

APOLOGIA PRO SCRIPTURA SUA, OR MAYBE WE GOT IT RIGHT AFTER ALL

by Eugene G. d'Aquili

Abstract. A summary of the progress of biogenetic structuralism as an approach to the social and behavioral sciences is presented, from the publication of *Biogenetic Structuralism* in 1974 to the present. The difficulty that many scholars have found integrating neuroanthropology and comparative ethology into an understanding of cultural, and particularly of religious, phenomena over the past almost two decades is considered. More specifically, the articles of James Ashbrook and Mary Lynn Dell published in the same June 1993 issue of *Zygon* as this article are analyzed and responded to. These authors critique Eugene d'Aquili's work of integrating neuropsychology and religious experience primarily by analyzing *Brain, Symbol & Experience*, which d'Aquili co-authored with Charles Laughlin, Jr., and John McManus. H. Rodney Holmes's article in the same issue of *Zygon* analyzes the whole corpus of d'Aquili's religion and science work as it appeared over the years in the pages of *Zygon* and in other articles and books as well as in *Brain, Symbol & Experience*. This critique is likewise carefully considered and responded to. Finally a proposed trajectory of d'Aquili's (and Andrew Newberg's) future work in their ongoing project integrating neuropsychology and religious experience is elaborated. This involves, not only expansion of their general theoretical approach, but also empirical testing of hypotheses relating brain function to religious experience using PET scanning and some newer MRI visualization techniques.

Keywords: James Ashbrook, biogenetic structuralism, *Brain, Symbol & Experience*, comparative ethology, Eugene G. d'Aquili, Mary Lynn Dell, H. Rodney Holmes, MRI visualization technique, neuroanthropology, neuropsychology, PET scanning, religious experience, science-religion integration.

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The year 1974 marked the publication of *Biogenetic Structuralism*, which first presented the tenets of biogenetic structuralism to a skeptical and often hostile audience of social scientists. Along with Edward Wilson's *Sociobiology*, published in 1975, it was seen as inaugurating a serious neurobiological intrusion into the social sciences and especially into culture theory. By the time we published our second book, *The Spectrum of Ritual*, in 1979, there was considerably more acceptance for the approach taken in *Biogenetic Structuralism*, although by then the battle lines had been drawn, with a considerable polarization of opinion over the appropriateness of the application of neurobiology and comparative ethology to the social sciences. In one of the most generous acts by a world renowned academic that I know of, Victor Turner publicly acknowledged the importance that *The Spectrum of Ritual* had for him and announced his intention to subject his own monumental work to a biogenetic structuralist critique. This he did during his last major address, given at the Oriental Institute in Chicago in 1982, an event to which Ashbrook alludes in his article in this issue. Unfortunately, Victor Turner's tragic death shortly thereafter prevented him from systematically carrying out his intention. From that point on, biogenetic structuralism was taken very seriously; it was both vigorously attacked and supported. During a week-long series of meetings and symposia held in Rome during February 1991, biogenetic structuralism was subjected to serious scrutiny by a group of Italian and German social scientists, philosophers, and neuroscientists as a possible "program for the social sciences for the twenty-first century." The meeting was sponsored by the *Centro per Ricerche e Documentazione* in conjunction with the University of Rome. At a meeting of the Institute for the Advanced Study of the Social Sciences held at Rocca di Papa, the Italian sociologist Carlo Quaranta acknowledged the contributions of biogenetic structuralism to traditional anthropology, philosophy of science, and theory of religion and transpersonal psychology. However, he called for an extension of the theory to sociology and to the sociological issues of large populations. Partially as a result of this meeting, Charles Laughlin, John McManus, and their colleagues are currently expanding biogenetic structuralism into new and exciting areas. Andrew Newberg and I have opted for deepening and expanding our understanding of religious phenomenology, as well as expanding progressively into the area of neuroepistemology. This will basically constitute a deepening and expansion of the program which I have developed in the pages of *Zygon* since 1974.

