Appendix 1

MACLEAN'S TRIUNE BRAIN CONCEPT: IN PRAISE AND APPRAISAL

Paul D. MacLean is a pioneer, a trailblazer, a scientist, and thinker well ahead of his time.* As a humanist deeply interested in the larger questions of human life, he started out studying philosophy. Unable to find satisfactory answers to questions such as the origin and meaning of life...why humans in spite of their unrivaled intelligence, often behaved in seemingly irrational ways threatening their individual as well as species survival...he turned to medicine and the study of the human brain. He anticipated that the brain, as the biological substrate of these behaviors, held the key to better understanding of these fundamental questions as well as hopefully their answers. MacLean was, for many years, chief of the Laboratory of Brain Evolution and Behavior of the National Institute of Mental Health. In 1952, drawing upon the nineteenth century French scientist, Paul Broca's designation of the great limbic node which surrounded the brainstem of mammals, he introduced the conceptual term *limbic system* into the neuroscientific literature. In 1970 he introduced the concept of the triune brain, which became widely popularized after the publication of Carl Sagan's rather overly dramatic and simplified discussion of it in *The Dragons of Eden* (1977). MacLean, in further developing the triune brain concept, which aroused great interest in psychiatry, education, and the lay public, produced his detailed and highly documented volume, *The Triune Brain in Evolution: Role in Paleocerebral Functions* in 1990.

CRITICISMS OF MACLEAN'S MODEL

MacLean's triune brain concept has been acknowledged the single most influential idea in neuroscience since World War II (e.g., Durant in Harrington 1992: 268). Nevertheless, following the publication of his 1990 opus, MacLean received

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highly critical reviews in two prominent science periodicals, *Science* (October 12, 1990: 303-05) and *American Scientist* (September- October 1992: 497-98). Both reviews were written by neurobiologists and both reviewers claimed that MacLean's triune brain concept has had limited acceptance or been largely ignored by professional neurobiologists.[137]

Anton Reiner, at that time a recent graduate, wrote the review in *Science*, which was the more extensive of the two. After initially recognizing MacLean as a trailblazer of neuroscience, whose triune brain concept has been well-received outside the field of brain research, as the centerpiece of Sagan's popular, *The Dragons of Eden*, and frequently as the only discussion of brain evolution in psychiatry and psychology textbooks, Reiner makes several points in criticism of the triune brain concept.[138]

First, he notes that since MacLean introduced the concept, there has been tremendous growth in neuroscientific research that has greatly extended our knowledge of brain function and evolution. This statement, of course, carries the general implication, which Reiner later makes explicit, that the concept is out of date.

Secondly, in initiating a criticism of MacLean's concept of the limbic system, Reiner writes: "MacLean's presentation of the role of the hippocampus in limbic functions is not well reconciled with the current evidence that the hippocampus plays a role in memory."(1990: 304).

Thirdly, Reiner contends that current research indicates that MacLean's reptilian complex is not a reptilian invention but seems to be present in vertebrates all the way back to jawless fishes.

Fourthly, Reiner maintains that MacLean overreaches the evidence when he claims that the basal ganglia are the neural seat for the control of species -typical types of behaviors.

Fifthly, Reiner states that the limbic system, which widely used term MacLean authored as a pioneer neuroresearcher, is not properly represented by MacLean. Contrary to MacLean, as Reiner would have it, the limbic system did not appear first in early mammals. Amphibians, reptiles, and birds also have limbic features such as the septum, amygdala, a different-looking hippocampal complex, and maybe even a cingulate cortex.

Sixthly, Reiner asserts that MacLean assigns the functions of parental behavior, which Reiner claims that MacLean regards as uniquely mammalian, to the mammalian cingulate cortex, ignoring the fact that some reptiles (crocodiles), all

[137]For a highly favorable review of MacLean's 1990 book see the review by Emre Kokmen, M.D. of the Mayo Clinic, Rochester, Minnesota, in *J. Neurosurg.* V. 75, Dec, 1991, p. 998. In this appendix I focus on the reviews in *Science* and *American Scientist* because they have reached a wider audience and have become red flag reviews unjustifiably inhibiting the thoughtful application of the triune brain concept in related fields as well as in the psychological and social sciences.

[138]The criticisms made by Reiner are not necessarily in the exact order presented.

birds, and possibly even some extinct reptiles (dinosaurs) also engaged in parental behavior.

Seventhly, Reiner makes a couple of other criticisms of MacLean concerning a) his preference for correspondence over the more evolutionarily appropriate concept of homology and b)his apparently uncritical acceptance of Haeckel's idea that ontogeny recapitulates phylogeny.

Finally, although Reiner praises MacLean's motives and acknowledges the appeal of the triune brain concept for dealing with "big" behaviors that we are all interested in such as: "How does our animal heritage affect our behavior? Why do we do the things we do? Why can we not live together more harmoniously?"... he feels that there are some telling shortcomings as recited above, in MacLean's scholarship. He concludes that "neuroscience research ***can*** (emphasis mine) shed light" on these important human questions, "though ***perhaps*** (emphasis mine) not in as global and simple a way as MacLean has sought."

A CRITIQUE OF REINER'S CRITIQUE

Book reviews because of their very nature are usually overly brief. They usually cannot deal in depth with the points they take issue with. Reviewers, then, are often themselves guilty of the same kinds of oversimplifications and misinterpretations that they seek to expose in their reviews. When Reiner states..."I strongly believe the triune-brain idea to be wrong"... he is caught up in the same oversimplifying tendency that he claims unjustifiably to find troublesome in MacLean.

The triune-brain concept may be wrong in some of its particulars, right in others, but still be very useful and valid in its more general features. After all, at this stage of our knowledge of the brain although it is quite advanced over the 1960s and 1970s, there are not a great number of things we can say with absolute confidence...very few generalizations that are without arguable interpretations of more detailed research data. And Reiner takes apart but does not offer a replacement generalization. His analysis is destructive, not constructive. This type of analysis is the easy part of the job...almost anybody can do it.

