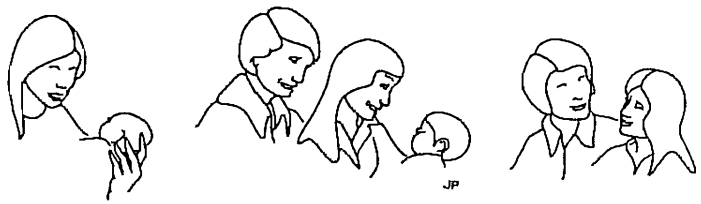
BONDING: A UNITARY PROCESS?



En Face

Mutual Gaze

HUMAN ETHOLOGY NEWSLETTER

JOAN S. LOCKARD, EDITOR	VOLUME 3	UNIVERSITY OF WASHINGTON
MARCH, 1982	ISSUE 5	SEATTLE, WASHINGTON 98195

A FLAG FOR SPRING

The masthead of this issue is flying the topic for our next forum. I have asked Nick Blurton-Jones to coordinate the responses. An elaboration of the question is given in the section SPRING FORUM.

ONE YEAR LATER

I would like to take this opportunity to thank four individuals whose help and donated time in the production of the newsletter have been invaluable: editorial assistant, Laurie Peterson; scientific programmer, Douglas Kalk; and artists Jocelyn Penner and James Congdon.

The past 12 months have seen a momentum to our Society that is truly exciting. The anxieties of a new science have given way to the challenge of the task ahead, with a feeling of comradery impossible in larger organizations. I wish to extend my appreciation to all those members whose contributions to the newsletter and active participation in 1981 allowed this prognosis.

To those whose term on the Executive Board ended with this issue, I. Eibl-Eibesfeldt, William McGrew, William Charlesworth, and Cheryl Travis, I am certain that the entire membership is grateful for the vital roles you have played in the founding of ISHE!

It is with equivalent enthusiasm that we welcome our new Board members: Michael McGuire, Esther Thelen, Ian Vine, and Ronald Weigel. They will serve for two years, overlapping in 1982 with Robert Adams, Gordon Burghardt, Wade Mackey, and Gail Zivin.

Committee assignments will be especially easy this time since most of our Board members are already serving in some capacity. It seems fitting that we ask Michael McGuire to chair the committee for long term goals, requesting simultaneously that Bill Charlesworth and I. Eibl-Eibesfeldt continue to give us the benefit of their experience. Gail Zivin and Ron Weigel have their work cut out for them with the upcoming international meeting in Atlanta. Bob Adams will continue to handle our recent literature section, Wade Mackey our human ethology abstracts, and Gordon Burghardt our membership. If Esther Thelen would take over the nominations committee and if Ian Vine, with Bill McGrew's continued help, would be willing to spearhead the European theater of our book review committee, then all immediate tasks would be covered.

THIS IS A TEST

Please adjust your set. If you can read the small print with relative ease, we have just saved a third of the production and mailing costs of our newsletter. A long term solution is not as simple. We will grapple with this issue at our Annual Meeting but do react now if this test is not to your liking. Your encouragement and comments since the last issue have been greatly appreciated and the suggested solutions do leave us alternatives:

"I am of the opinion that dues be doubled (to $10.00) and if possible, have the newsletter prepared by the editor and sent off to a publisher for printing and mailing. Not because it would look better (that 'handmade touch' has always been a hit with me) but because it will give the editor time to breathe between issues." -- Brian Gladue

"1) I think the newsletter is 'fancy' or 'professional' enough as you produce it. 2) I don't think that dues should be raised. Rather we should begin to be selective and perhaps shorten treatment of some topics to keep costs within budget. 3) Anything for a particular researcher (such as an enclosed questionnaire) should always be paid for (postage, printing, etc.) by that researcher. 4) I recommend that we not start publishing manuscripts." -- Jeanne Altmann

"Regarding the question of publishing, if it is simply a matter of format (and of $$), then the present style is perfectly adequate. If it involves excessive burden on the editor, then you have a very good reason for seeking an outside publisher." -- Ron Dare

"Regarding the newsletter: Either option is fine. Do whatever is easiest for you as editor." -- Bill McGrew

"Regarding Newsletter Blues, maybe it is time to have a more professional final product. Outside publishing might be a good idea. I'm willing to pay more. HEN is currently one of my best buys." -- Gary Mitchell

"To answer briefly the question of dues and publishing: From the beginning of the newsletter, I had the feeling that the $5.00 covered just the stamps. Also I think that the foreign members should have to pay more than the American. I like the typewritten version of the newsletter very much... I prefer a semi- confidential edition to a printed one. But you have the work and you have to say what is easiest for you." -- Etienne Colomb

"I suspect we would have to raise the fee considerably more if the newsletter were printed 'professionally'. It would probably be less expensive to just raise the fee, maybe to $7.50-$10.00, and let you keep doing it. How much is it currently costing to prepare an issue?" -- William Bailey

"Sorry about the financial difficulties for the newsletter. I noted that postage was charged 'First Class.' Not only would 'Printed Matter' save, you may look into non-profit organization mailing rates." -- Wolfgang M. Schleidt

"I'm sorry to hear about the financial difficulties of the newsletter... One option would be to mail the newsletter at a bulk rate... Some issues might necessarily have to go first class, like the ballot, for example. Another possibility is to shorten the length of the newsletter so that printing and mailing costs would be reduced. You might impose a 16-page limit on the present format. We could also impose an increase in foreign membership fees to cover the extra postage for airmail costs... if people don't want airmail, they could just wait for regular surface mail and get their newsletter later. We could also plan to increase the dues to about $7.00 for 1983." -- Cheryl Travis.

IN THE GUISE OF ALTRUISM

Since the last newsletter, several members have given gift subscriptions of the Human Ethology Newsletter to their university library or to their friends. This is a neat way to expand interest in our Society and increase membership. To help in this mutually beneficial endeavor, please forward the attached Library Recommendation Form to your Institution's library.

HAIR AND FEATHERS FLOCK TOGETHER

Wolfgang M. Schleidt sent the following remarks in reference to A BENCHMARK?, a discussion in the December, 1981 HEN on incremental changes and/or giant steps.

"George Oster of Berkeley may be offbase in his example that '... feathers require the process of evagination, while hair requires invagination.' I am, by training, a comparative morphologist, and at that time was taught that feathers are homologous to scales, and hair grows between scales, and is a new structure. A great (grand?) uncle of mine, Josef Schleidt, published some studies at the turn of the century, and I assume these still hold. If you know George Oster you may tell him to find out about this. He may look for a better example (if there is one). I assume he means mammalian hair. Birds 'make' hair from feathers; so, 'hair' of birds and 'hair' of mammals is an analogous structure, not homologous.

By the way, the same argument was made years ago against the theory that middle ear bones are homologous to fish jaw-joint. Relatively recently some fossil evidence was presented of some beasts which had a dual linkage, one behind the other, a kind of 'missing link'."

Schleidt, J. Uber Fruhstadien der Entwicklung von Schuppe und Feder. [On early stages in the development of scale and feather.] Archiv fur Mikroskopische Anatomie, Vol. 83, Abt. I (fur vergleichende und experimentelle Histologie und Entwicklungsgeschichte), pp. 118-129, 1913.

FORUM REACTION

"I truly appreciated the Kortlandt definition of human ethology (HEN 3(4): 8, 1981). I agree that the emphasis is not so much on methodology as on orientation, less on how you are doing research than on why you are. The biggest problem I have with my associates in psychology is not what they know about ethology but what they don't know. One "behavioral biologist" on my thesis committee couldn't understand how I could use a standardized setting to do an ethological study. He seemed even less enthused about the theoretical basis of the study. Most people here, while not concerned with phyletic history of the behaviors I'm interested in, for the most part will concede that the big hurdle is function. You can imagine the reaction to my suggestion that if I take some time to do a field study of parenting in mockingbirds I could possibly learn something that might help me better know/understand parenting in humans. An understanding of systems does not translate here." -- William Bailey

SPRING FORUM: Please respond by April 15

The topic for consideration is whether bonding is a unitary process in which mutual gaze and fondling (in the Klaus and Kennell sense, 1975, 1976) result in similar hormonal (emotional) changes, be it between parent- offspring or potential mates. This question may be meaningfully asked only if there is consensus as to the definition of the term and in what way (if any) it differs from the concept of attachment (e.g., Ainsworth, 1970, 1974; Bowlby, 1958, 1969). The subject is further complicated if we address the likelihood of "sensitive periods" and the measures by which we may adequately assess their chronic effects, if any. And finally, so as not to leave any room for an understatement of the breadth of this inquiry, is it then possible that "separation" in a bonded pair (at any age) results in other

physiological changes akin to depression.

Now, in 25 words or less: (1) What is bonding? (2) Is it similar for all age/sex classifications? (3) What are its proximal mechanisms? and (4) Is separation an antithetical process?

Ainsworth, M.D.S., and Bell, S.N. Attachment, exploration and separation: Illustrated by the behavior of one-year-olds in a strange situation. Child Dev., 1970, 41, 49-67.

Ainsworth, M.D.S., Bell, S.M., and Slayton, D.J. In M.P.M. Richards (Ed.) The Integration of a Child into a Social World. New York: Cambridge University Press, 1974.

Bowlby, J. Nature of a child's tie to his mother. Int. J. Psychoanal., 1958, 39, 350-373.

Bowlby, J. Attachment and Loss, Vol. 1. New York: Basic Books, 1969.

Kennell, J.H., Trause, M.A., and Klaus, M.H. In Parent-Infant Interaction, Ciba Foundation Symposium 33. Amsterdam: Elsevier, 1975.

Klaus, M.H., and Kennel, J.H. Maternal-Infant Bonding. Saint Louis: C.V. Mosby, 1976.

Least you be unhappy with Nick, let me assure you that he is not responsible for the topic but is willing to coordinate the responses. Please send your commentary to Nicholas Blurton-Jones, Graduate School of Education, UCLA, 405 Hilgard Ave., Los Angeles CA 90024.

WINTER FORUM: This issue

This forum is the first of probably several in the future that will attempt to determine the value of applying human ethology to other disciplines. The forum question posed in our September, 1981 newsletter by Thomas Wiegele and Roger Masters was:

How can human ethology illuminate the study of politics?

The following nine responses (one is a letter to the editor and the others are excerpts from books and meeting

presentations) provide an interesting mix of opinion regarding the influence of ethology on political science. We thank Thomas and Roger for taking the time to assemble this collection.

CAROL BARNER-BARRY
FROM: Longitudinal observational research and the study of basic forms of political socialization. In: Meredith W. Watts (Ed) Biopolitics: Ethological and Physiological Perspectives. ("New Directions for Methodology of Social and Behavioral Science") vol. 7, pp. 51-52, San Francisco: Jossey-Bass, 1981.