Brain, Symbol & Experience by Charles Laughlin, Jr., John

McManus, and myself ([1990] 1992) represents the most mature elaboration of biogenetic structuralism to date. Since it clearly has profound religious implications, and especially implications for the relationship of religion and science, it has served as the major focus of critique of my work in this issue of *Zygon*. James Ashbrook and Mary Lynn Dell confine their critiques exclusively to *Brain, Symbol & Experience*. Rodney Holmes includes it along with all the papers I have published in *Zygon* as the basis for his commentary on my work.

I am pleased to note that all three commentators are positive about the basic "vision" expressed in *Brain, Symbol & Experience* and my other works. They all seem to be persuaded of the importance of a credible neurophenomenology of human consciousness, especially for the meaning and significance of religious experience. None of them questions the importance of a multidisciplinary approach to human consciousness in general and to religious experience, in particular. The intent and broad outlines of our work seem to receive their approbation. Of course, each critic has reservations, to varying degrees, about how successfully and completely we fulfilled the task we set out to do.

First I would like to address some of Ashbrook's comments and concerns. In the beginning of the "Critique" section of his paper, Ashbrook shares with Mary Douglas some of the latter's reservations about our "new synthesis," especially with regard to symbolization. He quotes her in saying ". . . systems of symbols, though based on bodily processes, get their meaning from social experience. They are coded by a community with a shared history. . . . So the preliminary starting-point for this argument is that there are no natural symbols; they are all social" (1982, xix-xx). I would respond to this that none of us doubts the importance of social experience. I must point out, however, that the ability of a species to be social at all, and to indulge in social behaviors, is itself neurognostically structured.

Furthermore, the *process* of symbolization itself is likewise clearly neurognostically structured. What we are left with is the *content* of symbols. Although most symbols may be "arbitrarily" socially derived, even this process is embedded within a neurognostically determined matrix. Furthermore, it is far from certain that there are no natural symbols in terms of content. Research into "prepared learning" among certain animal species (Brown and Jenkins 1968; Williams and Williams 1969), into the nature of phobias among humans (Seligman 1970; Seligman 1971), and into what appear to be cross-culturally similar visions under similar circumstances such as the near death experience (Osis, Karlis, and Haraldsson 1977;

Ring 1982), all combine with the traditional arguments from the cross-cultural similarity of certain myth themes to produce renewed support for Jung's archetypal hypothesis or something like it. However, we are certainly not maintaining that the content of most symbols is neurogenetically determined, nor are we denigrating the immense importance of social experience in the selection, valuing, and transmission of symbol systems.

Although Ashbrook finds our concepts of polyphasic awareness fruitful, he has serious difficulties with "pristine perception." He questions whether we can "reject a neoplatonic perspective yet opt with Edmund Husserl for 'the ultimate, essential givenness of phenomena [standing] in pristine purity before the mind'" (Ashbrook 1993, 238). First of all, neoplatonic essences only have meaning when contrasted with an "objective" world of phenomena. The point of the Husserlian paradigm, as well as of mature contemplation, is precisely that one attains a state anterior to subjective-objective discrimination. In this regard, however, Ashbrook does have a point which we need to address. After multiple bracketings, epoche, the attainment of apodictic truth, or after mature contemplation, an individual still must use language heavily laden with a culturally determined "natural fallacy." How can an external observer determine, therefore, that states of pristine perception or apodictic truth are possible? Individuals may be self-deluded, and one "pristine perception" may be different from another. The answer is not that everyone, and scientists in particular, embark on a program of mature contemplation. That would be ideal, and although we advocate it in the book, it is not likely to occur on any large scale in the foreseeable future.

My own view is that the only reasonable answer to the question of whether "pristine perception" can occur, and is indeed pristine, is only likely to come from a heterophenomenological analysis (Dennett 1991). Such an approach makes no assumption about whether such a state exists or does not exist, but rather, in a systematic and controlled way, analyzes carefully the detailed verbal reports of individuals from as many cultural contexts as possible who claim to have the experience. Short of the experience itself, which eliminates all question of its reality, the only empirical answer to the question of whether a "pristine perception" is possible can only come from a rigorous heterophenomenological analysis. Unfortunately, this has yet to be done.

