cedents and Plate Motions in the Nordic Sea.

In addition to the main body of the text, a number of interesting and useful addenda and appendices are provided. Indeed the addendum by Johannessen, Johannessen, Sandven, and Davidson provide preliminary results of the Marginal Ice Zone Experiments (MIZEX) of 1983 and 1984. The inclusion of these preliminary results serve to underscore the fact that our knowledge of the Nordic Seas is still incomplete. Useful and exciting exploration and research is still taking place and should continue to take place for many years to come.

Overall, the community of Marine Geologists and Geophysicists, Physical Oceanographers and Underwater Acousticians owe a debt of gratitude to Burton Hurdle and his colleagues for producing this volume. It is a worthy successor to the 1909 landmark work of Helland-Hansen and Nansen.

ALAN BERMAN
Rosenstiel School of Marine and Atmospheric Science
University of Miami
Miami, Florida 33149-1098

Seismic Migration: Imaging of Acoustic Energy by Wave Field Extrapolation (A. Theoretical Aspects)

[Volume 14A (3rd edition) of the series, Developments in Solid Earth Geophysics]

A. J. Berkhout

Elsevier, Amsterdam, 1985.
xx + 445 pp. Price $65.50.

Over the past 15 years, the practice of seismic data processing has been profoundly changed by the introduction of wavefield imaging techniques based upon the scalar wave equation. Today, the migration of seismic data is in many ways a mature subject—the basic wavefield extrapolation techniques were developed during the 1970s and some form of migration is now applied to almost all industrial seismic reflection data. Still, migration remains a topic of active research, as increased computer power allows the application of more sophisticated techniques to complex data cases.

Professor A. J. Berkhout provided the first comprehensive text devoted to migration with Vol. 12 of this series, published in 1980. That text was subsequently revised and divided into two volumes: Vol. 14A covering migration theory, and Vol. 14B covering practical issues. In this third edition, Berkhout has further revised and expanded Vol. 14A to include discussions of migration using the two-way wave equation and the relation between migration and inversion theory.

Chapters 1–3 clearly and concisely review some necessary mathematical tools (basic vector analysis, 1-D and 2-D Fourier transforms); Chap. 4 introduces the acoustic wave equation and its basic properties; Chap. 5 provides an excellent discussion of the Kirchhoff integral and serves as a solid base from which the ideas of wavefield extrapolation are more fully developed in Chaps. 6 and 7; Chaps. 8–10 cover the most common migration methods, namely frequency-domain, Kirchhoff integral, and finite-difference techniques; Chap. 11 presents an extremely brief (8 pages) outline of seismic inversion and its relationship to migration; Chap. 12 compares several aspects of different migration techniques.

This volume in its previous editions has become a standard reference, in large part because it covers a wide range of migration methods. Comparisons between different techniques are handled through Berkhout's elegant description of migration as a spatial deconvolution problem. Using matrix operations to represent convolution in discrete space and time, he discusses both forward and inverse wave propagation in terms of impulse responses. In this framework, the various migration techniques can be described as spatial filtering (i.e., matrix inversion). This approach is particularly effective in comparing results for one-way and two-way wave equations, since the theory is couched in terms similar to propagator matrices.

Much of the added material on the two-way wave equation is useful and enlightening; in particular it emphasizes some of the important assumptions that underlie conventional (one-way) migration techniques. Berkhout's discussion of migration techniques based on the two-way equation, however, is burdened by some difficult notation and a certain lack of practical perspective. Some important practical matters such as the accuracy limitations imposed by data sampling and the importance of good velocity estimation may well be covered in a future edition of Vol. 14B, but an introduction to these issues would be welcome here.

In general, the reader interested in implementing or analyzing specific migration algorithms will find Vol. 14B a necessary companion, especially since that volume contains *all* the examples of migration applied to field and synthetic data. Volume 14B, however, was written to be self-contained and, in some instances, material in that volume repeats rather than complements the contents of Vol. 14A. Part of this problem is attributable to the fact that the theory and practice of migration cannot always be neatly separated; a more integrated discussion of some topics would be helpful. The reader unfamiliar with migration will certainly benefit by first reading Vol. 14B (or a general text on seismic data processing) to gain some practical perspective.

