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UNIFICATION IN BIOLINGUISTICS

Lyle Jenkins, Biolinguistics Institute, Cambridge, MA, USA

13.0 INTRODUCTION

Let's recall the meaning of unification. In physics unification has come to mean the synthesis of subfields. E.g., Newton is said to have unified terrestrial gravity and celestial mechanics, Maxwell unified electricity, magnetism and light and Glashow, Weinberg, and Salam unified electromagnetism and the weak interactions (the electroweak interactions), and so on.

Symmetry plays a - in fact - *the* fundamental role in unification in physics. As the physicist David Gross puts it:

Einstein's great advance in 1905 was to put symmetry first, to regard the symmetry principle as the primary feature of nature that constrains the allowable dynamical laws.

and

In the latter half of the 20th century symmetry has been the most dominant concept in the exploration and formulation of the fundamental laws of physics. Today it serves as a guiding principle in the search for further unification and progress. (Gross, 1996)

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Another physicist, Roger Newton, states the case even more strongly:

The ideal "theory of everything" might thus not be directly embodied in a set of equations, but in an all-encompassing symmetry principle from which the fields, the particles, and the dynamics would follow. (Newton, 2000)

Although the examples just given are from physics, the power of symmetry considerations extend to other natural sciences as well; e.g., chemistry. When researchers in organic chemistry talk of "Woodward Hoffmann rules," they are in essence talking about using symmetry considerations to predict the course of chemical reactions. Certain chemical reactions are said to be symmetry-allowed by the rules, while others are symmetry-forbidden.

Later on we will give some examples of symmetry considerations in biology, as well as biolinguistics. But first let us consider what is meant by unification in these fields.

The problem of integrating biolinguistics into the natural sciences has been termed the "unification problem" by Chomsky. In this case, of course, we are dealing with a biological system, with many additional levels of structure, a "complex system," as it is, in fact, sometimes called. For most biological systems it is convenient to break down such a unification problem into three areas of investigation:

- 1) Mechanisms (structure/function)
- 2) Development
- 3) Evolution

13.1 FOXP2 AND THE LANGUAGE GENE DISCUSSION

For some years now, there has been intensive study of the KE family, an English family exhibiting a hereditary disorder affecting speech and language (Jenkins, 2000). Last fall the gene underlying this disorder was isolated and identified as the FOXP2 gene, which is located on chromosome 7 (7q31) (Lai *et al.*, 2001):

The editors in *Nature Neuroscience* summed up the discovery of the FOXP2 gene as follows:

Ever since Chomsky suggested that humans have a 'language instinct', people have been debating the possible existence of genes that underlie our linguistic abilities. Now, in the first big triumph for the new field of 'cognitive genetics', such a gene has been identified. The data seem clear-cut, and the discovery has been greeted with justifiable excitement; the deficit seems specific to language, and unlike the weak associations that are common in behavioral genetic studies, this gene shows a strong Mendelian pattern of inheritance. (2001)

Where do we go from here? For one thing, the gene was found to code for a transcription factor; i.e., a protein that regulates gene expression by turning a gene on or off or otherwise modulating its activity. One line of investigation is to find out what other genes FOXP2 may

regulate, how it regulates these genes (turning them on or off, for example), and to determine whether any of these genes "downstream" are involved in speech and language in a more direct way.

It is also known that FOXP2 appears to act as a repressor in lung tissue (Shu *et al.*, 2001). The reader may be wondering what a putative "language gene," as it is sometimes referred to in the literature, is doing in the lung. The reason for this is that transcription factors like FOXP2 often act in a combinatorial fashion across different tissues.

What this means is that in cell division during development a new regulatory protein becomes expressed in one of the daughter cells, but not the other. This way only 3 regulatory proteins acting in combination are needed to produce eight different cell types. Combinatorial control is thought to be widespread as a means of eucaryotic gene regulation. For those familiar with linguistic models of language acquisition, it may help to think of parameter settings, whereby three binary parameters can specify eight different language types with respect to some structural feature. The idea then is that FOXP2 works in conjunction with certain factors in the lung to repress gene activity in the epithelium, whereas it might work together with other factors in the brain to regulate genes there that are directly (or indirectly) involved in speech and language.

Over the next few years Monaco's group plans "to identify the target genes that are being regulated by FOXP2 during neurogenesis." Moreover, they are collaborating [with Andy Copp's group at the Institute of Child Health in London in] in performing expression studies to determine when and where the gene is expressed in the developing brain." Finally, they are working with "Svante Pääbo's group at the Max-Planck-Institute in Leipzig on comparing the gene in primates and humans" (Enard *et al.*, 2002). For another use of such molecular techniques (zoo blots) to do multi-species studies of evolution, see (Jenkins, 2000).

The *Nature Neuroscience* editors sum up the prospects for these lines of investigation as follows:

Chomsky suggested that the structure of language, with its universal grammatical rules, is somehow embodied in our brains and in our genes. It now seems realistic to hope that these genes can someday be identified; the discovery of FOXP2 represents an encouraging first step toward that distant goal. (2001)

13.2 Emergentism

The study of the biology of language has been subjected to a barrage of unfounded criticism from a number of areas. Some of these criticisms appear to be directed at the foundations of the discipline, so it is important not to disregard them. From the point of view of unification we need to figure out whether or not there is anything being missed in our joint approach (including everything from LGB to Optimality Theory). One such criticism comes from Emergentism, the latest incarnation of Connectionism. In a 500-page edited tome called the

"Emergence of Language" Elman, Bates and others tout Emergentism as a "genuine" alternative to the "relatively large group of linguists who are willing to embrace a nativist view," although Chomsky is the only one specifically named (MacWhinney, 1999). Although "emergentism is also a much more difficult idea than either nativism or empiricism," the basic idea is that "outcomes can arise for reasons that are not obvious or predictable from any of the individual inputs to the problem."

