was indeed very slow and it is suggested that the cause of the increase in brain

size and lateralization which did take place in due course was not due to some fortuitous genetic mutation, but precisely from the "cultural invention" of rock throwing. If this was done regularly over many, many generations and with the right hand, as had been suggested above, those who could do it best had a better supply of animal protein, which in itself favors better brain function, and healthier offspring and thus it would have been a talent for which there would have been selection. So the events considered by Calvin seem more reasonably to have occurred in the opposite order.

Calvin indicates that an increase in left hemisphere size would have caused the right to enlarge equally on the basis of symmetry of development and this is reasonable. But the new tissue on the left side was there to enable it to handle the increased load coming from continuous and various right handed activities and those that developed from it such as thinking and conceptualization. The incidental increase in the right hemisphere was not in response to any need or environmental pressure. But at the same time, the tissue did not just develop and then lay around generation after generation waiting for some use to be developed for it. Whatever it was that was being handled in that area before the increase took effect would still go on and would involve the extra tissue, too. Presumably, it could do its job whether it was more efficient than before.

But Calvin says, "... other functions such as visual-spacial functions ... seem to have SETTLED IN opposite language." This appears to be another example of a misconception that turns up now and again in the literature of lateralization. Other writers have referred to functions being displaced, or moved or pushed out due to competition with other developing functions. But neither visual-spacial functions nor any other "settle in" anywhere. A function was there to begin with and while it may be enlarged, rendered more complex or more efficient, it does not, so to speak, "move in." A further objection lies in the implication that language was there first and that visuo-spacial functions "settled in" later. This cannot be true because all animals with two eyes, whether binocular vision had developed or not, have the problem of keeping tract of what goes on in both quadrants of the space in front of them. Because of decussation, the left eye or the left visual fields and therefore the right hemisphere must deal with matters both harmful and beneficial occurring to the left of the center line and vice versa for the left hemisphere. Thus, visual and spacial input from the left side was processed in the right hemisphere since long before the advent of the primates. So such functions were obviously in place long before language made its appearance. It may have been, however, that the right sided symmetrical development that followed upon the enlargement of the left to serve its needs may have provided enough extra tissue to enable the visual and spacial functions already there to develop more efficiently than those on the

left where there were numerous demands on the available neural tissue. This might account for why in modern man the right hemisphere is more efficient in these tasks. But it should be thought of as development more by way of a windfall of tissue than of specialization demanding more circuits.

"...tool sharpening seems a natural sequel to throwing..." Taken over a long enough time, the former surely came after the latter but this would have to be the longest "sequel" in history. Surely once hominids had masterd the matter of killing small animals at a distance, it would have served their needs and taxed their abilities for an awfully long time. Tools are found back about 2&1/2 million years ago, but that leaves about 1&1/2 million years between the time of "Lucy", a bipedal creature, and the earliest tools found. It is surely reasonable to suppose that a lot of years were used up in learning how to throw and be successful at it and having done so there would be no real need for tools other than those nature leaves strewn around the landscape---bits of broken rocks. Animals have long teeth and often claws with which to tear skin away from a carcass. But if hominids began their meat eating careers by scavenging, they had no need of tools because they would just go in and tear off hunks of the remaining meat. After learning to kill small animals themselves, a small piece of rather sharply pointed rock would serve to puncture the skin of the animal. The hominid could then put his fingers into the hole and tear the skin off. This would seem to have been an adequate solution to the problem of small butchery for many thousands of years.

It would take a very long time for hominids to perfect their ability to think and to conceptualize before they would be able to imagine a tool and a purpose for it and probably still longer to figure out how to arrive at a practical solution to the problem and to learn to manipulate rock against rock to bring such an imaginary item into existence. Even the stimulus for such a thought would not have occurred until a use for such a tool had become evident and this would not have been until he had worked his hunting skills up to the point of tackling animals larger than himself. Carcasses of such larger animals would require butchering to obtain the meat and this would have provided the environmental stimulus to invent something to solve the problem. A sharp edge of broken rock would do it and only after the problem in some way became complicated enough to require special shapes, sizes or types of tools would he be pushed into the mental processes necessary to solve them. Thus, sharpening followed throwing all right but probably by about a million and a half years.

Possibly the most surprising assertion made in Calvin's paper is that dealing with female throwing. He says, "...the first star pitcher may have been a mother." Nothing of importance is cited as a reason for assuming that females would have been concerned with throwing. He discusses the problems that would face a female hunter. "Predator-prey

chases present the unaided mother with the difficult choice of carrying the infant along for the ride (which is tiresome) or of leaving the infant temporarily unattended (which increases infant mortality via accidents or predation). A chase might also increase spontaneous abortions." Both of these are good arguments against female hunting, strangely. Yet he seems to see female hunting as being significant. Curiously enough he cites Lovejoy (1981) with approval because of his ideas about pair bonding, food sharing, carrying and other reproductive-social patterns. Yet Calvin pratically overlooks any male contribution to the hunting process.

He goes on to consider the observation of the tendency of females to carry their infants on the left side with the left arm "...the maternal preference for left-sided infant carry is what promoted female hominid right-handed throwing." Now picture a small, hominid female clutching her infant with her left hand, a stone in her right hand, trying to keep the child quiet so that she can sneak up on a wary victim and then, at the appropriate time, letting fly a stone. The vision is really ludicrous! One cannot imagine much accuracy or success under these conditions. It is doubtful if even a big league pitcher would be able to deliver many strikes even with his well educated right arm while he was holding a baby in his left arm. The whole idea would seem to be such as to outrage the good Bishop Occam as it is about as unparsimoniuos a solution to the problem of hunting as could be dreamt up. Even if females did attempt to hunt under the above conditions, they would soon find it impractical and the males who were not burdened with child care would take over. Essentially, males have ruled the world since hominid times for one very important but not immediately obvious reason, namely, that they do not have breasts! Those that do, have the obligation to nuture the young, those that do not are able to move about freely. So naturally, the males were the ones who learned to hunt by throwing rocks which accounts for their greater degree of lateralization because of everything that developed from the hunt. And hunting in turn, which involved moving out of sight of home base, is probably the reason why males even today are better than females at visuo-spacial activities in the right hemisphere. This is not merely a human observation since even male rats can run a maze better than females and presumably for the same reason.

