Biolinguistics - Structure, Development and Evolution of Language

Lyle Jenkins

Date: Tue, 25 Nov 1997

I. Introduction

The theme of this conference is "Generative Grammar since *Syntactic Structures*" (Chomsky, 1957). As Chomsky has noted, *Syntactic Structures* was based on "lecture notes for an undergraduate course at MIT:"

This [*Syntactic Structures*] was a sketchy and informal outline of some of the material in LSLT [*The Logical Structure of Linguistic Theory*], along with some material on finite-state grammars and formal properties of grammars from 1956. (Chomsky, 1975:3)

It is in LSLT, first circulated in 1955, that Chomsky sets out in more detail the theory of generative grammar. In this work "the 'realist' position" is assumed. The theory is "understood as a psychological theory that attempts to characterize the innate human 'language faculty'" (Chomsky 1975:37). Hence from its earliest beginnings the central motivation for the study of generative grammar (generative linguistics, I-linguistics, biolinguistics) has been to understand the biological basis for human language. [2]

We can divide up the study of the biology of language into three main areas: 1) language, 2) development of language and 3) evolution of language. The study of the first area, "language," tries to answer the question of what it means to know a language; i.e., to "know English," "know Japanese," etc. Let us briefly review the central notion of "language," following (Chomsky, 1996b).

The language faculty is that "component of the human mind/brain that is specifically dedicated to knowledge and use of language." The hypothesis that there is such a faculty specifically dedicated to language is sometimes called the "modularity hypothesis." The language faculty itself is sometimes called a "language organ." The language faculty is composed of two systems: a cognitive system, for storing information and, possibly, performance systems specifically dedicated to language. There are, we assume, also performance systems outside the language faculty.
These include sensorimotor systems (for articulation and auditory perception) and conceptual-intentional systems--classically, these are systems involving sound and meaning.

A "language," then, is a "state of the language faculty." We say that we "know" or "have" a language L, knowing; e.g., such things as rhyme, entailment, phrase boundaries, anaphoric relations, etc. A language generates expressions, each of which is a pair of interface levels, a PF (phonetic) representation and an LF (semantic) representation. These representations are generated in a computational component which combines (merges) items from a lexicon and rearranges them in various ways.

The study of the second area, "development of language," tries to answer the question about what internal states language passes (or "grows") through, from an initial, genetically determined, state (S-0) to a final steady state (S-n). The linguist's theory of the final state is called *grammar* and the linguist's theory of the initial state is called *Universal Grammar* (UG). Universal Grammar might be regarded as the theory for the biological *Bauplan* for human language. Finally, the study of the third area, "evolution of language," tries to answer the question of how language evolved in the species and discover what the evolutionary "design" specifications of language were, functional or otherwise.

During the time period under consideration; i.e., from *Syntactic Structures* to the present, we might distinguish, somewhat arbitrarily, three phases of biolinguistics, each corresponding to the three main areas of study. The first phase in the development of biolinguistics runs up through the late 1960s. This year marks the thirtieth anniversary of the publication of Lenneberg's *Biological Foundations of Language* (in 1967), which included an appendix on linguistics by Chomsky. We can get an idea from Lenneberg's works of the range of topics under active investigation on biology of language during this period, including: language acquisition, genetics of language disorders (dyslexia, specific language disabilities), language of deaf children, "wolf children," critical period, twin studies, family pedigrees, aphasia and language, evolution of language, etc. Much progress has been made in the study of many of these topics (and others) over the past thirty years: to mention only a handful of examples, linguistics (syntax, semantics, phonology), language acquisition (Bloom, 1994; Piattelli-Palmarini, 1980), specific language impairment (SLI); (Gopnik, 1997); (Van Der Lely & Stollwerck, 1996); (Rice, 1996); (Leonard, 1997), language perception (Jusczyk, 1997); sign-language (Poizner, Klima, & Bellugi, 1987); neurology of language (Geschwind & Galaburda, 1984); language-isolated children (Curtiss, 1977), creole language (Bickerton, 1990), split-brain studies (Gazzaniga, 1992), linguistics savants,(Smith & Tsimpli, 1995), electrical activity of the brain (Ojemann, 1983), etc.; for additional references, see (Jenkins, forthcoming).

During the first phase work was carried out in all three areas under consideration: 1) language, 2) development of language and 3) evolution of language, with perhaps the greatest progress made with regard to 1); i.e., specifying the notion "language." During the second phase on the other hand, beginning in the late 70s great progress was made in answering questions about problem 2), development in language. During this time, the Principles-and-Parameters model of language acquisition emerged. Finally, beginning in the late 80s, enough progress had been made with respect to questions 1) and 2), to begin raising questions about language design, function, evolution of language, etc. This research agenda became known as the Minimalist Program (Chomsky, 1995) and it is to these questions of language design that we now turn.

Language Design - "How perfect is language?"

Chomsky noted that when we begin to ask questions about language design, such as how optimal or perfect language is, considerations of economy arise, principles that are rooted in elegance rather than utility, "the kind of property that one seeks in core areas of the natural sciences, for example, searching for conservation principles, symmetry, and the like"

(Chomsky, 1991:49). Euler's view of economy was that "behind every phenomenon in our universe, we can find a maximum or minimum rule" (Hildebrandt & Tromba, 1996:34). Einstein captured the intimate connection between symmetry and design in the dictum "symmetry dictates design." We turn first to the idea of symmetry.

It was understood by the ancient Greeks that symmetry was a property both in geometric figures and solids, such as squares and cubes, as well as in the physical objects based on them. This led to the classification of the five Platonic solids (cube, tetrahedron, octahedron, dodecahedron, icosahedron). That there are only five of these solids illustrates the restrictive power of symmetry.

In order to understand what is meant by symmetry, let us consider the simple example of a square. If we rotate a square about its center by 90 degrees, 180 degrees, 270 degrees, or 360 degrees (= 0 degrees), the square remains unchanged. These four rotations are known as the rotational symmetries of the square. Similarly, the square possesses four additional reflectional symmetries, two about its diagonals, and two more about its horizonal and vertical midpoints. These eight symmetry "transformations" form a mathematical structure known as a "group," in this case the group of symmetries of the square. Characterizing symmetries by means of a group has the advantage of providing us with a measure of the degree of symmetry of a geometric or physical object, or as we see directly below, of a mathematical or physical equation. In addition, we can also employ the methods of group theory to perform calculations and predict properties of a symmetrical structure.

As we just noted, not only objects, but also equations may possess symmetries. As we may recall from high school algebra, the graphs of the equation for the parabola (y = x "squared") has symmetry under reflection about the y-axis. Note that if we substitute -x for x in the equation y = x "squared", the form of the equation remains unchanged. Another way to say the same thing is that the equation is "invariant" under the following reflection transformation: x -> -x. In fact one of the key insights of Galois, the inventor of group theory, was that you can associate groups to polynomial equations in such a way that the symmetry properties of the group can be used to predict whether a given equation has solutions. This work was later extended by Sophus Lie, who extended group theory in order to be able to predict the solvability of differential equations as well. The result was that group theory now provided a powerful tool to study the symmetry properties of all kinds of equations from Newton's second law (F = ma), to Maxwell's equations for electromagnetism to the equation(s) for the Theory of Everything.

As examples from biology where symmetry and economy interact in interesting ways, we might mention reaction-diffusion mechanisms (Turing structures), the double-helix, the structure of viruses, the structure of the genetic code, universal scaling laws, the occurrence of the Fibonacci numbers in phyllotaxis, and the bee honeycomb. We discuss the bee honeycomb and universal scaling laws below. For discussion of additional examples see (Jenkins, forthcoming); we also develop the idea there that symmetry considerations might play a role in the generation of syntactic word order patterns in human language.

