Letters

Biomedical Research: Ethics and Rights

Questions should be raised about biomedical research. The aim is to protect people. It is rarely considered necessary to protect the researcher, who is generally seen as the aggressor.

Two News and Comment articles by Barbara J. Culliton suggest to me that we need to begin worrying about the rights of the researcher. The first (22 Nov. 1974, p. 715) was entitled: "Patients' rights: Harvard is site of battle over X and Y chromosomes"; the second (27 June, p. 1284) reported the outcome: "Harvard researcher under fire stops newborn screening," the gist being that Stanley Walzer and Park Gerald gave way under pressure initially generated by a group informally led by Jonathan Beckwith of Harvard and Jonathan King of the Massachusetts Institute of Technology.

If research is to be halted on ethical grounds—and sometimes it should be—it should not be done by adversary proceedings in the media. Appropriate means exist. These include the funding agency (which approved the screening project), the faculty (which voted by about 200 to 30 in favor of continuing the project), and committees (such as that headed by Dana Farnsworth, which concluded that Walzer's work should continue).

Scientists deserve some protection from public adversary proceedings undertaken by other scientists acting without regard for their colleagues' rights to something approaching due process. Academic institutions with an interest in research have a clear and present need to formulate policies to guard the rights of the researcher.

FREDERICK HECHT
Child Development and Rehabilitation Center, University of Oregon Health Sciences Center, Portland 97201

The article on the cessation of the Harvard XYY screening program headed by Walzer and Gerald raises an important issue that could have unfortunate ramifications for future biomedical research.

Many features of contemporary society and its institutions give just cause for fears about disregard for the right and dignity of individuals and for their social and psychological needs. In this regard, Beckwith and King, as well as others, may have helped alert the academic community to such risks in the XYY screening program. It is a great satisfaction, therefore, to learn of the thorough and competent manner with which these matters were treated by the Benson and Farnsworth committees and of the overwhelming vote of confidence given by the Harvard Medical School faculty to Walzer and Gerald's research project.

On the other hand, Beckwith and King appear to have overlooked numerous scientific and humanistic aspects of this research. For example, in infantile autism the ratio of boys to girls afflicted is approximately 4 to 1, and chromosome screening may offer a potential for early detection, perhaps correlated also with parental chromosome patterns. The National Society for Austistic Children is an organization of parents and professionals devoted to the social and psychological needs of these children and to making their path through life as happy and useful as possible. In this organization there is universal support for research which seeks to test links between behavior and genetic parameters. It is estimated that the infantile autism syndrome may be present in 4 of every 10,000 births, and currently there are 80,000 afflicted children in the United States. Other similar organizations represent far larger populations of children who possess both more and less severe mental handicaps.

Biological and physical science is just now making a very small beginning toward understanding of neurological processes, and it is often at this stage that a few seminal pieces of research have great impact on the rate at which knowledge is acquired. It is not unreasonable that in the future the lives of hundreds of thousands of people might have been beneficially influenced by the notably promising Walzer-Gerald research program.

LELAND C. ALLEN*
Department of Chemistry, Princeton University, Princeton, New Jersey 08540

*The writer is founder and former president of the Central New Jersey Chapter of the National Society for Austistic Children.

Limits to Understanding?

Gunther Stent, in his article "Limits to the scientific understanding of man" (21 Mar., p.1052), thoughtfully suggests that there are impassable barriers to a full scientific comprehension of human behavior. He tells us that any explanation of "the complex phenomenon of man" is ultimately limited by the nature of the irreducible, inaccessible structures of the mind. It is certainly plausible that any attempt of the human nervous system to analyze itself *completely* presents some inherent limits. However, many of the barriers to the scientific explanation of our species noted by Stent might be products of his own assumptions and analytic tools, rather than being deducible from universal structures of the mind.

One could certainly agree with Stent that many structuralist theories (such as those propounded by Levi-Strauss) tend to be unverifiable; but here the structures are phantasms—neo-Platonic ideal forms with no material referent. When attempts are made to reduce "deep structures" to biobehavioral mechanisms (*1*), a great many of the epistemological problems raised by Stent obligingly vanish (along with the deadening weight of Cartesian dualism).

For structuralist anthropology in particular, we need to ask why inaccessible and unverifiable structures deduced from linguistic phenomena (myths, rules of kinship, and so forth) should be taken as the basis of "all human customs and institutions." Indeed, the structuralist view of "mind" presents insoluble problems for scientific explanation, but these problems are greatly ameliorated by viewing minds as neurophysiological process, and as only one factor in an equation that includes evolutionary history and a continual dialectic with the environment.