In the section concerning biogenetic structural theory entitled "Theological Implications" Ashbrook presents an exciting possible application of biogenetic structural theory to different kinds of

religious orientation as opposed to specifically mystical experience. Drawing on David Tracy and Paul Ricoeur, Ashbrook describes two trajectories or patterns of religious belief. Ashbrook writes, "One is the diffused phenomenology of manifestation; the other, the focused hermeneutics of proclamation. In manifestation, the locus of the holy can be anywhere and everywhere as discerned in mystical presence and natural symbolism; in proclamation the locus of the holy is specified and identified by virtue of historical claims and behavioral imperatives." Ashbrook goes on to suggest that such patterns imply differing neurognostic bases. He suggests that the tradition of religious manifestation is based on a dominance of right-hemisphere function and that the orientation of religious proclamation is based on a preponderance of left-hemisphere activity. He goes on to describe David Tracy's third pattern, praxis, including theologies of politics and liberation. Ashbrook maintains that the orientation of religious praxis may imply a neurognostic parallel of limbic integration, including "strategies to nurture one another in ways that are environmentally adaptive."

Although as presented, this triadic system may be a bit too pat, nevertheless I think Ashbrook is on to something that is extremely important. I have always thought that basic philosophical orientations are based on assumptions derived from various "fundamental perceptions of reality." In papers previously published in *Zygon* (d'Aquili 1978; d'Aquili 1983), I suggested in passing that such basic perceptions of reality are based upon the isolation (partial or total deafferentation) of certain neurocognitive operators. Thus, the fundamental perception that what is "really real" is being, duration, causality, or dialectic interaction of thesis, antithesis, and synthesis, results from a philosopher's ability to deafferent meditatively those neural structures or operators which underlie conceptualization, temporal ordering, abstract causal sequencing, or dyadic opposition in the processing of input from everyday life. Such deafferentation would result in an absence of content and in the subjective experience of the "absolute operation" of the neurocognitive operator itself. This is all highly speculative, of course, but Ashbrook does invite us to go down this road, certainly with respect to theological styles and orientations. It may be profitable to extend this line of inquiry to include the fundamental perceptions and consequent assumptions upon which various philosophies are based.

Unfortunately, I must accept Ashbrook's critique of our paying insufficient attention to gender differences in the organization of experience. However, the suggestion that *Brain, Symbol & Experience* represents "an unintended masculine epistemology" may go a bit

far. Ashbrook is correct in pointing out that there is accumulating evidence for a degree of sexual differentiation of the brain. He is reminding us not to neglect the impact of sex differences on epistemology and phenomenology. His point is well taken, although I must add the cautionary note that there is danger in overemphasizing the differences. We are all, women and men, capable of clear, formal communication, even if at times that may be difficult. Furthermore, I strongly suspect that most humans, at least under certain circumstances, are capable of experiencing the cognitive stylistic mode of the other sex even if not that sex's total experience of the world.

Ashbrook's suggestion that our image of "mutual interpenetration" of neural systems might possibly be an "unintended reference to homosexual activity" sent me running to an analyst. It may be several years before I can make any comment on this suggestion. Nevertheless, we should not take lightly the suggestion that there certainly are metaphors with gender implications and that we should raise our consciousness to awareness of this fact.

Another of Ashbrook's valid critiques is that, overall, we have tended to neglect Western religion in general and the Western mystical tradition in particular. I can only state that this issue is being addressed with work currently in progress.

Although Ashbrook may disagree with the authors on certain points, it is clear that he has understood our work very well. He has taken us to task on a few areas which need attention and has suggested new areas into which we might extend the application of biogenetic structural theory. For this we thank him.

The major emphasis of Dell's critique is on a much-needed call for a practical application of biogenetic structural theory to the areas of psychotherapy, to contemporary myth and symbolism, particularly in the men's and women's movements, and to faith formation theory. She makes specific suggestions of scholars within these areas with whom the authors of *Brain, Symbol & Experience* could and should dialogue. I will comment upon these helpful suggestions below.