To see how one might study the problem of optimal design in a biological system, let us consider the case of the hexagonal pattern of the bee honeycomb.[3] Pappus of Alexandria ascribes his remarkable design to "a certain geometrical forethought," that the bees were endowed with:

There being, then, three figures which of themselves can fill up the space round a point, viz. he triangle, the square and the hexagon, the bees have wisely selected for their structure that which contains most angles, suspecting indeed that it could hold more honey than either of the other two.

This argument anticipated later economy principles. It may well be one of first clear cases of the application of an optimum principle to cognitive behavior. As D'Arcy Thompson notes,

The use by Pappus of "economy" as a guiding principle is remarkable. For it means that, like Hero with his mirrors, he had a pretty clear adumbration of that principle of *minima*, which culminated in the *principle of least action*, which guided eighteenth-century physics, was generalised (after Fermat) by Lagrange, inspired Hamilton and Maxwell, and reappears in the latest developments of wave-mechanics. (Thompson, 1992)

In fact, the bee example is taken from Book V of Pappus, which is totally devoted to isoperimetric problems; i.e., problems dealing with the the areas of figures with equal perimeters and volumes of solids with equal surface areas. This reference to "geometric foresight" in the bees could now be interpreted in various ways -- e.g., one possibility is that bees can perform certain kinds of geometric computations. Another possibility is that bees are genetically programmed to perform in a way such that honeycomb construction naturally emerges due to geometrical and physical constraints in the bee's environment.

The next step was taken by Kepler who investigated the three-dimensional version of the honeycomb problem.[4] He determined that the bee cell assumed the shape of a rhombic dodecahedron with three rhombic surfaces at the base:

This, then, is the geometric figure, as near as possible to a regular solid, which fills space, just as the hexagon, square, and triangle are the fillers of a plane surface. This, I repeat, is the figure which bees form in their combs, with only this exception that the cells have not got roofs of the same kind as their keel. (Kepler, 1966:11)

The "keel" refers to the three rhombic surfaces at the base of each honeycomb cell. Kepler is credited with the first insights into the crystallographic classification of sphere-packing into the cubic lattice, the face-centered cubic lattice, and the (three-dimensional) hexagonal lattice.[5] He understood that honeycomb construction resulted from the packing of rhombic dodecahedrons and that "rhomboids are roomier than cubes" (p. 19). This is then another variant of the "economy" argument of honeycomb design.

Kepler's discovery "passed unnoticed, so that the astronomer Miraldi was later credited with determining the rhombic dodecahedral shape of the bee cells as well as the angles of the rhombic surfaces. Then Euler developed his ideas of maxima and minima mentioned earlier. So Reaumur hypothesized that perhaps Miraldi's rhombic dodecahedron was a "minimal configuration" and might be that which "employs the minimum of surface for a given content: or which, in other words, should hold the most honey for the least wax" (Thompson, 1992:529). Reaumur put the problem to Johann Samuel Konig, who later ignited the famous quarrel over priority to the principle of least action between Maupertuis and Voltaire. Konig calculated the predicted angles and was "agreeably surprised" to find that his calculations agreed to within 2 minutes with Miraldi's measurements. He was actually so surprised that he "asserted that the bees had solved a problem beyond the reach of the old geometry and requiring the methods of Newton and Leibniz." Fontenelle, the secretary of the French Academy, "denied intelligence to the bees but nevertheless found them blindly using the highest mathematics by divine guidance and command."

Colin Maclaurin also took up the study of the honeycomb. He had participated in the development of the calculus after Newton and Leibniz, as every calculus student who has had to calculate the Maclaurin series may have suspected (Marsden & Weinstein, 1985). But for whatever reason, he decided to solve for the honeycomb angles without using "any higher Geometry than was known to the Antients [sic]."[6] He calculated the angles to be 109=9A 28' 16", and its supplement, 70=9A 31' 44". It turned out that Konig had made a mistake in determining the square root of 2 and that the angles calculated by Maclaurin agreed precisely with Miraldi's measurement.
This led to the dictum that the bees "had been proved right and the mathematicians wrong, an error spread by Lord Brougham. However, it was eventually realized that this was an overstatement since, in reality, the bee cells "are far from identical, and do no more than approximate to an average or ideal angle." The consensus was that Miraldi's "measurements" were actually idealizations that he had made and that he "had really done well and scientifically when he eked out a rough observation by finer theory, and deemed himself entitled thereby to discuss the cell and its
angles in the same precise terms that he would use as a mathematician in speaking of its geometrical prototype."

Darwin devotes a whole section in The Origin of Species to the "Cell-making instinct of the Hive-Bee" and even performs a series of experiments with bee-hives himself. He seems to favor Reamur's theory discussed above:

We hear from mathematicians that bees have practically solved a recondite problem, and have made their cells of the proper shape to hold the greatest possible amount of honey, with the least possible consumption of precious wax in their construction. (Darwin, 1958:242)

Darwin concludes that the hive-bee has "the most wonderful of all known instincts:"

Beyond this stage of perfection in architecture, natural selection could not lead; for the comb of the hive-bee, as far as we can see, is absolutely perfect in economising labour and wax. (Darwin, 1958:249)

However, D'Arcy Thompson thinks that "Darwin had listened too closely to Brougham and the rest" and cites a list of imperfections in honeycomb construction observed by experimenters. He considers it "fantastic to assume (with Pappus and Reaumur) that the bee intentionally seeks for a method of economising wax." Instead he favors the idea "that the beautiful regularity of the bee's architecture is due to some automatic play of the physical forces." D'Arcy Thompson is familiar with the work of Plateau and others on soap films and soap bubbles, which led to the theory of minimal surfaces and notes certain similarities between soap films and honeycombs (e.g., 120=9A angles and hexagonal structure) and argues for a [surface] "tension effect:"

...it seems much more likely to me that we have to do with a true tension effect: in other words, that the walls assume their configuration when in a semi-fluid state, while the watery pulp is still liquid or the wax warm under the high temperature of the crowded hive. In the first few cells of a wasp's comb, long before crowding and mutual pressure come into play, we recognise the identical configurations which we have seen exhibited by a group of three or four soap-bubbles, the first three or four cells of a segmenting egg. (Thompson, 1992:542)

We will not be able to resolve this fascinating centuries-old puzzle here. There is little known about the genetic program for honeycomb construction and there are scattered reports that make stronger claims for the geometrical abilities of the bees that we will not be able to go into here. We simply wanted to present a typical case of a design problem for a biological system and show how considerations of optimality, minimality, symmetry, and other mathematical and physical factors as well as genetics can interact in various subtle ways. However, we will leave the reader with one last interesting fact. We can say with certainty that Darwin was wrong in saying that the honeycomb is "absolutely perfect" in economy terms. It was proven by the Hungarian mathematician Fejes T=F3th in his paper "What the Bees Know and What They Don't Know" that the bee cell is not perfectly optimal. There is a slight imperfection. He formulated the "isoperimetric problem for honeycombs" and was able to come up with at least one better result (Hildebrandt & Tromba, 1996:227). As we recall, Kepler had found that the "keel" or base of the cell consisted of three equal rhombi. T=F3th found a slightly better cell base consisting of two rhombi and two hexagons.[7]

We mention also in passing that Barbara Shipman has made the intriguing proposal that there might be a connection between the hexagonal patterns found in the dance "language" of honeybees and geometrical structures in six-dimensions called "flag manifolds," surfaces which in turn play a role in the theory of quantum mechanics. Note that the hexagonal patterns of the dance language are different than hexagonal honeycomb structures. For a discussion of this work, see Adam Frank's account "Quantum Honeybees" (Frank, 1997).