For a third edition, Vol. 14A has several noticeable problems in its design and production. Most annoying is the fact that in several places quoted papers are not found in the reference list at the chapter's end. Also, for a text of this scope, the index is short (three sparse pages) and uninformative. Finally, poor layout on some pages makes it difficult to separate text from figure captions, a problem compounded by the production of the book from a typed manuscript. Any reader willing to part with the purchase price deserves better than this.

Overall, this new edition of Berkhout's text is distinguished by its wide coverage of both new and established material; the problems noted above are generally of secondary importance. Together with the companion volume on practical issues, this book is one of a handful of essential references for anyone involved in the development of migration theory or technique.

BRUCE GIBSON
Rice University
Houston, Texas 77251

The Biology and Evolution of Language

Philip Lieberman

Harvard University, Cambridge, 1984.
viii + 379 pp. Price $27.50.

Although biology and psychology are neighboring disciplines, psychologists have failed to reach a consensus as to whether biology can shed any light on the properties of the mind. By and large, psychologists have refused to acknowledge that biology may provide a tool to understand the mind. Many have dismissed claims that the mind's functional properties could be explained or constrained by biology. In contrast, many linguists, ethologists, philosophers, and adherents of rational psychology have proposed that biology provides the causal account necessary to explain behavior and its development. Indeed, psychologists like Lenneberg (1967), and linguists like Chomsky (1978), have suggested that biology must play a major role in cognitive science; linguistic ability results from universal grammar and grammar is part of man's endowment rather than the result of learning. The rationalist explanation for language and its acquisition, has become so successful, even with those who once dismissed biological explanations of psychological causation, that debates over the role of biology in explaining the foundations of behavior have become rare. Nonetheless, the rationalist position is currently being challenged once again.

Contemporary conceptions of the form-function interplay remain elusive even today. Indeed, biology has been used in two ways to explain the causes of cognitive capacities. A group I will call *biological behaviorists* believe that organisms are initially empty and that the environment is endowed with properties that can induce change. In addition, they maintain that the cortex is structured to accommodate to the information in the environment. Furthermore, they believe in continuity between all organisms and suggest that this continuity can be explained by recapitulation. In

contrast, a second group, that I will call the *biological cognitivists*, describe the structural and functional properties of organisms, namely their *specificity*, rather than trying to discover the history of the organism's becoming, namely, its *stabilization*. Without rejecting evidence in favor of stabilization when such evidence is available, they remain dubious about the virtues of speculation based on stabilization routes. They believe that when natural categories are established and behavior types have been sorted into such categories, little is gained by postulating *ad hoc* forces that, supposedly, are responsible for the existence of such categories. If, for instance, language memory and attention are useful categories, the forces that established them are not obvious. Finally, the biological cognitivists are not convinced that recapitulation or any other evolutionary theory's descriptive tools can provide an adequate explanation of the psychological or linguistic functioning of man.

Philip Lieberman's *Biology and Evolution of Behavior* (Harvard University Cambridge, 1984) is a book that has the virtue of explicitly listing the prototypical arguments that characterize the position espoused by biological behaviorism. The twelve chapters of this book are organized to try and demonstrate that parallel distributed processes underlie speech as well as all other complex behaviors. Much of the structure of speech can be understood from careful inspection of the peripheral organs that are used to convey or decode the signal. Furthermore, since massive mutations are never, according to the author, conducive to survival, it is imperative to view the emergence of any structure as resulting from multiple minimal mutations. Thus language must surely have evolved from the minimal serial changes that intervene between man and his ancestors. The rest of the book tries to muster some data favoring these views.

The aim that Lieberman sets for himself in this book is to explain how language has evolved to its present form. Therefore, the stress throughout this essay is on biological continuity. Whenever language-specific devices are established, Lieberman claims, they concern mainly the input–output aspects of speech. Neuronal networks of the kind postulated in PDP models are proposed as the hardware that accounts for the general linguistic and cognitive aptitudes of man and animal alike. Indeed, syntax and semantics result from the same mechanisms that make all other cognitive behavior possible and, furthermore, these mechanisms are Lieberman argues, already present in mollusks:

> Mollusks thus can share some of the biological, neural mechanisms that are involved in human thought. Mollusks could "think" by using these neural mechanisms, though they could handle neither the range nor the complexity of the problems that human beings usually think about—p. 33.