"Observational research is not a technique that has received very wide use among political scientists. Since the inception of contemporary empirical work in political science, the major data-gathering technique has been survey research. This is understandable, since surveys allow the researcher to collect large amounts of data from numerous respondents in a relatively short period of time. Also, it is a reasonable way of studying questions that involve people's attitudes and information about political phenomena. One weakness of survey research, however, has been its inability to generate reliable information on actual political behavior. At best, the researcher can get data on reported behavior--either the behavior of the informant or the behavior of persons known to the informant. Observational research, conversely, is well suited to the study of actual political behavior; the researcher (or his assistants) know what activities the subject actually was involved in because they saw the subject involved in them. What is more difficult to ascertain, of course, is the meaning that subjects attribute to their behavior, a difficulty also present in survey and other empirical research...

One discipline in which there has been extensive use of observational research is ethology. Ethologists observe the social behavior of animals, and generally they analyze their data within an evolutionary framework. Most relevant to political science is the body of ethological research concerned with the internal governance of animal social groups. The same method can be used to study social influence phenomena, such as power and authority, within human groups. [My own research] explores that use...with special reference to the potential relevance of such research to the study of political socialization."

CLARA B. JONES
FROM: Letter to Joan S. Lockard, September 28, 1981.

"Regarding the Fall Forum topic, 'How can human ethology „ illuminate the study of politics?' it seems to me that the question has been implicitly and explicity approached in each issue of the Human Ethology Newsletter, including 3:3, 1981 (e.g., in Charlesworth's and Masters' responses to the Washburn review and in Nye's letter). I also think that the discussions may reflect a general ambivalence if not confusion about what 'human ethology' is. I think I 'hear' writers saying that they are unclear about whether 'human ethology' and 'human sociobiology' are the same discipline.

As Tinbergen and others have pointed out, 'ethologists' have a 'commitment' (see Charlesworth) to apply Darwinian theory to the analysis of behavior. If it is possible to separate 'ethology' and 'sociobiology' along scholastic lines, I think it can only be done by viewing 'human ethology' as the study of 'species-typical' behavior patterns ('derived' activities, and the like, in the classical sense), generally viewed from a level above the individual; while 'human sociobiology' would necessarily view behavior from the individual level of analysis (intra- and inter-populational) and in its interactional context (neither being necessary from an ethological perspective).

Human ethologists are not, in my view, studying 'man's cultural achievements' (Charlesworth, again), per se (that idea suggesting to me some Cartesian notions of determinacy and descent), but the phenotypic relationship(s) between those achievements and their genetic consequences for individuals in the relevant population(s) (leading, often, to further 'achievements', this relationship, also, subject to evolutionary investigation).

Human ethology can 'illuminate... politics' through the study of correlates between ethnic (genotypic and ecotypic) differences and cultural (phenotypic) differences, within and between populations (e.g., 'culturgens' cum Lumsden and Wilson; or, the relationship(s) between attitudes and values, and actions), focusing, especially upon 'ultimate' causation, mechanisms and consequences. How, for example, does an individual's 'ideology' contribute to his/her 'inclusive' reproduction success?

While recent empirical studies reveal to this reader a definite discomfort about utilizing Darwinian theory in a hypothetico-deductive manner (e.g., Altmann's 1980 report from Harvard Press on baboon mother-infant behavior), I

think that attempts by primate ethologists and sociobiologists to, for example, measure genetic (via electrophoresis or 'heritability' measures, for example) and phenotypic-behavioral correlations; to clarify how communication signals may 'converge' between genotypes (cum Moynihan); to study how the same signals may be employed in different contexts (Moynihan, again); to investigate how individuals, and/or genotypic or phenotypic mutuals, might 'jam' the transmission systems of conspecifics to their own advantages; to assess the extent to which individuals (genotypes) can 'mimic' varying cultural patterns to their own advantage (see Moynihan on 'social mimicry'); and, to assess the extent to which the biostatistics of 'culturgens' (if, indeed, sociality is so fine-grained) may be a function of the biostatistics of Mendelian genetics stand out, in my view, as primary issues to 'illuminate' the ultimate significance of behavioral systems, including belief systems.

Most important to incorporate into 'human ethology,' I think, is the 'sociobiological' issue: How does behavior (regardless of its degree of genetic canalization) serve an individual's genetic self-interests? I hope that in the Human Ethology Newsletter we might address ways in which such concerns might be formalized."

ROGER D. MASTERS
FROM: The impact of ethology on political science. In: Albert Somit (Ed) Biology and Politics. The Hague: Mouton, 1976, pp 198-199.

"From the first serious attempts to create a rigorous science of politics in the modern sense (as distinct from an 'art' or 'philosophy' of politics), it seems fair to say that the prevalent model of a true science has been either mathematics or physics. Among political theorists, from the mechanism of Hobbes and eighteenth century philosophers like Helvetius to the nineteenth century positivism of Comte, physics increasingly became the standard of what would be scientific in a science of politics. Indeed, one eighteenth century group which pretended to have formulated a science of politics--the physiocrats--symbolizes this tradition in its very name...

Although the study of ethology and its application to human behavior has not been the only factor in challenging this attitude, the popularized works of Robert Ardrey, Konrad Lorenz, and Desmond Morris have reflected a movement from physics to biology as the scientific model to which political science should aspire. To be sure, what has often

been called the 'biological revolution'--and notably the extraordinary advances in molecular biology and biochemistry--have contributed to a renewed interest in the biological sciences. Moreover, it was George Gaylord Simpson..., an exponent of the neo-Darwinian 'synthetic' theory of evolution rather than an ethologist or biochemist, who asserted that 'biology...and no longer mathematics, is now the queen of the sciences.'

There are, it might be added, numerous considerations which converge to support this epistemological and methodological shift. Like biology--and unlike classical physics--the social sciences study populations of organisms that change over time. Like biology--and unlike classical physics--time is an essentially irreversible variable of decisive importance in most of the phenomena analyzed by political scientists. Like biology--and unlike classical physics--the perfectly controlled experiment is difficult if not impossible in political science. Like biology--and unlike classical physics--some form of technological or functional reasoning seems inherent in political life. Finally, like biology--and unlike classical physics--political science studies complex systems (human societies) which are self-replicating organizations of information. If nothing else, the convergence between biology and what has come to be called 'structuralism' in anthropology and linguistics suggests the importance of the parallels between biological and social science.

It can be argued, however, that the widespread interest in and respect for biology would not in itself have led political scientists to take a biologist like Simpson seriously when he asserted: 'I am content to define the social scienes as those branches of biology dealing with organisms that have language'. Rather, the emergence of ethology as a sub-field of biology devoted to the comparative study of animal behavior, and especially its popularization by authors who included human behavior in their comparisons, has apparently encouraged many social scientists to consider more seriously the kinship of their disciplines to biology...

Whatever the theories or empirical propositions that political scientists may borrow or derive from ethology, this shift in perspective may have exceptionally profound effects on the discipline. At the risk of using a word rendered trite by overuse, at this level it is entirely possible that political science is in the process of what Kuhn called a change of paradigm."

GLENDON SCHUBERT
FROM: "Ethological Politics." Paper presented at Symposium on Ethological Approaches to the Study of Politics. Annual Meeting of American Association for the Advancement of Science, Washington, D.C. (January 6, 1982), pp. 13-14.

"Both primate and carnivore models of social behavior are relevant to our understanding of hominid evolution, and hence both have an indirect bearing upon human behavior today. But neither living primates nor living social carnivores offer a 'better,' or even too close, a model to directly support cross-species comparative analysis of such social behaviors as territoriality, aggression, predation, and conspecific killing--all of which bear directly on such political behaviors as war, revolution, colonialism, and minority subjugation. The extent to which such behaviors have biological roots and hence reflect genetic as well as cultural components surely ought to be of concern to political science. Political evolution demands study in evolutionary, prehistorical terms; but the appropriate frame of reference is to be found in the unique phylogeny of hominids evolving into modern humans, and in a behavioral ecology that is focused on human adaptive--and maladaptive--responses to environmental opportunities, demands, and constraints, instead of on speculative deductions from evolutionary pipe-dreams even when they assume a mathematical disguise. We may well learn something about our species' capacities for warfare by studying it in the microcosm of the political behavior of primitive groups; there is no evidence indicating that we shall learn much about its elimination by studying that as an exercise in the applied theory of reciprocal altruism.

A few recent examples could be cited of attempts to understand contemporary political behavior utilizing ethological theory and methods; but the surface has barely been scratched in work done to date in relation to what would need to be attempted were political scientists generally to begin to start taking ethology seriously. And conversely, although a few examples can be cited of attempts by primatologists to understand the social structure of simians in political terms, such work has been undertaken so far with only the barest genuflection in the direction of the presumption that political science does not necessarily represent a completely species-specific body of knowledge. In short, political scientists need to know a great deal more about ethology before their observational research can begin to test the hypothesis that human biology has an important effect upon human behavior. And ethologists interested in the political behavior of other social animals ought to feel obliged to acquaint themselves better with

what already is systematic knowledge about the political behavior of humans."

ALBERT SOMIT
FROM: Introduction. In: Albert Somit (Ed) Biology and Politics. The Hague: Mouton, 1976, p. 10.

"What is it that political scientists should be studying? How can they best go about that inquiry? Here, the history of ethnology may be instructive. The field of animal behavior made relatively slow progress so long as ethologists limited their observations primarily to animals in zoos, experimental laboratories, and other man-made settings. A near quantum jump in knowledge was achieved when the realization dawned that, adequately to understand the behavior of an organism, it must be studied in the environment for which it has been designed by evolution--its natural habitat. This forced some drastic changes in the life style of ethologists, but the results were most impressive.

Much of what we deal with in political science is, at best, only indirectly related to political behavior; much of our inquiry takes place in what is, at best, a simulated political environment. To date, the greatest impact of ethology on political science has been in terms of concepts and explanatory theory. We might profit most, at least in the short run, if the ethologists' example persuades us to look at actual political behavior in an actual political arena."

JOHN WAHLKE
FROM: "Some Notes on Biology and the Study of Politics." Paper presented to the 50th Anniversary Meeting of the Canadian Political Science Association, May 1978.

"How...can ethology be of any use to political scientists? Two different strategies suggest themselves. One proceeds from the evolutionary inter-relationships among living creatures essentially by analogy. It seems obvious that any generalization applicable to all primates, or to all vertebrates, or to any other whole taxonomic class which includes our species must ipso facto apply somehow to human behavior also. Without observation and careful study of humans, but by reasoning from analogy to the observed behaviors of cognate species which have been studied, we can make valid statements about humans. This entails using various ethological concepts as 'templates,' so to speak, against which to compare human behvior in a search for

manifestations of analogous or homologous behavior. That is, the record of human behavior is searched for instances of territorial behavior, bonding practices, male dominance behaviors, and any others which may be of interest and known to occur among related species. This, to put the matter over-simply, is essentially the procedure followed in most of the popularized works approaching human behavior ethologically-- Ardrey, Morris, and, to a considerable extent, Lorenz, for example.