Let us turn now to questions of design of human language. In 1976 Chomsky formulated the fundamental questions of biolinguistics as follows (Chomsky, 1978):

(a) function
(b) structure
(c) physical basis
(d) development in the individual
(e) evolutionary development

We turn now to question (a), which, of course, implicitly involves (e), evolution, as well. Questions about the design and function of language have come up repeatedly in biolinguistics in the period under consideration (since *Syntactic Structures*); e.g., the functional explanation for grammatical transformations based on certain assumptions about short- and long-term memory (Miller & Chomsky, 1963) and the functional motivation for syntactic output filters (Chomsky & Lasnik, 1977). With the emergence of more restrictive models of language in recent years as a result of work on problems (b) structure (of language), and (c) development, it has now become possible to (tentatively) take up the consideration of questions (a) function, and (e) evolution; i.e., the question of language design. As Chomsky has put it:

Recent work also suggests that languages may be optimal in a different sense. The language faculty is part of the overall architecture of the mind/brain, interacting with other components: the sensorimotor apparatus and the systems that enter into thought, imagination, and other mental processes, and their expression and interpretation. The language faculty *interfaces* with other components of the mind/brain. The interface properties, imposed by the systems among which language is embedded, set constraints on what this faculty must be if it is to function within the mind/brain. The articulatory and perceptual systems, for example, require that expressions of the language have a linear (temporal, 'left-to-right') order at the interface; sensorimotor systems that operated in parallel would allow richer modes of expression of higher dimensionality. (Chomsky, 1996a:29)

He pursues this line of argument for the "displacement property:"

In the computation of lambda [i.e., a logical form representation - lj], there seems to be one dramatic imperfection in language design, at least an apparent one: the "displacement property" that is a pervasive and rather intricate aspect of language: phrases are interpreted as if they were in a different position in the structure, where similar items sometimes appear and can be interpreted in terms of conceptually natural relations. (Chomsky, 1996b)

Chomsky suggests that the reason for the "displacement property" might be found in terms of "interpretive requirements that are externally imposed." The idea here is to look at what appear to be imperfections in language design and, if possible, show that they are not really imperfections, but result from independently motivated constraints, in this case constraints imposed at the interface between language and interpretive systems external to language.

Let us mention a conceptually analogous argument from the physical sciences. A cusp formation is observable at the interface of certain kinds of crystals; for a photograph, see (Peterson, 1988:69). Metallurgists had thought that this imperfection was due either to a dislocation in the
crystal or to the fact that the crystal had formed under non-equilibrium conditions. Taylor and Cahn showed that neither need be the case (Taylor & Kahn, 1986). They demonstrated that this crystal "imperfection" was expected under equilibrium conditions given certain necessary assumptions about symmetry (anisotropy) and economy (minimization of energy). In particular, the cusp formation turned out to be one of twelve minimal surfaces predicted by their theory.

Lasnik presents a case study of how biolinguists go about studying the mechanisms of language and at the same time try to learn something about the question of language design (Lasnik, 1997). He reviews work on phrase movement over the past few decades on based on a variety of languages, including English, Spanish, Irish, Japanese, Chinese, Palauan, Chamorro, Ewe, etc. An example from English follows:

Who do [you think [that John believes [that Mary said [that Tom saw _] ?

Here, the question phrase *who* has moved to the front of the sentence from the object position after *saw*. The question is, does *who* move in one fell swoop to the beginning of the sentence or does it move step-wise ("successive-cyclically" is the technical term) to the front; i.e., through the position of *that* at the beginning of each clause. Chomsky originally presented evidence for the step-wise hypothesis, and supporting evidence was subsequently discovered in a number of other languages in favor of this idea (Lasnik provides extensive references). One might ask why language is designed with "short movement" rather than "long movement?"

Lasnik notes that it has been suggested that ease of processing might be part of the design motivation. Some of the evidence for this comes from languages where the position corresponding to *that* in the above example is morphologically marked (Irish) or syntactically distinguished (Spanish) at the beginning of each clause, so that one can, so to speak, follow "a trail" of markers from the moved item *who*, down to the position where it originated. However, Lasnik cites research on other languages where no such markers are found (English), or even where there is no visible phrase movement whatsoever, although the interpretation of the sentence is the same as if certain "movement" constraints had applied
(Japanese, Chinese). He concludes that the design question for "short" movement is still open. However, at least one can see what kinds of evidence one can look for to investigate the question.[8]

Suppose, thirty years ago, when long movement was standardly assumed in linguistic work, we had asked why language is designed such that it has long movement. Consider now the following possible answers: long movement facilitates reproduction, winning friends and influencing people, communication, gossiping, ease of processing. Or the answer is natural selection. We can easily see why such answers are useless as explanations for language evolution. For, today, having learned that there is short movement, not long movement, we ask again why language was so designed. The non-answers are the same: gossiping, winning friends, etc. However, by pinning down the syntactic mechanisms better, we at least begin to see how an argument could be made for processing in some of the cases and to understand what kind of evidence counts for or against such a hypothesis.

II. Dennett - "Mandarin Mind-Games" vs. "Solid, Lab-Based Science"

In a review of Jackendoff's *Patterns of the Mind* (Jackendoff, 1994) and Pinker's *The Language Instinct* (Pinker, 1994), Dennett makes the following very odd statement:

The general attitude among cognitive scientists has been that since the linguists seemed to think they could do it all without benefit of controlled experiments (they just consulted their grammatical intuitions, which had long since been sullied by their own theories) and since they thought the brain was irrelevant, they could just go off and play their mandarin mind-games amongst themselves, while the rest of us got on with solid, lab-based science. (Dennett, 1994)

What makes Dennett's critique most bizarre is that the very books that he is reviewing, Jackendoff's *Patterns of the Mind* and Pinker's *The Language Instinct*, refute his remarks. Both make it very clear that intutions about language 1) are central to the study of language and the brain, not irrelevant "mind-games" and 2) that the other areas of the
biology of language that they discuss depend, directly or indirectly, on these intutions. These views are expressed not only in the two books Dennett reviews, but are at the very core of Jackendoff's and Pinker's technical work as well.

In the section on *Functionalism* (pp. 45-50), Jackendoff reviews the status of linguistic judgments. He examines the simple principle that, in English, "the subject of a sentence (normally) precedes the verb." He notes that the brain must have some way of distinguishing and manipulating the class of "verbs" from other kinds of words. The same is true for sequences of words that function as "sentences," as well as for sequences which can function as a "subject" (or an object) within a sentence. Jackendoff notes that the way that we study the consequences of such principles is by doing a "linguistic experiment:"

It turns out that among the kinds of experiments that can be done on language, one kind is very simple, reliable, and cheap: simply present native speakers of a language with a sentence or phrase, and ask them to judge whether or not it is grammatical in their language, or whether it can have some particular meaning. In fact, we have already done a number of these experiments in the course of the previous chapters. I presented various strings of words such as "Harry thinks Beth is a genius" and "Amy nine ate peanuts," and I judged whether they were or were not possible sentences of English. If all went well, you had no trouble agreeing with my judgments. That's all there is to it. The idea is that although we can't observe the mental grammar of English itself, we can observe the judgements of grammaticality and meaning that are produced by using it. (p. 46)

Jackendoff notes that the study of ambiguous linguistic examples, like "visiting relatives can be boring" (either relatives are visiting you or you are visiting relatives) is comparable to the study of Necker cubes and other visual illusions in the vision sciences. Likewise, the study of impossible sentences has some conceptual similarities with the study of "impossible figures" in vision such as the "Trident" figure.