ERIC A. SMITH
Department of Anthropology, Cornell University, Ithaca, New York 14850

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1. C. D. Laughlin and E. G. d'Aquili, *Biogenetic Structuralism* (Columbia Univ. Press, New York, 1974); E. H. Lenneberg, *Biological Foundations of Language* (Wiley, New York, 1967).

A caveat is indicated for Stent's pessimistic essay. First, he seems to be following the conventional conditioned clichés of professional philosophers. Second, he gets into semantic traps in so doing. Third, he is conveniently sketchy in his neurophysiological surmises. Fourth, he seems to discount historical factors in the development of scientific endeavor. And, fifth, he seems not to realize that "Occam's razor" remains a useful tool in logical analysis.

Philosophy and science cannot easily be harmonized until there is agreement between philosophers and scientists on the meaning of the symbols they may use in trying to communicate with each other. Thus, "mind," as a philosophical cliché, has little meaning for neurologists or neurophysiologists who are concerned with brains. Similarly, "man" is a semantic trap for scientists who are concerned with people. In the scientific study of the structures and functions of brains, it is not necessary to postulate "mind" any more than, in the scientific study of heat, was it necessary to postulate "phlogiston."

Stent wisely notes adaptive survival factors in the evolution of brains, and he admits that brains may function as whole organs. That ontogeny recapitulates phylogeny is generally accepted ever since E. H. Haeckel demonstrated it. The ancient postulate of "soul" and the Cartesian hypothesis of the duality of "mind" and matter were outmoded by mid-19th-century science, but conditioned vestiges hang on, especially among philosophers. "Self" is a term of recognition of one's individuality, and seems to be acquired in one's early infancy.

The general materialistic slant of biology was given in 1847 by the manifesto of those young Berliners, Carl Ludwig, Hermann von Helmholtz, and Emile DuBois-Reymond, to the effect that all living processes, including consciousness, may be explained in terms of physics and chemistry. Current biological scientific effort proceeds rather successfully according to this theory. Further reduction goes to number, probability, and statistical induction of generalities from a multitude of particulare. These are epistemological factors in the biological as well as in the physical sciences. Far from being "innate" features of hypothetical "minds," they are characteristics of the universe in general, of which individual humans and their respective brains are a part. But it does take brains to comprehend them.

Our brains seem to have evolved in one-to-one correspondence to what is outside of them. Time and space are characteristics of what is outside of brains, and brains seem to have evolved in correspondence with these characteristics. So with other presumed "innate" properties of brains. That events occur sequentially all over the universe, including within brains, is no reason, as Hume showed, to postulate "causality" in relation to them. The principles of thermodynamics inexorably point "Time's Arrow," in Harold Blum's pleasant phrase.

Stent's essay is provocative and may help bring about further discussion between philosophers and scientists. To be meaningful, such discussions must first remove semantic traps which may ensnare both philosophers and scientists. Science proceeds toward independently verifiable information (the "truth") about ourselves and our environment, with the realization that this information is tentative, and subject to revision as more or better evidence is obtained. It also accepts the dictum of Edwin Grant Conklin, president of the AAAS in 1936, that unwelcome "truth" is better than cherished error. This is a value judgment, but it aids scientists to move along—to what limits of understanding of ourselves and our environment, who can say?

CHAUNCEY D. LEAKE
University of California, San Francisco 94143

In proposing a limit to the scientific understanding of man, Stent may be mistaking structural for scientific understanding. The stimulus-response connectionism of earlier psychological learning theories (*1*) and some later discoveries about the functional architecture of the visual system (*2*) do point toward a structural theory of brain functions and is exemplified in some current theories (*3*). The difficulty arises when structuralism is pushed to the neurological limit of recognition by single cells. Such a scheme on the human scale is implausible because of the inadequate information capacity and inherent rigidity. To account for how the operation of the nervous system transcends the limits of its components, Stent suggests we must attribute to the structure something more—an incorporeal "self" or homunculus, which lies beyond scientific understanding. While philosophical replies to structuralism and discussions of the whole-parts question are numerous (*4–6*), I wish to point out a possible conceptual block and mention some recent studies that bear on the question of the limit proposed by Stent.