From the standpoint of ethology, such a strategy may be justifiable. But it is not very appropriate for political scientists, who, qua political scientists, are interested not in the grand total of all human behavior or the sum total of all facts in human history, but in political and governmental phenomena. The strategy of lifting concepts out of the general body of ethological literature in hopes of finding analogues for them in human behavior...is not a set of directions for studying and explaining observed variations or uniformities in politically relevant human behavior, but a vague charge to go out and find some dependent variables to describe. Abraham Kaplan's warning against succumbing to 'the law of the hammer' is well known. The strategy of inquiry just described might by analogy be described as succumbing to 'the law of the concept.'

A more effective strategy, therefore, is to begin inquiry (as Kaplan recommended) with genuine puzzlement, confessed ignorance, about some noteworthy set of political events or phenomena, with genuine curiosity about 'What in hell is going on here?' Then, accepting as hard knowledge the most general ethological principle that the problematic or puzzling behavior can be 'unpacked' for study by discovering to what extent programmed, fixed action patterns are involved in it and how one can formulate working hypotheses as first approximations to explanation. And then, of course, one must design and conduct empirical research to test more specific and manageable research hypotheses derived from that. In sum, political scientists can do better by beginning with a provocative explanadum and proceeding logically to work out the explanans than by going the other way round, i.e., by starting out with an explanation of something-or-other in hand to look for something that it might explain."

MEREDITH W. WATTS
FROM: Editor's notes and introduction. In: Meredith W. Watts (Ed) Biopolitics: Ethogical and Physiological Perspectives. ("New Directions for Methodology of Social and Behavioral Science") vol. 7, pp. 1,11, San Francisco:

Jossey-Bass, 1981.

"The synthetic biopolitics refers to an interstitial discipline with an identifiable set of intellectual concerns, methodological interests, and a growing number of scholars. Where, in the period from 1963 to 1969, there was a total of 21 written documents clearly in the biopolitical tradition, there were 91 in 1970-1974 and 176 between 1975 and 1979. One of the first papers appeared in 1964, and reviews of the literaure have appeared every few years since then. Those interested in the area, though hardly identifiable with sociobiology per se, have been noticed and picketed by the critics of sociobiology -- a rare tribute to the imagined or predicted impact of biopolitics. Perhaps most important, there has emerged in the biopolitical literature a recognition that it is time for the rigorous development of empirical research. The appearance of the first biopolitics text is another sign of growing vigor...

Common to all the [work] in this [field] is an interest in the evolutionary history and the biological substrate of human sociopolitical behavior. It is by no means implied that any of these phenomena can be reduced to purely biological concepts. In fact, it is not argued that any of these phenomena is biologically determined; one needs only to accept that there is a biological component and that social behavior has biological parameters. This minimal acceptance is all that is needed for general social scientists to be able to take notice of these developments and consider their possible contributions.

Biopolitics does not attempt to displace any existing approach but rather to provide the social sciences with the theoretical and empirical richness of the life sciences perspective. Its success will very likely be determined, not by the displacement of some current approach, but from integration with contemporary and conventional usage. For example, if we assume that humans are both rational, cognitive creatures and biological entities with appetites and needs, then the integration of biological wth cognitive, phenomenological, and behavioral perspectives is more than a homiletics -- it is a theoretical and empirical probability.

THOMAS C. WIEGELE
FROM: Biopolitics: Search for a More Human Political Science. Boulder, Colo.: Westview Press, 1979, pp. 146,148.

"Ethology has given us many useful organizing concepts including territoriality, bonding, imprinting, and

ritualized behavior. Ethology also has the powerful advantage that many of its insights have grown out of a comparative perspective on animal behavior... [However,] the most productve avenues for the political scientist to exploit in developing a more operationally comprehensive definition of human naure [may] lie in the life sciences that are devoted exclusively to the study of man. The bodies of knowledge that have dealt with the human organism directly and empirically include medicine, psychopharmacology, neuroanatomy, biochemistry, epidemiology, human biology, psychophysiology, human physiology, human endocrinology and behavioral ecology. Each of these is a significant discipline in its own right; each has focused on the human species; each has the potentiality of adding to our understanding of political society. Much of the work relating to the study of elites, conflict and aggression, and even the general political system has grown out of these life sciences."

FRED H. WILLHOITE, JR.
FROM: Rank and reciprocity: Speculations on human emotions and political life. In Elliott White (Ed) Sociobiology and Human Politics. Lexington: Lexington Books, 1981, pp. 241,255.

"...How is one to understand the field observations of intraspecies lethal violence among, for example, chimpanzees, gorillas, several types of monkeys, wolves, lions, hyenas, African wild dogs? Is it really 'bourgeoismorphic' to interpret such behavior as competitive -- for status, mates, or other resources -- when the observed situation seems to make most sense in those terms? It seems entirely possible that ideological prejudice could prevent accurate understanding of animal behavior; the sword of 'unmasking' cuts more than one way.

I am not implying that sociobiology is, or should be, immune from trenchant criticisms. Sociobiological theorists have sometimes given the impression that they consider the whole symbolic realm of human culture as a mere epiphenomenon expressing and partially masking genetic imperatives. This probably reflects an understandable lack of experience and naivete in dealing with human data, and anthropologists and other social scientists are well equipped to point out errors and mistaken assumptions on the part of evolutionary biologists. As I have indicated, some of the latter have begun to develop much more sophisticated conceptualizations of the evolution of human behavior, while continuing to insist that it must be interpreted within a fundamentaly Darwinian framework.

It seems to me unwise at this time for political theorists to commit themselves unreservedly to one particular version of evolutionary theory. Biologists who mutually agree on the reality and fundamental significance of the evolutionary process still disagree on many important points of interpretation. However, I do regard it as perfectly legitimate to consider sociobiological speculations about human evolution and attempt to discern some of their potential implications, as long as the provisional and exploratory nature of this kind of thinking is clearly understood. If one desires to theorize within the boundaries of empirical science, there is no choice but to attempt to develop an evolutionary conception of human nature and politics, however fumbling, error prone, and interminable the effort my be.

As a student and teacher of the great political theorists of the Western tradition, I greatly admire the boldly speculative character of Robert Triver's work. Starting from the fundamental assumption that the individual is the principal unit of selection within the evolutionary process, Trivers seeks to explore the implications of this assumption for our understanding of some of the most elementary social relationships -- for example, those between parents and offspring, or between siblings. I [have explored] a few implications for political theory of one type of social interaction discussed by Trivers -- 'reciprocal altruism,' exchanges of assistance or resources...

As genetically distinct but necessarily interdependent and cooperative beings, conditioned by a culture that stresses the worth of the individual and by living in complex, stratified, rapidly changing societies, we tend to place a high value on personal freedom but also feel anxiety and guilt about exploitation. Historical experience has shown that all-out unregulated economic freedom cannot persist. It has been unavoidably necessary for governments to become involved to an increasing degree in redistributive activities. A difficult and troubling question with which this trend confronts us is whether, and in what ways, its indefinite continuation would undermine the socioeconomic preconditions of personal and political freedom.

Governments that do attempt to control and regulate nearly all exchanges of goods and services, with their controllers claiming that they guarantee total economic justice -- truly reciprocal altruism -- are hostile to personal freedom. Furthermore, their rigid, elaborately graded, and unresponsible hierarchies represent cultural-evolutionary regression to the governmental system

of theocratic empires -- without providing the psychological consolations of the supernatural. And, as far as I know, there is no persuasive evidence that these political systems are in practice any less economically exploitative than the mixed economies associated with constitutional democracies."

ELLIOTT WHITE
FROM: Introduction. In Elliott White (Ed) Sociobiology and Human Politics. Lexington: Lexington Books, 1981, pp xi-xii.

"The fact that scholars and scientists differ and disagree, as they surely do in this [field], might be presented as an argument for a relativistic position, whether historically or sociobiologically based. Yet the alternative to an egalitarian relativism wherein each individual's 'truth' is merely that and no more might encompass the idea of a hierarchy wherein some individuals, by virtue of greater ability and experience, are more apt than others to envision the truth. Everyone may be fallible, but some are more fallible than others. We all recognize that the science of medicine is imperfect, but we still wish to consult the best doctor around...

In the case of humans, this possibility takes on an added dimension with what Etkin refers to as our unique 'capacity for foresight and planning.' If contemporary neurobiology should establish a scientific basis for such a capacity, as I believe it may well do (my paper at the 1979 American Political Science Association meeting on 'Sociobiology, Neurobiology and Political Socialization' elaborates this contention), then human neurobiology will move to the forefront in the explanation of human behavior and hence of science as well. In both Sociobiology and On Human Nature, Edward Wilson leaves this possibility open, making clear that for him sociobiology is only half, albeit an extremely critical half, of an emerging 'behavioral biology' that includes, as its other half, neurophysiology. As the latter term -- as used by Wilson -- implies, the study of the brain may stress a materialistic, genetically reductionistic view that will turn out to be generally compatible with the sociobiological perspective now taken by Wilson, Trivers, and others. If, however, the future course of neurobiology happens to be more independent and perhaps more mentalistic, then behavioral biology -- as a union and ultimately a synthesis of neurobiology and sociobiology -- will also develop more independently of contemporary sociobiology, with the latter also undergoing qualification on its own terms with the passage of time.

To venture a further unscientific speculation, I believe that such an emergent behavioral biology will be brought to bear on science itself. The 'behavioral biology of science' will acknowledge the profound human genetic variability that characterizes human populations and therefore the scientific community itself; it will acknowledge individual differences in background and experience as well as in intellect; and it will, finally, accept the possibility that the hypotheses, concepts, and findings that characterize the process of scientific inquiry exist and are subject to validation on their own terms, irreducible to a merely genetic or environmentalist level.

I will make one final unscientific prediction. Such a developing behavioral biology should also form the basis for what Gunther Stent in The Coming of the Golden Age calls a 'classical paradigm' for political and social science. Up to now political and social science have had no consistent approach. The current behavioral umbrella has suffered from divisive splits in the past and is now, in any case, in the process of being overturned by the increasingly strong winds emanating from the life sciences. Thus I believe that what Stephen Toulmin has called the 'would-be discipline' of the social sciences is now being replaced with its classical paradigm. This development, following Stent, means that the general concepts that increasingly will guide future inquiry and will themselve find a fuller validation a differing levels of explanation are presented and explicated for the first time in a systematic fashion. In classical genetics at the turn of the century, the concept of the gene both guided future research and came itself to be understood on the level of molecular biology, whereupon genetics entered a new phase in its development as a field.

I will not speculate here on the course of political and social science following their classical era, because I believe that we will not transcend our new status until both sociobiology and neurobiology -- both of them also fledgling fields -- are ready to transcend theirs. And the prospect for their doing so before the passage of generations or even centuries seems to be slight."

BOOK REVIEWS

For this issue we have reviews of two recently published books. The first is a contribution from one of our members and the second is reprinted from two different outside sources.

THE ROOTS OF HUMAN BEHAVIOUR. By Myron A. Hofer. San Francisco: W.H. Freeman and Co. 331 pp. (1981)

Reviewed by Peter K. Smith, Department of Psychology
University of Sheffield, England

This is a well-written, lucid and concise book which gives an overview of the psychobiology of early development, suitable for an undergraduate course.