The Biology and Evolution of Language is a book that tries to reinstate most of the basic tenets and claims of behaviorism without advancing arguments to make such rehabilitations appealing or convincing. When Lieberman tries to show that language results from the operation of general cognitive abilities mediated by parallel-associative networks this is done in such a programmatic fashion that if he turns out to have been right it would be a coincidence. Maybe PDPs underlie all faculties yet no cogent arguments are advanced to convince us of this. We could, for instance, suggest that the outcome of the next French election is related to the change in the size of skulls over the last two millon years. However, it seems a rather impractical way to set about predicting the outcome of that election. Likewise, language might be related to PDP models though it is rather impractical, at this point, to try and predict something about language processing and structure from that construal.

The scientific project that Lieberman puts forward has a flavor that persists throughout the book which can be best illustrated with a few quotations like the following:

> I shall argue that the formal rules of Chomsky's "fixed nucleus" are ultimately related to the way that lizards wiggle their tails—p. 35,

or

> The goal set forth by Chomsky and Saussure is inherently impossible in light of the data on biological variation that are the basis of evolution by means of natural selection—p. 13,

> Thus there cannot be any speaker or hearer in a population who has the grammar of Chomsky's ideal speaker-hearer. The properties of the abstract average, if that is what we mean by the competence grammar or langue, can be determined only by studying the variations that typify the linguistic behavior of individual members of a population—p. 14.

The arguments advanced by Lieberman seem to me unconvincing as are the data on which they are based. Even within biology the position espoused by Lieberman does not seem to hold. Consider how F. Jacob (1970) in the *La Logique du Vivant* copes with the notions of biological variation. Though we are all aware that variation is preeminent in biology he states (my own translation) that

> Few phenomena are as pervasive in the world of living beings as the formation of things alike... . In fact, the genetic program is made up by the combination of essentially invariant elements.

Lieberman's reasoning must predict that the program of genetics is in fact impossible and ill inspired. Although he does not go as far as saying this, he makes an equivalent charge against Chomsky's program. These quotations illustrate the often unsubstantiated claims and *a priori* arguments used by Lieberman mostly against Chomsky and the movement he has actively promoted for the past quarter of a century. Of course, it is difficult to be convinced by a book that drones on endlessly about the mistakes made by others regardless of their success. Here is an example of the kind of blanket statement that must remain, I hope, unconvincing:

> The attempts of logicians, including Russell, Chomsky, and Montague (1974), to develop formal systems of logic that are unambiguous probably are inherently flawed—p. 82.

I have always been fascinated by G. K. Chesterton's understanding of the nature of the scientific enterprise. Indeed, in one of his most charming stories he has Father Brown state, "a man of science isn't trying to prove anything. He's trying to find out what will prove itself." Unfortunately, while reading *The Biology and Evolution of Language* one is frankly amazed by Lieberman's attempt to show that Chomsky and his enterprise are empty. He is certainly not Chesterton's man of science. In all likelihood Chomsky's influence can be attributed to scientific achievement, that is, to the usefulness of his ideas and Lieberman's constant criticism smacks more of crusade than of intellectual argument. But Lieberman does not stick exclusively to Chomsky. His distaste is broad and many cognitive scientists are put on the Index, which strangely means that they will not be mentioned in the index even when they are cited in the text. Thus Alvin Liberman is not only excluded from the index but is not even cited in the section (pp. 135–136) on Motor Theories of Speech Perception and I also wonder whether Eimas is excluded due to carelessness or prejudice. It is also curious that Eimas, one of the first to systematically study infants to assess the mind's disposition for language, is only mentioned in a very subsidiary fashion as if he had contributed little of importance to our understanding of the biological foundations of language. Given the omissions mentioned above it is not too surprising that Fodor is also not found in the index either. Maybe he is also on Lieberman's black list. I might add that in the section on lateralization the discovery of a right-ear superiority effect in dichotic listening tasks is attributed to Swaffield, Shearme, and Holmes, 1961, rather than to Kimura, 1961, as is generally the case. Given the publication dates, it is, of course, nice to see the reinstatement of an author who had been lost from bibliographic reference but this does not justify simply dropping Kimura altogether. In fact, had Lieberman really wanted to set the record straight he should have referred to Licklider who published a report to the same effect in 1948.