But how is Emergentism a genuine alternative to the various programs in biolinguistics? No one engaged in minimalist investigations or optimality approaches, etc. denies that "outcomes can arise that are not obvious or predictable from any of the individual inputs to the problem." Chomsky, for one, hasn't denied the "Emergence of Language" (the title of the book), far from it. In fact, here is what he had to say about the matter in 1968:

This [human language] is an example of true "emergence" -- the appearance of a qualitatively different phenomenon at a specific stage of complexity of organization.

He stressed that this "poses a problem for the biologist", much as Philip Anderson had earlier stressed that the existence of emergent properties in physics, like superconductivity, posed problems for the physicist in his famous essay "More is Different."

According to Bates,

The emergentist alternative is committed to the idea that knowledge itself is not innate, but emerges across the course of development, through the interaction of innate architecture, innate timing, and input to the cortex. [p. 10]

Generative linguists are said to subscribe to "representational innatism." Mis-representational innatism might be a better term. What the authors have done is take the study of developmental biology, subtracted out architecture and timing constraints and then call the residue "representational innatism." They then ascribe this concoction to the "relatively large group of linguists who are willing to embrace a nativist view." Putting aside the fact that no linguists subscribe to the fiction called "representational innatism," let's take a look at the "genuine" alternative of Emergentism; in particular, what Elman calls the "conspiracy theory of language:"

Language is simply the result of a number of tweaks and twiddles, each of which may in fact be quite minor, but which in the aggregate and through interaction yield what appears to be a radically new behavior. It is in this sense that language is a conspiracy.

It appears to be based on two tenets, the first of which is:

The early view, for example, that complex behaviors might be directed by single genes, has given way over the past several decades to the realization that even apparently simple traits such as eye color reflect the coordinated interaction of multiple genes.

The technical name for the "coordinated interaction of multiple genes" is "polygenic inheritance." Here Elman has the history of genetics wrong. Although the Emergentists may have come to this "realization" over "the past several decades," it has been a known fact in genetics since around 1909. Nilsson-Ehle demonstrated polygenetic inheritance in wheat color at that time. There had been a debate then about how to reconcile discrete Mendelian characters with continuous phenotypes like height, intelligence, etc. Nilsson-Ehle settled the debate with his studies (Jenkins, 1975). A few years later Davenport suggested in a study of inheritance of eye color in humans that at least two genes were involved. Thus the idea that "simple traits such as eye color reflect the coordinated interaction of multiple genes" is not something "realized" in recent times, as Elman apparently thinks, but has been common knowledge in genetics for about ninety years.

The second tenet of the "conspiracy theory of language" is that "small alterations in developmental pathways can lead to very large differences in outcome." Again this is by now a well-documented property of biological systems. Cf. Jacob, who asked "what accounts for the difference between a butterfly and a lion, a chicken and a fly, or a worm and a whale." He concluded that it wasn't biochemical innovation, but new regulatory circuits:

The minor modification of redistributing the structures in time and space is enough to profoundly change the shape, performance, and behavior of the final product. (Jacob, 1978)

Chomsky explicitly noted the parallel between Jacob's formulation and the Principle and Parameters approach to language:

In a system that is sufficiently intricate in structure, small changes at particular points can lead to substantial differences in outcome.

Note that Elman's formulation here is nearly identical with Chomsky's formulation twenty years earlier (the words in brackets are from Elman):

..small changes [alterations] at particular points [in developmental pathways] can lead to substantial [very large] differences in outcome

We are hardly dealing with a "genuine alternative" here. Moreover, what Elman refers to as "tweaks and twiddles," Jacob once referred to as "tinkering" (*bricolage*) in the evolutionary context.

Summing up, the conspiracy theory of language doesn't represent an alternative to standard biolinguistics. Both polygenetic inheritance and developmental constraints are part and parcel of developmental biology and hence of biolinguistics. What Emergentists call "conspiracy," biolinguists call "biology." More important than the terminology, though, is the fact there exist a number of testable developmental models of language, with a wide coverage of the world's languages, based on the Principles-and-Parameters approach, but no comparable Emergentist model.

One final point -- to study emergent properties, it would be more profitable to study just about any other field of natural science -- condensed matter physics, for example – rathern than human language. Take, for example, superconductivity. Here the stages of organization, the inputs and the outputs, are much better understood. Superconductivity can be technically characterized as the breaking of the electromagnetic gauge invariance (Weinberg, 1992). The theories of electromagnetism, of symmetry breaking and of gauge invariance are all well defined. Here we know the rules to get from one level to another so that we can meaningfully ask in what sense superconductivity is an emergent phenomenon. In human language, on the other hand, there are gaps, not to mention huge holes, in our understanding of the representation of language in the brain, in language genetics, and in the evolutionary history of human language. To study emergent properties, it makes more sense to look at problems in simple systems where a lot is known, not in complex systems, in which so little is known. For the same reasons, to study the laws of motion, Galileo made the reasonable decision to roll balls down inclined planes, not cats.

As for innateness, it is important not to get hung up on definitions. Otherwise, the result is a definitional game, in which innateness is defined in a way that corresponds to nothing anyone believes in, as when Emergentists define "representational innateness." Concepts evolve over time. The notion of "atom" meant different things to Rutherford, Bohr, Heisenberg and Schroedinger, as quantum theory was developed. Likewise innateness has meant different things to successive scientific generations. It is useful to think of innateness as a shorthand for a network of mechanisms which gets spelled out in ever greater detail, starting with Mendelian factors, polygenic inheritance, the one gene-one enzyme hypothesis, the discovery of DNA's role in inheritance, the Central Dogma, promoters and repressors, split genes, alternative splicing, tensegrity, and on up to cells, tissues, organs, and the dynamics of complex networks. We don't need to define innateness. In the final analysis, Nature tells us what it means.