Much of the thrust of the book is on mechanisms in early development, in human and in other species. There are thorough discussions of the properties and growth of neurones, and neuronal networks; of elementary forms of learning and of behaviour organisation; of prenatal and neonatal behaviour; of the effects of the intrauterine environment; and the influences of nutrition, hormones, and sensory stimulation and environmental interaction on brain development.

The text is generally factually well-informed and up-to-date, but in an otherwise strong chapter on the early parent-infant relationship, the many criticisms of the Kennell and Klaus work on very early mother-infant bonding are not mentioned. The concluding chapters on language, play, and sexual and aggressive behaviour will not seem so strong, to a psychologist, as the rest of the book. The treatment of aggression is particularly scanty, and the work on dominance in children is not mentioned, nor indeed other ethological work on peer interactions, the latter topic only taking up directly one page of text.

The strength of the book is in psychobiology rather than psychology. It would be difficult for a student to read this book and still have simplistic views of the nature/nurture issue. Despite some claims in the introductory and final chapters, however, it goes very little of the way toward giving an evolutionary basis to early development. The first 15 pages are on evolutionary theory, and give a fair resume of recent developments, including sociobiology; but these points are seldom taken further in the text, apart from the standard example of Bowlby's attachment theory. Perhaps this just shows that the sociobiology of early development is still largely unexplored or at least unsystematised territory. For an understanding of mechanisms of early development, the book has much to ofer. It is well-produced, and reasonably well illustrated.

THE MISMEASURE OF MAN. By Stephen Jay Gould. New York: W.W. Norton. 352 pp. (1981)

Reviewed by Robert Kagan
Kennedy School of Government, Harvard University

(December 9, 1981, p. 22.)

Through most of history, belief in an ordered ranking of beings, from beast to man to angel and demigod to god, has predominated over all views of creation. That same hierarchical structure also has been imposed on the human race.

Theognis of Megara in the 6th Century B.C. divided men into the good and the bad on the basis of their noble or ignoble birth. He believed a quality of mind separated the two, a quality that the Greeks called "gnome" or judgment, "not infallible, but constant enough to assure its possessor the advantages of good moral behavior." Theognis believed that only a few men possessed this judgment, that it was lost through interbreeding between the noble and ignoble, and that the Greek polis, which put in such close proximity the base- and well-born, would ultimately lead to the degeneration of the Greek peoples.

It is surprising how little man's understanding of the hierarchy of human intelligence changed over the next 2,500 years. Today we no longer talk of noble and ignoble birth, nor would we propose, as Theognis and, later, Plato did, that only men and women born of the best parentage should rule.

But as recently as 40 years ago, as Stephen Jay Gould points out in "The Mismeasure of Man", scientists, sociologists and other learned people proposed tightened U.S. immigration laws, discriminatory education and forced sterilization as means of protecting the purity of a race from the degenerative effects of interbreeding with those of innately low intelligence. Even today William Shockley advocates voluntary sterilization of those with hereditarily low IQs.

What separates these modern believers in racial purity and innate intelligence from their ancient forebears has been a claim to scientific objectivity. According to Mr. Gould, Plato's "Noble Lie" -- that men and women were born of a certain metal, whether gold, silver or brass, indicating their place in society -- has given way to the

The Inferiority Complex

The Mismeasure of Man
by Stephen Jay Gould.
Norton, 352 pp., $14.95

R. C. Lewontin

The first meeting of Oliver Twist and young Jack Dawkins, the Artful Dodger, on the road to London was a confrontation between two stereotypes of nineteenth-century literature. The Dodger was a "snub-nosed, flat-browed, common-faced boy...with rather bow legs and little sharp ugly eyes." Nor was he much on English grammar and pronunciation. "I've got to be in London tonight," he tells Oliver, "and I know a 'spectable old genelman lives there, wot'll give you lodgings for nothink...." He was just what we might have expected of a ten-year-old streetwise orphan with no education and no loving family, brought up among the dregs of the Victorian *Lumpenproletariat*.

Oliver's speech, manner, and posture were very different. "'I am very hungry and tired,'" he says, "the tears standing in his eyes as he spoke. 'I have walked a long way. I have been walking these seven days.'" Although he was a "pale, thin child," there was a "good sturdy spirit in Oliver's breast." Yet Oliver was born and raised in that most degrading of nineteenth-century institutions, the parish workhouse, deprived of all love and education. During the first nine years of his life he, "together with twenty or thirty other juvenile offenders against the poor-laws, rolled about the floor all day, without the inconvenience of too much food or clothing."

Where amid the oakum pickings did Oliver find the moral sensitivity and knowledge of the English subjunctive that accorded so well with his delicate form? The solution of this, the central mystery of the novel, is that Oliver's blood was upper-middle-class, though his nourishment was gruel. Oliver's whole being is an affirmation of the power of nature over nurture. It is a nineteenth-century prefiguration of the adoption study of modern psychologists, showing that children's temperaments and cognitive powers resemble those of their biological parents whatever may be their upbringing. Blood will tell.

Dickens's explanation of the contrast between Oliver and the Artful Dodger is a form of a general ideology that has dominated European and American social thought for the last 200 years, and is the central concern of Stephen Jay Gould's book—the ideology of biological determinism. According to this view, the patent differences between individuals, sexes, ethnic groups, and races in status, wealth, and power are based on innate biological differences in temperament and ability which are passed from parent to offspring at conception. There have, of course, been countercurrents of "environmentalism" emphasizing the malleability of individual development and the historical contingency of group differences, but, with the exception of Skinnerian behaviorism, all modern theories of social development have assumed an irreducible nontrivial variation in innate abilities among individuals and between groups. Occasionally, the political consequences of extreme biologism have been so repugnant that environmental and social explanations of group differences have held temporary sway. So, the practical application of biological race theory by the National Socialist state discredited biological theories of racial and ethnic superiority for about thirty years, but by 1969, with the publication of Arthur Jensen's monograph *How Much Can We Boost IQ and Scholastic Achievement?*, it was once again not only respectable, but even popular, to argue that blacks owed their inferior social position to their inferior genes.

Because biological determinism is a structure of social explanation that uses basic concepts in anatomy, evolutionary theory, genetics, and neurobiology, often in a corrupted form, its critique demands the powers of a historian of ideas and a professional biologist. Because the scientific methods and concepts involved are rather abstruse, criticism also requires a first-class writer. Fortunately, Gould is a professional historian, an evolutionary biologist and anatomist of great accomplishment, and a master at explaining science. *The Mismeasure of Man* is his examination and debunking of the scientific face of the fiction of Oliver.

Dickens's view of the origin of human variation was hardly exceptional; it permeated nineteenth-century literature. At times it appeared only incidentally as part of the substrate of unspoken assumption as, for example, in *Felix Holt*, when Esther Lyon is set to learning French on the assumption that her French ancestry will make it easy for her. At others, it is a central preoccupation, as in Eliot's *Daniel Deronda*. Daniel, the adopted son of a baronet, is a typical young English *milord*, whom we first meet at a fashionable Continental gambling spa. But then, mysteriously, in his young manhood, he develops an interest in things Hebrew, falls in love with a Jewish girl, becomes converted. The reader is not entirely astonished to learn that Daniel's mother was, in fact, a Jewish actress. The Law of Return, it seems, is only an expression of the inevitable.

A preoccupation with the power of blood was not simply what the French know as "the madness of the Anglo-Saxons." Eugène Sue, the most popular French author of the mid-nineteenth century, created in *Les Mystères de Paris* the archetype of the noble prostitute, somehow unsullied and saintly in the midst of her sordid existence. She was, of course, the abandoned child of a morganatic marriage. Among the *goyim* at least, the true character apparently can be transmitted through the paternal line. But it is in the Rougon-Macquart novels of Zola that biological theories of character are given their most careful articulation. The Rougons and Macquarts were, it will be recalled, the two halves of a family descended from a woman whose first, lawful, mate was the solid peasant Rougon, while her second, illicit, lover was the violent, unstable Macquart. From these two unions arose an excitable, ambitious, successful line, and the depraved, alcoholic, criminal branch that included Gervaise and Nana. When Coupeau, Gervaise's husband, is admitted to the hospital for alcoholism, the examining physician asks him first, "Did your father drink?" As Zola says in his preface to the cycle, "Heredity has its laws, just as does gravitation."[1]

Zola's "experimental novels," as he called them, were the outcome of developments in physical anthropology as a scientific, materialist discipline, developments to which the first part of *The Mismeasure of Man* is devoted. In America, Samuel Rogers Morton had, in the 1830s and 1840s, measured large numbers of skulls of different human groups, including long-dead Incas and ancient Egyptians. The Anthropological Society of Paris had been founded in 1859 by Paul Broca, the leading European exponent of the theory that high intelligence and character were a consequence of larger brains, so that the mental qualities of individuals and races could be judged from the sizes of their skulls. The appearance, in the same year, of the *Origin of Species* gave rise to an evolutionary view of human differences that placed each physical type on an ascending scale of progress from our apelike ancestors. In particular, criminals were seen as ataviams, apelike in both mind and body, but in a variety of forms, so that the founder of criminal anthropology, the Italian Cesare Lombroso, could tell a murderer from an embezzler at a glance. But Broca and Lombroso were only the inheritors of a long tradition that began with the natural philosophers of the eighteenth century.

The reductionist materialism of Descartes's *bête machine* and La Mettrie's *homme machine* led inevitably to the anthropometry of Broca and Lombroso. If mind is the consequence of brain, then are not great minds the products of great brains? Indeed, phrenology was a perfectly sensible materialist theory. Since acquisitiveness is a product of a material organ, the brain, then highly developed acquisitiveness should be the manifestation of the enlargement of one region of the brain. On the not unreasonable (although factually incorrect) assumption that the skull will bulge a bit to accommodate a bulge in the cerebral hemisphere, we might well expect an enlarged "bump of acquisitiveness" among the more successful members of the Exchange, not to mention Jews in general.

Moreover, less developed races should have less developed brains, women should have smaller cranial capacities than men, the lower classes more sloping foreheads than the bourgeoisie. Thus one should be able, by the appropriate physical measurements, to characterize the mental, moral, and social attributes of individuals and groups. There are, however, two problems with this theory. First, there is the factual error. Despite all claims to the contrary, there are no differences in brain size or shape between classes, sexes, or races that are not the simple consequence of different body size, nor is there any correlation at all between brain size and intellectual accomplishment. Second, there is the conceptual error. Intelligence, acquisitiveness, moral rectitude are not *things*, but mental constructs, historically and culturally contingent. The attempt to find their physical site in the brain and to measure them is like an attempt to map Valhalla. It is pure reification, the conversion of abstract ideas into things. While there may be genes for the shape of our heads, there cannot be any for the shape of our ideas. It is with an exposure of these two errors of biological deter-

[1] Emile Zola, preface to *La Fortune des Rougons* (Librairie International A. Lacrois, Verboeckhoven, 1871).