First, I would like to consider an issue which she treats in the beginning of her critique, an understanding of which is essential to approaching any profoundly interdisciplinary work. I am grateful to Dell for making this issue explicit. The issue is the sense of uneasiness which any real expert in a field feels when confronted with the data from his field of expertise being presented in a deeply interdisciplinary work. By deeply interdisciplinary I mean that the data from various disciplines are presented in an interpenetrated manner rather than simply juxtaposed. The data from one discipline, therefore, are

presented more or less filtered through other disciplines. Even when great care has been taken to preserve the integrity of those data, the effect is somewhat unnerving to the expert. It is not that the data are misrepresented or erroneous, but rather that they acquire an unusual tint, tone, or feel which can be quite disconcerting. This sense of uneasiness is even more markedly felt by Holmes, as we shall see below. Dell writes, "I must admit I was not favorably impressed on my first reading 'The Nature of Neurognosis,' the authors' effort to encapsulate all of neuroscience relative to consciousness into one small chapter" (Dell 1993, 218). In another place she writes, "I was caught off guard by references to Piaget in material that is rightfully the domain of Alf Brodal." (Dell 1993, 218). Her uneasiness sent Dell to consult a number of standard neurological and psychiatric texts for a more orthodox perspective on brain, structure, and function. After quoting several of these she concludes:

Alas, the medical textbook definitions may be more familiar, but they certainly offer no significant advantages for interdisciplinary dialogue than those offered by the authors. In the strict scientific sense, the sections on the limbic system, sleep physiology, and prefrontal cortex can certainly be made more detailed and rigorously accurate, but is that the purpose of a pioneer work of an interdisciplinary nature? I suspect not. (Dell 1993, 219)

Although it is obvious that no interdisciplinary treatment can present any of its component disciplines with a depth and rigor that would fully satisfy an expert in that field, I have a sense that this is not the major source of the uneasiness. Having encountered the same vague disquiet from experts in philosophy, anthropology, and theology, as well as from some neuroscientists, I have become convinced that the real problem derives from the interpenetration of data from varying fields, something which yields a vaguely unfamiliar feel to material that an expert in a field rightly expects should feel very familiar. With respect to this issue Dell notes, "I realize that my criticisms, though they have validity, reflect on a minor scale the academic territoriality that historically has prevented interdisciplinary endeavors and the inquiries this work attempts to foster." Dell has done us a great service in making explicit a problem which usually remains an implicit disquiet on the part of any expert approaching a deeply interdisciplinary work.

Dell then goes on to exhort the authors to dialogue with researchers in psychopathology and psychotherapy with a view to expanding the application of biogenetic structural theory and evolving a general theoretical underpinning for various apparently divergent modes of psychotherapy. She feels that the key to such an overall understanding both of psychopathology and the various psychotherapies might

lie in an increased attention to the neurophenomenology of consciousness. I could not agree more with Dell's position on this issue. An in-depth consideration of the relationship of biogenetic structural theory to the work of Frank, Wolberg, Langs, and others cannot help but be beneficial to both sides in the dialogue.

Before I leave this area, there is one issue about which I must disagree with Dell. She states, "trances, fugues, memory blackouts or amnesia, somnambulism or sleep walking, and certain automatic behaviors are other alterations of consciousness that vary in their degree of pathology." I may be misunderstanding Dell, but the sense I get is that altered states or phases of consciousness necessarily represent pathology to a greater or lesser extent. If this is what she is saying, I must strongly disagree. Alternate phases of consciousness may or may not represent pathology. Insofar as they are correlated with increased fragmentation of the personality, they certainly do represent pathology. But insofar as they represent a heightened or increased integration of the personality, they can represent a marked increase in adaptation to the environment and in personal growth over baseline reality. An altered phase of consciousness, *in and of itself*, represents neither pathology nor increased mental health. An alternate phase of consciousness may represent either one depending on how it is integrated into the personality structure of the individual and how integrated or fragmented that personality structure may be itself. It is high time that psychiatry and psychology stop identifying altered phases of consciousness, as such, as pathological. If I have misunderstood Dell, I apologize, but this is a point that needs emphasis in any case.