But in his apparent eagerness to discredit and take apart MacLean's useful generalization, he also fails to study his subject closely and therefore engages in some very careless scholarship. He makes significant omissions, outright errors, and substantial misrepresentations of MacLean's work. Let's look at the points Reiner raises one by one.

1. ***Reiner blatantly misstates the facts when he claims that the triune brain concept as well as MacLean's book is outdated and lacks up-to date documentation.***

Reiner's first point i.e., that there has been a great growth in knowledge about the brain since MacLean first announced his triune brain concept in the 1960s and 1970s implies that MacLean has left the concept untouched and undocumented since that time and has therefore not considered any of the more recent findings. The implications of this statement are belied by the currency of research cited by MacLean and included in his discussions. In backing up his case for the alleged outdated ideas and data in the book, Reiner baldly states "only a handful of papers from the '80s are cited"(Reiner 1990: 305). This statement is categorically false and easily contradicted by a count of bibliographic items. The bibliography of this work

contains over 180 entries (a big handful indeed!) which date from 1980 to at least 1988 and over 220 entries that date between 1975 and 1979. This amounts to at least 400 entries of rather recent documentation...keeping in mind, of course, that the publication date of MacLean's book and Reiner's review was for both 1990.

2. ***Reiner ignores or misstates the facts when he says, "MacLean's presentation of the role of the hippocampus in limbic functions is not well reconciled with the current evidence that the hippocampus plays a role in memory."***

The phrasing of this statement indicates that MacLean is unaware of or fails to report on the extensive research indicating the role of the hippocampus in memory. Such an implication is totally unwarranted. MacLean devotes fully two chapters to reporting and discussing such research. These chapters even have memory in their titles. Chapter 26 is titled *Microelectric Study of Limbic Inputs Relevant to Ontology and* ***Memory*** (emphasis mine). Chapter 27 is titled *Question of Limbic Mechanisms Linking a Sense of Individuality to* ***Memory*** (emphasis mine) *of Ongoing Experience.* These chapters deal at length with the role of the hippocampus in memory and propose an integrative role for the hippocampus in tying learning to affect or emotion (For a summary of MacLean's discussion on these matters, consult 1990: 514-16).

3. ***Claiming that the reptilian complex is not a reptilian invention, Reiner misrepresents MacLean's position.***

On the third point, Reiner contends that current research indicates that MacLean's reptilian complex is not a reptilian invention but seems to be present in vertebrates all the way back to jawless fishes. This is largely a taxonomic question. At what point do we declare something to be a fish, an amphibian, an amniote, a reptile, or a mammal? And do we view mammals as branching off from the amniote tree before we have distinct reptiles in the line of descent? Or do we prefer the more likely probability that mammals descended in a line from the ancient mammal-like reptiles of the pre-dinosaur Permian-Triassic periods called therapsids, who represent a branching of the ancient reptile line (cotylosaurs). Therapsids appeared approximately 230 millions years ago, and approximately 50 million years before the emergence of the great dinosaurs of the Jurassic and Cretaceous periods.

MacLean knows these facts and clearly acknowledges them, while supporting a lineage for mammals that traces back to the therapsids, of the synapsida subclass that branched off from the diapsida line that eventually produced the great dinosaurs many years later. This is the standard position in evolutionary theory today. One might wish to compare the phylogenetic tree in MacLean (1990: 34) with Butler and Hodos (1996: 72), Strickberger (1996: 396) and Hickman, et al. (1984: fig. 27-1). And it is the accepted position of standard zoology texts (e.g., Miller and Harley 1992, Hickman, et al. 1984, 1990). Mammals, and ultimately us humans, then, did not evolve from dinosaurs but from a parallel lineage that split much further back in geologic time.

If the term Reptilian Brain or Reptilian Complex causes confusion with modern reptiles, and because the reviewers don't wish to read MacLean's work closely, the Reptilian Complex could be thought of, and perhaps redesignated, as the ancient amniote complex or even the early vertebrate complex. And, of course, as MacLean acknowledges thoroughly, this early brain complex is not the reptilian brain of

modern reptiles but it is also not the same as that of the early vertebrates, amniotes, or therapsids. At several points in his book, MacLean makes this unequivocally clear by his reference to stem reptiles (cotylosaurs) (MacLean 1990: 33, 82), those early reptiles from which both the diapsid and synapsid lines branched off. To assure the proper evolutionary context, MacLean also uses the term *protoreptilian* in his initial definition and adds the clarifying comment that he refers to the reptilian complex (or R-complex) only for brevity's sake (see MacLean 1990: 15-16, 244, 519). This protoreptilian, or stem reptile brain, has been altered by modifications which include those produced by differentiation and elaboration of earlier structures (e.g., see MacLean 1990, 243). These modifications, to include differentiations and elaborations, provide, in addition to their previous maintenance and behavioral functions, neural circuitry in support of the enhanced limbic structures of mammals. These enhanced mammalian limbic structures necessarily engage and enhance prior circuitry in the brainstem. And together these enhanced limbic and brainstem circuits provide support for the greatly enhanced neocortex (or isocortex) which eventually got the sufficient modifications that permitted language and the development of complex technological societies.[139]

4. ***Reiner misrepresents MacLean's position on the basal ganglia.***

On the fourth point above, Reiner states that he knows of no one other than MacLean who believes the basal ganglia to be the neural seat for the control of species-typical types of behaviors (Reiner 1990: 305). This statement is a misrepresentation of MacLean's position as well as an admission of ignorance on the part of Reiner. In the first place, MacLean never uses the inclusive term "neural seat." Further MacLean is not talking about all species typical behavior but only some. He specifically excludes from this discussion such mammalian class/species typical behavior as maternal nursing and play, which are attributed primarily to other brain parts and treated in other chapters of the book.