The New York Review of Books, 28, 12-16, 1981.

numerical rankings of the IQ test. But, he argues, "determinist arguments for ranking people according to a single scale of intelligence, no matter how numerically sophisticated, have recorded little more than social prejudice."

Mr. Gould offers quite a bit of historical evidence to support this claim. "The Mismeasure of Man" is a rogue's gallery of consciously and unconsciously dishonest scientists and pseudo-scientists--craniometrists who juggled their measurements of skulls so that the mean cranial capacities of white, Northern European men always came out the largest; social and behavioral scientists such as H.H. Goddard who touched up photographs of his famous "Kallikak" family to make them look demented and demonic; and mental testers such as Lewis Terman, who tried to measure the IQs of great men who had been dead for hundreds of years (Cervantes and Copernicus each managed only 105). Not all of these scientific frauds were committed in the name of racism and class privilege, but enough of them were to raise serious doubts about the possibility for scientific objectivity in the study of human intelligence, and it is Gould's primary aim to lend weight to these doubts.

Anyone interested in the current debate over human intelligence, however, will be disappointed by this book, for Mr. Gould fails to give it serious treatment. Instead he devotes fully a third of "The Mismeasure of Man" to craniometry, a crackpot "science" if every there was one, the main premise of which was that large cranial capacity meant high intelligence. In fact, as Mr. Gould points out, large heads generally sit atop large bodies. But by focusing so much attention on the obviously misguided and often demonstrably racist works of a discredited science, Mr. Gould obscures, intentionally I believe, some of the really hard issues raised by modern intelligence testing.

Why, for instance, do some groups score better than others on almost all the tests ever devised for measuring intelligence? Mr. Gould has no answer. He treats modern studies of human intelligence and heredity as direct descendants of the earlier frauds, besieged by the same prejudices and base motives, particularly our "persistent, indigenous racism." Except for a brief discussion of Arthur Jensen, he does not deal with present-day theories of intelligence and attacks them only by inference. He is simply convinced that any study that reveals differences between races, any study that makes much of hereditary intelligence, indeed, any study that attempts to measure intelligence at all is necessarily tainted.

Mr. Gould's crucial argument in "The Mismeasure of Man" is that any attempt to "reify" intelligence, to suppose that there is some thing in the human mind that can be measured, is fallacious. He makes a daring and largely successful attempt to explain, and then debunk, the complicated statistical theory of factor analysis, upon which is based the notion of a scientifically measurable intelligence factor for all humans. He does indeed demonstrate that statistical analyses of intelligence are faulty. They may even be forever doomed to failure.

But he cannot disprove what has been obvious to all men at all times, that some human beings are smarter than others in every way, and that their children tend in general to inherit this mental superiority. This basic understanding must not be allowed to affect this country's laws or social policies, but neither can it be wished away or debunked.

"The Mismeasure of Man" is very well-written. As a history of bad science and social science, and a thorough lesson on how scientists often fit their facts to their prejudices, it is important reading. But as an open-minded treatment of the intelligence question, it falls short, for in the end Mr. Gould becomes subject to the same criticism that he levels against the measurers of man. He knows what he believes and he sets out to prove it. He may even be subject to the political and cultural presssure of his own period, for if there are prejudices that plague the social sciences today, those prejudices more likely seek to suppress evidences of inequality than to exploit them.

THE MISMEASURE OF MAN. By Stephen Jay Gould. New York: W.W. Norton, 352 pp. (1981)

Reviewed by R.C. Lewontin, Agassiz Professor of Zoology
Museum of Comparative Zoology, Harvard University

minism that Gould's *The Mismeasure of Man* is largely concerned.

The first problem is to explain how the zoologists and anthropologists of the nineteenth century could find, so consistently, that, for example, the brains of whites are significantly larger than the brains of blacks when, in fact, there is no difference between them. The answer seems to be, according to Gould, that the most eminent zoologists and anthropologists simply rigged the data. When Samuel Morton, in his *Crania Americana* of 1839, showed conclusively that American Indians had smaller craniums than Caucasians, he did so by including a large number of small-brained (because small-bodied) Inca skulls in his Indian sample, but at the same time excluding a number of Hindu small-skulled specimens from his Caucasian sample. When Gould recalculated the data using all of Morton's measurements, the difference between Indians and Caucasians disappeared. Paul Broca, faced with some very small brains of some very eminent professors, invented *ad hoc* corrections for age and postulated disease. As a last resort he appealed to the imperfection of institutions:

> It is not very probable that five men of genius would have died within five years at the University of Göttingen.... A professorial robe is not necessarily a certificate of genius; there may be even at Göttingen some chairs occupied by not very remarkable men.[1]

It is amusing to see Broca explaining away, correction by correction, a reported 100-gram superiority of the brains of Germans over Frenchmen. When, despite his best efforts, Broca found some measurements placing blacks higher than whites, he decided that, after all, those measurements were of no interest. And on it goes. The "objective facts" of science turn out, over and over again, to be the cooked, massaged, finagled creations of ideologues determined to substantiate their prejudices with numbers.

In his debunking of the "data" of anthropometry, Gould follows the model set by Leon Kamin's brilliant muckraking in the byre of IQ studies,[2] but with somewhat different conclusions about the nature of scientific inquiry. Science, he argues, is a social activity, reflecting the reigning ideology of the society in which it is carried out, the political exigencies of the time, and the personal prejudices of its practitioners. Racist scientists produce racist science. It is not that they deliberately falsify nature, but that their unconscious prejudices lead them to largely unconscious biases in their methods and analyses, biases that bring them to comfortable conclusions. There are, after all, many ways of explaining observations. How are we to decide among them, except in the light of unspoken assumptions and predispositions?

Like Kamin, I am, myself, rather more harsh in my view of the matter. Scientists, like others, sometimes tell deliberate lies because they believe that small lies can serve big truths. How else are we to understand the doctored photographs discovered by Gould in the report by the American psychologist Henry Goddard on the pseudonymous Kallikak family whose good (*kalos*) and, bad (*kakkos*) branches were the living counterparts of the Rougons-Macquarts?

For his part, Sir Cyril Burt, perhaps the most influential psychologist of the twentieth century, *knew* that intelligence was almost perfectly determined by the genes and he was quite willing to make up the data to prove it to people who needed that sort of thing. (His most notorious fabrication was aimed to show that identical twins brought up separately would still be of equal "intelligence.") Burt may indeed have been, as Gould says, "a sick and tortured man" during the last years of his life, but even his biographer, Professor Hearnshaw, admits that Burt was none too scrupulous about numbers at any time.[4] Whether deliberately or not, there is no evidence that scientists are falsifying nature any less in the twentieth century than they did in the nineteenth.

By the beginning of the twentieth century, the belief that great men had big heads and great criminals big noses had pretty much disappeared from the scientific scene, although it was still part of popular consciousness. When Agatha Christie's young Tommy sees a communist trade-union agitator for the first time, he observes that the fellow

> was obviously of the very dregs of society. The low bestling brows, and the criminal jaw, the bestiality of the whole countenance, were new to the young man, though he was a type that Scotland Yard would have recognized at a glance.[5]

In place of measurements of skull and limb, biological determinist science began to measure intelligence itself. The IQ test, created by the French psychologist Alfred Binet in 1905 as a diagnostic instrument to help teachers help children, became, in the hands of its English-speaking adaptors, Henry Goddard, Lewis Terman, and Charles Spearman, an instrument for arraying everyone along a single scale of mental ability.

Much of the history of the political use of IQ testing in America, especially in helping to justify the Immigration Act of 1924, has been recounted by Kamin, who demolished the "data" purporting to show the heritability of IQ differences. Unfortunately, the story of the Cyril Burt frauds is nowhere told in its full richness. Even the summary by Kamin in the book containing his "debate" with H.J. Eysenck[6] is too brief to provide the excitement of psychology's Watergate, which had its own Woodward and Bernstein (Kamin and Oliver Gillie), its outraged denials by Burt's supporters, and its final days of capitulation in the face of the overwhelming evidence of wholesale fakery. And Gould has other fish to fry. *The Mismeasure of Man* looks beyond the politics, the data, and the frauds to address the central epistemological issue about intelligence: "Is there anything to be measured?"

IQ tests vary considerably in form and content. Some are oral, some written, some individual, some given in groups, some verbal, some purely symbolic. Most combine elements of vocabulary, numerical reasoning, analogical reasoning, and pattern recognition. Some are filled with specific and overt cultural referents: children are asked to identify characters from literature ("Who was Mr. Micawber?"); they are asked to make class judgments ("Which of the five persons below is most like a carpenter, plumber, and bricklayer? 1) postman, 2) lawyer, 3) truck driver, 4) doctor, 5) painter"); they are asked to judge socially acceptable behavior ("What should you do when you notice you will be late to school?"); they are asked to judge social stereotypes ("Which is prettier?" when given the choice between a girl with some Negroid features and another with a doll-like European face); they are asked to define obscure words (sudorific, homunculus, parterre).

Moreover, the circumstances of testing are laden with tensions. Gould, after reviewing the content of the Army classification tests of the First World War, describes at length the intimidating and alien atmosphere in which the tests were given. Complex commands were given just once, in a military style, in English to men many of whom were recent immigrants and some of whom had never before held a pencil. When Gould gave the Army Beta Test, designed for illiterates, in the prescribed style to his Harvard undergraduates, sixteen out of fifty-three got only a B and six got a C, marking borderline intelligence.

The claim is made by their supporters that IQ tests measure a single underlying innate thing, general intelligence, which itself does not develop during the lifetime of the individual, but is a cause of the individual's changing overt behavior. In the jargon of educational psychology, "fluid" intelligence becomes "crystallized" by education. Intelligence, so viewed, is not what is learned, but the ability to learn, a fixed feature immanent to different degrees in every fertilized egg.

The evidence that there is a unitary intellectual ability is that the results of different tests and of different parts of the same test are correlated with each other. Children who do well on pattern recognition tend to do well in numerical reasoning, analogical reasoning, and so on. But the claim is spurious. IQ tests, like books, are commodities that can yield immense profits for their publishers and authors if they are widely adopted by school systems. A chief selling point of new tests, as announced in their advertising, is their excellent agreement with the original Stanford-Binet test. They have been carefully cut to fit.

Moreover, the agreement of the results of various parts of the same tests has also been built into them. In order for the original Stanford-Binet test to have won credibility as an *intelligence* test, it necessarily had to order children in conformity with the *a priori* judgment of psychologists and teachers about what they thought intelligence consisted of. No one will use an "intelligence" test that gives highest marks to those children everyone "knows" to be stupid. During the construction of the tests, questions that were poorly correlated with others were dropped, since they clearly did not measure "intelligence," until a maximally consistent set was found. The claim that something real is then measured by these selected questions is a classic case of reification. It is rather like claiming, as a proof of the existence of God, that he is mentioned in all the books of the Bible.