Jackendoff compares this indirect study of mental grammar to the attempt to measure the mass of the electron (or the sun) -- if we can't make the measurements directly, then we perform them indirectly. However, from our point of view, the study of linguistic judgments is just as "direct" as a brain-imaging study -- these are just two different kinds of evidence. To use Jackendoff's word-order example, if an imaging study were to lead to the conclusion that the subject must always follow the verb in English, then we would dismiss the imaging experiments as being inconsistent with very strong linguistic evidence.

Jackendoff points out that judgments vary more among speakers about some linguistic examples such as "what did he wonder whether to fix?," and "what did he meet a man who can fix?," etc., where "judgments of crucial sentences aren't so crystal-clear and reliable." In this case the linguist will consider "trying more possibilities" and "consulting more people." He notes that in principle at least other kinds of experiments have or might someday provide crucial evidence, including "computer simulations," "reaction-time procedures," "measurement of brain waves," or even, down the road, the way "neural circuitry "encodes "certain kinds of patterns and not others."

So linguists aren't holding back Dennett from doing the "solid, lab-based science" that he is allegedly conducting. If Dennett and his cognitive colleagues wish to present us with a neurological account of language, they can go right ahead, starting with how English speakers know that it's "the book" and not "*book the." Or how they know that "the man put the book on the table" is a possible sentence of English, but not the mirror sequence "*table the on book the put man the." And these are only two among tons of crystal clear intuitions about English, not in the least "sullied" by linguistic "mind-games." The fact is that, as of 1997, neither Dennett (nor anybody else) can give a neurological account of even the simplest cases (like "*book the") that are central to any explanation of what it means when we say that "X knows English." The fact of the matter is that if we were to ban intuitions from the study of language, Dennett may as well shut down the doors to the language part of his cognitive lab.

The Connectionist "Debate" over Innateness

There has recently been an extensive philosophical debate within the connectionist community about the use of such terms as "innate," culminating with the multi-author 446-page-long book called *Rethinking Innateness*. The connectionists have conceived the debate as one between them and (primarily) (bio)linguists, as evidenced by the section "Twelve arguments about innate representations, with special reference to language" towards the end of the book. However, it is a one-sided debate in that no (bio)linguists are taking part in it, since they are unable to figure out what the issues are supposed to be about. So the debate is really an internal debate among the connectionist community with two schools of thought. Five of the authors think that "innate" should be re-thought and re-defined, while one author thinks the word should be "dispensed with entirely:"

> While the central arguments and concepts of this book represent our collaborative efforts, in any enterprise involving several authors with very different backgrounds there are bound to be areas of disagreement that cannot be resolved. One of these concerned the title, with which MJ [Mark Johnson] wishes to put on record his disagreement. In MJ's view the term "innate" is better dispensed with entirely, as opposed to being re-thought. [RI:xviii]

For bio(linguists), "innate" is just a technical term, which gets spelled out by empirical research on real languages, like English, Russian, Japanese, Mohawk, etc. It is of no interest to convene a committee of six biolinguists to sit down and legislate a definition for it. Instead you look at passives, relative clauses, quantifier scope, prosodic phenomena, etc., across a wide range of languages and try to tease out the parts that are learned and the parts that result from the unfolding of the developmental genetics program for human language. Ultimately, one hopes to spell out these processes further in terms of neural circuits, genetic development, etc. What we end up with will be the specification of what it means for language to be innate. This is exactly the standard procedure in any other area of biology; e.g., see Scheller and Axel's *How Genes Control an Innate Behavior* (Scheller & Axel, 1984). Whether this is the right way to proceed can only be judged by its successes (or failures) to achieve new insights. In fact, as the connectionist Mark Johnson, has himself noted [with John Morton], the analysis of behavior into "innate" and "acquired" components by the ethologists Tinbergen and Lorenz led to a "dramatic explosion" in knowledge:

> The study of ontogeny, the development of the individual, was guided for many years by the framework erected by the early ethologists, most notably Niko Tinbergen and Konrad Lorenz. They and some of their followers analysed behaviour and its development into "innate" and "acquired" components and introduced a range of different putative mechanisms such as sign stimuli, fixed action patterns and the hydraulic model of drive states. The use of such concepts resulted in a dramatic explosion of our knowledge in this field. (Johnson & Morton, 1991:6)

I think it is fair to say that there has been a similar "dramatic explosion" in our knowledge about universals of language and parametric variation in biolinguistics over the last forty years. But, instead of pursuing the sensible approach of Tinbergen and Lorenz, the connectionists persist in getting hung up with absurd interpretations of the word "innate."

For example, Jeffrey Elman, one of the authors of *Rethinking Innateness*, comes up with the following formulation:

What we mean by innate and what Steve Pinker means by innate are different...He is saying language is literally in the genes. We say it can't be in the genes because genes don't work that way. Specialized brain circuits arise not from a genetic plan but from experience pouring into the brain and growth factors helping it mature. (Blakeslee, 1997)

It is worth noting that it is only in the cognitive "sciences" that one has to defend oneself against such nonsensical objections. To see how nonsensical it is, let's replace "Pinker" with "Hubel and Wiesel" and "language" with "vision":

What we mean by innate and what Hubel and Wiesel mean by innate are different....They [are] saying vision is literally in the genes. We say it can't be in the genes because genes don't work that way. Specialized brain circuits arise not from a genetic plan but from experience pouring into the brain and growth factors helping it mature.

To a biologist working on vision, it is well-understood that vision is not "literally in the genes," but arises by genetic specification of "specialized brain circuits." It is just as well-understood that these circuits must arise from some kind of a "genetic plan" and many details are currently being learned about the molecular design specifications for this plan, especially in lower organisms. As Hubel notes:

My own guess is that the primary visual cortex, and perhaps the next few stages too, are all wired entirely according to genetically coded instructions. [Hubel 216]

It is also clear to any biologist that human language is not "literally in the genes," either, but likewise depends on genetic specification of "specialized brain circuits." Hubel goes on to note that in the case of language the fact that cortical circuits have been wired does not preclude a role for experience:[9]

This is not to say that other regions of cortex are necessarily wired without benefit of experience. Most neurologists would guess that the circuits responsible for language are mainly cortical--and no one would contend that we are born knowing the details of our native tongues. [Hubel 216]

Elman et al. present the D'Arcy Thompson's analysis of the construction of combs by the honeybee as an example of emergent behavior (Elman, Bates, Johnson, Karmiloff-Smith, Parisi, & Plunkett, 1996). They then go on to claim that such examples and others are incompatible with the "nativist" approach to language and lend support to their "connectionist" program. However, the connectionists are throwing out the baby with the bathwater. At the moment, the biolinguistics program offers the best hope of the interaction between genetic program and environment.

In the case of language, the "emergent" phenomena that we hope to account for includes wh-movement in Russian, incorporation in Mohawk, clitic movement in Spanish, verb-second placement in Germanic, and a host of other phenomena. We must also account for the myriad constraints and variations that we observe across languages. The task for the linguist then is to analyze each language carefully and attempt to extract the general principles common to all languages and identify the limited ways in which they can vary.

This must be done with the same care and thoroughness as in the study of honeycomb. What one has come up with is a proposal of some general principles which permit restricted parametric variation (the Principles-and-Parameters model).

What the connectionists propose is to throw out the grammars of Russian, Mohawk, Spanish, etc. and the theory of Universal Grammar which account for the "emergence" of these grammars. This leaves them with no answer for one of the fundamental questions in biolinguistics: "What constitutes knowledge of language (Russian, Mohawk, Spanish, etc.)? This move is somewhat similar to Deacon's move (see below). He rejects biolinguistic theories of language and the initial state, but has nothing to put in its place, beyond a literary metaphor of a "virus," somehow based on connectionist principles.