Dell's suggestion that biogenetic structuralism may inform the theoretical understanding of both the men's and women's movements particularly with regard to their mythopoetic and affective base, as well as being informed and deepened by them, is helpful. Dell's suggestions reflect Ashbrook's concerns about our not dealing with the phenomenology of different gender experiences, but her commentaries suggest the possibility of very practical applications of biogenetic structuralism to current extremely important gender-based social movements.

Finally, with regard to theology and faith formation, Dell suggests dialogue with Matthew Fox, William Placher, Roberta Bondi, and James Fowler. Certainly dialogue with all these individuals holds the promise of being extremely fruitful, and, as with the gender issues, such dialogue would allow biogenetic structuralism significant practical application. Since most of my own work has become focused on mysticism and mystical states, I am particularly sensitive to the fact

that a dialogue with Matthew Fox could be quite productive. Although I have some reservations with certain aspects of Fox's approach, his emphasis on practical mysticism as potentially transformational in society at large cannot but be fascinating to a scholar whose primary interest is the fundamental nature of mystical experiences.

One particular criticism of my work has been that it is all very nice from a theoretical and academic perspective, but it refers to experiences that occur only among a handful of people and therefore cannot possibly have any social relevance. Although major mystical experiences may occur only among the few, minor but very meaningful experiences seem to occur among many more people than we thought. Fox certainly asserts this, and furthermore, he sees mysticism as one of the essential ingredients in the desperately necessary transformation of our world. To a theoretical scholar, such world renovation projects are not of much concern, but to all of us as human beings, they most certainly are.

My final comment on Dell's critique is that her citing of Fowler's work in faith development theory is so obviously constant with biogenetic structuralism as to need hardly any further explanation. She states, "the fact that *Brain, Symbol & Experience* and faith development theory rely so heavily on Piaget, epistemic process, and anthropologic facts, make the two systems very compatible." I would say that not only are the two systems compatible, but a serious dialogue between them would probably result in an exponential elaboration of both.

Again, I must thank Dell for creating a lot of work for us. Such work can only result in greater refinement of biogenetic structural theory and in an increased understanding of the possibilities of its social application.

Finally, we come to Rodney Holmes's commentary (1993, 201-16). This *opusculum* is certainly the most witty, the most complimentary and flattering, and potentially, the most damaging critique I have ever received. Reading it was something of an emotional roller coaster. After I had finished, I was not certain whether to assume a place with Plato, Aristotle, Darwin, and Husserl, or whether I should apply for a subsidy on the basis of mental disability. I certainly thank him for his appreciation of my vision, overall strategy, and importance of the general program. When it comes to my neuroscience, however, Holmes's uneasiness (which Dell shared at first) obviously progressed to increased skepticism and eventually to tentative rejection. Clearly, there are some issues in this

area that need to be discussed, and a number of areas of obvious misunderstanding.

Holmes's first critique is that there is insufficient EEG evidence for the neurophysiological events which I postulate underlie mystical experiences. Furthermore, Holmes maintains that this evidence should be readily available. He states,

EEG studies of the lesser trance states would have easily detected a brain functioning in this alternate mode. EEG studies of various meditative states are legion, but in mainstream neuroscience the results are judged to be highly controversial. The Marxist sociologists of science might be correct when they say this is because the powerful establishment refuses to accept the data. But in my judgement the data predicted by d'Aquili's model would be so unambiguous that verification, or in Popper's terms, falsification by objective criteria has long been possible, often attempted, and not yet achieved. (Holmes 1993, 208)

It is simply not true that in *mainstream* neuroscience EEG studies of mystical experiences have often been attempted. There have certainly been many studies by special interest groups, i.e. religious or philosophical movements, which have a vested interest in positive results. By and large, the results of EEG studies by such groups tend to support my model, but like Holmes, I hold them suspect. I have never used their data in support of my models of mystical states. Unfortunately, mainstream science is scarcely interested in mystical states, and although some work has been done on meditation and concentration, practically nothing has been done on mystical states themselves. There is a very good reason for this. Not only is there little interest in the subject in mainstream neuroscience, but it is very difficult to generate these states, even with mature contemplators. What little work has been done has examined mainly novice meditators with whom the chances of attaining any mystical state while hooked up to an electroencephalograph are almost nil. As I have said, even with mature contemplators, the chances are relatively small.