A good deal of *The Mismeasure of Man* is taken up with a lucid explanation of the abstruse statistical method used by mental testers to extract a single dimension, *g*, that is supposed to measure general intelligence. This method, factor analysis, takes a collection of different measurements and combines them into a single weighted average, where the weights are derived from the observed correlations between the measurements. The error, as explained by Gould, is not in the arithmetic, but in the supposition that, having gone through the mathematical process, one has produced a real object,

[1] Paul Broca, *Bulletin Société d'Anthropologie* 2 (Paris, 1861), pp. 139-207 (quoted by Gould).

[2] Leon Kamin, *The Science and Politics of IQ* (Halsted Press, 1974).

[4] L.S. Hearnshaw, *Cyril Burt: Psychologist* (Cornell University Press, 1979).

[5] Agatha Christie, *The Secret Adversary* (Dodd, Mead, 1922).

[6] H.J. Eysenck versus Leon Kamin, *The Intelligence Controversy* (John Wiley, 1981). While billed as a debate, this book in fact consists of two independent summary pieces on IQ, followed by brief rejoinders. Eysenck, formerly one of Burt's strongest supporters, here casts his vote for impeachment but says it doesn't matter because the rest of the data on the heritability of IQ is so good. This has become the standard way of handling the Burt frauds, since the facts can no longer be denied.

or at least a number that characterizes one. As Gould points out, the price of gasoline is well correlated with the distance of the earth from Halley's comet, at least in recent years, but that does not mean that some numerical combination of the two values measures something real that is their common cause. Even with Gould's help, the reader may remain mystified. The very complexity of the statistical manipulation is part of the mystique of intelligence testing, validating it by making it inaccessible to nonexperts. After all, look how complicated quantum mechanics is, and you can use it to blow up the world.

Gould's view of the biological determinists is that they are doubly blinded, first, by their own racial and ethnic prejudices, and second, by what Gould calls "Burt's real error," the vulgar reductionism that leads them to reify an abstract statistical entity. Yet the analysis is somehow incomplete. With its emphasis on the racism of individual scientists, and on their epistemological naïveté, *The Mismeasure of Man* remains a curiously unpolitical and unphilosophical book. Morton, Broca, Lombroso, Goddard, Spearman, and Burt make their appearance as if from a closet, and smelling a bit of mothballs. They are "men of their time," displaying antique social prejudices which on occasion come back to haunt us in the form of "criminal chromosomes" and a brief eruption of Jensenism. Their biological determinism appears as a disarticulated cultural artifact, nasty and curious, like cannibalism, but not integrated into any structure of social relations.

Biological determinism is the conjunction of political necessity with an ideologically formed view of nature, both of which arise out of the bourgeois revolutions of the seventeenth and eighteenth centuries. These revolutions were made with the slogans, "Liberty, equality, fraternity" and "All men are created equal." They meant literally "all *men*," since women were excluded from social power, but they did not mean "*all* men," since slavery and property qualifications continued well into the nineteenth century. Still, one can hardly make a revolution with the cry, "Liberty and equality for some!" The problem for bourgeois society (and for socialist society, as well) is to reconcile the ideology of equality with the manifest inequality of status, wealth, and power, a problem that did not exist in the bad old days of *Dei Gratia*. The solution to that problem has been to put a new gloss on the idea of equality, one that distinguishes *artificial* inequalities which characterized the *ancien régime* from the *natural* inequalities which mark the meritocratic society. As the Harvard psychologist Richard Herrnstein puts it:

> The privileged classes of the past were probably not much superior biologically to the downtrodden, which is why revolution had a fair chance of success. By removing artificial barriers between classes, society has encouraged the creation of biological barriers. When people can take their natural level in society, the upper classes will, by definition, have greater capacity than the lower.[7]

[7]Richard Herrnstein, *IQ in the Meritocracy* (Atlantic/Little, Brown, 1973), p. 221.

Equality then becomes equality of opportunity, and those who fail do so because they lack intrinsic merit. But if we truly live in a meritocratic society, how do we account for the obvious passage of social power from parent to offspring? It must be that intrinsic merit is passed in the genes. The doctrine of grace is replaced by the Laws of Mendel.

The emphasis in *The Mismeasure of Man* on racism and ethnocentrism in the study of abilities is an American bias. IQ testing was widespread in France long before there were significant numbers of Algerians there, and Sir Cyril Burt's most influential educational invention, the British eleven-plus exam, long antedated the influx of West Indians and Pakistanis. Lombroso's criminal anthropology had nothing to do with race and ethnicity, but with the same *classes laborieuses, classes dangereuses* that concerned Eugène Sue. In America, race, ethnicity, and class are so confounded, and the reality of social class so firmly denied, that it is easy to lose sight of the general setting of class conflict out of which biological determinism arose. Biological determinism, both in its literary and scientific forms, is part of the legitimating ideology of our society, the solution offered to our deepest social mystery, the analgesic for our most recurrent social pain. In the words of Charles Darwin, quoted on the title page of *The Mismeasure of Man*, "If the misery of our poor be caused not by the laws of nature, but by our institutions, great is our sin."

The disarticulation of social relations, the alienation of man from land, the creation of what C.B. MacPherson calls "possessive individualism"[8] began in the fourteenth century with the market-town corporations, and slowly became the dominant mode of our society. They brought with them an alienation and objectification of nature. The natural world was seen less and less as an organic unity, an extension of the Mind of God. Like the body social, the body natural came to be an assemblage of elements, interacting with each other, yet each possessing its intrinsic and independent properties. No longer do we "murder to dissect," but rather do we expect to discover the true nature of the world by taking it to bits, the bits of which it is truly made. In this sense Descartes was as much a founding father of our society as Paine or Jefferson.

It is easy to criticize the vulgar materialism of Spearman and Burt, who thought of intelligence sometimes as a form of elementary energy, sometimes as a liquid that could be crystallized, but it is not clear that anything else could be expected from them. The reification of intelligence by mental testers may be an error, but it is an error that is deeply built into the atomistic system of Cartesian explanation that characterizes all of our natural science. It is not easy, given the analytic mode of science, to replace the clockwork mind with something less silly. Updating the metaphor by changing clocks into computers has got us nowhere. The wholesale rejection of analysis in favor of an obscurantist holism has been worse. Imprisoned by our Cartesianism, we do not know how to think about thinking. □

[8]C.B. MacPherson, *The Political Theory of Possessive Individualism* (Oxford University Press, 1962).

MINI COMMUNICATIONS

A Comment on the Selective Advantage of Male Subordination to Females in Primates ("Female-Dominance")

Clara B. Jones
Museum of Comparative Zoology
Harvard University

Hrdy (1981) reviews those species displaying female-dominance and observes that this unusual trait may occur in three conditions: (1) where the reproductive output by each member of a monogamous pair is equivalent (implying that a male in such a case would have little to gain by dominating his mate); (2) where seasonal breeding corresponds with intense male-male competition for mates, leading to the "conservation" of energy at other times of the year, and (3) where the trait is "primitive." While it is probably true that female-dominance is a "phylogenetic trait" among the lemurs (A. Jolly, personal communication), it is not clear that the equivalency of (genetic) interests between mates ever obtains to compensate asymmetries between them (see Trivers, 1972; Power, 1980; Alatalo, et al., 1981; Payne, 1979; Gladstone, 1979; Wade, 1979; Kleiman, 1979) or that it could benefit males to be energetically conservative (see Schoener, 1971; Downhower and Armitage, 1971) relative to females in the same conditions. It is my purpose in this note to suggest a simpler, testable hypothesis that will collapse the three categories of Hrdy into a more general construct.

Relations between the sexes in primates are generally discussed within the conceptual framework of differential rank or status (e.g., Hausfater, 1975), and characteristic asymmetries (e.g., size or fighting ability) among individuals apparently lead to social hierarchies in most group-living animals (see Wilson, 1975). Agonistic relations between sexes may represent a form of intersexual competition that can be analyzed in terms of Darwinian "sexual selection" (the differential reproduction of genotypes which accrue large quantities of and/or high quality males; see Emlen, 1973). As such, females (or some sub-class of females) may be viewed as a limiting resource for which males compete, and males who determine the yield (in offspring) of a significant proportion of females each generation are most successful from a Darwinian perspective. It is in this sense that males are expected to exploit females and to dominate them socially.

There seems to be general agreement among students of primate societies that males are almost always dominant to females (e.g., Alexander, et al., 1979). Occasionally, however, females may command males in a consistent pattern that appears in mechanism and function to compare with the more common cases of male dominance (see Jolly, 1966). Hrdy's recent review (1981) indicates that the display of female-dominance may be more variable within and between seasons than patterns of male dominance and that this variation may reflect synchronous reproductive states among females and their effects upon male-male and male-female relations. Despite distinctions that may remain to be understood among hierarchical patterns, Hrdy (1981) has most recently stressed that the phenomenon of female-dominance supports the conclusion that "primates are not totally locked into a pattern of male dominance."

In an atempt to gain some insight into the characteristics of female-dominance, I studied a captive pair of Lemur fulvus fulvus (E. Geoffroy 1812) at Riverbanks Zoological Park, Columbia, South Carolina for approximately eleven crepuscular and daylight hours of discontinuous observation in April, 1980 (see Harrington, 1975 for certain other aspects of this subspecies' behavior). The female displaced the male 64 percent of the time (47 times out of 74). Comparing proportional male and female supplantations with a "goodness of fit" design, significant deviations resulted from a 50:50 expectation ($p<0.01$, $\chi^2=7.84$, $df=1$). I was particularly interested to note that the female never used aggressive (i.e., escalated) behavior to supplant the male, although he often applied cuffing or biting to displace her. Thus, the female in my brief study dominated the male through the outcomes of "ritualized" (e.g., tongue-flicking) and other non-damaging behavior (e.g., head-on approach with eye contact) rather than the expression of higher rates or likelihoods of escalated response, suggesting that the nature of intersexual interaction in conditions of female-dominance may differ qualitatively from the more common pattern. In particular, females may dominate males because of the latter's "aggressive restraint."

Nevertheless, the essential question remains: Why might males restrain their agonistic responses to females? Male interests will usually dominate female interests since male reproductive success will be limited only by the number of mates which each can control, while female reproductive success is limited by the amount of energy extractible from the environment that can be converted into offspring (Otte, 1974). Male selfishness, however, can be limited by the deleterious effects of male behavior upon the reproductive

success of individual females (Downhower and Armitage, 1971). In these circumstances, males may forego behavior theoretically optimal to their own sex in favor of behavior optimal to females (Parker, 1974). While it seems clear that male selfishness may often benefit the reproductive success of females (e.g., Orians, 1969), a male may be expected to display selfishness even where its deleterious effects upon individual females are significant if his reproductive success is thereby enhanced (Downhower and Armitage, 1971). The occasional occurrence of female-dominance, however, demonstrates that there are certain environmental mosaics that favor males who submit, on average, to their female conspecifics. By inference, then, I propose the hypothesis that males will adopt subordination to females where males who dominate females leave fewer offspring, on average, than males who do not.