The Connectionist "Debate" over Modularity

Another popular (and private) debate among connectionists is whether the brain is modular; i.e., has specialized systems for language, vision, mathematics, etc. As an example of modularity, consider the following case:

In 1988, a 27-year-old man identified as CK sustained head injuries in an auto accident that left him with a strange impairment: He has normal eyesight and cognition, but he can't recognize objects. He can laboriously identify them by their component parts, but often they just look like "blobs." Yet he easily recognizes faces.

...The researchers put their subject through 19 types of experiments which included showing him upside-down faces, cartoon faces, partial faces, animal faces, and faces made out of vegetables. The man performed as well as or better than controls did in recognizing most types of faces, but his performance plummeted with an upside-down face or a photograph that had been "fractured." The authors conclude that the facial recognition system depends on the face being upright, and on preservation of the spatial relationship between two of three key features: eyes, nose, and mouth.

There is such a wide range of evidence of this sort and other kinds, indicating that the brain is modular so that most neuroscientists tacitly assume modularity as a working hypothesis. But this is apparently not the universally the case, as Blakeslee reports from the annual meeting of the Society for Neuroscience in an interview with the connectionists (including Jeffrey Elman and Mark Seidenberg), who she labels the "west pole camp" (Blakeslee, 1997):

But people in the west pole camp say there is not a shred of evidence for such fixed mental organs. In fact, they point out, research has shown that the brain can be surprisingly adaptable in moving different functions from one area to another, depending on need. For example, young children who have their entire left hemispheres (where, according to Pinker, a language organ is located) surgically removed because of epilepsy go on to develop language areas on the right side of the brain.

It is difficult to tell whether this curious discussion reflects a new low in work on language in the cognitive sciences or in New York Times science reporting. However, it has been quite well understood for decades in work on neurology of language that there is a wide range of developmental variation in language; see Geschwind and Galaburda

(Geschwind & Galaburda, 1987:67) (and the extensive cited literature there), who distinguish between "standard" dominance and "anomalous" dominance (language on the right side or both sides of the brain). Chomsky, noting that studies had shown that the language that develops in individuals with their left hemisphere removed is not fully normal (Dennis & Whitaker, 1976), suggested that one might investigate whether the deficiency involved computational or conceptual mechanisms of language (Chomsky, 1980:56). He also noted that Norman Geschwind had "pointed out that one must be careful in drawing conclusions concerning the inherent properties of the two hemispheres from such cases, since cortical development in the subjects in question was not normal to begin with" (Chomsky, 1980:264). For additional discussion, see Corballis, who attributes to Pierre Marie the idea (as early as 1922) that "the two hemispheres of the brain might have equal *potential*, at least for the representation of speech" (Corballis, 1991:287).

So talk here about surgery on epileptic patients is beside the point. Moreover, it is known that when fruit flies are heat or ether-shocked, homeotic mutants with legs instead of antenna growing out of their heads (*Antennapedia*) can result (Kauffman, 1993:508). Do we now conclude that there are no such things as "legs" either (or genetic programs)? The truth is that there is a fixed range of natural variation in all genetic developmental programs, whether that of language or of the fly, and this development can be derailed even further by environmental trauma or insult, sometimes in quite dramatic ways. It is the task of the biologist to discover the mechanisms that permit this variation.

Deacon's View of Language as a Parasite or Virus

Deacon rejects the standard view of biolinguistics (and of biology) that language, like vision and other faculties of the mind, is rooted in the brain. For Deacon, language is "outside brains:"

> I think Chomsky and his followers have articulated a central conundrum about language learning, but they offer an answer that inverts cause and effect. They assert that the source of prior support for language acquisition must originate from *inside* the brain, on the unstated assumption that there is no other possible source. But there is another alternative: that the extra support for language learning is vested neither in the brain of the child nor in the brains of parents or teachers, but outside brains, in language itself. (Deacon, 1997:105)

Deacon himself has embraced the view that language is some kind of "parasitic organism," or perhaps a "virus" that infects the brains of young children in order to reproduce itself.

> In some ways it is helpful to imagine language as an independent life form that colonizes and parasitizes human brains, using them to reproduce. (Deacon, 1997:111)

Deacon is thus explicitly rejecting the standard picture of biology of language which says that the development of language proceeds like any other biological system, whether the eye, the chick hind limb, or the heart. Language develops by passing through a series of developmental states, the course of which is affected by experience and maturation. In this standard picture of language acquistion, the crucial events affecting language development -- gene regulation, the development of neural circuits, synaptic communication, etc. -- are taking place *inside* the brain, not outside.

As far as we can tell, the move to language viruses solves no outstanding problems in the biology of language, but does create a whole host of problems for Deacon. According to Deacon, language does not evolve in the brain, but "evolves" outside the brain. Hence the subtitle of his book, The Co-Evolution of Language and the Brain. By what mechanisms does this language "virus" evolve and how does it work? For ordinary biological viruses, these mechanisms are well understood. At this point it is fair for the biologist to ask: "Well, how does your virus "grow" and how does it "evolve?" At this point Deacon invokes the "parasite" theory of Christiansen: "my own view is probably closest to that proposed in a recent paper by Morton Christiansen." So let us turn to that paper.

Christiansen interprets language as "an organism":

Following Darwin, I propose to view natural language as a kind of a beneficial parasite--i.e., a nonobligate symbiont--that confers some selective advantage onto its human host without whom it cannot survive. (Christiansen, to appear)

The reference to Darwin is to some remarks that he made about the "struggle for existence" among "the words and grammatical forms in each language:"

A language, like a species, when once extinct, never...reappears...A struggle for life is constantly going on among the words and grammatical forms in each language. The better, the shorter, the easier forms are constantly gaining the upper hand...The survival and preservation of certain favored words in the struggle for existence is natural selection

The move to parasites on the basis of this metaphoric description by Darwin is dubious.

We must remember that Darwin did not have the knowledge of genes, DNA, neurogenetics of behavior, etc. that we have today. To fill this gap he postulated gemmules in his theory of pangenesis and described language as "organic beings." If Darwin, the scientist, knew what we do today, it seems reasonable to think he would drop pangenesis and the theory of language as "organic beings" and would be trying to describe the body and the mind within the framework of developmental biology that we have today.

Christiansen (and Deacon) are in the position of Darwin in the 1800s. They have locked themselves into a metaphoric description of language[10] with no known account of the "beneficial parasite" or language "virus" in terms of standard developmental biology: As Christiansen puts it, "the fact that children are so successful at language learning is therefore more appropriately explained as a product of natural selection of linguistic structures, rather than natural selection of biological structures, such as UG [universal grammar]." The burden is on Christiansen to come up with overwhelming arguments against the standard biolinguistic approach, before we allow ourselves to be driven into embracing a metaphor in some domain outside biology, perhaps even outside the natural sciences, in some Platonic heaven.

So let us examine the central argument against "principles of UG:"

At this point it is furthermore illuminating to recall that the putative principles of UG are not established, scientific facts...The Government and Binding framework....underlying UG is merely one amongst many linguistic theories--albeit perhaps the most dominant one.