The problem with the single cell homunculus is not the bugaboo of explanation via infinite regression, but the constraint on an approach to understanding which results from assuming that explanation necessarily involves reduction of the problem to a fixed locus. Consider an alternative to the "grandmother" cell whereby recognition is, as in Fiegl's argument (*4*), an array of activity across hundreds of cells. Such a system would be more amenable to handling, as variations built on a basic pattern, the many facets of Grandmother—an elderly person, a close relative, serves Thanksgiving dinner, won't let you play with the pump, and so forth. The gain in information potential is readily demonstrated by assuming that ten cells are involved in "person recognition." Functioning as individual units, they can "recognize" grandmother and nine other persons. Functioning as an array they can achieve 2^{10} unique patterns or recognition of 1024 persons by each cell being either active or not. If meaningfulness is further defined by the temporal sequence of activity in the array, these ten cells could encode 10! or some 4 million persons. While the information capacity of such arrays is most easily estimated with binary arithmetic, this does not mean that the memory role of each cell cannot function like a word rather than a bit in a computer memory. If we assume some 6 million foveal pathways to mediate visual acuity, the potential of arrays become astronomical—yet necessary if we consider the number of image details involved in perception.

Suggestions that perception resides in a pattern of cell activity are no longer confined to the isomorphic field effects proposed in Gestalt theory (*5*). Since the discovery that human spatial sensitivity is mediated by visual pathways selectively sensitive to spatial frequency (*7*), concepts of how cortical responses are synthesized to achieve figure detection involving cells with localized element sensitivity (*8*) have been replaced by concepts involving cells with relatively nonlocal sensitivity constrainted mainly in the frequency domain (*9*). While the former approach lent itself readily to the idea of synthesis in convergent branching (yielding "grandmother," "grandfather," and so forth, cells), in the latter, image details are maintained only as the simultaneous response of many cells. Both statistical (*10*) and interferometric (*11*) models have been proposed to account for how the responses of large numbers of cells may be utilized in perception and learning. Some recent sensory evidence indicates that the frequency processing of images may also extend into the temporal domain (*12*) and thereby could mediate perception of change and movement in nonlocal ways similar to spatial processing (*13*).

The organization of the components of the central nervous system in space and time adds sufficient potential to obviate speculation that understanding of man lies beyond science, at least for the present. The current limit to the scientific understanding of man remains man's understanding of science. We need to develop the research tools for studying nonlinear, dynamic neural networks (*6*). Possibly more important, psychology may have to undergo a theoretical revolution comparable to the change from classical to quantum physics. Explanations of perception, memory, and so forth, as dynamic, nonisomorphic, and yet invariably transformed, patterns of activity across thousands of nerve cells may be more difficult to conceptualize but are just as coherent as explanations based solely on structure.

T. H. NILSSON
Department of Psychology,
University of Alberta,
Edmonton, Canada T6G 2E1

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Stent states that structuralism admits the possibility, and indeed argues for the actuality, of innate knowledge. However, he does not consider the theories of Jean Piaget, whose book (*1*) on structuralism is at considerable odds with the picture painted by Stent of a structuralism that is more or less equivalent to Kantian philosophy. Piaget's theory is a synthesis of the theories held by Hume (positivism) and Kant (critical idealism). It argues that all psychological processes are active processes. That is to say, contrary to Hume's assumption, living organisms, in adapting to their environments, do not passively absorb the structures of those environments. Rather, the latter are assimilated in accordance with the organisms' own structures, that is, with such things as specific neural architectures. More important for Piaget, organisms have central control over their peripheral sensory processes. Included are processes connected with motor-efferent systems, and thus with overt or interiorized motor processes. Stent, then, is correct in arguing that perception and related behavioral capacities are dependent (to use his terminology) on the "soul" or the "self," but he is incorrect about the supposed scientific inaccessibility of such central control processes. To modern neurophysiologists, psychobiologists, and behavioral psychologists, such processes are well within the range of meaningful scientific study.

Piaget has unfailingly argued that thought and sensorimotor processes must be understood in terms of the above motor processes. Such a hypothesis has empirical consequences which can be verified. To take an example from perception, it has been argued that visual input for a human perceiver is not composed of stationary "pictures" but—because the perceiver has visual receptors that are provided with motor systems—these inputs will tend to be transformations imposed by the perceiver on the environment. It has been proposed by Piaget and others that the successions of inputs engendered by the transformations comprise a mathematical group, that is, that the same given structure persists throughout the series. Since the outer environment as such does not have group structure, organisms therefore construct their environments by means of their overt or interiorized motor transformations. Thus, in adapting to their environments, organisms bring—not innate concepts, as Kant would have it—but a set of external or interiorized transformations which they impose on the environment. The latter, according to Piaget and others, provide the invariants of great generality, the logic structures, sought by Kant. Moreover, Piaget's theory is one in which the stability of transformation rules does not lead to an automatic assumption of their innateness. His theory therefore has opened the door to a fruitful study of origins and psychological development which was not engendered, say, by Kantian philosophy.