Finally, the following, "Certainly the tendency for both right-handedness and language to be represented in the left hemisphere is compatible with a common origin in a lateralized motor sequencer." This quotation echoes others already commented on. Right handedness and the things that developed from it were the cause of the motor sequencer not the result of it. Language, of course, is a left hemispheric specialty (in all right handers and many left handers) simply because it is an outgrowth of activities basically right handed and left hemispheric, namely hunting. Then, there is this,"...perhaps a sequencer common to language and throwing is more lateralized

in modern males." Compared to whom, modern females? Obviously, since females have not been too involed in throwing, in historical times at least! Compared to homind males? If that is what is meant why should it not be so? Disregarding entirely Calvin's ideas about female throwing, hominid males 2-3 million years ago were the hunters, probably used their right hands and induced lateralization and the development of feedback which, in turn, was responsible for learning about time which, in turn, was basic to the development of sequencing mechanisms. Males have been doing the same thing throughout human existence and naturally, developed and perfected lateralization of left hemisphere specific characteristics such as analysis, rational thought, sequencing, language, etc.

In summary, it is suggested that when bipedal males, having gotten use to meat in their diet, sought to obtain it directly rather than by scavenging, they did so by learning to kill at a distance with thrown rocks, that the ones most successful at doing this did it with their right hands and their successful method was imitated by others. Those who were most successful got the most animal protein for themselves and their families. Animal protein is conducive to better brain function and probably to brain development so that there was a vital rather than a vicious circle here. Genes involved in hunting skills would increase "fitness" and thus would be subject to selection and maintenance of those skills. Continued use of the right hand resulted in developments in the left hemisphere which were not duplicated in the right hemisphere because the left hand was not so involved. Learning to think, in the sense of manipulating experience, led to conceptualization and both resulted from the experience of killing at a distance. This naturally led to the neural mechanisms for these abilities to be closesly associated with the neural tissue controlling eye, arm and hand in the left hemisphere. Since skill in the activity of hunting involved timing and analysis, they too are attributes of the left hemisphere. Since all of this resulted from throwing, that act was indeed the most important event in Man's history after that of achieving bipedalism which was a prerequisite to it.

References

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MINI COMMUNICATIONS

Two articles were submitted for the section on Mini Communications this time; the first by Clara Jones, on the selfishness of altruism in vervet monkeys, and a second provocative topic of stupidity by Jim Welles.

Are Female Cercopithecines Altruistic To Their Daughters?

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Primatologists have only recently begun to address with any rigor questions regarding the relationship(s) between within-group (i.e., population sub-unit) genotypic and phenotypic variation, on the one hand, and such variation between "groups" and populations, on the other hand. Studying vervet monkeys (Cercopithecus aethiops) in nature, Cheney, et al. (Behav. Ecol. Sociobiol., 9, 153-161) investigated "the relation between behavior and individual reproductive success", in particular, the differential effects upon annual reproduction and other behavioral parameters occasioned by "non-random mortality." Cheney, et al. ("the authors") found no evidence of differential reproductive success, but found, nevertheless, significant differences in the activities performed by females and their offspring as a function of female rank. They concluded that patterns of rank-dependent mortality within groups (by disease for low-ranking females, by predation for high-ranking females) favored differential behavioral response to minimize the likelihood of death, but the authors did not assess alternative evolutionary hypotheses to explain their failure to find differences in female reproductive parameters as a function of rank-related behavioral variation. The authors assess their findings in terms of selfishness, rejecting "group"-level explanations (see the authors' pg. 158), apparently because they made the assumptions that individuals maximize annual reproductive success; that survivorship, rather than fecundity, is maximized in their (heterogeneous) habitat; and, that factors favoring group life are equivalent to factors favoring social life. I submit this note to suggest that the authors' data can be richly interpreted in terms of hypotheses that weigh "group", especially matrilineal (as distinct from nepotistic), interests.

Cheney, et al. tested the hypothesis that annual reproductive success is a function of differential female rank (interference competition) and differential mortality (implying that rank is a main effect), concluding that "there was no relation between dominance rank and birth rate". Since

differential reproductive success, in addition to dispersal, represent those genetically "effective" responses over which individuals may gain some neurological (physiological) control, and since "each activity performed by an individual can be thought of as incurring a certain probability of death and a certain probability of successful reproduction" (McCleery, R.G., in Krebs, J. & N. Davies, eds., Behavioural Ecology, Sunderland, MA: Sinauer, 1978, pp. 377-410), the finding that annual reproductive success for female vervets fails to reflect other phenotypic differences among them leads to two competing inferences from Darwinian theory: (1) that the "effective" components of between-individual behavioral variation exactly cancel one another out despite the particular causes of mortality and of variability in the forms of inter-female behavioral response; or, (2) that behavioral differences among females are not "effective" when analyzed on the basis of direct annual reproduction.

The authors point out that "non-random mortality" may lead to differences in annual reproduction among female vervets where predation pressures are relaxed, suggesting that direct selection is "effective" in these regimes. However, because they reject the opportunity to assess their contrasting results, under conditions of strong predation pressure, in terms of "group" factors, the authors do not evaluate alternative hypotheses that would derive from the assumption that individual vervet females maximize "inclusive fitness", the organizing principle of "sociobiology" (see Markl, H., Evolution of Social Behavior: Hypotheses and Empirical Tests. Basl: Verlag Chemie, 1980). To the observer, within-and between-individual behavioral variation may be a highly visible and subjectively compelling component of many primate societies, and the authors state that "the distribution of causes of mortality suggests that individuals living in the same social group may confront different selective pressures, and may, as a result, respond differently to similar social and environmental variables." However, students intending to identify patterns and relationships within behavioral variation that may be stable across generations, predictable by environmental (including social) regime and genetically "effective" are bound to employ alternative hypotheses from the general evolutionary law in an attempt to explain data and constrain inferences. The authors' interesting results that female vervets do not differ in other ways behaviorally may be interpreted rigorously in terms of inclusive, especially matrilineal, interests.