In part II on the *Striatal Complex with Respect to Species-Typical Behavior*, MacLean repeatedly emphasizes that the traditional view that the striatal complex is primarily involved in motor functions represents an oversimplification. He writes that the purpose of the present investigation is to test the hypothesis that the striatal complex plays an "essential" role in certain species typical behaviors as well as certain basic forms of behavior common to both reptiles and mammals (MacLean 1990: 243). At one point after reciting the evidence, MacLean says that the results "suggest that the medial globus pallidus (a structure of the basal ganglia) is a site of convergence of neural systems involved in the species-typical mirror display of gothic-type squirrel monkeys." (MacLean 1990: 189). And, a little further on, that "findings indicate that in animals as diverse as lizards and monkeys, the R-complex

[139]The use of the term "additions" is deliberately avoided here because it has been the source of some confusion (see Butler and Hodos 1996: 86). New brain structures do not spring de novo out of nowhere but rather evolve from the differentiation of previously existing structures. When differentiations become sufficiently established, they are often referred to loosely as "additions." This does not deny that seemingly new additions may possibly and occasionally arise, but the intent here is to emphasize the phylogenetic continuity that underpins the concept of homology

is ***basically involved*** (emphasis mine) in the organized expression of species-typical, prosematic communication of a ritualistic nature." (1990: 189).

Additional research, some predating some postdating Reiner's review and of which Reiner is apparently ignorant, adds further support to MacLean's hypothesis. For example, J. Wayne Aldridge and colleagues from the University of Michigan in a research report titled "Neuronal Coding of Serial Order: Syntax of Grooming in the Neostriatum,"(1993) conclude that there is "direct evidence that the neostriatum ***coordinates the control*** (emphasis mine) of rule-governed behavioral sequences." This study builds upon a series of earlier studies of species-typical grooming behavior of the rat (e.g., Berridge and Fentress 1988; Berridge and Whishaw 1992; Cromwell and Berridge 1990). These earlier and more recent studies certainly support MacLean's hypothesis that the striatal complex plays an essential role in some species typical behaviors of a ritualistic nature.

And, of course, there is the growing body of clinical evidence, going well back into the 1970s and 1980s, that neurological disorders in humans (such as Parkinson's, Huntington's, and Tourette syndromes), which involve damage to the neostriatum, produce specific deficits in the sequential order of movement, language, and cognitive function.(e.g., Holthoff-Detto, et al. 1997; Cummings 1993; Benecke, et al. 1987; Marsden 1982, 1984; Oberg and Divac 1979). Such serial order patterns in behavior are phylogenetically old as well as pervasive and often constitute the basis of identifying so-called species-typical behaviors.

5. ***Reiner misrepresents the facts when he claims that MacLean says the limbic system first appeared in mammals.***

On the fifth point above, Reiner again misrepresents MacLean's position. MacLean does not claim that the limbic system first appeared in early mammals. He acknowledges that limbic features appear in fishes, reptiles, and birds, but are rudimentary and poorly developed as compared with those of mammals (MacLean 1990: 247, 287). According to MacLean's view, then, it is not the presence or absence of limbic features themselves in ancestral amniote or reptilian vertebrates, but rather the significant and prominent development of limbic features in mammals which is appropriately of interest in understanding the evolution of characteristically and uniquely mammalian behavior. Further, care must be exercised in making comparisons across existing modern species. We can only infer that the structures and undeveloped and/or rudimentary homologues of such structures in modern species were also present in ancestral lines. Brains don't fossilize, so the point can't be made conclusively. The currently accepted inferential position in neuroscience is that there are homologues of limbic structures going well back into vertebrate history, although these homologues in modern species are often difficult to establish and sometimes downright dubious (Striedter 1997; Veenman, et al. 1997).[140]

[140]The accuracy and utility of the concept and term limbic system has itself been a separate topic of some disagreement in recent years. Some authors state that it does not represent a truly functional system and the term should be discarded. Others defend its use. Most texts continue to find the term useful and because of its longtime usage it will probably remain in the literature. Some recent and prominent scholars illustrate the controversy well. Pierre Gloor of the Montreal Neurological Institute, McGill University, in his thorough-going work *The Temporal Lobe and Limbic System*, by the very use of the term in the title indicates his position. Further on in the text, while acknowledging the controversy he writes that this

6. ***Reiner displays careless scholarship and misrepresents the facts of neuroscience, evolution, and animal behavior as well as MacLean's position on parental behavior and the cingulate cortex.***

Another example of careless scholarship on Reiner's part is in the sixth point above. He claims that MacLean assigns the functions of parental behavior, to the cingulate cortex and that MacLean regards parental behavior as uniquely mammalian. According to Reiner, MacLean's alleged position "ignores the fact that some reptiles, such as crocodiles, and all birds engage in parental behavior, not to mention the possibility suggested by paleontological data that some extinct reptiles, namely dinosaurs, also engaged in parental behavior."(Reiner 1990: 305).

Such a blanket claim suffices to make one wonder if Reiner felt it worth his while to even consult the book he is reporting on. Firstly, MacLean does not "assign" parental behavior to the cingulate cortex. He reports the recent (at that time) research on maternal mechanisms in the septal or medial preoptic area (MacLean 1990: 351-53) and indicates that this area may have provided the initial potentiality for full scale mammalian maternal behavior (MacLean 1990: 354), which would include play and the development of empathy. The very title of his chapter 21 is ***Participation*** (emphasis mine) *of Thalamocingulate Division in Family-Related Behavior.* Participation is participation not unilateral and unequivocal assignment. And MacLean uses the systemic term thalamocingulate to indicate intra-limbic nuclei and cortical connections, not simply cingulate cortex as Reiner states.

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system in mammals exhibits an organization that is sufficiently different from that characterizing other areas of the cerebral hemisphere to merit such a designation (Gloor 1997: 106).

And well-known neurologist, Richard Restak tells us that based upon a large body of experimental work, it is appropriate to conclude that, "depending on the areas stimulated, the limbic system serves as a generator of agreeable-pleasurable or disagreeable-aversive affects."(1994: 143). Nevertheless, there is little agreement among neuroscientists concerning the contributions of the different components, and their mutual influence on each other (1994: 149).