Furthermore, the mystical states which may be generated by meditation are what I have termed the major ones. Minor states, such as a profound sense of religious awe or numinosity, do not tend to occur from meditation, but almost always spontaneously. Although it is probably true that minor states occur more frequently than the major states, owing to their spontaneous nature there is close to zero probability that one would happen to occur under laboratory conditions. The simple fact is that one is more likely to obtain a major mystical state under laboratory conditions with mature contemplators than minor mystical states, even if the latter spontaneously occur more frequently in life. A sense of profound religious awe is

rarely generated spontaneously in a laboratory. If mainstream science were seriously interested in obtaining the EEG correlates of mystical experience, chose the correct subjects, and assiduously and patiently pursued the project, we would undoubtedly have many more examples of major mystical states. But, given the elusive nature of the phenomena to be studied, I doubt that we would be flooded with data.

Next, I would like to answer several points which Holmes insists are minor, but which get repeated enough in his critique to call our neuroscientific competence into question. First, there is the famous issue of how many neurons are in the brain. We all agree that no one has ever counted them and probably no one ever will. Holmes began to get uneasy when he read that we stated that there are 10 trillion nerve cells in the brain. Precisely what we stated was "some authorities estimate that the nervous system is composed of more than 10 trillion nerve cells, and many times that number of glial and other support cells" (Laughlin, McManus, and d'Aquili [1990] 1992, 35). We were trying to make the point that there were indeed a great many nerve cells, and that some authorities estimate as many as 10 trillion in the *entire nervous system* (not just the brain) Some authorities do assert this (Klopf, [1982] for example), but in any case, this represents a lot of nerve cells. Presenting the matter this way is very different from asserting outright that there are 10 trillion nerve cells in the nervous system as a given and certain scientific fact. This seems to be a case of twisting a stylistic issue to imply ignorance of fact.

Another example of this is contained in Holmes's statement, "as mentioned above, major cells are often connected by interneurons; contrary to the authors' claim, however, interneurons are very well studied" (Holmes 1993, 208). What we actually stated was the following: "one reason why this feedback relationship has not been given the importance it deserves in neurophysiology is that although most neurons are local circuit neurons, they have been the hardest to study. The most accessible neurons for study—those with long processes called *long axon neurons*—are the least numerous and are relatively less affected by local feedback loops" (Laughlin, McManus, and d'Aquili [1990] 1992, 37). This statement is absolutely true. It does not claim that interneurons have not been studied, or even that they have not been studied reasonably well. It does claim that by far the greatest effort has gone into the study long axon neurons simply because they are the most accessible. The amount of information on the function of long axon neurons is significantly greater than the amount of information on the function of interneurons.

A third minor point is Holmes's taking us to task for our estimate of the concentration of cells in the cortex. In fact, we were quoting Sir John Eccles (Laughlin, McManus, and d'Aquili 1990, 37); although one may disagree with his metaphysical dualism, Eccles has unquestioned credentials as a mainstream neuroscientist.

Enough for minor issues. Now let us turn to more important concerns.

We are accused of panpsychism presumably because of the use of language in a section entitled "The Hedonistic Neuron." As is clear from the text, the title of this section is derived from the title of Klopff's book *The Hedonistic Neuron: A Theory of Memory, Learning, and Intelligence* (1982). Our preference for Klopff's model (although alternative models would fit just as well with biogenetic structural theory) clearly obliges us to defend his language, although it is not always the happiest. Klopff's position is that neurons purposely seek to maximize excitation (depolarization) and to minimize inhibition (hyperpolarization). Thus, excitation is equivalent to "pleasure" at the level of the organism and inhibition equivalent to "pain" or "displeasure." Following Klopff, we state that the goal of the cell is not merely to fire (output), but rather to fire in a way that increases overall excitation.