He lists some other alternatives, including Categorial Grammar, Dependency Grammar, Lexical Functional Grammar, and Generalized Phrase Structure Grammar.[11] This is true, but doesn't help out the "parasite" theory any. For all of the above theories have something that the parasite theory doesn't: viz., each of them is a real, not metaphoric, theory of language: each talks about specific antecedent-anaphor relations, phrase dislocation, word order, Case, etc. in a variety of languages--English, Turkish, Mohawk, etc. Moreover, each of them can be interpreted in various ways as theories of language acquisition with an initial state and parametric variation. They can be also used to interpret standard data from specific language disorder, aphasia, etc., consistent with current understanding. They can all be instantiated in parsing or processing theories, using the kind of functional arguments that Lasnik reviewed (above). The "parasite" theory on the other hand is immune from such empirical verification, as Christiansen himself notes:

This does, of course, not relieve the present theory from the burden of providing explanations of language universals--predominately couched in terms of learning and processing constraints. Indeed, its future success as a theory of the evolution of language depends in part on whether such explanations are forthcoming.

Christiansen cites recent work on "processing constraints" that hold hope for the parasite project, but as we have seen, processing constraints and language design in general are an integral part of all the other alternative UG theories, so the parasite theory, if and when it is forthcoming, will still have to be evaluated against the others on a case-by-case basis.

The remaining arguments of Christiansen are simply that language learning *might* proceed not by language-specific mechanisms, but by general mechanisms of "hierarchically organized sequential structure" and that these general mechanisms could have been the products of adapation by evolution. However, it is idle to speculate further, since we lack even rudimentary predictions from the parasite model to compare with a wide range of linguistic constructions across many languages that have been studied often in great depth in standard biolinguistic models of UG. Christiansen says that "it is important to note the explanandum [for the parasite model] is the behavioral data, not the theory-laden constructs that these empirical observations have given rise to (albeit linguistic theories do provide useful guidelines and descriptive frameworks)." At the same time, it is also important to note that once the "theory-laden constructs" with the wealth of explanatory insights that they provide into varied constructions across many languages have been tossed out, one is left, for the time being, holding nothing more than a parasite metaphor in some Platonic heaven. To the degree that Deacon's program depends on Christiansen's parasite analysis, it would appear that, for the moment at least, Deacon's entire program collapses, for the reasons given above, since he has no characterization at all for the central notion of biolinguistics; viz., language. At one point Deacon makes the following curious observation:

The adaptation of the parasite to its hosts, particularly children, provides the basis for a theory of prescient language learning. Though this is a caricature, it is no less so than the nativist and empiricist alternatives, and it captures much more accurately the dynamic push and pull of biases that have shaped both languages and the human brain.
(Deacon, 1997:113)

It seems odd that Deacon would characterize the central concept of his theory as a "caricature." It is either a serious empirical proposal or not. If it is, then the term "metaphor" seems more appropriate than "caricature" for the language parasite. The "nativist" alternative, on the other hand, as noted, has fully characterized the technical term "(I-)language" and provided a wealth of evidence for particular views about the role that the concept plays in biolinguistics.

Pinker's Principle -- "God or natural selection"

In *How the Mind Works*, Steven Pinker seeks to extend his claims about natural selection from the language domain, which he discussed previously in *The Language Instinct*) to the mind as a whole (Pinker, 1994; Pinker, 1996). Since, as we argue below (and in more detail in (Jenkins, forthcoming)), his claims about natural selection and human language are either incoherent or irrational, it is important to ask whether these ideas have been dropped or not.

Pinker states that *How the Mind Works* rests on three central claims: 1) that the mind is a system of mental organs, 2) that the mental organs are computational in character, and 3) that the organs of computation are "a product of natural selection." As we will show, neither claim 1) nor claim 2) are new to research into the mind. These have been standard assumptions in work on (bio)linguistics for years and have many classical antecedents as well. Only 3) embodies a new claim and, we argue, represents a regression in work on evolution of language, as well as of mind.

Let's look briefly at Pinker's claim 1), that the mind is a system of mental modules or organs:

The mind, I claim, is not a single organ but a system of organs, which we can think of as psychological faculties or mental modules. (p.27)

Compare this formulation with the one given by Chomsky (in 1976):

We may conceive of the mind as a system of "mental organs," the language faculty being one. Each of these organs has its specific structure and function, determined in general outline by our genetic endowment, interacting in ways that are also biologically determined in large measure to provide the basis for our mental life. Interaction with the physical and social environment refines and articulates these systems as the mind matures in childhood and, in less fundamental respects, throughout life. (Chomsky, 1978)

However, the claim only becomes interesting if one is able to discover and spell out the "specific structure and function" of the individual mental organs. In this regard one might argue that the study of the language faculty, in particular work on generative grammar during the last forty years, has provided some of the best evidence to date to validate the assumption of a mental organ specifically dedicated to language.

The assumption that there is a faculty (or mental organ) for language actually goes back to the earliest days of generative grammar (LSLT, for example). The more general idea has many classical antecedents as well, as Chomsky has pointed out in many places. Compare Descartes' remarks on the innate "faculty of thinking" and the innate ideas of "movements and figures" and "of pain, color, sound and the like" (Chomsky, 1966:67). It does strike one as peculiar that in a 660-page volume on the mind, Pinker remembers Descartes in passing for his proposal about the pineal gland, but not for his substantial contributions to theory of mind, nor that of the Port-Royal grammarians. One might get the impression that the theory of cognition and ideas about innate faculties of mind date back to the Sloan Foundation's funding of cognitive science centers in the U.S.

Let us turn to the second claim of Pinker in *How the Mind Works*; viz., that the "mental organs are computational in character." Again this view has been a commonplace in work on biology of language for years. Here is one formulation, given by Chomsky for language:

Suppose that what we call "knowing a language" is not a unitary phenomenon, but must be resolved into several interacting but distinct components. One involves the "computational" aspects of language--that is, the rules that form syntactic constructions or phonological or semantic patterns of varied sorts, and that provide the rich expressive power of human language. (Chomsky, 1980:54)

In actual linguistic practice, the notion of applying computations to abstract representations goes back to the beginning of generative grammar (*Morphophonemics of Modern Hebrew*), and even further back -- to structuralism (Bloomfield's *Menomoni Morphophonemics*) and to ancient times (Panini's Sanskrit grammar).12

Since, as we have seen, Pinker's assumptions 1) and 2) are standard assumptions in research on biology of language, the only assumption that need concern us here is Pinker's further assumption 3) in *How the Mind Works* that the organs of computation are "a product of natural selection." As far as we can tell, Pinker has simply carried over unchanged the incoherent and/or irrational formulation of natural selection directly from the *Language Instinct* into *How the Mind Works*. He repeats the claim, popularized by Dennett (Dennett, 1995), that cognitive science is threatened and besieged by "Darwin-hating academics", which include any and all of his critics; see the blanket reference (Pinker, 1996:165, note on p. 573). Stephen Jay Gould, we learn from Dennett, is out to "poison minds," and, according to Pinker, is "nasty and strident" and is "scrambling things so that his opponents have horns and he has a halo" (Yemma, 1997).

However, what is being objected to is not Darwin, but Pinker's incoherent and irrational formulation of (ultra-)Darwinism. In *The Language Instinct*, Pinker offers only two options for evolution of language, "God or natural selection." If these are the only choices, then God is the best choice. Creationism or belief in "intelligent design" ((Behe, 1996)) is coherent, but irrational. For example, Behe has proposed that biochemical laws can only be explained by "intelligent design." Behe's theory is coherent. It has two parts--a scientific part consisting of the known laws of biochemistry and an "intelligent design" part which says that whatever science hasn't explained yet is "explained" by "intelligent design." It is irrational, because everytime a new law is found, the explanation is the same. If the law is A, then "intelligent design" is the explanation. If the law is ~A, then "intelligent design" is the explanation again. A no-lose situation. However, we are worse off with Pinker's invocation of "natural selection," because his principle is not only incoherent, but also irrational. It is irrational for the same reasons just given--it can "explain" A or ~A, but it is also incoherent since natural selection can explain nothing without a space of physical possibilities to operate in.