Cheney, et al. present numerous features of the interactive patterns of vervet monkeys, some extracted from the literature and some from their remarkable field data set. Considering all features that they discuss, matriline self-interest may explain the derivation and/or maintenance of the following patterns: female relatives, possibly sisters, occupy adjacent rank among many cercopithecines, and a female vervet usually assumes her mother's relative dominance position;

female vervets are generally recruited into their natal groups; for vervets, there may be "no consistent rank-related differences in birth rates" or "offspring survival"; brothers or male "peers" apparently associate after dispersal, and males may not disperse for long distances (see McCracken, G. & Bradbury, J., Science, 198, 303-306, 1977); high-ranked females and their offspring may expose themselves to risks from predation by giving "alarm calls" and by leading group progressions, respectively (thus, high-ranked individuals may act as "donors", low-ranked females, as "recipients" of these apparently altruistic activities); high-ranking females are more aggressive during inter-group encounters to conspecifics in other groups than are low-ranking females (again placing themselves in an apparently altruistic position with respect to low-ranking members of the same group but in a selfish position with respect to females of other groups).

Patterns that may be explained by individual and matriline self-interest are these: interference by high-ranking females may be a significant component of mortality due to illness in low-ranking females during the dry season and of differential reproduction success under certain environmental regimes; low-ranking females exhibit "friendly interactions" to non-group members during intergroup confrontations (thus, low-ranking members of one group act as "donors" to or solicitors of individuals of a second group, placing high-ranking females of the first group in a potentially altruistic position); low-ranking females initiate most "friendly" activity with high-ranking females. It is important to note that high-ranked and probably related female vervets act to maintain group cohesion, in general, while low-ranked and probably related females behave in ways that may be destabilizing or that may favor group sub-division ("fissioning"). Such sub-population structure may generate ethological isolation under appropriate conditions (see Sade in Markl, op. cit.).

While the results under consideration might be interpreted in terms of sexual selection and its classical prediction that females maximize offspring "quality" rather than "quantity", sexual selection and "inclusive-fitness maximizing" are not necessarily incompatible (see West-Eberhard, M.J., Proc. Am. Phil. Soc., 123, 222-234, 1979), since both mechanisms may minimize competition through "sympathetic" features (e.g., through patterns of female mate selectivity). This conclusion is strengthened for vervets by reports from the literature indicating that females may be dominant to males under certain conditions (Rowell, T., Anim. Beh., 19, 625-645, 1971) and that, in any event, adult female dominance relations within and between sex for this species may be highly variable and often relaxed in nature (P. Whitten, pers. comm. & unpublished data; see Jones, C.B., Human Ethology Newsletter, 3, 23-26, 1982 & 3, 18-20, 1982); that vervets occur in and show adaptations to seasonal habitats (Gartlan, J. & Brain, C. in Jay, P., Primates: Studies in Adaptation and Variability. New York: Holt, Rhinehart & Winston, 1968, pp. 253-292; Rowell,

T.E. & Richards, S.M., J. Mammal., 06, 58-69, 1979), including certain heterogeneous conditions that appear to favor "female emancipation" among the birds (e.g., Emlen, S.T. & Oring, L.W., Science, 197, 215-223; Gowaty, P.A., Am. Nat., 118, 851-859; Jones, C.B., op. cit.); and, that male vervets "mimic" female sexual signals (Wickler, W., in D. Morris, ed., Primate Ethology. New York: Anchor Press, 1967, pp. 89-189; Crook, J.H., in Campbell, B.G., ed., Sexual Selection and the Descent of Man. Chicago: Aldine Press, 1972, pp. 231-281), apparently to facilitate social life (Moynihan, M., Evolution, 22, 315-331).

The authors strongly suggest (p. 160) that social competition within groups may structure vervet populations. Interference competition may lead to ethological isolation within and between vervet groups through differential patterns of association among group members. In this regard, the causes of mortality as a function of rank which the authors report will, alone, predict uncomplementary reproductive tactics among females since disease, a seasonally predictable factor, represents a cost to group membership (see Freeland, W., Science, 197, 215-223, 1981) for low-ranking females who may benefit from patterns of group destabilization or sub-division (e.g., by attempts to ascend the dominance hierarchy or to emigrate). Mortality upon high-ranking individuals, on the other hand, was primarily a function of non-seasonal and unpredictable predation, conditions favoring group cohesion (Alexander, R.D., Ann. Rev. Ecol. Syst., 5, 325-383; also see Freeland, op. cit., Chapman, R.C., Science, 201, 365-367, 1978).

In the present study, the causes of mortality and the behavioral variability that may have depended upon those causes were apparently not converted by females into effectively different annual yields (offspring). However, as McCracken & Bradbury (op. cit.) make clear, it is important to understand those regimes in which population structure at phenotypic and genotypic levels may be evolutionarily meaningful, particularly with respect to higher-order processes. Such enlightenment may be facilitated by the primatologist's appreciation that social subordination itself may be a competitive reproductive "strategy" (West, M.J., Science, 157, 1584-1585, 1967) and may only reflect "despotism" where individuals have no other direct or indirect reproductive alternatives. The authors' results can be assessed in terms of hypothetical and estimated (see their p. 154) matrilineal effects, including the selective display of individual response, even if the null hypothesis that survival and fecundity is random with respect to matriline cannot be tested directly with their observations (see Feldman in Markl, op. cit.). To the extent that matriline is a significant effect for vervets, individuals expected to display relatively high degrees of within-group relatedness may maximize matriline life and/or total numbers of direct descendants rather than reproductive rate on an annual basis, at least

where predation pressures are intense. This report by Cheney, et al. suggests that female vervets in conditions of relatively "relaxed" predation pressure may maximize both annual reproductive rate and matriline life and/or total numbers, and provides some understanding of the relationships between interference competition, "inclusive-fitness maximizing" and the potential for ethological isolation in primate groups. Any discussion proposes that it is not necessary to invoke nepotism or kin selection in order to explain the author's results and predicts that sisters are not generally altruistic to one another, all other things being equal, implying that selection acts directly on mothers, apparently favoring those who aid daughters.

I thank R.C. Lewontin and H. Markl for comments on an earlier draft.

Toward a Psychology of Stupidity

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Abstract. Stupidity is usually considered to be a "lack of intelligence" and is commonly defined as such. However, the proposition offered here is that, rather than being an intellectual failing, stupidity is a mild personality disorder -- a slightly maladaptive characteristic of essentially all normal humans.