The use of the words *pleasure* and *pain*, even though they are put in quotation marks in the text, apparently leaves us open to a charge of panpsychism. The use of the word *goal* obviously leaves us open to the charge of Aristotelian teleology in Holmes's mind. Furthermore, Holmes maintains that we suppose that most neurons are excitatory or facilitatory and that there are very few inhibiting neurons. This position is allegedly necessary for the subsequent positing in my later works of reverberating circuits underlying the generation of mystical states. There are so many misunderstandings here that I scarcely know where to begin.

First of all, the use of the words *pleasure* and *pain* with reference to neuronal behavior is clearly an analogy to higher-order structures such as the entire brain. Analogical meaning is not literal meaning. We do not believe that neurons are happy or sad little creatures that consciously formulate goals and try to attain them. Such a position would indeed be both panpsychistic and absurd. The analogical nature of the terms *pleasure* and *pain* is clearly indicated by their being presented in quotation marks.

Which brings us to the use of the word *goal*. Apparently for Holmes, the mere sight of the word conjures up the spectre of Aristotelian teleology. The fact of the matter is that in evolutionary and developmental biology in particular, the use of the word *goal* and

of concepts relating to it is very common simply because it is impossible to present the Darwinian mechanism of natural selection every time one wishes to say, for example, that glucose-6-phosphate dehydrogenase was evolved *for the purpose of* metabolizing glucose. No one believes in the inherent purposefulness of that process. The enzyme arose because of selective pressures upon various molecular types. But the use of the term *goal* is essential shorthand language if scientific works are to be readable. Such usage is technically called teleonomy, a very different animal from teleology.

What we were trying to maintain is that since excitable tissue evolved, it evolved for an evolutionary purpose (again not teleology). Since presumably its adaptive function is precisely its excitatory nature, it *tends* (again not teleology) to maintain a state of maximal excitation. Given that an essential property of neural tissue is a refractory period after discharge, it became necessary to evolve a mechanism of presynaptic inhibition allowing the discharging neuron to recover and thus to become efficiently responsive to the next incoming stimulus. Thus, inhibitory mechanisms are essential to maintaining maximal excitatory function of neural tissue. The statement that excitatory tissue tends towards maintaining maximal excitation in no way denies the incredible importance of inhibitory mechanisms. Indeed, I would maintain that inhibitory mechanisms evolved precisely to guarantee maximal efficiency of excitation, and in general, maximal excitation.

Because of his initial misreading of our intent (possibly because we were not careful enough with our language and permitted some ambiguity to slip in), Holmes seems to assume that we think that there are very few inhibitory cells and that the vast majority are facilitatory. Yet we state, "the intrinsic motivation of each cell is to fire (see Bergstrom 1969; Berridge and Rapp 1979), unless inhibited from so doing by inhibitory cells (a large portion of all neurons are in fact inhibitory!)" (Laughlin, McManus, and d'Aquili [1990] 1992, 39). The exclamation mark was obviously intended to emphasize the point and to counteract any misunderstanding about the tremendous importance of inhibition.

Once Holmes got the idea that it was necessary to our theory to have an overwhelming preponderance of facilitatory neurons, however, he apparently supposed that this was essential for setting up the reverberating circuits which I propose in the "Mystical States" paper published in this issue of *Zygon*. This is simply not correct. In the schematic drawing of the proposed mechanism for the *Via Negativa* in that paper, the reader will note that the very first step involves partial deafferentation of the posterior superior parietal area

originating from inhibitory neurons in the right prefrontal cortex. This is clearly an inhibitory mechanism and actually begins the whole process, at least in the *Via Negativa*. Likewise, the final climactic step in both the *Via Negativa* and the *Via Positiva* involves maximal inhibition resulting in total bilateral deafferentation of the parietal area mentioned. In the models presented in this paper, and in those Newberg and I are working on for minor and spontaneous mystical experiences, inhibition is an important, and often central, element.