13.3 RETHINKING THE NATURE-NURTURE DEBATE

In recent work Karmiloff and Karmiloff-Smith (K&K-S) propose "rethinking the naturenurture debate" in language (Karmiloff and Karmiloff-Smith, 2001). To judge from K&K-S, the nature-nurture debate appears to be a hotly contested issue in psychology (they dedicate the final chapter to it), as is the "debate" about innateness (cf. the recent volume "Rethinking Innateness") (Elman *et al.*, 1996). However, in biolinguistics and, more generally, in biology, there is no longer any debate about these issues – they have long since been laid to rest by everyone (with the possible exception of the media). There is simply no debate about whether the fruit fly *Drosophila* or the Chinese language is genetically determined *or* environmentally determined – both genetics and the environment play critical roles. Furthermore, this has been understood in the biological sciences for decades. Ironically, K&K-S concede as much in one passage, in the case of biolinguistics:

These and other unique features of grammar have led Chomsky and his disciples to make claims about the innateness of language. Everyone agrees that there must be some innate component to the human capacity to learn grammar, because we are the only species capable of it. And everyone agrees that children need linguistic experience to learn the actual words that make up their particular native tongue. (Karmiloff and Karmiloff-Smith, 2001:105)¹

So "everyone agrees" on the need both for "some innate component" and for "linguistic experience." End of the nature-nurture debate in biolinguistics.

However, K&K-S vacillate about the "debate" elsewhere in the book. They find the "nature versus nurture dichotomy is not a useful one" (p. 6) and correctly note that "it is not a question of nature or nurture; rather it is about the intricate interaction between the two" (p. 7). But then several pages later they change their minds and elevate the "debate" to the "crucial nature-nurture debate" and even dedicate the concluding chapter of the book to the subject (*Rethinking the Nature-Nurture Debate*). However, the "debate" can't be both "not useful" and "crucial" at the same time. But whatever they imagine the debate to be, it is a one-sided debate limited apparently to psychology, since as they noted in the citation above, everyone agrees² that there are innate and environmental components to language, so there is nothing to "rethink."

But let's take a look at what the authors conclude after "rethinking the nature-nurture debate." In summing up this section (and the book as a whole), they argue that what is needed is a *truly developmental perspective* of language [italics is ours]:

By placing an ever-greater emphasis on seeing how brain pathways gradually change over time, rather than looking at the adult end state, we will finally be able to chart the full development of language acquisition from fetus to adolescent. (p. 225)

If we omit the phrase "rather than looking at the adult end state," this statement would be completely innocuous, though hardly novel. But we have to ask why anyone would propose that studying earlier developmental stages is an alternative to studying the last stage (the "adult end state"). No biologist would propose that the study of the developmental stages of Drosophila's courtship song is an alternative to the study of song in adult flies.

The authors' peculiar formulation of the "developmental perspective" appears to stem from serious misunderstandings of the literature on universal grammar and language acquisition. For example, in a discussion of various models of language acquisition, they refer to "Chomsky's deterministic view of language acquisition," which "sharply contrasts" with the model under discussion:

Chomsky considers adult end-state grammar to be the result of evolution and not of progressive language learning. [p. 139]

¹ There are many better arguments for a genetic component to language than "because we are the only species capable of it," but these are omitted here, since K&K-S are conceding the point anyway.

² And "everyone" here includes all of the approaches they discuss in their book ("structural approaches," "bootstrapping approaches," "sociopragmatic approaches," "cognitive approaches," "processing approaches," "construction-based approaches," and "network models").

The authors appear to totally confuse language development with language evolution. We are talking about development here, not evolution; as the authors say, the time span "from fetus to adolescent", not evolutionary time spans of hundreds of thousands and millions of years. The acquisition of adult grammar, e.g., learning nouns and verbs, results from development, not evolution. No one in the literature has taken the absurd position that vocabulary learning takes place not by "progressive language learning," but instead by "evolution." In fact, earlier in the very same chapter, the authors admit as much:

While experience of a specific language is necessary for learning words and for setting the innately specified parameters, nativist theories regard experience as secondary to the grammatical capacities that they claim were created by evolution. (p. 110)

Summing up, there is no "nature-nurture" debate in biolinguistics, despite what appears from time to time in the media and in some parts of the psychological literature. Every approach to biolinguistics recognizes the critical role of both internal mechanisms and of environment.³ For similar reasons, it makes no sense to talk about a "nativist" approach based on notions of "innateness" standing apart from other approaches.

It is of interest to note that biologists have a much easier time comprehending the implications of work in biolinguistics than do many linguists or psychologists. For example, in a recent discussion of human behavior, *The Splendid Feast of Reason*, the biologist Jonathan Singer points to language acquisition as a "useful paradigm for other kinds of human behavior" (Singer, 2001). He notes that common sense might suggest that "language acquisition is the quintessential example of a behavior that is entirely attributable to nurture, to environmental influences" and that "enormous diversity" of languages might make a "common genetic basis for language" seem "implausible." Nevertheless,

...a revolution started 50 years ago by Noam Chomsky has led to the widely accepted view that there is indeed a profound genetic basis for language acquisition in humans....Although the details of these arguments are complex, the manifesto of the Chomskian revolution is essentially that the human brain—structured by the actions of the genes via their protein products—somehow encodes for a "universal grammar" that is intrinsic to all human languages, however much specific languages may differ.

Singer notes that this implies that "these genes...apparently program for the construction of a neural network in the brain into which the elements of specific languages are expeditiously incorporated by each of us."