Consider the following statement by Pinker: "...unlike Chomsky, I believe that natural selection is the key to explaining the structure of the mind." Pinker's statement here makes no more sense than to say that the electron is "the key" to explaining the atom. Just as the

atom cannot be understood apart from other factors (the nucleus, for one), the mind (body, etc.) can't be understood apart from the physical space that the organism is embedded in. Let's consider a relevant example. West et al. propose a model based on physical and geometric constraints to explain "quarter-power scaling" (West, Brown, & Enquist, 1997). As they note, a cat is a hundred times as large as a mouse so that one might expect a cat's metabolic rate to be one hundred times greater (Banegas, 1997). It is
actually only about thirty times greater and is derived by taking the square root of the square root of the body mass and cubing it. This empirical result has been known for decades as Kleiber's Law. They derived quarter-power scaling from three assumptions: 1) "that a space-filling fractal-like branching pattern is required to supply life-sustaining fluids to all parts of the organism," 2) "that the final branch of the network-the twigs of a tree or the capillaries of a circulatory system-are the same size regardless of a species' body mass," and "that the energy used to transport resources through the network is minimized."

Note that we could "derive" quarter-power scaling by invoking the Pinkerian formula: "the key is natural selection." Of course, this "explanation" would hold even if the denominator were five, six or seven instead of four. Observe further that appeals to reproductive fitness or finding mates don't help here either. West et al. are, of course, not
denying the obvious fact that metabolic rate is important for evolutionary fitness, not to mention survival. To the contrary, West notes that the answer to scaling phenomena is "essential to understanding how evolution maximizes fitness." However, they know that to move beyond descriptive laws like Kleiber's Law to explanation one must seek a deeper understanding of the physical and geometric factors involved in the design. As one of the co-authors (Brown) puts it, "given the constraints under which life developed and evolved, it isn't surprising that the diversity of living and fossil organisms is based on elaboration of a few successful designs."

Similarly, Pinker has branded as a "howler" the idea that "many properties of organisms, like symmetry, for example, do not really have anything to do with a specific selection but just with the ways in which things can exist in the physical world" (Pinker, 1996:168). According to the dictionary, a howler is "a ridiculous, bad mistake." Although he is singling out Chomsky as the culprit, the idea has a long tradition, from the ancient Greeks through Galileo and Kepler, and in modern times D'Arcy Thompson, Turing, and most recently in work on non-linear dynamics. Pinker's argument against this view is that "most things that exist in the physical world are *not* symmetrical, for obvious reasons of probability: among all the possible arrangements of a volume of matter, only a tiny fraction are symmetrical." Compare Pinker's statement with Malenkov's description of a crystal:

A real crystal contains impurities and defects, so that not all the unit cells are equivalent, and a real crystal is therefore strictly speaking not periodic. Even if we ignore defects and impurities, atoms in a real crystal at any temperature above absolute zero (0 K) practically never occupy the places corresponding to coordinates determined by means of diffraction methods because they are involved in thermal motion. So if we could glance at the instantaneous position of atoms--take a "snapshot" of a crystal structure--we should see neither periodicity nor symmetry and the picture would be much less beautiful. (Malenkov, 1997:45)

As Pinker observes, most *things* that exist in the physical world are not symmetrical. But most physicists are interested in studying the symmetrical world of structures and physical laws hidden behind Pinker's messy and assymmetric world. As Malenkov goes on to say:

In the words of the great Russian poet Alexander Blok, "Erase the accidental traits and thou shalt see: the world is beautiful." Diffraction experiments give us information about atomic positions averaged over time and space, thus "erasing accidental traits." We can, of course, obtain information about the thermal motions of atoms in crystals; in fact, the very same diffraction experiments enable us to find so-called temperature factors, which are measures of average displacements of atoms from their average positions. But for most crystallographers, instantaneous "snapshots" are ephemeral and irrelevant, and the only images of crystals that concern them are the averaged idealized structures with their beautiful symmetry and periodicity.(Malenkov, 1997)

Malenkov goes on to show the "beauty of disorder," that even "structures which are in principle irregular and disordered, such as are found in liquids and in some amorphous solids, can also be very elegant and beautiful."

Summing up, in the years since *Syntactic Structures* (and *LSLT*), which marked the beginning of the modern era for biolinguistic research, we find encouraging progress in all areas, particularly with respect to questions (a) language and (b) development, but even with respect to question (c), we have begun to raise interesting questions about language design. Much has been learned much about the nature of the design specifications of language by studying the grammars of many natural languages and by extracting general principles of Universal Grammar from these languages to account for acquisition. Futher empirical conditions on language design come from a variety of different kinds of studies: lateralization, modularity, plasticity, maturation, critical period. We noted earlier that the task of the biolinguist is to spell-out these mechanisms, not to re-think the definition of innate. We suggested further that the study of language design should be guided by considerations of economy and symmetry, as should the study of any physical system. As noted, we might reasonably regard the grammatical (a)symmetries of language as the "spectral lines" of the language faculty, pointing the way to the still-distant study of the equations of our minds.

Footnotes:

[1] To appear in the *Web Journal of Formal, Computational & Cognitive Linguistics*.

[2] We use the term biolinguistics in what follows for this endeavor with exactly the same meaning as (generative) linguistics or I-linguistics. For a history of the origin of the term "biolinguistics," see (Jenkins, forthcoming). The term biolinguistics properly reflects the fact that the study of generative grammar in principle draws on evidence from a variety of domains -- linguistics (syntax, semantics, phonology), neurology, genetics, etc. In contrast, terms like neurolinguistics and psycholinguistics have sometimes been incorrectly interpreted to mean that only certain kinds of evidence are relevant to the study of language; e.g., "psychologically real" evidence, in the case of psycholinguistics.

[3] We draw here extensively on the fascinating account by D'Arcy Thompson and some later material provided in Hildebrandt and Tromba (Hildebrandt & Tromba, 1996; Thompson, 1992).

[4] The investigation of the honeycomb problem arose in the course of Kepler's investigation of the hexagonal form of the snowflake. Chomsky has compared the study of optimal design of language to the study of the design of the snowflake. This would seem to be in harmony with Kepler's vision. Lancelot Law Whyte, in the accompanying commentary to the *Six-Cornered Snowflake* believes that Kepler "saw the need for a mathematics of morphogenesis" and that "*this* [Kepler's] *essay is the first recorded step towards a mathematical theory of the genesis of inorganic or organic forms*."

[5] Apparently, Thomas Harriot had previously discovered sphere-packing principles independently. (Kepler, 1966:v)

[6] According to D'Arcy Thompson, "it was characteristic of Maclaurin to use geometrical methods for wellnigh everything, even in his book on Fluxions." In Newton's work, the rate of change of a variable x was the "fluxion" of x.

[7] It has been suggested by the connectionists that the honeycomb example just discussed presents a problem for biolinguistics. However, we think that they simply misunderstand the conventional technical meaning of "innate." We return to the issue below.

[8] The above is a condensed version of the much more thorough and interesting analysis given by Lasnik.

[9] I mention in passing that my cat has "growth factors" and also had plenty of language "experience pouring into the brain," but my wife and I were unable to get it to talk, although it can see just fine.

[10] The move to parasites and viruses is purely voluntary in this case. Darwin had no choice in the case of gemmules, since there was no better theory around.

[11] He asserts that these alternative theories do not necessarily have transformations in the sense of the Government and Binding framework. But this is irrelevant, since the alternatives all have an equivalent way of expressing the dependency between a moved phrase and its gap.