The issue about our being unable to tell the difference between a neuron and an organism is one that does not merit much space for rebuttal. The question of precisely what is an organism is more a philosophical question than a scientific one. Obviously, there are single-celled organisms. There are also cases in which it is not clear whether a conglomerate of multiple cells is an organism or each individual cell is an organism. It boils down to whether one wishes to emphasize as essential to an organism its reactivity and adaptation to the environment or its ability to survive in an environment by itself separate from associated, related cells. In any case, it is an old debate, and certainly not an essential point in biogenetic structural theory. It should scarcely be an attack on our biological competence.

A more subtle and important critique is the issue of neural connections going from nucleus to nucleus and not from structure in general to structure in general. That is, of course, absolutely correct, and it underlies Holmes's concern that the reverberating circuits presented in my models of mystical experiences may not actually exist. Clearly, the connections from structure to structure do exist, and that is documented in the paper. Considering our emerging knowledge of the richness of connections between nuclei, I consider it far more probable that the circuits postulated exist than that they do not. Obviously, Holmes believes the reverse.

Clearly, both our opinions are just that: opinions based on soft evidence. The best way at this time to determine who is right is by positron emission tomography or by some of the newer techniques of magnetic resonance imaging. Holmes states, "I fear (and I am as certain as I can be short of doing the literature search myself) that the circuit proposed in this piece (d'Aquili and Newberg 1993) does not exist at the level of actual connections between specific nuclei" (Holmes 1993, 211). It is difficult to know on what Holmes bases this certitude, especially considering our ever-increasing knowledge of the richness of connections among nuclei. If in fact Holmes went to the literature, did an exhaustive search, and could not document this circuit, all that would mean was simply that it was not documented.

It would state nothing about whether the circuit exists or not. Although our knowledge of internuclear connections is growing exponentially, there are still huge gaps in that knowledge (by Holmes's admission). What does seem to be emerging is the richness, not the paucity of connections. Therefore, since we know the structures are connected, and since there appears to be an incredible richness of internuclear connections, it would seem to me more reasonable to assume that these circuits exist than that they do not. However, only empirical investigation will tell which of us is correct.

I wish to thank Holmes not only for his praise of the vision and philosophy, but mostly for his *very careful* (dare I say overly careful) reading of our neuroscience. Many of his problems unfortunately arise from our lack of use of cautious enough language, appropriate qualification, and from allowing certain ambiguous statements to slip by. His careful critique has certainly reminded me of the importance of the greatest caution even in the presentation of relatively minor issues. It is clear as well that part of the difficulty arises from differences of opinion concerning disputed topics about which only soft evidence exists, sometimes pointing in opposite directions.

In conclusion, let us move from the level of microanalysis to the level of macro-statement. At this level, Holmes and I would certainly agree that we humans are the "religious savants" of the living world, and that our most truly human nature *is* our religious nature, even if we disagree about the precise neuroanatomy and neurophysiology upon which that religious nature is based.

Let me conclude with just a few words about where I am going with my work. When I speak of my work, I am not speaking of the biogenetic structural project in general. Charles Laughlin, John McManus, and their colleagues are expanding biogenetic structural theory into new areas. For my part, with my associate Andrew Newberg, I intend to continue work on religious phenomenology in general and mystical experiences in particular. Newberg and I are in the process of negotiating with nuclear medicine departments at several universities with the purpose of subjecting aspects of the theory I have been developing since 1972 to empirical testing using PET scanning, some of the newer techniques of MRI, or possibly some combination of the two. The vision and the philosophy may be interesting, but they will only become compelling when they can be linked to a neurophysiological substrate with certainty.

In addition to continued expansion into the area of religious phenomenology, Newberg and I have become fascinated by the neuroepistemology implied by aspects of mystical experience.

Neuroepistemology opens up an essentially new world to traditional philosophy, a world in which we hope to be among the first explorers. The project may be grandiose, but many doubted that we could come this far. To stop questioning, to stop exploring, to stop proposing the seemingly outrageous (so long as it is grounded in the science of one's time and empirically tested) is to deny the essentially religious nature of one's humanity.

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