The term "mentally retarded" is generally used when referring to those at the lower end of the IQ curve -- under 70. Rather than being used as a synonym for this condition, "stupidity" might more aptly be employed for a common form of maladaptive behaviors which are so typical of all people at all intellectual levels. This suggestion is grounded in the simple observation that many very intelligent people have done some incredibly stupid things.

Of course, some stupid acts may be performed outside one's field of expertise, but that does not explain Watergate or the Edsel. Such blunders are more comprehensible in terms of a dysfunctional mental attitude that obscures reality by commitments to nonadaptive beliefs.

Such behavior is very common in people, even if it is irrational and mildly neurotic. If normal, common characteristics are adaptive, stupidity is adaptive only in the sense that it permits people to adjust to rather than cope with the inconsistencies of life. In this process of adjustment, stupidity can manifest itself in either of two ways: by conformity to an ill-adaptive status quo, or by the initiating of nonadaptive innovations.

To the extent that stupidity is negatively correlated with intelligence, it might be said to have been dealt with in the psychological literature on intelligence. However, if it is indeed a common personality malfunction, it has not really been considered by behavioral scientists at all. It is most peculiar that those who have spent decades watching cockroaches, running rats through mazes, shocking monkeys and computing questionnaires have failed to comment upon one of the most crucial phenomena of the human mind.

The antecedents of human stupidity are, of course, discernable in the animal world. Nesting wasps (Ammophelia) show no adjustment of their behavioral routine following experimental removal of food placed in the nest (Evans & Eberhard, 1970). And the Garcia effect in vertebrates has shown the limitations of laboratory learning in a number of species (Smith et. al., 1982). Similar difficulty in human learning can be attributed to beliefs established through the experience of social conditioning.

It would be well to make clear, at this point, that a basic conclusion of this article is that stupidity is a very subjective phenomenon. There will be few smug or glib generalizations as to exactly what it is or is not in any specific situation. In fact, it is by its very nature circumstantial and impossible to define operationally. (On this latter point, it is similar to intelligence -- a phenonmenon which has hardly suffered from research neglect, such theoretical handicaps notwithstanding).

Almost all efforts to pin stupidity down disappear into a fog of subjectivity. One person's stupidity is another's superstition is another's religion. Deeds called stupid turn out to be brilliant. Others extolled as brilliant come to be considered moronic (Pitkin, 1982). In fact, one of the surest ways to evaluate someone's "SQ" is to wait. Time is the great revealer. Who can now say, with assurance, that it is or is not stupid for civilized countries to spend $1 million/minute on armaments? Even historians may not eventually agree, but they certainly will have a clearer basis for disagreement than do contemporary analysts.

"Inclusive fitness" may be a convenient answer for another subjective question -- Stupid for whom? Certainly the hero who knowingly sacrifices himself for his group isn't stupid as such. But this assumes that he accomplished only what he foresaw and intended -- and nothing else. What we foresee is preconditioned, and "intention" is often difficult to determine beforehand. Judging all consequences of an act may make conclusions murkier still. And who does the judging and by what standard? Only in rare cases can human behavior be considered as objectively stupid.

One such classic case was described by Walter Pitkin in

his remarkable book, A Short Introduction to the History of Human Stupidity, upon which much of this article is based. He relates that there was a group of Eskimos living on a bay. They expected to be attacked by an invading enemy, and so, to protect their town treasures -- a bell -- they decided to drop it in the bay. Naturally, they wanted to be able to retrieve it themselves when the danger was past, so when they threw it overboard, they marked the side of the boat! Now that is stupid.

Although most examples are not this clear-cut, there can be no doubt that stupidity plays a major role in human affairs. This examination of that role is not, of course, intended to provide a balanced perspective on all human behavior but rather offers a balance to those prevailing. While it has proved useful to analyze human behavior by considering people as problem solvers, perhaps it would be equally profitable to consider them as problem creators. Really, most of our social and political problems are human creations, not acts of nature (volcanic eruptions, bad weather, etc.).

A thorough inquiry into the nature of stupidity would perforce entail investigations into the relative effects of climate, diet, disease, drugs and age. However, our concern here will be limited to what might be referred to as the normal functioning of humans under good to ideal environmental conditions.

Stupidity under any condition can be considered an imbalance of mental processes. It is an incapacity to perceive and react to a situation as a whole. There is usually a fixation upon one factor in a situation to the point of blotting out or de-emphasizing all else (Pitkin, 1932). Thus, stupidity might be defined as a maladaptive disorganization of selected information.

According to this definition, stupidity has much in common with classic defense mechanisms, in that it can deny and does distort reality. However, whereas defense mechanisms usually function to reduce anxiety, stupidity may be just as likely to increase anxiety. In toto, it functions to make the level of anxiety independent of external conditions. Stupidity can both cause and result from an inability to cope with perceived exigencies.

In fact, most of this subjective phenomenon of stupidity begins with mis-perceived conditions. Such errors may result from misdirected attention and perceptual defense (Erdelyi, 1974). That is, attention may be directed to irrelevant stimuli, and information gathered may then be filtered by a schema.

The "schema" is Piaget's term for the strategy that a person uses in dealing with the world. It is essentially

synonymous with Adler's "world picture" and Bolles' (1979) "focus of control." It is the set of basic beliefs a person has about how the world works and how objects relate to each other. It is built up by assimilating new information and then accommodating to that information.

However, as the schema develops, it may tend to become either conservative (in that it may protect its integrity by rejecting or distorting data) or independent (in that it may assert itself by inventing data). Thus, the basic types of stupidity are:

1) Schematic dominance -- sticking to a schema when its alteration would promote adaptation,
2) Schematic degeneration -- changing a schema when the original was more appropriate than that reconstructed, and
3) Fantasy -- creating a schema for a world which does not (yet) exist. (Of course, to this last form of madness we owe such cultural mutations as Emancipation and the airplane).

Stupidity is thus a matter of throwing out a good schema or adhering to a bad one, with the determination of good and bad being the subjective aspects of the process. Most people are too emotionally involved with themselves at times of decision and action to be objective judges as to what is or is not in their best interests.

Unfortunately, intellectual learning does not appear to be much of a safeguard against stupidity. Humans have a tremendous ability to profit from experience, but we also have a tremendous inability to profit from experience. A highly complex set of learned attitudes and values (the schema) is fashioned as we interact with our environment, but we learn to think, feel and act in ways that both help and hinder us.