On the other hand William Blessing, a neuroscientist at Flinders University, in his study of the lower brainstem, feels that emphasis on the limbic system has detracted from the study of brainstem mechanism, that it has been "plagued by its anatomical and physiological vagueness and by the lack of precision with which the term is used."(Blessing 1997: 15). Further, he feels the term should be dropped from the literature(Blessing 1997: 16).

A third recent author, neuroscientist Joseph LeDoux (1996: ch. 4) argues that because the limbic system is not solely dedicated to the single global function of emotion, a claim that MacLean fully recognizes in his chapters on memory (1990: chs: 26 & 27), that the concept should be abandoned. LeDoux apparently prefers a single functional criterion for the definition of a system, whereas MacLean seems to prefer a combination of functional and anatomical criteria. Le Doux concludes his argument by stating: "As a result, there may not be one emotional system in the brain but many."(1996: 103). Compare this with the concluding line of the definitional description by Kandel, et al., authors of the most widely used textbook on neuroscience and behavior: "The limbic system contains neurons that form complex circuits that play an important role in learning, memory, and emotion."(1995: 708).

The use and value of the conceptual term limbic system, then, seems to depend on one's research focus and how one chooses to define a system. It might be added that the definition of what constitutes a system is controversial in all disciplines, not just in neuroscience.

MacLean cites good evidence for thalamocingulate participation in "nursing, conjoined with maternal care"(MacLean 1990: 380). After all, lesions in certain portions of the cingulate cortex interfere with nursing and other maternal behavior (Stamm 1955, Slotnick 1967), not with blanket parental care as Reiner asserts.

It may be too early or simply erroneous in neuroscience to assign anything specifically and finally to any exclusive part of the limbic area. There is more likely some localization of minor function, but for most behaviors of any scale there seems to be fairly wide-ranging neural circuitry that may be interrupted by lesions at many different points. For example, recent research on maternal behavior (nursing, retrieval, nest-building) in rats has focused on the medial preoptic area with its connections to other limbic structures and the brain stem (Numan 1990). Alison Fleming and her colleagues (1996), summarize what we know about the neural control of maternal behavior. Not only the medial preoptic area with its brainstem projections, but also other limbic sites are involved, including the amygdala (Numan, et al. 1993; Fleming, et al. 1980), hippocampus (Terlecki and Sainsbury, 1978; Kimble, et al. 1967), septum (Fleischer and Slotnik 1978), and cingulate cortex (Slotnik 1967, Stamm 1955). Most emotions, emotional behaviors, and emotional memories seem to be distributed, involving multiple pathways. Specific behaviors and categories of behaviors can be interrupted by lesions at varying points in these multiple pathways. More recent research has again confirmed that the cingulate cortex is involved in emotion and motivation (Stern and Passingham 1996). In a recent research report John Freeman and colleagues conclude that the neural circuitry formed by interconnected cingulate cortical, limbic thalamic and hippocampal neurons has fundamentally similar functions in the affective behaviors of approach and avoidance (Freeman, et al. 1996).

Like any good scientist with an open mind, MacLean, at the close of his chapter on participation of the thalamocingulate division in family-related behavior, calls for more neurobehavioral research to explore the extent of this participation (MacLean 1990: 410). It is also noteworthy that MacLean is one of the few thinkers in neuroscience who shows concern for the neural substrate of such family based behavior, characteristic of mammals, as play and the underpinning but illusive quality of empathy. Although such characteristics have been reported on behaviorally (e.g., for play, see Burghardt 1988, 1984; Fagen 1981), they have largely been ignored in the search for neural substrates, not because they are unimportant, but because of the extreme difficulty in defining and objectifying them. But the evidence clearly points to neocortical as well as limbic cortical and subcortical representation (e.g., see Fuster 1997: esp. 169; Frith 1997: 98; Frith 1989: 154-55). One of these days, hopefully, mainstream neuroscience will direct more serious research toward a better understanding of these difficult and ignored questions which are so critical to a full understanding and appreciation of humanity.

Reiner also indiscriminately uses the blanket term "parental behavior" coupled with attributing that same blanket usage to MacLean. In this usage, Reiner shows a remarkable deficit of scholarship, naivete, or both. MacLean is not discussing all parental behavior. He is discussing those nurturing behaviors that are the most distinguishing characteristic of mammals and a fundamental part of their taxonomic classification and differentiation from birds and reptiles. These behaviors must be found in either new structures or modifications to existing structures. As Butler and

Hodos point out, new structures may be added to organ systems, but modification of existing structures appears to be more common (1996: 86). The jury is still out on the neurophysiology of these defining mammalian behavioral features. What's more, with the emphasis on cognition in neuroscience, there has been surprisingly little attention paid to the extensive work on the neural and hormonal basis of the motivational and emotional aspects of maternal care. This is openly acknowledged by leading scholars in the brain science field (e.g., Rosenblatt and Snowden 1996; LeDoux 1997: 68; Kandel, Schwartz, and Jessell 1995).

The blanket term "parental care" as used by Reiner in his criticism of MacLean amounts to condemnation by indiscriminate generalization. Parental care has been defined by a leading authority as "any kind of parental behavior that appears likely to increase the fitness of the parent's offspring" (Clutton-Brock 1991: 8). It is a very broad and inclusive term. The term includes nest and burrow preparation. The very production of eggs itself is included. This kind of "parental care" is found in the earliest vertebrates with very primitive brains indeed. If the all-inclusive definition of parental care can be stretched to include the production of eggs and digging a hole to place them in, perhaps it could conceivably be stretched to include even the sharing of cellular membranes during asexual reproduction by single-celled organisms.