[12] The foundations of the theory of computation was developed in modern times, by Turing, Church, Post and others.

Bibliography

Banegas, D. (1997, Summer). The Tree of Life: Tree of Life: Scientists Model Nature's System of Fractal-branching Networks. *Santa Fe Institute Bulletin*.

Behe, M. J. (1996). *Darwin's Black Box: The Biochemical Challenge to Evolution*. New York: The Free Press.

Bickerton, D. (1990). *Language and Species*. Chicago: University of Chicago Press.

Blakeslee, S. (1997, November 4). Recipe for a Brain: Cups of Genes and Dash of Experience? *New York Times*.

Bloom, P. (Ed.). (1994). *Language Acquisition: Core Readings*. Cambridge, Massachusetts: MIT Press.

Chomsky, N. (1957). *Syntactic Structures*. The Hague: Mouton.

Chomsky, N. (1966). *Cartesian Linguistics*. New York: Harper and Row.

Chomsky, N. (1975). *The Logical Structure of Linguistic Theory*. Chicago: University of Chicago Press.

Chomsky, N. (1978). Language and Unconscious Knowledge. In J. H. Smith (Ed.), *Psychoanalysis and Language, Psychiatry and the Humanities* (pp. ??). Yale University Press.

Chomsky, N. (1980). *Rules and Representations*. New York: Columbia University Press.

Chomsky, N. (1991). Linguistics and Cognitive Science: Problems and Mysteries. In A. Kasher (Ed.), *The Chomskyan Turn* (pp. 26-53). Cambridge, Massachusetts: Basil Blackwell.

Chomsky, N. (1995). *The Minimalist Program*. Cambridge, Massachusetts: MIT Press.

Chomsky, N. (1996a). Language and Thought: Some Reflections on Venerable Themes. In *Powers and Prospects: Reflections on Human Nature and the Social Order* Boston: South End Press.

Chomsky, N. (1996b). Some Observations on Economy in Generative Grammar. In *unpublished manuscript*.

Chomsky, N., & Lasnik, H. (1977). Filters and Control. *Linguistic Inquiry*, 11, 1-46.

Christiansen, M. H. (to appear), Language as an Organism: Implications for the Evolution and Acquisition of Language. *Cognition*.

Corballis, M. C. (1991). *The Lopsided Ape, Evolution of the Generative Mind*. New York: Oxford University Press.

Curtiss, S. (1977). *Genie: A Psycholinguistic Study of a Modern Day "Wild Child"*. New York: Academic Press.

Darwin, C. (1958). *The Origin of Species*. New York: New American Library.

Deacon, T. W. (1997). *The Symbolic Species: The Co-Evolution of Language and the Brain*. New York: W. W. Norton.

Dennett, D. C. (1994, June 23). Review of Ray Jackendoff, *Patterns in the Mind: Language and Human Nature*, and Steven Pinker, *The Language Instinct: How the Mind Creates Language*. *London Review of Books*, p. 10-11.

Dennett, D. C. (1995).*Darwin's Dangerous Idea*. New York: Simon and Schuster.

Dennis, M., & Whitaker, H. (1976). Language Acquisition Following Hemidecortication: Linguistic Superiority of the Left Over the Right Hemisphere. *Brain and Language*, 3, 404-433.

Elman, J. L., Bates, E. A., Johnson, M. H., Karmiloff-Smith, A., Parisi, D., & Plunkett, K. (1996). *Rethinking Innateness*. Cambridge, Massachusetts: MIT Press.

Frank, A. (1997, November). Quantum Honeybees. *Discover*, p. 80-87.

Gazzaniga, M. S. (1992). *Nature's Mind*. New York: BasicBooks.

Geschwind, N., & Galaburda, A. M. (Eds.). (1984). *Cerebral Dominance: The Biological Foundations*. Cambridge: Harvard University Press.

Geschwind, N., & Galaburda, A. M. (1987). *Cerebral Lateralization*. Cambridge, Massachusetts: MIT Press.

Gopnik, M. (Ed.). (1997). *The Inheritance and Innateness of Grammars*. New York: Oxford University Press.

Hildebrandt, S., & Tromba, A. (1996). *The Parsimonious Universe*. New York: Springer-Verlag.

Jackendoff, R. (1994). *Patterns in the Mind*. New York: BasicBooks (HarperCollins).

Jenkins, L. (forthcoming). *Biolinguistics: The Unification Problem*. Cambridge: Cambridge University Press.

Johnson, M. H., & Morton, J. (1991). *Biology and Cognitive Development*. Oxford: Blackwell.

Jusczyk, P. W. (1997). *The Discovery of Spoken Language*. Cambridge, Massachusetts: MIT Press.

Kauffman, S. A. (1993). *The Origins of Order*. Oxford: Oxford University Press.

Kepler, J. (1966). *The Six-Cornered Snowflake*. Oxford: Oxford University Press.

Lasnik, H. (1997). On the Locality of Movement: Formalist Syntax Position Paper. In *1996 Conference on Functionalism and Formalism in Linguistics*. University of Wisconsin, Milwaukee:

Leonard, L. B. (1997). *Children with Specific Language Impairment*. Cambridge, Massachusetts: MIT Press.

Malenkov, G. (1997), The Beauty of Disorder. *Science Spectra*, p. 44-51.

Marsden, J., & Weinstein, A. (1985). *Calculus*. New York: Springer-Verlag.

Miller, G. A., & Chomsky, N. (1963). Finitary Models of Language Users. In R. D. Luce, R. Bush, & E. Galanter (Eds.), *Handbook of Mathematical Psychology* (pp. 419-492). New York: John Wiley.

Ojemann, G. A. (1983). Brain Organization for Language from the Perspective of Electrical Stimulation Mapping. *Behavioral and Brain Sciences*,?(6), 189-230.

Peterson, I. (1988). *The Mathematical Tourist*. New York: W. H. Freeman.

Piattelli-Palmarini, M. (Ed.). (1980). *Language and Learning: The Debate Between Jean Piaget and Noam Chomsky*. Cambridge, Massachusetts: Harvard University Press.

Pinker, S. (1994). *The Language Instinct*. New York: William Morrow and Company.

Pinker, S. (1996). *How the Mind Works*. New York: W.W. Norton.

Poizner, H., Klima, E. S., & Bellugi, U. (1987). *What the Hands Reveal about the Brain*. Cambridge, Massachusetts: MIT Press.

Rice, M. L. (Ed.). (1996). *Toward a Genetics of Language*. Hillsdale, New Jersey: Lawrence Erlbaum Associates.

Scheller, R. H., & Axel, R. (1984). How Genes Control an Innate Behavior.

Scientific American, 250, 54-62.

Smith, N., & Tsimpli, I.-M. (1995). *The Mind of a Savant: Language Learning and Modularity*. Oxford: Blackwell.

Taylor, J. E., & Kahn, J. W. (1986). Catalog of Saddle Shaped Surfaces in Crystals. *Acta Metallurgica*, 34(1), 1-12.

Thompson, D. A. W. (1992). *On Growth and Form*. New York: Dover.

Van Der Lely, H. K. J., & Stollwerck, L. (1996). A grammatical specific language impairment in children: An Autosomal Dominant Inheritance? *Brain and Language*, 52, 484-504.

West, G. B., Brown, J. H., & Enquist, B. J. (1997). A General Model for the Origin of Allometric Scaling Laws in Biology. *Science*, 276(5309), 122-126.

Yemma, J. (1997, October 3). Survival of the theorists: Professors battle over Darwin's concept of evolution. *The Boston Globe*, p. 1.