As we learn, the same strategies that help us cope also restrict our abilities to respond and learn. This is an inherent liability of the mnemonic techniques that give learned material some structure and provide cues for its recall. These devices both aid and interfere with behavior, for they limit the context in which information may be used (Smith et. al., 1982).

Schemas can make life much easier because we don't have to stop to figure out the solution to a given problem from the beginning each time. We simply pull out the "Appropriate" schema and use it. However, this method of response can lead to "functional fixedness," if customary use of data interferes with their use in novel situations (Smith et. al., 1982). A schema may keep us from taking a fresh look at problems and thus limit our abilities to find solutions.

Usually, most people do not try to test their understanding of the world (their schemas) to discover if they are right or not. They try to impose their interpretations on

reality and each other. As it happens, the human mind is such a subjective instrument, that innumerable views and reconstructions of the physical and social worlds can be and have been proffered throughout the ages. The world might be a happier and quieter place if most people were indeed objectively testing their schemas against nature or nurture, but we are usually committed to our schemas in self-confirming interactions with our environments. We do not test reality except to find it wanting; we try to confirm our beliefs.

Stupidity is really rooted in the human capacity to believe. This can be overwhelming. Schemas can turn perceptions into almost anything and provide subjectivity a vague and self-sustaining base for interpretation. This point was rather dramatically demonstrated by a study on ipecac -- conventionally considered a vomit inducer. When it was administered as a new drug to relieve nausea to women suffering from morning sickness, some reported immediate relief and remission of symptoms (Hass, et. al., 1959). So what is ipacec? It may be a vomit inducer to someone who is objective -- that is ignorant, but it can be a vomit suppressor to some who believe it to be one.

The important point is that the schema is a self-determining phenomenon, defining to a large degree what information will influence its further development. The human experience is essentially a subjective one, with schemas formed by compounding personal experiences with the physical and social worlds. For most people, the decisive source of a given belief system is their language group. Whether or not a schema corresponds to or correlates with personal experience is usually predetermined by the fact that personal experience is shaped by the schema. The remarkable thing about humanity is not its long tradition of stupidity but that we have, indeed, survived as long as we have.

This suggests that there must be some survival advantage to stupidity. If there is, it could be that stupidity keeps people unaware of impinging circumstances with which they could not cope anyway. There can be little advantage in being overly concerned about problems beyond one's range of response. Thus, stupidity helps by making people insensitive to chaotic and conflicting stimuli which offer only the prospect of unresolvable conflicts. The hypersenstive may worry and fret beyond their means; the oblivious are more at ease with both themselves and their world. Of course, the latent stupidity in both cases may not be obvious, but the potential becomes clear when the hypersensitive react unnecessarily to their disadvantage, or the hidebound are eventually forced to respond in situations where their limited reactions are pointedly maladaptive.

An alternative, complementary explanation to the idea of survival advantage could be that stupidity weeds itself out through cultural selection while being replaced by a de novo

process of synthesis inherent in the subjective nature of the human condition. In this latter respect, stupidity may be the cost of having a wellspring of new ideas as a source of cultural innovation. Further, there is also the possibility of transmission of stupidity across generations, since not only truths are passed down from ancestors, but superstitions, errors, and outmoded ideas as well.

The role of stupidity in our future evolution may be enhanced by the fact that the technological conditions framing our experiences are changing faster than we can adapt. The subjective aspect of adjusting to technological progress is primarily a matter of determining "basics." What is an adaptable means to an eternal end? What is to be conserved? What is expendable? Life itself is no longer considered sacred as such; hospitals can but may not prolong life in cases considered beyond hope of recovery. Thus is an old ethic challenged by the engineer and scientist.

In the contemporary world, as always, mythology begets pathology. Emotional amalgams of hopeful philosophies based on dubious information becloud adjustment. A seemingly obvious example is found in those who retain a commitment to the schema of expansion, growth, and development in a world of limited resources. An alternative schema emphasizing quality rather than quantity of life is making some inroads, but western civilization still seems commited to finding out how much misery can be created and tolerated in the name of progress.

It is sad enough when stupidity occurs naturally or spontaneously, but it has long since been institutionalized. The individualistic stupidity of the 19th century has become incorporated and is now organized, computerized and centralized by the Federal Government -- the folks who brought us the Susan B. Anthony dollar and Viet Nam. The bureaucrats have now adopted programmed ignorance as their basic public relations "stragedy" for the American nuclear arms program. The pitfalls of this opportunistic induction of stupidity were recently and clearly spelled out by Edward Teller, uncle of the H-bomb.

> "Educating people about the nature and actual perils of nuclear weapons is almost impossible when elementary facts are guarded by strict regulations of secrecy. Given such conditions, dangerous myths develop and proliferate. Some of them have grown from misinterpreted scientific studies; others seem to be based on simple wishful thinking. They all have one common characteristic; so long as they are believed, they obstruct an accurate assessment of our problems and will prevent the development of workable plans to preserve peace." (Reader's Digest, 11/82)

So stupidity abounds eternal. It was, is and probably always will be an overwhelming factor of human life. Although there is unquestionably an intellectual aspect to it, stupidity is primarily a function of our subjective schemas. These determine perceptions and contribute to the slightly disordered condition of the normal mind. This prevents us from coping with many major problems successfully. Worst of all, our failure to recognize, analyze and appreciate the neurotic nature of stupidity prevents us from coping with our inability to cope. And that is the ultimate stupidity.

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BOOK REVIEWS

DARWIN TO DNA, MOLECULES TO HUMANITY by G.L. Stebbins
W.H. Freeman, San Francisco, 1982 and
GENERAL THEORY OF EVOLUTION by V. Csanyi
Akademiai Kiado, Budapest,
Studia Biologica Hungarica No. 18, 1979
(translated by Anna Balazs, 1982)

Reviewed by Ian Vine
University of Bradford, England

These two books are similarly ambitious in attempting to deal with a rather comprehensive range of evolutionary phenomena, from the molecular level upwards, and in seeking unified approaches which do justice to the remarkable potency of Darwin's theortetical insights into the processes of evolution. But in other respects, they are very different. Stebbins sets out to provide a textbook for the non-specialist which takes account of recent advances and new emphases coming mainly from molecular biology, ecology, ethology and sociobiology. His impressive expertise is apparent, not only in a generally admirable coverage of theoretical issues, but in a broad and erudite survey of the empirical course of evolutionary developments from single-celled organisms through to modern humans.