But specifically...what about parental care in modern reptiles? Contrary to Reiner's claim, MacLean reports on parental care in crocodiles (MacLean 1990: 136-37) and also in some species of skink lizards (MacLean 1990: 136, 248-249). A recent review article on parental care among reptiles by Carl Gans of the Department of Biology, University of Michigan, brings us up to date. Gans claims that the most spectacular example of reptilian parental care takes place among crocodiles. Both parents respond to the call of hatchlings who vocalize underground while emerging from the eggs. The adults dig them up and transport them to water in their large buccal pouch(Pooley 1977). The young are then washed and stay shortly in association with the adults. After a relatively brief period, however, the juveniles' response to the adults reverses. The juveniles disperse suddenly into small, nearby channels where they may dig themselves tunnels. Gans notes:

> In view of the fact that crocodylians may be ***cannibalistic*** (emphasis mine), there seems to be both an inhibition of cannibalism in the parents and an inhibition of a possible adult avoidance reaction in the neonates (1996: 153).

This kind of short-lived parental care during which the cannibalism of parents is inhibited may be impressive in reptiles, but it is a far, far cry from the highly developed family-related behavior in mammals; behavior which is so further developed in the human species that it extends often throughout an entire lifetime and becomes the basis for a vastly extended social life. The ***equating*** of parental care in reptiles with parental care in mammals is simply ludicrous. It is this mammalian family behavior that concerns MacLean, and the neural substrate is appropriately sought in the brain modifications that became prominent with the appearance of mammals.

7. ***Reiner's further inaccuracies: recapitulation, homology, and correspondence, etc.*** Near the end of his review Reiner makes the following isolated statement: "MacLean also errs in his apparent sweeping acceptance of Haeckel's idea that ontogeny recapitulates phylogeny." Again Reiner distorts and

misrepresents. From a close review of the book it is by no means altogether clear that MacLean "sweepingly" accepts Haeckel's concept. In fact he only refers to it once (MacLean 1990: 46) while at the same time noting the well known exceptions. Haeckel's concept has been replaced in neuroscience today by the principles of von Baerian recapitulation. The von Baerian version holds that while ontogeny does not recapitulate phylogeny in the thoroughgoing Haeckelian sense, it does recapitulate the features of an organism in terms of the organism's general to more specific classification. In other words the von Baerian principles state that the more general features of an organism develop before the more specific features (Butler and Hodos 1996: 51-2). The issue, however, is still not so clearly settled. The emergent discipline of evolutionary developmental biology is looking more closely into such questions (Hall 1992, Thompson 1988). For instance, evolutionary biologist Wallace Arthur, in summarizing the main themes of this emerging discipline, writes:

> No single comparative embryological pattern is universally found or can be described as a 'law'. Von Baerian divergence, its antithesis (convergence) and a broadly Haeckelian (quasi-recapitulatory) pattern can all be found, depending on the comparison made (1997: 292).

On the additional point that MacLean prefers to think in terms of correspondence rather than homology probably reflects his functional-behavioral orientation. In fact it is specifically in discussing the issue of the relationship between structure and behavior that (MacLean 1990: 37) makes this comment. Later, he returns to a more standard use of homology (MacLean 1990: 228). There is, in fact, presently no sure fire way of demonstrating that homologues have the same one-to-one functions or produce the same one-to-one behaviors across species. In reporting that MacLean, at one point, expresses preference for the term correspondence because of the confusion in the definition of homology, Reiner shows what can only be considered a misplaced and sophomoric "gotcha" exuberance. He writes that MacLean's comment "should leave Stephen J. Gould, not to mention all other students of evolution, aghast," adding that such a comment constitutes a "very critical misjudgment to make in a work on evolution."(Reiner 1990: 305).

This is truly a naive, if not preposterous statement by Reiner. Could it be that Reiner is not aware of the long history of the pervasive problems associated with the definition of homology? For example, Leigh Van Valen, of the biology department of the University of Chicago, in the first sentence of his frequently referenced article on homology and its causes, writes: "Homology is the central concept of anatomy, yet it is an elusive concept."(1982: 305). Further on, in view of the persistent definitional ambiguities, Van Valen practically equates the two terms homologue and correspondence when he writes: "In fact, homology can be defined, in a quite general way, as ***correspondence*** (emphasis mine) caused by a continuity of information" ...although in a footnote Van Valen admits that correspondence itself needs further definition beyond the scope of his paper (305: fn. 1; cf. Roth 1994). Although there has been some sharpening of the concept of homology, with emphasis on phyletic continuity, the ambiguities have by no means been adequately resolved (Arthur 1997: 171-77; Hall 1994, 1996).

And there is the haunting question that is still wide open for research and investigation ...do most homologous behaviors share a homologous structural basis or can homologous behaviors be rooted in nonhomologous structures? (see Hall

1996: 29 fn. 23). The recent report by William Blessing on the lower brainstem raises the question of multiple neural representations of body parts and behavior, in that behavior originally represented and controlled in the brainstem of an earlier vertebrate may maintain its brainstem representation, but be controlled by an added representation in the frontal cortex of a more highly developed mammal. Such multiple representations at different levels as the brain became more complex would certainly confuse the issue of a straightforward homologous match of structure and function (1997: 1-18; see also, Brown 1977).

Research on very limited aspects of function are often suggestive but far from conclusive even on such limited function. Establishing homologues of the prefrontal cortex can be particularly vexing. A recent research article by Gagliardo and colleagues, "Behavioural effects of ablations of the ***presumed*** (emphasis mine) 'prefrontal cortex' or the corticoid in pigeons" (Gagliardo, et. al., 1996), indicates, not only in its discussion and conclusions, but in the very title itself, the uncertainty, ambiguity, and cautions that currently characterize such research efforts (see also Fuster 1997: 7-11).