Once we substitute the idea of "innate" in K&K-S with more current terminology such as "genes" that "program," it can readily be seen that all approaches to the biology of language are "nativist" in this sense. Secondly, the term "developmental approach" is a misnomer when

³We omit consideration here of *tabula rasa* theories, which apart from being empirically untenable, no one publicly espouses.

applied to biolinguistics. All approaches to the study of the biology of language study development; i.e., language acquisition.⁴

13.4 LIEBERMAN'S CRITIQUE OF THE BIOLINGUISTIC APPROACH

In the preface to *Eve Spoke: Human Language and Human Evolution*, Philip Lieberman announces the following objective:

Noam Chomsky's theories concerning human language and the organization of the human brain also fail to fit with these new insights into the nature and evolution of the biological bases of human language. I will discuss some of Chomsky's claims and present data that refute them. (Lieberman, 1998:xv-xvi)

However in many cases, Lieberman doesn't even bother to present what he calls "Chomsky's claims," but instead presents his own caricature of these claims, which he then proceeds to "refute." As we will see, it is not work on biolinguistics that Lieberman is attacking. Most linguists wouldn't recognize Lieberman's distortion of their work. For example, Lieberman holds that work on Universal Grammar is a "worldwide religion," a belief system with "disciples," citing unnamed sources:

Chomsky's disciples (some of his leading advocates often refer to the theory as a worldwide religion) believe that a "universal grammar" is genetically encoded into every human brain. (Lieberman, 1998:10-11)

It is well-established in biology that the physical structure and behavior of organisms are determined by the the interplay of genetics and environment. Apparently the idea that the same holds for language is regarded by Lieberman as religious dogma. Lieberman attacks work on the biology of language in a number of areas, but these can be broken down for convenience of exposition into three major categories: 1) language, 2) development, and 3) evolution.

13.4.1 On Language

Lieberman's argument against an "innate universal grammar" is nearly incoherent:

Paradoxically, the primary evidence that Chomskian linguists cite to support their claim that an inborn universal grammar exists is the failure of the algorithmic approach similar to digital computer programs. [p. 125]

⁴ Of course, a particular study of language might focus primarily, or even exclusively, on development, evolution, etc., but the study of development is a central part of the subject matter of every approach to the biology of language.

The reference here to the "algorithmic approach" is presumably based on Lieberman's misconception that work in generative linguistics derives from "digital computer programs:"

Moreover, for the past thirty years the linguistic study of language has been dominated by Noam Chomsky at MIT. Superb neurophysiologists who understand that brains are not the biological equivalents of digital computers work at MIT. However, Chomsky's model of the brain still seems to be based on the digital computers that were being developed at MIT when he first developed his linguistic theories in the mid-1950s. (Lieberman, 1998:99)

Lieberman has it exactly backwards. It was Lieberman's field of speech analysis (among others) that was swept over by euphoria for digital computer models, not generative (bio)linguistics. Compare the following skeptical comments from Chomsky himself:

The technological advances of the 1940's simply reinforced the general euphoria. Computers were on the horizon, and their imminent availability reinforced the belief that it would suffice to gain a theoretical understanding of only the simplest and most superficially obvious of phenomena--everything else would merely prove to be "more of the same," an apparent complexity that would be disentangled by the electronic marvels. The sound spectrograph, developed during the war, offered similar promise for the physical analysis of speech sounds. The interdisciplinary conferences on speech analysis of the early 1950's make interesting reading today. There were few so benighted as to question the possibility, in fact the immediacy, of a final solution to the problem of converting speech into writing by available engineering technique. And just a few years later, it was jubilantly discovered that machine translation and automatic abstracting were also just around the corner. (Chomsky, 1968:3)

The skepticism about digital computers in the 50's and 60's was in generative linguistics, not in speech analysis, at least at MIT, as Lieberman should remember, since he was there during that era. Chomsky goes on to say:

Correspondingly, there is no reason to expect that the available technology can provide significant insight or understanding or useful achievements; it has noticeably failed to do so, and, in fact, an appreciable investment of time, energy, and money in the use of computers for linguistic research--appreciable by the standards of a small field like linguistics--has not provided any significant advance in our understand of the use or nature of language. These judgments are harsh, but I think they are defensible. They are, furthermore, hardly debated by active linguistic or psycholinguistic researchers. (Chomsky, 1968:4-5)

Once we replace Lieberman's misleading reference to "the algorithmic approach similar to digital computer programs" with the phrase "generative linguistics," we have the following claim:

The primary evidence that Chomskian linguists cite to support their claim that an inborn universal grammar exists is the failure of generative linguistics.

That is, Lieberman's claim about linguists is analogous to the following claim about chemists:

The primary evidence that chemists cite to support their claim that atoms and molecules exist is the failure of chemistry.

The claim about linguistics is as false as the claim about chemistry, but it does appear to be what Lieberman is saying, for he spells out the claim in more detail as follows, calling it "the linguistic argument:"

Premise 1: "...generative linguists have failed to adequately describe any language."Premise 2: "...children acquire language."Conclusion: "...the principles underlying language must therefore be innate."

Lieberman claims that linguists make this "linguistic argument" although no one would make the following argument:

Premise 1:	"Thousands of experts have been unable to make industrial robots as adaptable as human beings."
Premise 2:	[Humans can bolt bumpers onto cars.]
Conclusion:	"The instruction set for bolting a bumper in place on a Toyota is innate."

Note that Lieberman provides no citation or other reference to what he calls "the linguistic argument," which he appears to attribute (in the next sentence) to the "Chomskian position." Moreover, he doesn't even bother to address the standard argument for "innate universal grammar" given by linguists over the last forty years; viz., the well-known "argument from poverty of stimulus," or the huge literature on language typology and acquisition. So we could stop here and dismiss the "linguistic argument" as Lieberman's invention. However, it is of interest to follow his incoherent argument a little further to see where it is headed.

In an effort to establish the (alleged) premise 1: "...generative linguists have failed to adequately describe any language," Lieberman presents a history of generative linguistics, which he calls "toy linguistics," that is as fanciful as his claim about generative linguistics originating from digital computers.

Over the past forty years it has become apparent that linguists following Chomsky's principles have produced a sort of toy linguistics. Despite decades of intensive effort only a small subset of the sentences of any language can be described by means of syntactic rules. These sentences typically are the examples presented to demonstrate the power of this algorithmic method in introductory courses and expository texts such as Jackendoff's book and Steven Pinker's 1994 book, *The Language Instinct*. As the linguistic corpus expands the number of putative rules begins to approach the number of sentences. The rules of grammar become torturously complex and ultimately fail.