In contrast, Csanyi's monograph covers analogous ground at a predominantly abstract level, presumes considerable acquaintance with several specialist literatures, and gives a condensed presentation of a particular and provocative general theoretical integration at variance with Stebbins' cautious and catholic approach. These differences are perhaps best summed up in how they define evolution itself. For Stebbins, it is strictly just "irreversible transformations of the genetic composition of populations, based principally on altered interaction with their environment and guided by natural selection" (page 9). For Csanyi, it is a general process in which "an open system receiving an energy flow shifts from the state of equilibrium, [and] in its structures replicative information arises that continually increases, converging toward a maximal value until the whole system becomes one coordinated replicative unit" (page 93).

Where Csanyi appears to see evolution as uni-directional and progressive, Stebbins sees it as predominantly conservative, inclining towards the currently fashionable 'punctuated equilibrium' view. Although rejecting any clear cut divide between micro and macro processes, he sees the emergence of new species as predominately due to environmental changes that create a new challenge or a new niche. This

fits in with his ecological stress on population-environment interactions, on the coevolution of interdependent species, and autocatalysis, and with an emphasis on normalizing selection and epigenetic constraints on change. Yet Stebbins remains willing to follow Dobzhansky in acknowleding evolutionary 'transcendence' - occasional profoundly novel and significant transformations such as the origin of life or the emergence of the human mind. He would deny, however, that such events are predictable from any general laws of evolutionary progress.

Stebbins' treatment of both the theory and overall course of evolution is stimulating and informative, although occasionally rather too condensed for a non-biologist to follow every detail (e.g. in his treatment of the genetic code). But the attraction of this text for human ethologists is that he devotes a third of it to hominid evolution and to human behavior and its determinants. His account of our evolutionary emergence is basically standard and cautious, stressing that the diversity of primate adaptations permits rather few direct inferences about our genetic heritage. He tentatively traces back the rapid evolution of intelligence in hominids to selection factors first associated with exploiting the increasing availability of hard foods such as nuts (a variant of Jolly's theory), and the related adaptiveness of tool use. (It must be noted, though, that his dating of various hominid stages is erratic - compare Fig. 11-1, Table 11-1, and associated text.)

He attributes our evolutionary transcendence to the three main traits of 'artisanship', 'conscious time-binding,' and 'imaginal thinking', suggesting that although essentially a product of greater neocortical complexity they together permitted the shift to a new, primarily cultural, mode of adaptation. The novel phenomenon of a conscious mind, largely moulded by social learning through symbolic language, is held to create an evolutionary discontinuity partly because the products of mind become a major feature of the environment responsible for selecting human traits. This is one reason for Stebbins' skepticism about the more naive forms of human sociobiology. Nevertheless, his treatment of this topic is generally fair, and, as elsewhere, his watchword is caution against oversimplification and premature judgement. An important omission is any discussion of selection for reciprocal altruism; and he fails to make clear the important distinction between fitness-increasing effects of behaviors and their proximal motivating mechanisms. But his account of the complexities of genetic influences on behavior is valuable, as is his analysis of cultural transmission and selection of adaptive traits and of disanalogies between biological and cultural evolution. He suggests, plausibly, that genetically facilitated desires for approval and power are likely to be stronger in humans than more direct motives to maximize inclusive fitness. Regrettably he cites no relevant human ethological research.

In a well-informed discussion of cultural evolution, Stebbins offers useful criticism of attempts to interpret human history in over-biologized terms and to discover all-inclusive general laws. Instead, he again emphasizes the importance of specific population-environment interactions and other broad principles which, at times, yield unpredictable quantum jumps when cultures face new environmental challenges. Although the future of humanity is uncertain, he concludes on a somewhat naive note of optimism regarding our capacity to cope with known threats to survival. Overall, despite some weaknesses which make it imperative that the book is read critically, this is a valuable teaching text on evolution from which not only the beginning student can profit.

Part of the interest of Csanyi's work is that he approaches evolution sharing several of Stebbins' emphases, but is led to offer a more deterministic account of how transcendent shifts of level can occur. Although there are no obvious signs of defective translation, I remain uncertain whether my unease over several of his central concepts is just due to unfamiliarity with some of the specialist areas he draws upon. His approach appears to have both the strengths and weaknesses of systems theoretic analyses; and my doubts about his attempt to formulate quite general laws of evolutionary process and direction stem largely from the elusiveness of the presumed mechanisms, as he moves the molecular chemical level up to levels describing human actions and their products. His central claim seems to be that in open systems, selection favors structures which can store energy for longer times, notably self-reproducing structures. Over time, these tend to evolve towards a state of perfect replicative accuracy; but also functionally distinct structures tend to become coordinated (as 'hypercycles') and evolve autocatalytically so that they converge into new, integrated, replicating units with maximal internal stability. Evolution can now take off again at a new level, proceeding through analogous phases. In principle, we end up with the whole system replicating perfectly in coordinated fashion.

The principal levels that Csanyi identifies and describes with his theory are molecular, cellular, organismic, and ecological; but much of the interest of his analysis arises from his attempt to extend it to other systems linked to the last three of these, identified respectively as 'neural' (for 'concepts' or memory traces within a brain), 'cultural' (for 'ideas' or supraindividual, collective, concept superstructures), 'technical' (for object-making and replicating processes). It should be immediately apparent just how ambitious is Csanyi's project, and how admittedly speculative are parts of the anlaysis - although others are at least supported by an impressive array of references. I must confess my own skepticism regarding such an all-embracing enterprise, on both empirical and philosophical fronts, while acknowledging the attractions of grand integrative theories

which may simplify the unmanageable complexities of the real world. In a way, Stebbins' and Csanyi's viewpoints neatly counterbalance each other. And both agree that ultimately one cannot predict the detailed form of useful innovations. In its own terms, Csanyi's monograph also offers many stimulating ideas, and may prove to be of heuristic value to students of evolutionary processes.