Investigations of these particular conditions may provide an understanding of the social and non-social factors that minimize reproductive benefits to males from agonistic intersexual behavior. Hrdy's (1981) analysis shows that temporal constraints upon breeding may represent one such set of factors.

I thank Alison Jolly for discussion of primate hierarchical patterns and encouragement to advertise my observations of Lemur fulvus fulvus.

Alatalo, R.V., Carlson, A., Lundberg, A. and Ulfstrand, S. The conflict between male polygamy and female monogamy: The case of the pied flycatcher Ficedula hypoleuca. Am. Nat. 117:738-753, 1981.

Alexander, R.D., Hoogland, J.L., Howard, R.D., Noonan, K.M. and Sherman, P.W. Sexual dimorphisms and breeding systems in pinnipeds, ungulates, primates and humans. In: Evolutionary Biology and Human Social Behavior: An Anthropological Perspective, edited by N.A. Chagnon and W. Irons, pp. 402-435. North Scituate, Mass.: Duxbury Press, 1979.

Downhower, J. and Armitage, K. The yellow-bellied marmot and the evolution of polygamy. Am. Nat. 105:355-370, 1971.

Emlen, J.M. Ecology: An Evolutionary Aproach. Reading, Mass.: Addison-Wesley, 1973.

Gladstone, D.E. Promiscuity in monogamous colonial birds. Am. Nat. 114:545-557, 1979.

Harrington, J.E. Field observations of social behavior of Lemur fulvus fulvus E. Geoffroy 1812. In: Lemur Biology, edited by I. Tattersall and R.W. Sussman, pp. 259-279. New York: Plenum Press, 1975.

Hausfater, G. Dominance and Reproduction in Baboons (Papio cynocephalus. Contributions to Primatology, Vol. 7, Basel: S. Karger, 1975.

Hrdy, S.B. The Woman That Never Evolved. Cambridge: Harvard Press, 1981.

Jolly, A. Lemur Behavior. Chicago: Chicago University Press, 1966.

Kleiman, D. Parent-offspring conflict and sibling competition in a monogamous primate. Am. Nat. 114:753-759, 1979.

Orians, G. On the evolution of mating systems in birds and mammals. Am. Nat. 103:589-603.

Otte, D. Effects and functions in the evolution of signaling systems. Ann. Rev. Ecol. Syst., 385-417, 1974.

Parker, G. Courtship persistence and female guarding as male time-investment strategies. Behaviour, 48, 157-184, 1974.

Payne, R.B. Sexual selection and intersexual differences in variance of breeding success. Am. Nat. 114:447-452, 1979.

Power, H.W. On bluebird cuckoldry, human adultery, and the caricaturing of sociobiology. Am. Nat. 116:705-709, 1980.

Schoener, T.W. Theory of feeding strategies. Ann. Rev. Ecol. Syst. 369-404, 1971.

Trivers, R. Parental investment and sexual selection. In: Sexual Selection and the Descent of Man, edited by B. Campbell, pp. 136-179. Chicago: Aldine, 1972.

Wade, M.J. Sexual selection and variance in reproductive success. Am. Nat. 114:742-746, 1979.

Wilson, E.O. Sociobiology: The New Synthesis. Cambridge: Harvard Press, 1975.

RECENT LITERATURE

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

Books:

Boice, R. Ethological Psychology. Monterey, Calif.: Brooks/Cole, 1981.

Chiarelli, A.B. and Corruccini, R.S. Primate Behavior and Sociobiology. Berlin, New York: Springer-Verlag, 1981.

Chiarelli, A.B. and Corruccini, R.S. Primate Evolutionary Biology. Berlin, New York: Springer-Verlag, 1981.

Crabtree, J.M. and Moyer, K.E. (Eds) Bibliography of Aggressive Behavior: A Reader's Guide to the Research Literature, Vol. I. New York: Alan R. Liss, 1981.

DeCatanzaro, D. Suicide and Self-Damaging Behavior: A Sociobiological Perspective. New York: Academic Press, 1981.

Denny, M.R. (Ed) Comparative Psychology: An Evolutionary Analysis of Animal Behavior. New York: Wiley, 1980.

Dixson, A.F. The Natural History of the Gorilla. New York: Columbia University Press, 1981.

Eisenberg, J.F. The Mammalian Radiations: An Analysis of Trends in Evolution, Adaptation, and Behavior. Chicago: University of Chicago Press, 1981.

Farber, B. Conceptions of Kinship. New York: Elsevier North Holland, 1981.

Fogle, B. Interrelations Between People and Pets. Springfield, Ill.: Charles C. Thomas, 1981.

Hamburg, D.A. and Trudeau, M.B. (Eds) Biobehavioral Aspects of Aggression. New York: Alan R. Liss, 1981.

Hopper, A.F. and Hart, N.H. (Eds) Foundations of Animal Development. Fairlawn, N.J.: Oxford University Press, 1980.

Immelman, K. Introduction to Ethology. New York: Plenum, 1980.

Immelman, K., Barlow, G.W., Berkeley, L.P. and Main, M.B. Behavioral Development. New York: Cambridge University Press, 1981.

Jolly, A. A World Like Our Own: Man and Nature in Madagascar. New Haven: Yale University Press, 1980.

Kellerman, H. Group Cohesion: Theoretical and Clinical Perspectives. New York: Grune and Stratton, 1981.

Krebs, J.R. and Davies, N.B. (Eds) Behavioural Ecology: An Evolutionary Approach. Sunderland, Mass.: Sinauer Associates, 1978.

Krebs, J.R. and Davies, N.B. (Eds) An Introduction to Behavioral Ecology. Sunderland, Mass.: Sinauer Associates, 1981.

Maple, T.L. and Hoff, M.P. Gorilla Behavior. New York: Van Nostrand Reinhold, 1981.

Midgley, M. Beast and Man: The Roots of Human Nature. New York: New American Library, 1980.

Reynolds, P.C. On the Evolution of Behavior: The Argument from Animals to Man. Berkeley: University of California Press, 1981.

van den Berghe, P.L. The Ethnic Phenomenon. New York: Elsevier North Holland, 1981.

von Cranach, M., Foppa, K., Lepenies, W. and Ploog, D. (Eds) Human Ethology: Claims and Limits of a New Discipline. Cambridge, England: Cambridge University Press, 1979.

Wilson, E.O. Sociobiology: The Abridged Edition. Cambridge, Mass.: Harvard University Press, 1980.

Wilson, P.J. Man, The Promising Primate: The Conditions of Human Evolution. New Haven: Yale University Press, 1980.

Articles:

Beckstrom, J.H. Sociobiology and interstate wealth transfers. Northwestern University Law Review, 1981, 76, 216-270.

Boesch, C. and Boesch, H. Sex differences in the use of natural hammers by wild chimpanzees: A preliminary report. Journal of Human Evolution, 1981, 10, 585-593.

Boucher, J.D. and Brandt, M.E. Judgment of emotion: American and Malay antecedents. Journal of Cross-Cultural Psychology, 1981, 12, 272-283.

Crockenberg, S.B. Infant irritability, mother responsiveness, and social support influences on the security of infant-mother attachment. Child Development, 1981, 52, 857-865.

DeNicola, D.R. Sociobiology and religion: A discussion of the issues. Zygon, 1980, 15, 407-423.

Engstrom, F.M., Roche, A.F. and Mukherjee, D. Differences between arm span and stature in white children. Journal of Adolescent Health Care, 1981, 2, 19-22.

Goldenthal, R., Johnston, R.E. and Kraut, R.E. Smiling, appeasement, and the silent bared-teeth display. Ethology and Sociobiology, 1981, 2, 127-134.

Goody, J.R., Duly, C.J., Beeson, I. and Harrison, G. Implicit sex preferences: A comparative study. Journal of Biosocial Science, 1981, 13, 455-466.

Gottman, J.M. and Ringland, J.T. The analysis of dominance and bidirectionality in social development. Child Development, 1981, 52, 393-412.

Hand, J.L. Sociobiological implications of unusual sexual behaviors of gulls: The genotype/behavioral phenotype problem. Ethology and Sociobiology, 1981, 2, 135-146.

House, A.E., House, B.J. and Campbell, M.B. Measures of interobserver agreement: Calculation formulas and distribution effects. Journal of Behavioral Assessment, 1981, 3, 37-58.

Hughes, A.L. female infanticide: Sex ratio manipulation in humans. Ethology and Sociobiology, 1981, 2, 109-112.

Itzkowitz, M. and Nyby, J. A parental sex difference in child custody suit. Ethology and Sociobiology, 1981, 2, 147-150.

Kimble, C.E. and Yoshikawa, J.C. Vocal and verbal assertiveness in same-sex and mixed-sex groups. Journal of Personality and Social Psychology, 1981, 40, 1047-1054.

LoPreato, J. Toward a theory of genuine altruism in Homo sapiens. Ethology and Sociobiology, 1981, 2, 113-126.

Mackie, A.J. Attachment theory: Its relevance to the therapeutic alliance. The British Journal of Medical Psychology, 1981, 54, 203-212.

Main, M. and Weston, D.R. The quality of the toddler's relationship to mother and to father: Related to conflict behavior and the readiness to establish new relationships. Child Development, 1981, 52, 932-940.

Matthews, K.A., Batson, C.D., Horn, J. and Rosenman, R.H. Principles in his nature which interest him in the fortune of others...: The heritability of empathic concern for others. Journal of Personality, 1981, 49, 237-247.

Peel, R.A. Natural selection, social evolution and economic strategy. Journal of Biosocial Science, 1981, 13, 377-390.

Polsky, R.H. and McGuire, M.T. Naturalistic observations of pathological behavior in hospitalized psychiatric patients. Journal of Behavioral Assessment, 1981, 3, 59-82.

Porter, R.H. and Laney, M.D. Attachment theory and the concept of inclusive fitness. Merrill-Palmer Quarterly of Behavior and Development, 1980, 26, 35-51.

Porter, R.H. and Moore, J.D. Human kin recognition by olfactory cues. Physiology and Behavior, 1981, 27, 493-495.

Rosen, A.J., Sussman, S., Mueser, K.T., Lyons, J.S. and Davis, J.M. Behavioral assessment of psychiatric inpatients and normal controls across different environmental contexts. Journal of Behavioral Assessment, 1981, 3, 25-36.

Smith, J.B., Madsen, C.H. and Cipani, E.C. The effects of observational session length, method of recording, and frequency of teacher behavior on reliability and accuracy of observational data. Behavior Therapy, 1981, 2, 565-569.

Stein, M. A biopsychosocial approach to immune function and medical disorders. The Psychiatric Clinics of North America, 1981, 4, 203-222.

Taylor, R.B. and Ferguson, G. Solitude and intimacy: Linking territoriality and privacy experiences. Journal of Nonverbal Behavior, 1980, 4, 227-239.

Wilcox, S. and Katz, S. The ecological approach to development: An alternaive to cognitivism. Journal of Experimental Child Psychology, 1981, 32, 247-263.

Williams, B.J. A critical review of models in sociobiology. Annual Review of Anthropology, 1981, 10, 163-192.