When one reads an assessment as wide of the mark as this, one must charitably assume that Lieberman has been out of touch with work in linguistics for some decades. This impression is reinforced when we find that Lieberman's evidence for the alleged "failure" of generative linguistics rests solely on a single study of a fragment of Parisian French:⁵

Charles Gross in 1979 showed that the algorithmic approach failed for a fragment of the grammar of Parisian French; subsequent failures have been unreported. (Lieberman, 1998:126)

Lieberman appears to believe that there is a conspiracy afoot in linguistics to hide any failures from public view. In linguistics, as in any science, it is standard procedure to discuss [include] data that the theory can handle in the same article as the data that the theory cannot handle, in the hope that other researchers working on the problem can gain greater understanding. In the particular article referred to here, *The Failure of Generative Grammar*, there was no "failure" reported, despite the apocalyptic title. Rather, Gross made the uncontroversial observation that in linguistics, as in biology or physics, there are always exceptions to theories and, similarly, data that remain unaccounted for. He concluded that one can sidestep this intrinsic property of science, by abandoning the search for explanation and by restricting oneself to data collection and description. But this is his, or any other scientist's, personal decision, and has no further consequences for the problems of biology of language. What Lieberman proposes in place of empirically well-supported theories of Universal Grammar (his "toy linguistics") is that humans acquire languages with the same associative principles that pigeons and dogs use to learn. He neither specifies these principles nor does he show how a single linguistic property in a single language follows from them. We return to this proposal again below.

As the only support of his claim that "subsequent failures [of the algorithmic method];" i.e., of generative grammar, have been "unreported," Lieberman provides the following footnote:

MIT's alumni magazine, *Technology Review*, for example, announced in 1990 a major project that would make use of Chomsky's most recent research to implement a language translating system, but no progress on the project was subsequently reported. (Lieberman, 1998:160, fn. 22)

There does not appear to be any such announcement in any of the issues of the *Technology Review* for 1990, but quite possibly Lieberman is referring to the October, 1991, issue of the magazine, which contains the article "Language Busters," which discusses research in computational linguistics in the Artificial Intelligence Laboratory at MIT in the early 1990's, in particular work on principle-based parsing (Berwick *et al.*, 1992) and language translation systems by Robert Berwick, Sandiway Fong, Bonnie Dorr, and Michael Kashket, among others (Horning, 1991). However, all of the systems discussed were subsequently implemented and were the subject of doctoral theses that were later completed, as well as of other articles in the computer science literature. Lieberman would have immediately discovered this for himself,

⁵ Presumably Lieberman means the linguist Maurice Gross, not Charles Gross, who works on object detection in macque monkeys.

had he taken the time to consult the scientific literature, instead of his alumni magazine. In fact some of these have been available for some years for downloading free of charge for research purposes; e.g., Fong's PAPPI system, Dorr's UNITRAN system, etc. Far from being "failures" that went "unreported," this work has contributed to an explosion of interesting and important work in the field of computational linguistics.

13.4.2 "Everest Linguistics"

Numerous examples of modularity from language and the brain sciences in general have been reported, which have motivated the widely-accepted assumption that the brain is made up of specialized brain mechanisms. However, Lieberman ridicules the idea that language is modular, comparing the idea to phrenology:

Phrenologists tried to relate bumps on the skull to very specific aspects of behavior, such as avarice, honesty, or mathematical ability. Phrenology has been dismissed as quack science–bumps on the skull do not correspond to expected behavior–but its essence survived in Broca's work and, more recently, in Chomskian "modular" brain theory. (Lieberman, 1998:102)

What makes this attack on modularity irrational, is that Lieberman advocates brain modularity for the area in which he himself does research:

The complex ballet constantly performed by the muscles of our speech anatomy--our lips, tongue, vocal cords, and so on--is choreographed by specialized brain mechanisms that also appear to make complex human thought possible. (Lieberman, 1998:4)

The implication here is clear. The assumption of modularity; i.e., "specialized brain mechanisms," is fine for Lieberman, who is doing real hard-headed science on the lips and the tongue. But modularity is off limits to others in the cognitive sciences–they must be doing quack science–bumps on the skull and the like. This position is irrational enough. But Lieberman even manages to top this by proposing what must rank as one of the most misguided experiments ever proposed in the cognitive sciences. Lieberman mounts an expedition to the top of Mount Everest to test (and, he claims, disconfirm) the modularity thesis for language.

Lieberman's proposed "test" of the modularity thesis is to let people climb 24,000 feet up Everest (40% less oxygen at the top) and then give them a "battery of speech, syntax and cognitive tests:"

If speech and cognition were controlled by distinct parts of the brain, as linguists and neurologists had long believed, the climbers' mental sluggishness and their speech problems would increase at very different rates. On the basis of recent advances in brain imaging and linguistic theory, however, we expected a different outcome. The same neural circuits that control the tongue, the larynx and the lips during speech are implicated in complex thought, we had come to believe. But was that theory right?

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Would the climbers' speech and thought deteriorate in near lockstep, all the way up the mountain?" (Lieberman, 1997:23)

Lieberman finds that voice-onset time decreases; i.e., "pat" becomes "bat" and "god" becomes "cod" and also that "by the time they reached the upper camp, they needed 54 percent more time to understand sentences easily grasped by any six-year-old." His conclusion is summarized in the subtitle to the article: "In the thin air of the Himalayas, linguists discover that speech and thought are inextricably linked." Lieberman takes this experiment to demonstrate the demise of the modularity thesis.

But Lieberman refutes this bizarre line of argument himself: "The climbers' brains and the rest of their bodies, also slowed to a crawl." If this is so, why haven't we linguists also discovered that speech and walking [cell metabolism, etc.] are "inextricably linked?" Why wasn't this conclusion published in the *Nature* article, where Lieberman published the data? But, of course, Lieberman could not have slipped this faulty argumentation past the *Nature* reviewers. A reviewer would want to know why a Mount Everest expedition is needed to test the modularity thesis. The same thing could have been done a whole lot more safely (114 have died on Everest) and in a more controlled fashion in Lieberman's lab at Brown University.