MARGARET MEAD AND SAMOA:
THE MAKING AND UNMAKING OF AN ANTHROPOLOGICAL MYTH
by Derek Freeman
Harvard University Press, Cambridge, 1983

As we have been unable to obtain a complimentary copy of this book in order to have it reviewed by one of our members, below is a brief description that was printed in a flyer from Harvard University Press.

"In 1928 Margaret Mead announced her stunning discovery of a culture in which the storm and stress of adolescence do not exist. Coming of Age in Samoa has since become a classic - and the best-selling anthropology book of all time. Within the nature-nurture controversy that still divides scientists, Mead's evidence has long been a crucial "negative instance," an apparent proof of the sovereignty of culture over biology.

In Margaret Mead and Samoa, Professor Freeman presents startling but wholly convincing evidence that Mead's proof is false. On the basis of years of patient fieldwork and historical research, Freeman refutes Mead's characterization of Samoan society and adolescence point for point. Far from the relaxed transition to adulthood that Mead ascribed to permissive childrearing and tolerant sexual attitudes, Samoan adolescence, Freeman demonstrates, is a time of frequent stress in an authoritarian society with punitive methods of childrearing and restrictive regulations against premarital sex.

Freeman's book thus corrects a towering scientific error. But his aim is not to blame Margaret Mead so much as to understand how her error could have occurred and become basic to the doctrine of cultural determinism. The result is a detective story in the history of science, one filled with engrossing details about cultural anthropology's battle with the eugenics movement, about Mead's relationships with her most important colleagues, Ruth Benedict and Franz Boas, and finally about her poor preparation for the field and the likelihood that she was duped by her adolescent informants. Beyond these particulars lie painful but important generalizations about how the truth in science can sometimes be obscured by theory and how theory can sometimes be twisted by ideology."

RECENT LITERATURE

Readers are invited to send literature that they would like included in RECENT LITERATURE to: Robert M. Adams, Dept. of Psychology, 145 Cammack Bldg., Eastern Kentucky University, Richmond, KY 40475.

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BULLETIN BOARD

Election results of the American Society of Zoologists are: Dr. Donald S. Farner, President-Elect 1983; Dr. James Dent, Treasurer through 1985; and Dr. Joseph L. Simon, Member-at-Large.

A postdoctoral position is available July, 1983 at the University of North Carolina at Chapel Hill in the Developmental Program in Psychology. The position involves collaborative research on vocal development in birds and human infants and would require some teaching. Interested persons (especially those with a background in developmental psychobiology, animal behavior and/or infancy) should send a letter of application, vita and 3 reference letters to: Dr. Meredith West, Dept. of Psychology, University of North Carolina, Davie Hall 013A, Chapel Hill, NC, 27514.

The September 1982 issue of Science News reports discovery, in Kenya, of an 8-million-year-old fossil by Richard Leakey and Hidema Ishida. This fossil appears to have both human and ape-like characteristics with a human-type jawbone containing 5 teeth, however, it does not resemble modern or ancient humans nor apes.

Reported in the October 1982 issue of Science News was evidence suggesting that man evolved into a tool-maker in the Middle East or Asia instead of the African savannas. Fossils of the species Homo erectus (ancestor to Homo sapiens) were discovered in Israel with speculations that the ancient humans left the Middle East to "invade" and colonize eastern Africa. Another noteworthy article in the same issue was the topic of prelinguistic communication with very young children. A study conducted at Cornell University used a sound spectroscope to analyze the intonations of mothers' speech with their babies. Distinctive tones for different kinds of sentences and usage of certain pitch patterns in certain contexts (such as a raised pitch when trying to attract attention) were indicated. The report suggests that pitch is "used to carry information about mothers' intentions and feelings" and that these early pitch patterns could possibly provide a framework for later language development.

The Ninth Congress of the International Primatological Society, held last August, was reported in the August 1982 issue of Science News. Discussed in this publication were studies completed by ISHE members, Susan M. Essock-Vitale, Michael T. McGuire, Martin S. Smith and Marc D. Hauser. Author Wray Herbert made note on Essock-Vitale and McGuire's theory of individuals being more altruistic toward close kin

than toward distant because close kin carry more of the individual's genetic legacy. The article also highlighted Smith's suggestion that grandparents tend to invest more in the survival of their daughters' children than their sons' children because paternity is always less certain than maternity. Hauser's theory on the behavior of old age was abstracted as he concedes older subjects tend to engage socially less with other non-kin group members in that they have "depleted investment potential, become more discriminating in social interaction, wasting less energy on activity with no potential for genetic payoff."

New journals published by Academic Press include Brain and Cognition, edited by Harry A. Whitaker, which is a complement to Brain and Language, and focuses on monitor sensory processes, visual and spatial processes, memory, emotion, sex differences, and will accept research papers, clinical case histories, reviews and commentaries. Dr. Whitaker also edits Brain and Language which publishes papers on speech, hearing, reading, and writing as they relate to any aspect of the brain and brain function.

Volume 12 of the Journal of Human Evolution, Academic Press, edited by A.B. Chiarelli, is a recent issuance. It deals with current interest in cytogenetics and heredity along with paleontology and taxonomy as the journal aims at unifying the many disciplines involved in the study of human evolution.

Another recent publication by Academic Press is Developmental Review: Perspectives in Behavior and Cognition, edited by Grover Whitehurst. It presents analyses of social policies as they affect human development, historical examinations, and critiques of method and design.

Human Neurobiology is a new journal which compiles data relating to perception, cognition, memory, emotion, sleep and speech and publishes articles concerned with visual rehabilitation, brain stimulation, language and addiction. This is available through Springer-Verlag New York, Inc. with Volume 1 (1982) and Volume 2 (1983) both ready for ordering.

UPCOMING MEETINGS

The Midwest Regional Meeting of the Animal Behavior Society is scheduled for April 8-10, 1983 at the University of Missouri-St. Louis and St. Louis University. Invited paper sessions include "Recent developments in bird song study," "primate social organization," "ethology of infants and children." Contact Zuleyma T. Halpin, 1983 Midwest ABS Meeting, Dept. of Biology, Univ. Missouri-St. Louis, 8001 Natural Bridge Road, St. Louis, MO, 63121.