There is an awful lot of assuming going on in some quarters of neuroscience on this issue, which simply cannot be settled at this time based on the empirical evidence. This is one of the problems and cautions that must be acknowledged when generalizing across species...say from rats to humans. In maternal behavior, for example, can we say factually that the medial preoptic area plays the same part in the maternal behavior of humans that it does it the rat brain? No, we cannot. At least not yet. But neuroscientists, after first hedging themselves, and following homologous logic, seem inclined to think so. Nevertheless, it is entirely within the realm of possibility that we may find that it does so only in part or not at all. As neuroresearcher Joseph LeDoux notes: "Some ***innate*** (emphasis mine) behavioral patterns are known to involve hierarchically organized response components." (1996: 120). And further on he adds: "Species differences can involve any brain region or pathway, due to particular brain specializations required for certain species-specific adaptations or to random changes."(1996: 123). And neurologist Richard Restak points out that in the case of animals multiple limbic areas may increase, modify or inhibit aggression. He notes further that even the same area may increase or inhibit responses under different experimental conditions and depending on the animal selected for experiment. As an example, he points out that the destruction of the cingulate gyrus (a limbic component) increases aggressive behavior in cats and dogs, whereas, on the contrary, such an operation has a calming effect in monkeys and humans (1994: 149).

Or perhaps, as Blessing suggests, there are multiple representations. Then we might have to go to correspondence rather than homology (even homoplasy might not apply, since homoplasy, or parallel evolution, would probably not apply in such closely related species) to account for the behavioral circuitry. In other words the corresponding neural circuitry--that circuitry controlling maternal behavior --may be found in the same, slightly differing, multiple, or perhaps (though highly unlikely) even totally different structural homologues or modifications.

In fact, if homology is correct and functionally, to include behaviorally, uniform...that is, the same structures account for the same functions and behaviors across classes, orders, and species... this finding would support the triune brain

concept as set out by MacLean, which says generally that the protoreptilian complex common to both reptiles and mammals functions largely the same in both classes. This finding would also support MacLean's position that the expanded circuitry areas of the mammalian complex bear characteristically mammalian functions and are the circuitry for characteristically mammalian behaviors such as nursing, a defining taxonomic feature of mammals (which, in part distinguishes them from reptiles and birds).

In a final series of somewhat negatively gratuitous comments Reiner writes about some of MacLean's legitimate speculations. For example, Reiner states "...and mathematical skill (he thinks the cerebellum could be involved)"(Reiner 1990: 305).

And why not? See MacLean's discussion on the subject (MacLean 1990: 548-52). Recent research has indicated that the cerebellum is not just a motor mechanism, but is also likely involved in higher cognitive and perhaps even language function. Especially relevant is the rather well-supported hypothesis that indicates a cerebellar mechanism involved in all tasks that require precise temporal computations. This could well suggest an involvement in mathematical processes. True, the evidence is insufficient to permit firm conclusions as to the cerebellar role in higher cognitive processes, but it is a research direction which needs further refinement and is currently pursued by a number of neurobiologists (Daum and Ackermann 1995; Dimitrov, et al. 1996; Altman and Bayer 1997: esp. 749-51).

Overall, given the outright errors, careless scholarship, misrepresentations, and sophomoric, prejudicial tone of Reiner's review, it probably should never have been allowed to appear in a publication of the stature and influence of *Science.* Such prejudicial reviewing should perhaps raise serious questions of standards if not ethics in the academic-scientific community.

CAMPBELL'S REVIEW IN AMERICAN SCIENTIST

The review by Campbell in *American Scientist* (1992) is a much shorter review than that of Reiner. It brings up some of the same points, but is less prejudicial in its tone. Since it is less detailed it expresses primarily the preferences and value judgements of the reviewer. Campbell repeats Reiner's erroneous charge about outdatedness. He writes: "...that except for a very few papers, most of the references were published prior to 1980"(1992: 498). It has already been noted that this "handful" of items amounts to more than 180 citations. One suspects that Campbell proceeded from his preconceptions and found what he expected to find. Campbell ends his review with the statement: "Unfortunately, the data presented are, ***to some degree*** (emphasis mine), outdated, and the evolutionary reasoning is unsophisticated."(1992: 498). The use of the term "unsophisticated" by the reviewer is a good example of gratuitous abuse of review. It is a sweeping value-laden term that communicates more about the reviewer than the reviewed. For anyone who has closely read MacLean's detailed and thoughtful work, such blanket judgments are not warranted. The evolutionary reasoning is, on the contrary, quite thoughtful, well-presented, and sophisticated. Such blanket judgments tell us more about the sociology of neuroscience and neuroscientists that they do about the subject matter of the discipline itself.

THE COMMENTS OF BUTLER AND HODOS

In their recent comprehensive and overall admirable work on comparative vertebrate anatomy, Butler and Hodos attempt to formalize the assignment of MacLean's work to the relics of history. Their comments reflect the standard oversimplified criticisms, misrepresentations, and errors that have become popular to repeat ever more unreflectively. Butler and Hodos assign the triune brain concept, inaccurately and indiscriminately, to a category they called "theories of addition." And without any detailed discussion or analysis, of the very significant indisputable points of accuracy in MacLean's concept, they write that the past three decades of work in comparative neurobiology "unequivocally" contradicts MacLean's theory (1996: 86).

It seems almost incredible that two such qualified authors should accept the same flagrant misrepresentations, inaccuracies, and oversimplifications of MacLean's work that have become commonplace in some sectors of neurobiology over the past decade. It appears that they merely parroted the errors and misrepresentations of Reiner and others rather than reading MacLean's 1990 work closely and open-mindedly. Or perhaps they simply took their understanding from Carl Sagan's overpopularized and oversimplified presentation in *The Dragons of Eden* and didn't consider the issue worth looking into further. There is no point in repeating the responses given earlier to Reiner's review. The same points hold for Butler and Hodos' comments. The rebuttal points are clearly made and easily accessible to verification by anyone who chooses to make the effort. The categorical statement by Butler and Hodos that the extensive body of work in comparative neurobiology over the past three decades unequivocably contradicts MacLean's theory, which they apparently have not read, constitutes on that point poor, if not irresponsible, scholarship.