But by then it would have occurred to the reviewer that this experiment is surely ill-designed to study modularity questions. Released from ethical constraints, a neurologist from Mars studying modularity would ask what happens if tissues are supplied differentially with oxygen. Such a researcher wouldn't shut down the oxygen supply to the whole brain. This would be tantamount to doing electrical brain stimulation experiments by electroshock therapy. Also, given that people on Everest suffer "excruciating headaches, vomiting and disorientation...and when asked to walk in a straight line...weave and veer like drunkards," these might not be the optimal conditions to quiz speakers on linguistic constraints from German and Bulgarian and the like. However, Lieberman insists, paradoxically, that "intellectually...the Everest expedition was a highpoint in my career," more so than his thirty years of work on "infants, children, 'normal' adults, dyslexic adults, elderly people and patients suffering from brain damage" and work on "fossilized skulls of human ancestors, as well as the anatomy and behavior of monkeys, chimpanzees and apes."

13.4.3 On Development

As mentioned before, Lieberman proposes abandoning all empirically well-supported theories of Universal Grammar in favor of the unfounded speculation that language learning proceeds "by making use of the associative cognitive processes that allow pigeons, dogs, and humans to abstract general principles from specific occurrences." That is to say, Lieberman proposes a language acquisition device which takes linguistic data as input ("specific occurrences") that are then analyzed by "associative cognitive processes." Finally, these yield as output "abstract general principles."

Compare Lieberman's proposal with a testable proposal about language; e.g., Baker's work on languages; more specifically, on noun incorporation and other phenomena in Iroquoian languages (Baker, 1996). Baker has exhibited the kinds of knowledge that Mohawk speakers have and how this knowledge might in part follow from universal principles ascribed to the initial state. We can test whether or not the grammatical knowledge of the Mohawk speaker follows from the instantiation of Baker's principles. Since Baker has specified the knowledge and the principles, Lieberman at least has an opportunity to refute Baker. Lieberman, on the other hand, has not told us what "abstract general principles" he is claiming that Mohawk speakers have nor does he tell us what the "associative cognitive processes" are that "pigeons, dogs, and humans" have that yield the alleged principles. In this sense, Lieberman is immune from refutation. Moreover, had Lieberman taken the trouble to formulate any "abstract general principles" for any language, he might well have discovered for himself that "associative cognitive processes" have proven to be woefully inadequate for the task, at least for those associative models of language that are explicit enough to test.

In a discussion of the article on word segmentation by Saffran *et al.* (1996), Lieberman claims that the authors showed that "no innate knowledge of possible word structure was necessary," rather, "general associative learning sufficed" (Lieberman, 1998:130). What Saffran *et al.* actually concluded was that "some aspects of early development" may result from "innately biased statistical learning mechanisms" (p. 1928), an idea which had already been proposed in the earliest work on generative linguistics (in Chomsky's *The Logical Structure of Linguistic Theory*), and later on, in other work; see Pinker (Pinker, 1997). Note also that the reference to "biased" in "innately biased" is superfluous, since the authors haven't told us how it adds anything to the more traditional terminology, "innate statistical learning mechanisms." But whatever the distinction is thought to be, Saffran *et al.* make clear that innateness is involved in both cases:

Innate biases in statistical learning may be different in important ways from innate knowledge of linguistic principles. But both of these implementations involve types of innateness. (Saffran *et al.*, 1997:1181)

Hence Lieberman, who agrees with the empirical results of the study, apparently also accepts "innate(ly) biased" mechanisms.

So do Bates and Elman (1996), who wrote the commentary that accompanied the article by Saffran *et al.* They note that "even if we assume that a brain (real or artificial) contains no innate knowledge at all, we have to make crucial assumptions about the structure of the learning device, its rate and style of learning, and the kinds of input that it 'prefers' to receive." In other words, one is ultimately forced to postulate some innate mechanisms. And, in Elman and Bates' response to a series of letters about the word segmentation study and the commentary, they make the same point clear: "the central debate in our field is not about innateness per se, it is about the nature of this ability" (Elman and Bates, 1997), a point that was also explicitly made throughout *Rethinking Innateness* (Elman *et al.*, 1996).

Lieberman also states that "Bates and Elman note that the demonstrated power of associative learning of human infants in this linguistic task obviates the need for a Chomskian innate language organ that specifies the details of syntax" (p. 160, fn. 25). Here Lieberman is misrepresenting both Bates and Elman, as well as Saffran *et al*. The experiments performed by Saffran *et al*. concerned word segmentation, not acquisition of syntax, as they themselves make clear:

A second question concerns how a statistical mechanism might apply to the acquisition of syntax. Pinker assumes that an extension from words to grammar would involve using the same sequential statistic; he then argues that this statistic is insufficient to capture the nature of grammar. We agree. (Saffran *et al.*, 1997)

Nor do Bates and Elman claim that the results on word segmentation "demonstrate" anything about syntax. As Saffran *et al.* correctly note,

In contrast, Bates and Elman assume that infants can perform a range of statistical analyses, and they express confidence that, somewhere in the mix, such capabilities will be sufficient.

But the fact that Bates and Elman "assume" and "express confidence" that something, "somewhere in the mix," can "capture the nature of grammar" does not constitute a demonstration, as Lieberman seems to believe.

Lieberman refers to "the Chomskian claim that a language gene produces an identical universal grammar in all 'normal' human beings" (p. 161). In the section entitled "The Language Gene," Lieberman says that it is even alleged that "evidence for a 'language gene' has been found that 'proves' Chomsky's theory," the evidence being based "studies of Myrna Gopnik and her colleagues at McGill University, which have been cited by Chomsky and his advocates." Lieberman provides no citation from such studies, because there are none, much less any that claim that a language gene "'proves' Chomsky's theory." In this regard, note that Fermat's Last Theorem can be proved, but not linguistic theory, nor the theory of general relativity for that matter. The notion that work on genetic language disorders is motivated by a search for "the language gene" to "prove" linguistic theory in a way not even possible in as successful a physical theory like general relativity is a fantasy. Apart from some of the popular press perhaps, talk of the "language gene" is found only here in Lieberman's work as well in some connectionist work. Gopnik and her colleagues, on the other hand, have always carefully qualified their statements.