The 4th Biennial Conference of Ethology and Behavioral Ecology of Fishes is scheduled for May 9-12 at Illinois State University in Normal, Illinois. This is a conference/workshop of fish biology consisting of an exchange of ideas, methodology and research results. Students are encouraged to attend. Abstracts are due March 25th and those interested should contact the College of Continuing and Public Service at Illinois State University at (309) 438-8691.

The New York Academy of Sciences is holding a conference on Timing and Time Perception for May 10-13 at the Barbizon-Plaza Hotel in New York City. The conference will bring together researchers from the field of animal psychophysics dealing with temporal discrimination and scientists involved in human timing and the organization of temporal patterns. Deadline for pre-registration is April 25th and requests for information should be addressed to the Conference Director, c/o NYAS, 2 East 63rd Street, New York, NY, 10021 or call (212) 838-0230.

The American Association for the Advancement of Science is planning its annual meeting at the Renaissance Center in Detroit May 26-31. Approximately 150 symposia are scheduled to cover 20 areas of interest (i.e., earth sciences, engineering, cell biology, health care, agriculture, etc.). For registration information, write to AAAS-Dept. R, 1515, Massachusetts Ave., N.W., Washington, D.C. 20005 (non-members can join by completing a registration form) before May 20, 1983.

Victoria, B.C., Canada is the location for the June 29-July 2, 1983 meeting of the International Society for Research on Aggression. The Society is interdisciplinary and is involved in research with animal and human subjects. Write to Gordon W. Russell, Dept. of Psychology, University of Lethbridge, Lethbridge, Alberta, Canada T1K 3M4 for further information.

We are pleased to be meeting with the Animal Behavior Society this year at Bucknell University in Lewisburg, PA June 19-24. May 1st is the deadline for advanced registration. Highlights

of the program include: John Scott symposium, June 20th (Monday); Conservation Biology symposium, June 22nd (Wednesday); Clark Fox symposium, June 23rd (Thursday). The Human Ethology Business Meeting is planned for Tuesday evening, June 21st. Other events include geologic and historical tours, special events speakers and workshops, social functions and the annual banquet. For further information regarding arrangements at the University, contact Cheryl Kyes, Animal Behavior Program, Bucknell University, Lewisburg, PA, 17837, or telephone (717) 524-1431. Terry Christenson can be reached, Program Officer, at the Dept. of Psychology, Tulane University, New Orleans, LA, 70118 or phone (504) 865-5331.

Munich, West Germany is the site of the 7th Biennal Meeting of the International Society for the Study of Behavioral Development scheduled from July 31st to August 4th. For further details contact Prof. Paul B. Baltes, Max-Planck.Inst. for Human Development & Education, Lentze-Allee 94, D-1000 Berlin 33, Federal Republic of Germany or Prof. Rolf Oerter at the University of Munich, Am Stadtpark 20, D-8000 Munich 50, Federal Republic of Germany.

The Albert Einstein College of Medicine is holding the Fourth Cape Cod Institute from June 27th to September 2nd. The Institute proposes an in-depth, in-person exposure to the ideas and methods of leading clinicians and researchers. It consists of a series of separate week long programs that meet Monday through Friday from 9AM until Noon. Each program is approximately $295 and interested persons should contact the Office of Continuing Professional Education, Albert Einstein College of Medicine, Montefiore Medical Center, 111 East 210 Street, Bronx, NY, 10467 or telephone (212) 920-6676.

Notice of Symposium Planned for 1983 Animal Behaviour Division of the American Society of Zoologists: Entitled "Paternal Behaviour" and organized by Richard E. Brown of Dalhousie University, Novia Scotia, Canada, "the function of this symposium is to examine the occurrence of paternal care in a wide variety of animals and to examine the circumstances under which paternal care occurs and the factors which affect the expression of paternal care."

Future Meetings:

The International Primatological Society is to hold the Xth IPS Congress in Nairobi, Kenya between August 12-17, 1984. The XIth IPA Congress will be at Gottingen, Federal Republic of Germany.

Society for Neuroscience - call for papers, May 13th, for the 13th Annual Meeting scheduled for November 6-11 at the Hynes Auditorium at the Sheraton-Boston Hotel, Boston. Other meeting dates for the Society are: October 10-15, 1984 at Anaheim; October 20-25, 1985 at Dallas; November 9-14, 1986 at

Washington, D.C. Contact the Society for Neuroscience, 9650 Rockville Pike, Bethesda, MD, 20814 for further information.

The International Association for the Scientific Study of Mental Deficiency (IASSMD) is extending an invitation for the 7th International Congress to be held in New Delhi, India, March 1985. IASSMD is an international organization concerned with promoting scientific inquiry into all aspects of mental handicap. Although the theme has yet to be finalized, Dr. Annalise Dupont, chairperson of the Programme Committee, is welcoming suggestions. Dr. Dupont may be contacted at the Demographic-Genetic Research Department, 8240 Risskov, Denmark.

Meeting Reminders:

American Psychological Association's annual meeting to be held at Anaheim, Ca. this August (26-30). For further information, non-members should contact David B. Miller, Dept. of Psychology, U-20, University of Connecticut, Storrs, CT, 06268, (203) 486-3516.

Also coming up this summer is the 1983 meeting of the American Society of Primatologists scheduled for August 7-10. Contact Dr. David M. Taub, Program Chairman, P.O. Box 557, Yemassee, SC, 19945 concerning keynote speakers, invited symposia, etc.

Registration Form
"Human Life History Strategies Workshop"

Date: May 21, 1983
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[ ] I would like to attend the workshop.

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Mail to: Drs. Nicholas G. Blurton Jones & Ronald M. Weigel
Department of Education
University of California, Los Angeles
Los Angeles, CA 90024

For information, call: (213) 825-8315.

INTERNATIONAL SOCIETY FOR HUMAN ETHOLOGY

Membership and Newsletter

The ISHE was formed with the goal of promoting ethological perspectives in the study of humans. It encourages empirical research that addresses the questions of individual development, environmental, ecological and social processes which elicit and support certain behavior patterns, the function and significance of behavior, and comparative and evolutionary problems. The Society maintains an elected executive board and a number of committees, publishes a quarterly newsletter, collates an annual selection of human ethology abstracts, and meets annually in conjunction with the Animal Behavior Society, the International Primatological Society or another major society.

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International Society for Human Ethology
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