Papers presented at the symposium "Human Sociobiology: New Research and Theory," November 11-14, Northwestern University. Organized by Napoleon A. Chagnon and William Irons, Dept. of Anthropology, Northwestern University, Evanston IL 60201.

Berte, N. (Northwestern) Some evolutionary implications of K'ekchi' labor transactions.

Bugos, P.E. (Northwestern) Genealogical kinship as a predictor of social behavior among the Ayoreo Indians of Bolivia.

Campbell, D. (Syracuse) Alternative paths to ultrasociality in humans.

Chagnon, N.A. (Northwestern) Kindemcom: The fourth style in the study of kinship.

Daly, M. and Wilson, M. (McMaster) Homocide and domestic conflict in urban America: The Detroit case.

Flinn, M. (Northwestern) An ethological analysis of maternal kin biases in a rural Trinidadian village.

Fredlund, E. (Penn State) Incest and relatedness in the Shitari-Yanomamo population.

Freedman, D. (Chicago) Tribal fissioning on the Australian continent: A reconstruction.

Hames, R. (Nebraska) Exchange balance and relatedness in Ye'kwana gardening.

Hawkes, K. (Utah) How much is enough? 'Hunters' and 'limited needs.'

Hewitt, J. (Northwestern) Optimal foraging approaches to the archaelogical record.

Hurd, J. (Penn State) Sex differences in mate choice among the Nebraska Amish of central Pennsylvania.

Irons, W. (Northwestern) Testing Lack's theory of optimal family size.

McCommon, C. (Penn State) An analysis of mating strategies among black Caib females.

Melancon, T. (Penn State) Sexual selection in age-structured populations.

Sade, D., Cheverud, J. and Chepko-Sade, D. (Northwestern) Are monkey societies products of group selection.

Smith, E.A. (Washington) Behavioral ecology and human sociobiology.

BULLETIN BOARD

Human Ethology Abstracts IV, edited by Larry Stettner and Karen Olson is available for $3.00 postpaid from ASMER, P.O. Box 57, Orangeburg, New York 10962. A reminder to everyone -- please send abstracts (150 words, APA format) this year to Wade Mackey for inclusion in HEA V. Wade's address is: Division of Social Sciences, Iowa Wesleyan College, P.O. Box 369, Mt. Pleasant IA 52641.

The Human Biology Council is an international non-profit organization formed in 1974 and currently having 450 members. Its official journal, Human Biology, is published quarterly and contains research reports, review articles, and book reviews. The major focus is on problem-oriented and theoretical approaches to variation in human populations and individuals. The following subject areas in human biology are common among members' interests: body composition, physique; bone morphology, dentition; demographic factors, including fertility, mortality, migration; genetic and familiar mechanisms; growth, maturation, aging, secular trends; physiology of organ systems; socio-cultural influences; and temperature, altitude, disease, nutrition, and other environmental

factors. For membership information write to: Roger M. Siervogel, Fels Research Institute, Wright State University School of Medicine, Yellow Springs OH 45387.

Animal Behaviour Abstracts provides coverage of the applied aspects of ethology. In compiling each quarterly issue, over 5500 international research journals, books and reports are regularly monitored. Each issue contains approximately 1400 abstracts. For information and a free sample issue, write to American Behaviour Abstracts at: P.O. Box 1, Eynsham, Oxford OX8 1JJ, England; or at 1911 Jefferson Davis Highway, Arlington VA 22202, USA.

Behavioral Ecology and Sociobiology publishes (quarterly) original contributions and short communications dealing with quantitative studies and experimental analysis of animal behavior on the level of the individual and the population. It also contains articles on the functions, mechanisms, and evolution of ecological adaptations of behavior. Editor is H. Markl, University of Constance, Federal Republic of Germany. Publisher is Springer-Verlag.

Environment and Behavior is an interdisciplinary journal that publishes empirical and theoretical work on the influence of the physical environment on human behavior at the individual, group, and institutional levels. For example: theory of architecture/behavior relations; new research methods; evaluations of buildings or urban settings; beliefs, meanings, values, and attitudes of individuals or groups concerning various building types; studies of planning, policy, or political action. The Associate Editor for Architecture and Environmental Design Research is Gary T. Moore of the School of Architecture and Urban Planning, University of Wisconsin-Milwaukee. Reviews are conducted by an editorial review board comprised of leading researchers, scholars, and practitioners. For information, write to the General Editors: Robert B. Bechtel and William H. Ittelson, Environmental Psychology Program, University of Arizona, Tucson AZ 85712.

A new Ph.D. program in architecture with a concentration in Environment-behavior studies will begin in Fall, 1982 at the University of Wisconsin in Milwaukee. Environment-behavior studies in architecture are concerned with the mutual interrelationships between people and the environment and with applications enhancing the quality of life through environmental policy, planning, design, and education. The school offered its first master's degree in architecture with an emphasis in environment-behavior studies in 1975. For information write to Uriel Cohen, Dept. of Architecture, School of Architecture and Urban Planning, University of

Wisconsin, Milwaukee WI 53201.

Aggressive Behavior, the official journal of the International Society for Research on Aggression, is published quarterly. It is a multidisciplinary journal with an editorial board drawn from a broad range of academic fields. It is devoted to the empirical and theoretical analysis of conflict and the scientific understanding of aggression in humans and animals. Recent papers have examined such diverse topics as brain mechanisms in aggression, terrorism, rape, laboratory studies of children and adults, animal studies of natural and experimentally induced aggressive behavior, and analyses of delinquency in streets and schools. Each issue contains a comprehensive international bibliography of literature on the field of aggressive behavior. Editor-in-Chief is Ronald Baenninger, Dept. of Psychology, Temple Universiy, Philadelphia PA 19122. For membership information write to Robert J. Blanchard, Dept. of Psychology, University of Hawaii, Honolulu HI 96811.

Roger Sperry, professor of psychobiology at the California Institute of Technology, recently received the Nobel Prize in Physiology and Medicine for his now classic "split-brain" experiments which led to the discovery of the distinct functions of each hemisphere of the brain. Sperry shares half of the $180,000 total prize with the Harvard team of David H. Hubel and Torsten N. Wiesel, who won for their discoveries of the brain's mechanisms for processing visual information.

Harald G. Wallbott, Dept. of Psychology, University of Gieben, West Germany, writes in the Winter issue of Video-Informationen (5(2):14-17, 1981) that when using a hidden camera, ethical issues as well as legal problems concerning protection of the rights of a person have to be considered. Written consent should include all information necessary for the subject or his legal representative to be able to evaluate what will be done with the tapes now and in the future. If it is necessary to record subjects without their prior knowledge, (since many records become worthless once subjects are aware of the fact that they are being recorded), then they must be permitted to view the tapes, decide if they should be erased, and either agree or refuse to sign the written consent. If a consent is refused, the tapes have to be erased immediately. In any case, records should not be used in a way that could prove harmful, insulting, or discriminating to subjects, even if they will never view the record or find out what has been done with it. References provided by Wallbott are:

American Psychologicl Association ad hoc Commitee on Ethical Standards in Psychological Research. Ethical Principles in the Conduct of Research with Human Participants. Washington: American Psychological Association, 1973.

Canada Council. Report of the Consultative Group on Ethics. Ottawa: The Canada Council, 1977.

Lavender, J., Davis, M., Graber, E. Film/video research recordings: Ethical issues. Kinesis, 1979, 1, 9-20.

Rosenbaum, M. The issues of privacy and privileged communication. In M.M. Berger (Ed.) Videotape Techniques in Psychiatric Training and Treatment. New York: Bruner/Mazel, pp. 198-206, 1970.

Wolstenholme, G., O'Connor, M. (Eds.) Ethics in Medical Progress. Boston: Little, Brown and Co., 1966.

UPCOMING MEETINGS

Northwest Scientific Association Annual Conference. March 17-19, 1982 in Walla Walla College, College Place, Washington. Papers are invited in Evolutionary Biology, Science Education, Social Science, Zoology, and for a Symposium on Biotelemetry and Radio Tracking. For information, contact: Clyde L. Webster, Chemistry Dept., Walla Walla College, College Place WA 99324.

International Conference on Infant Studies. March 18-21, 1982 in Austin, Texas. For information, contact: Tiffany Field, Mailman Center for Child Development, Dept. of Pediatrics, University of Miami Medical School, P.O. Box 016820, Miami FL 33101.

IXth World Congress of Social Psychiatry. July 5-9, 1982 in Paris. Contact: PMU 9e Congres Mondial de Psychiatrie Sociale, BP 246, 92205 Neuilly-sur-Seine, France.

International Association for Cross-cultural Psychology. July 19-23, 1982 in Aberdeen. Contact: J.B. Deregowski, Dept. of Psychology, King's College, Old Aberdeen AB9 2UB, Great Britain.

XXth International Congress of Applied Psychology. July 25-31, 1982 in Edinburgh. Contact: Centre for Industrial Consultancy and Liaison, University of Edinburgh, 16 George Square, Edinburgh EH8 9LD, Great Britain.

Seminar Counseling Across Cultures. August 3-16, 1982 in Honolulu. Contact: The Institute of Behavioral Sciences, 250 Ward Ave., Suite 226, Honolulu, Hawaii 96814.

Xth World Congress of Sociology. August 23-28, 1982 in Mexico City. Contact: Fritz Schutze, Universitat Kassel, Fachbereich 4, Heinrich Plett-Str. 40, D-3500 Kassel, West Germany.

10th Annual Meeting Canadian Assn. for Physical Anthropology pour l'Anthropologie Physique au Canada. November 18-21, 1982 in Guelph, Ontario. For information write to: Susan Pfeiffer, School of Human Biology, University of Guelph, Guelph, Ontario, Canada, N1G 2W1.

International Conference on Social Psychology and Language. July 18-22, 1983 in Bristol. Contact: Peter Robinson, University of Bristol, School of Education, 35 Berkeley Square, Bristol BS8 1JA, Great Britain.

Meeting Reminders

International Human Ethology Meeting. August 8-13, 1982 at the Colony Square Hotel, Peachtree and 14th Streets, Atlanta, Georgia. Held conjointly with the International Primatological Society, and the American Society of Primatologists. All members of ISHE should have received registration materials by now. If you have not (or for information about the meeting in general), contact: Cathy Yarbrough, Congress Office, Yerkes Regional Primate Research Center, Emory University, Atlanta GA 30322 (404-329-7709).

Deadlines are:

Advance Registration: March 31, 1982
Student Dormitory Registration: June 20, 1982
Colony Square Hotel Registration: June 30, 1982

For information about ISHE contributions to meetings, contact: Ron Weigel, Human Ethology Laboratory, Neuropsychiatric Institute, UCLA, Los Angeles CA 90024.

Animal Behavior Society Meeting. August 15-20, 1982 at the University of Minnesota, Duluth. Deadline for receipt of abstracts is April 26, 1982. For information contact Terry Christenson, Dept. of Psychology, Tulane University, New Orleans LA 70118. (504-865-5331)