13.4.4 On Evolution

Turning now to evolution, we note that Lieberman makes the following claim(s):

Chomsky once categorically stated that human language couldn't have evolved by means of the processes that Charles Darwin proposed in his modestly entitled book *On*

the Origin of Species. Chomsky has recently retreated from that stance, but we will see that his version of the biology and evolution of human linguistic ability is not consistent with the general principles of evolutionary biology and studies of the brain bases of language and speech that we'll discuss.

As to the first (historical) claim, there was no such categorical statement made by Chomsky (and consequently no retreat). We have reviewed Lieberman's charges that standard biolinguistic theories of language and of language development are "inconsistent" with biology. In each case, we have shown his objections to be unfounded. At the same time we have argued that his own proposals, while not inconsistent with biology, are empirically vacuous.

In conclusion, the glaring gap in Lieberman's discussion of the biology of language is that he has not come to grips with the fundamental question of the discipline, "What constitutes knowledge of language?" Or, what does it mean to say that "John knows English (Bulgarian, Mohawk, etc.)? In short, one cannot seriously discuss the "biology of language" without characterizing "language." It is not enough to deride forty years of linguistic scholarship (not to mention two thousand years of previous work) as "toy linguistics." If Lieberman believes that pigeons, dogs and humans can derive the "abstract general principles" of Bulgarian and Mohawk from "specific occurrences," then, for starters, he has to reveal what those "abstract general principles" are. Recall that he has thrown out the "algorithmic approach" (his term) which has been used with such success by linguists to analyze hundreds of constructions in thousands of languages over the past fifty years. Until Lieberman lists these "abstract general principles," we can't even get the discussion off the ground. In particular, it is impossible to judge whether these alleged principles are modular (or not); e.g., whether they are subsystems distinct from vision, music and mathematics or speech articulation and perception.⁶

An additional problem was seen to arise for Lieberman in connection with questions of language development or acquisition. First, since he hasn't spelled-out the "abstract general principles" for English or Japanese, he has no way of finding out whether the "associative cognitive processes" that pigeons, dogs, and humans allegedly have can compute those principles. Secondly, he has failed to specify what the "associative cognitive processes" are that yield the principles. So Lieberman's language acquisition theory is doubly untestable. Or to put it another way, since he didn't face the first question 1), "what constitutes knowledge of language?", he has no way of deciding the question, "how is knowledge of language acquired?" either positively or negatively. Universal Grammar is a testable theory with consequences for many languages, hence part of normal science. Lieberman's untestable system, while not a "worldwide religion," does appear to be a belief system held by one person.

Finally, Lieberman has a third problem with respect to the question of "how language evolved (in the species)." He is interested in how human language "evolved by means of the processes

⁶ We have already noted earlier that Lieberman is a firm believer in modularity in his own work; compare also his reference above to "the complex ballet...choreographed by specialized brain mechanisms."

that Charles Darwin proposed." But again, since we don't know what either the "abstract general principles" or the "associative cognitive processes" are, we have no way of figuring out how they could have evolved. On the other hand, we have seen that research in biolinguistics has provided in depth studies of a wide variety of languages so that both questions of universality and variation, along with questions of evolutionary design can be and are being fruitfully investigated.

13.5 BEYOND EXPLANATORY ADEQUACY

Chomsky recently posed the question whether we might "seek a level of explanation deeper than explanatory adequacy, asking not only what the properties of language are, but why they are that way" (Chomsky, 2001), comparing these properties to those of a snowflake in another context (see Chapter 15). At least since the time of Kepler, scientists have wondered about the mysterious pattern formation seen in the snowflake:

There must be some definite cause why, whenever snow begins to fall, its initial formations invariably display the shape of a six-cornered starlet. For if it happens by chance, why do they not fall just as well with five corners or with seven? (Kepler, cited in (Stewart, 2001)

What kinds of principles might guide us in answering questions about complex systems from the snowflake to human language? We have already noted that very general principles in the physical world such as symmetry and optimality constrain the dynamics of physical systems to account for what appear on the surface to be disparate phenomena.

E.g., not only did the formulation of Maxwell's equations unify the phenomena of electricity, magnetism and light, but also symmetry properties of these equations led directly to relativity theory (Lorentz invariance) and modern field theory (gauge invariance).

Stewart notes that the process of snowflake formation is a pretty intricate system in itself:

is the process a phase transition? Yes. Is it a bifurcation? Yes. Is it symmetry-breaking? Yes. Is it chaos? Yes. Is it a fractal? Yes Is it a complex system? Yes.

(Stewart, 2001)

Symmetry and optimality may also restrict the form of biological systems and thereby have a unifying effect; examples are pattern formation (e.g., spirals in sunflowers), synchronous oscillations, visual hallucinations, speciation, locomotion of microorganisms, gaits, flocking, navigation, the structure of the genetic code (supersymmetry of matter and force), the ability of

a cat to land on its feet (Falling Cat Theorem), a striking case of unification (gauge symmetry). As an example from biology, let us take the case of speciation.

13.6 Symmetry Breaking as an Origin of Species

Stewart, Cohen and Elmhirst have proposed a new way of looking at the problem of the formation of species using insights gained from the modern study of dynamical systems; the presentation here is based on (Stewart, 2001); for technical details, see Golubitsky and Stewart (2002). In the view of Darwin (*On the Origin of Species*), 13 species of finch evolved on the Galapágos Islands from a common ancestor by gradual changes (e.g., in beak length) brought about by natural selection. Similarly, some 5 or 6 million years ago humans diverged from a common species. Stewart notes several problems with this picture. First:

Once, about five million years ago, the distant ancestors of chimpanzees and humans were all part of a single species. Today they are not. How did the species diverge? Was Darwin right to think it was simply a gradual drifting apart? But if there was a selective advantage in changing, why didn't they all drift in that advantageous direction? Why did some go the other way?