THE UTILITY AND VALIDITY OF MACLEAN'S TRIUNE BRAIN CONCEPT

The triune brain concept may have its faults. But such faults have been patently misrepresented in some cases and grossly exaggerated in others. Whatever its faults may prove to be, the triune brain concept gets at a fundamental truth. The mammalian modifications, differentiations, and elaborations to the early vertebrate and ancestral amniote brains had the effect of introducing endothermy (warm-bloodedness), maternal nursing, enhanced mechanisms of skin contact and comfort, as well as enhanced visual, vocal, and other cues to bond parents to offspring and serve as the underpinning for the extended and complex family life of humankind. The mammalian modifications, therefore, added greatly enhanced affectional, other-interested behavior to the primarily (although not exclusively) self-preservational, self-interested behaviors of ancestral amniotes and early vertebrates (not necessarily their modern representatives).

The simplistic representation and attempted demolition of MacLean's triune brain concept is not good science. Reiner's review, where it has any validity at all, is like discovering a termite or two in the bathroom wall -- and then proceeding to pronounce a full alarm that the house is full of termites -- only to find that it is necessary to treat a couple of boards in the subflooring. Further, in his deconstructive, analytic fervor, Reiner has not offered an alternative higher level generalization. The review represents a dysfunction common to a lot of scientific

practice ... that of an analytical approach that takes apart but can't put back together. Perhaps we should call it analytic myopia. Being not interested in the bigger questions of humanity that we so desperately need help on, and lacking an interest in therapy, these analytic myopics continue their fine-grained focus. Fine-grained focus is fine, laudable, and very much needed. It becomes analytically myopic, however, when it fails to place in context what it finds and defines, when it employs sloppy scholarship, and when it attempts prejudicially to destroy or deconstruct that which it lacks the imagination and courage to put together.

On the other hand the theories of brain evolution that Butler and Hodos review favorably and the synthesis which they present at the end of their book focus on the immunohistological, hormonal, and morphological mechanics (1996: 463-73). They say, in fact, almost nothing at all about behavior or the significance for behavioral evolution for the various mechanisms of evolution they identify. And they make no attempt whatsoever to confront the larger behavioral questions of humanity where we need help and guidance from neuroscience in defining the neurobiological basis of human nature in order to establish links up the scale of generalization with the social sciences. The theories they present are only of interest to the technical aspects of neuroscience. They are not, however, incompatible, but rather tend to support MacLean's concepts when these concepts are thoughtfully considered and not inaccurately reported, misrepresented, or grossly oversimplified.

The key point in comparing these theories with that of MacLean's is that they are comparable, at best, only in part. They ask and respond to different questions. MacLean tries to address the larger questions of human nature and behavior. The others show no interest in such questions but address the fine grained technical questions of anatomical and functional evolution. At the level where they meet they do not contradict each other but are largely compatible. At the point they diverge they primarily address different questions. This is, I think, the root of the tension between the two. MacLean's concept facing up the scale of generalization is useful and has been appropriately well-received in the therapeutic sciences, and is also very useful for the social sciences. On the other hand, it has not been, but may yet become, more useful and better received in other quarters of neuroscience ...especially when subjective experience is eventually given its due in the study of consciousness. There are, in fact, recent signs that the importance of subjective experience, which is of great interest to MacLean, is beginning to be more fully recognized in the newer studies of consciousness (Hameroff, et. al. 1996).[141]

The triune brain concept may need modification, then, as the body of neuroscience grows...but certainly not outright rejection. With appropriate clarifications, it is still by far the best concept we have for linking neuroscience with the larger, more highly generalized concepts of the social sciences. This is true even if its level of generality has limited utility for some neuroscience researchers who are doing ever more fine-grained research into neural architecture and function.

[141]See especially the article by Stubenberg (1996); also, Galin (1996: 121) who writes: "I assert that what is most interesting about mental life for most ordinary people is not mechanism, not performance, not information processing; it is what it feels like! Subjective experience!" Searle (1997) provides a general criticism of the emerging consciousness literature. See also the assessment by molecular neuroscientist Smith (1996: 471-74).

The transition from early vertebrate to amniote to synapsid reptile to mammal was in behavioral effect the transition from a nearly exclusively self- preserving organism with relatively little or less complex social life to, at least in part, a nurturing, "other-maintaining", "other-supporting", or "other-interested" organism. And that makes all the difference in the world for human evolution. Our other-maintaining mechanisms combined with our self-preserving ones provide the biological glue as well as the dynamic for our remarkable behavioral evolution, our social life, and ultimately the crucial social and political factor of our moral consciousness.

The qualitative differences between the familial and social behaviors of even the most caring of reptiles (say, modern crocodiles), birds or social insects and the mammal we call human are overwhelmingly evident. Humans with their social, cognitive, and language skills, for better or for worse, dominate the planet and no other species comes close. Any neurobiologist who cannot see or appreciate the difference is suffering from analytic myopia or some form of misplaced species egalitarianism (e.g., see Butler and Hodos 1996: 3-4). The proper study of humans is humans and to some extent their lineal antecedents. The triune brain concept generalizes a fundamental truth out of much that is yet unknown and uncertain in neuroscience. And this generalization, when properly understood, appreciated, and applied, is the most useful bridging link, thus far articulated, between neuroscience and the larger and pressingly critical questions of humanity's survival...as well as the hoped for transformation of humanity into a truly life-supporting, planet-preserving and enhancing custodial species.

When other neuroscience researchers reach the conceptual point in their grasp of the discipline where they feel an increasing obligation to take a more holistic view and proceed to move up the scale of generalization in order to confront the larger questions of human life, they will likely produce concepts closely resembling the triune brain. Homology and behavioral evolution will almost inevitably take them in that direction. When that time comes, if the triune brain concept has been buried in the scrap heap of scientific history, it will be exhumed, refurbished, and honored. Frankly, despite its current lack of popularity in some quarters of neurobiology, I do not think it will be consigned to the scrap heap. I think that it will continue to be influential, and with appropriate modifications as research progresses, provide an important underpinning for interdisciplinary communication and bridging.