In regard to Piaget's hypothesis that there is a link between early sensorimotor and highly advanced cognitive structures, it may be noted that the physicist Bohm argues (*2*) that there is a greater fundamental structural affinity between primitive sensorimotor processes and the conceptual structure of modern relativity theory than there is between the former and Newtonian physics, with its absolute entities of mass, time, and so forth. In fact Bohm argues that science is a process of perceiving. Thus, Stent's statement that man's human (semantic) structures and intuitions have to be "denatured" in order for him to understand his modern science seems to be contradicted.

In fact, the supposed limits to the scientific understanding of man are not apparent if one chooses Piaget's rather than Kant's model of structuralism.

JOHN W. GYR
Mental Health Research Institute,
University of Michigan, Ann Arbor 48104

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Stent holds to the Kantian doctrine of a priori categories of experience, but he makes no reference to Piaget's vast work in "genetic epistemology," which provides an alternative to the a priori explanatory model. Stent's statement,

> The most basic of these concepts and ideas are precisely the Kantian a priori notions of time, space, and causality. The meaning of these terms is intuitively obvious and grasped automatically by every child in the course of its normal intellectual development, without the need to attend physics classes.

can be answered by noting that the child does in fact attend physics classes during the sensorimotor stages of his development. Those classes are rigorously taught by the nonhuman and human environment. So effective are these physics classes that most children come through as experts—so expert, in fact, that the fruits of their experiences pass for a priori, faultlessly matched-to-the-world givens.

ROBERT BARASCH
Route 1,
Plainfield, Vermont 05667

Nilsson's suggestion that an alternative to the "grandmother cell" could be "an array of activity across hundreds of cells" does not help to resolve the dilemma of having to posit a scientifically intractable soul or self as the psychological end point of visual perception. Admittedly, it is true that, as Nilsson points out, a tremendous cellular economy is achieved by representing the "grandmother" percept by the joint activity of a set of multifunctional grandmotherhood "facet" cells, rather than by a single cell. But why does Nilsson not go all the way, economy-wise, and suggest that the visual pathway ends at its narrowest funnel, namely the "6 million foveal pathways"? From a logico-semantic point of view, Nilsson's proposal is, in fact, entirely equivalent to the "grandmother cell," which if it did exist, would, in any case, have to be driven by the "facet" cell set he posits. Thus, Nilsson has not faced the deep question to which I addressed myself, namely, How does meaning arise from nerve cell activity?

The same criticism applies to the work cited by Nilsson on the implications of spatial frequency tuning in the visual pathway. Whereas the proposal made in these articles that the brain subjects the visual input to a Fourier analysis may be a useful formalism for handling the data of certain psychophysical and neurophysiological experiments, it has no standing as a theory of perception. Nevertheless, I fully support Nilsson's plea that "we need to develop the research tools for studying nonlinear, dynamical networks."

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Even though, in my attempt to discuss the scientific understanding of man in a brief essay, I could refer to only a tiny fraction of the large number of contributors to that vast subject, I do, in retrospect, agree with Gyr and Barash that I really *ought* to have mentioned Piaget's important contributions to the study of cognitive development. Just as Lorenz made the Kantian a priori a part of modern evolutionary biology, so did Piaget bring it into accord with modern ontogenetic biology and the genetic commonplace that the phenotype is the product of a dialectic between genes and environment. I do not agree, however, with Gyr's opinion that Piaget's theory of the connection between thought and sensorimotor processes has brought the central psychological concept of soul or self "well within the range of meaningful scientific study." That theory has indeed "opened the door to a fruitful study of origins and psychological development," but it shares with other structuralist theories the impossibility of validation.

To appreciate the scientific status of Piaget's genetic epistemology, we may consider the example of one of its verifiable empirical consequences offered by Gyr. According to Gyr, the sensorimotor hypothesis provides an argument to show why the human eye is provided with motor systems that cause visual inputs to be transformations imposed by the perceiver on the environment. Whereas it is certainly impressive for a contemporary psychological theory to argue in favor of a fact known to Helmholtz a century ago, it is characteristic of the structuralist nature of the sensorimotor hypothesis that it would not have been falsified by the (conceivably) negative finding that the human eye, like that of the owl, lacks an extraocular musculature. That is to say, Piaget's sensorimotor hypothesis, though couched in quasiphysiological terms, is on an epistemological par with Freud's theory of the Oedipus complex, which also has verifiable empirical consequences, but, nevertheless, cannot be validated by critical experiments or observations. I intend it as no criticism of Piaget when I include him among the structuralists whose theories I categorized as "merely plausible, being, maybe, the best we can do to account for the complex phenomenon of man." On the contrary, I believe that to claim for these theories a status which they do not have is to render their authors a disservice.

GUNTHER S. STENT
Department of Molecular Biology, University of California, Berkeley 94720

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