Secondly, species are organisms that can interbreed. In order for species to diverge, and to keep the genes from mixing, something must stop them from interbreeding. Ernst Mayr proposed that now and then a group of organisms becomes isolated from the rest of its species by a barrier, like a mountain range, and is then able to evolve separately. This idea is called *allopatric speciation*.

However, some cases of speciation do not fit well into this pattern. So other researchers have considered mechanisms that do not require organisms to be split by geography (*sympatric speciation*). These mechanisms consider speciation to be *symmetry-breaking bifurcations*. One such mechanism is sexual selection. Another is that proposed below by Stewart and colleagues.

In the traditional view of speciation, the system changes gradually – "continuous changes produce continuous effects," as Stewart puts it. However, this only happens when "stable states remain stable." However, if an external parameter that the system depends on hits a critical value, the change can be sudden:

Bifurcations occur when the state of the system changes from being stable to being unstable; the system then seeks a new stable state, which may mean a big change.... Symmetry breaking is a particular type of bifurcation behavior, found in symmetric systems.

The mathematical models of Stewart et. al. make some surprising predictions, the first being that "this kind of speciation event is very sudden quite unlike Darwin's gradual accumulation of tiny changes." The second prediction is that

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the two species "push each other apart," away from the original common body plan. If a species of birds with medium-sized beaks splits into two, then one group has shorter beaks, the other has longer ones, and suddenly there are very few birds occupying the old middle ground.

The role of selection in this example is that it removes the hybrids (the "middle ground") with the medium size beaks (perhaps the medium size seeds are no longer available in sufficient quantity).

Summing up, the model of speciation of Stewart et. al. is modeled at the level of the phenotype, not the genotype, and predicts sudden speciation events ("punctuated equilibrium") with discontinuous bifurcations.

13.7 LANGUAGE AS A COMPLEX SYSTEM

From the earliest work on grammar in biolinguistics, there has been an effort to understand the invariant or universal properties of language. E.g., Chomsky notes that syntactic theory must be formulated in a "structure-dependent" fashion so that operations such as the one in English that inverts subject noun phrases and verbs ("Is that person here today?) are permitted in universal grammar, but not operations that reflect a string or interchange odd and even words (Chomsky, 1965). Moreover, invariants of a topological character such as nesting and connectedness in tree graphs have been studied. This shift of focus to invariants or universals is conceptually similar to the shift that occurred in physics when Einstein made symmetry "the primary feature" of study (see citation earlier). In the case of physics one is searching for symmetry operations that leave the system (the laws of physics) unchanged (invariant). Such operations include the space and time translations and rotations and movement with a uniform velocity. In the case of language one seeks the grammatical operations, which leave the biolanguage invariant. These operations include the structure-dependent operations just discussed among many others.

Another example is the work of Nowak, Komarova, Niyogi and colleagues who, in a series of papers analyze language acquisition and evolution by studying the dynamics of populations of speakers, using (in part) symmetry considerations of the kind discussed earlier. The work assumes built-in grammar mechanisms:

Children must have a built-in sense of what grammar is. The linguist Noam Chomsky called this innate mechanism universal grammar. It is written in our genes and generated by neuronal circuitry in our brains. (Nowak, 2000)

Building on the work of many others, Nowak *et al.* assume for the case of language acquisition that Universal grammar (UG) contains two parts: 1) a rule system that generates a "search space" of candidate grammars, $\{G_1, G_2, ..., G_n\}$ and 2) a "[learning] mechanism to evaluate input sentences and to choose one of the candidate grammars that are contained in his [the learner's] search space." One of the main questions to be determined is "what is the maximum

size of the search space such that a specific learning mechanism will converge (after a number of input sentences, with a certain probability) to the target grammar."

The question for language evolution then is "what makes a *population* of speakers converge to a coherent grammatical system." A homogenous population (all individuals have the same UG) is assumed. Nowak *et al.* derive the following set of ordinary differential equations, which they call the "language dynamics equations" which give the population dynamics of grammar evolution:

$$\frac{dx_{j}}{dt} = \sum_{i=1}^{n} f_{i} q_{ij} x_{i} - \phi x_{j}, \ j = 1, \dots, n$$

Figure 1 - The language dynamics equations. (Nowak and Komarova, 2001)

We will not go into detail here, but will mention only that f_i represents "the average payoff [for mutual understanding] for all those individuals who use grammar G_i " and contributes to biological fitness (the number of offspring they leave). The q_{ii} "measure the accuracy of grammar acquisition" of the offspring from their parents. The x_i denotes "the relative abundance of individuals who use grammar G_i ." The variable ϕ denotes the "average fitness or 'grammatical coherence' of the population, "the measure of successful communication in a population."

Nowak *et al.* use the language dynamics equations to study the conditions under which UG will result in "grammatical coherence." A number of factors can be varied in order to run computers simulations: population size, assumptions about UG's search space, about the learning mechanism (e.g., "memoryless" or "batch" learning, etc.). One can also model questions in historical linguistics. Again symmetry and stability (stable and unstable equilibria) are useful in the study of the language dynamics equations, as they were in the example of origin of species discussed earlier.

13.8 CONCLUSION

Some years ago Chomsky made a distinction between problems, which "appear to be within the reach of approaches and concepts that are moderately well understood," and mysteries, that "remain as obscure to us today as when they were originally formulated" (Chomsky 1975:137).

This theme was recently taken up by Horgan in *The End of Science*, in which he raised the question to Chomsky as to whether the sciences, including the social and neurosciences sciences, were approaching their end:

Chomsky insisted that "there are major questions for the natural sciences which we can formulate and that are within our grasp, and that's an exciting prospect." For example, scientists still must show—and almost certainly will show—how fertilized cells grow

into complex organisms and how the human brain generates language. There is still plenty of science left to do, "plenty of physics, plenty of biology, plenty of chemistry." (Horgan, 1996:153)

In conclusion, the study of the genetics of language is generating a steady stream of interesting problems (apart from mysteries) that should keep researchers quite busy for years to come.

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