Appendix 2

THE PRIMARY ALGORITHMIC LEXICON

The primary [task] *is to show that the apparent richness and diversity of linguistic phenomena is illusory and epiphenomenal, the result of interaction of fixed principles under slightly varying conditions*
(Noam Chomsky 1995: introduction and chapter 4)

The purpose of this appendix is to indicate the correctness of Chomsky's insight quoted above. Chomsky indicated but never achieved a demonstration of the illusory and epiphenomenal nature of the richness and diversity of linguistic phenomena. An analysis of the primary algorithmic basis of both syntax and the lexicon permit substantial progress in achieving this goal. The simplifying effect of the primary algorithm on syntax has been demonstrated in preceding chapters. Here I will focus on the lexicon.

Such a demonstration has been tried before. An earlier attempted simplification of English parts of speech along functional lines was proposed by Fries (1952). He divided words into four major classes named Class 1, 2, 3, & 4. The four classes corresponded functionally to nouns, verbs, adjectives, and adverbs, which make up the bulk of the words in sentences. Fries reported that these four classes, without counting repetitions, accounted for over 93 percent of words in a sample of 1000. All other parts of speech he grouped as "function words" (1952: 65-110). Fries, however, was limited in his theoretical approach because he had no concept of the universal and simplifying algorithmic basis of language and, further, he could not show the basic derivational similarity of many lexical items.

The primary algorithmic syntactic structure allows a further refinement of Fries classification system. This primary algorithmic syntax indicates that only nouns and verbs are basic. Modifiers, such as adjectives and adverbs secondarily attach to the basic categories of nouns and verbs. The function words can be thought of as Fries grouped them. Such a scheme represents a remarkable simplification of linguistic phenomena.

The primary algorithm, however, allows us to go much further in reducing the richness and variety of the actual contents of the lexicon...the words. A remarkable number of words are variants of elements of the primary algorithm itself. Listed below are variants of primary algorithmic terms used in this book. Following that will be other examples from the lexicon of any standard English dictionary. Any reader, by personal effort, may *extend* the list and further *incorporate* a vast number of other lexical items.

1. Variant primary algorithmic terms used throughout the pages of this book: order, incorporation, synthesis, thesis, antithesis, part, whole, unity (primal, cosmic, absolute), equilibrium, equilibration, argument, content, deduction, induction, subject, object, concept, discrimination. Any noun is, of course, an incorporation or an ordering

chaos, extension, process, mulitiplicity, movement, change, reaching, relating, induce, deduce, imbalance. Any verb, of course expresses extension, reach, relation or being.

2. Some synonyms, antonyms, or derivations of primary algorithmic terms:
agree, agreement, analyze, analysis arrange, arrangement, assemble, assembly, associate, association, build, building, collect, collection, combine, combination, concept, conception, connect, connection, coordinate, conjunct, conjunction, constitute, constitution, consume, contract, covenant, compact, create, creation, devour, discriminate, eat, forge, form, formation, deformation, gather, gathering, govern, government, group, include, inclusion, induce, institute, institution, introject, join, joining, link, linkage, model, mold, nation, opinion, organ, organism, organize, organization, paradigm, part, pattern, produce, relate, relationship, select, selection, society, structure, swallow, system, systematize, thought, unify, unite, union.

3. A large group of words combine the algorithmic meaning of incorporation and extension. A few illustrative examples are such words as:
appoint, appropriate, assemble, assimilate, augment, capture, clutch, collect, collection colonize, colonialism, control, conquer, coopt, dominate, embezzle, employ, enslave, enthrall, entrance, imprison, inclose, embrace, encarcerate, encircle, encompass, enfold, engage, engorge, enlist, enroll, ensnare, entrap, envelope, expropriate, gather, grasp, group, immobilize, imperialism, include, maintain, obtain, retain, seize, steal, subdue, suborn, subordinate, transcend, transfix, use, utilize

4. Below are words selected from the only first seven pages of the "A" section of Websters New Illustrated Dictionary as they relate to incorporation and extension.
Incorporation:
abandon -- to leave a previous association, attachment, organization, or incorporation
abash -- discompose; disconcert; disorganize, disincorporate

abate -- to reduce; take away; to deduct as a part of a payment or some whole
abdicate -- to renounce a claim, a possession, a thing incorporated to oneself
abhor -- dislike to incorporate, be a part of, or associated with
ablate -- to remove from a whole (incorporation).
abnegate -- deny; withdraw from an incorporation
abolish -- demolish an order or incorporation
abominate -- to strongly reject for incorporation
abort -- to bring to a premature conclusion (incorporation)
abrade -- to scrape away a part of something
abrogate -- demolish something (an incorporation)
absolute -- perfect in order or incorporation
absolve -- to make eligible for re-incorporation
absorb -- assimilate, incorporate
abstain -- to refrain from incorporating
abstemious -- incorporating sparingly
abstinence -- refraining from incorporating
abstract -- separate from an incorporation; detach
abstruse -- hard to mentally incorporate
absurd -- cannot be incorporated; out of fit
abut -- to touch, or join (incorporate)
accept -- to agree to incorporate
access -- admittance to incorporation
accident -- chance disrupting of an event, state, or incorporation
acclaim -- incorporate with honor
acclimate -- to adapt or become incorporated to a new environment.

Extension:
abbreviate -- shorten
abduce -- draw or lead away
abduct -- to carry away (wrongfully)
aberrance -- wandering from straight path
abet -- to aid (an extension) wrongdoing
abide -- to delay extending; stay
ability -- able to extend, and reach out
abjure -- withdraw from an extension; abandon
able -- having power; capable of extending and incorporating
abridge – shorten (an extension)
abroad -- extended away from
abrupt -- halting an extension quickly
abscind -- to cut off an extension
abuse -- to extend and incorporate injuriously

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