Let me add, more substantially, that there are two presuppositions adhered to thoughout the history of modern psychology, neither of which seems warranted to me. The first, at least implicitly, states that if we want to study growth and/or acquisition the best way to go about it is to measure it. The second, also implicitly, states that the *panglossian* accounts of nature are true because they are common sensical. Voltaire uses Dr. Pangloss to illustrate this position, i.e., why do we carry glasses? Because our noses are made to carry glasses, so spectacles we carry, etc. In retrospect, let us acknowledge that the millions of marks put on walls by an equally large number of loving parents did not result in any breakthrough regarding the understanding of growth. In contrast, the study of the underlying biological processes involved during the period of growth have yielded highly useful theories and results about growth. As for the panglossian view, Gould and Lewontin (1979) have written a poweful critique of this position. Let us acknowledge that the position, though espoused by most evolutionary biologists, is too powerful to be of great use. Indeed, it can "explain" everything. At times I have argued that it can explain chemical reactions or astronomy. Recently, my friend Piatelli-Palmarini (in press) has quoted A. Coutinho who has asked the following rhetorical question:

> Why do all stones fall to the ground when let free? Simple, because they have been so selected. All those which tended to fly upwards were lost a long time ago.

Many of Lieberman's arguments presented throughout this book are examples of panglossianism carried to the extreme. To such an extreme that they can be safely dismissed.

In Chapter 9 Lieberman speculates about the hardware needed to preserve language and thought and reaches the conclusion that it consists of (1) innate and species-specific input modules, (2) innate specialized neural

mechanisms that mediate not only language and thought but also motor control and so forth, and (3) very plastic general mechanisms that allow the organism to adapt to the environment. To some extent Lieberman's position as summarized above is so broad and generic that it is compatible with the standard line adopted in theories like those proposed by Chomsky (1965, 1978), Fodor (1983), and Forster (1979), among many others. However, Lieberman's aversion for the cognitivist movement is such that he persists in presenting his point of view as conflictual even when the disagreements are very minor. This is a shame since it detracts considerably from his point.

Regardless of my misgivings about his data and arguments it may be that Lieberman is, in the final analysis, right. Perhaps, cognitive psychology and linguistics are disciplines meandering down a path littered with errors and irrelevant quests. However, should the future prove this to be so, I am certain that it will not be for the reasons cited by Lieberman. There is no doubt in my mind that new theories and new data will be influenced by the *weltanschaung* brought about by the Chomskian revolution even if much of the work generated by its revolutionaries is overtaken by that of other scientists.

JACQUES MEHLER
Center for Cognitive Science and Psycholinguistics, C.N.R.S. and E.H.E.S.S.
54, Bd Raspail
Paris, France 76270

See author Lieberman's reply in the Letters to the Editor section of this issue [J. Acoust. Soc. Am. 80, 1521–1522 (1986)].

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LETTERS TO THE EDITOR

Editor's Comment

Because Jacques Mehler's review (p. 1558) of Philip Lieberman's book, *The Biology and Evolution of Language*, revealed a great divergence of basic viewpoint, Lieberman was invited to reply in a Letter to the Editor. Publication of both the review and the letter in this issue ends the controversy so far as the *Journal* is concerned.

DANIEL W. MARTIN
Editor-in-Chief

A reply to Jacques Mehler's "Review of *The Biology and Evolution of Language*" [J. Acoust. Soc. Am. 80, 1558–1560 (1986)]

Philip Lieberman
Department of Cognitive and Linguistic Sciences, Brown University, Providence, Rhode Island 02912

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Mehler takes an extreme Chomskian position and disputes the biological premises of my book, *The Biology and Evolution of Language* (Harvard U.P., Cambridge, MA). These premises are consistent with modern biological thought and data. Mehler distorts the tone, content, and intent of my book.

PACS numbers: 43.10.Hj, 43.70. – h, 01.30.Ee

One of the curious developments of the last 30 years is the quasireligious fervor of many of the disciples of Noam Chomsky. This development is perhaps not surprising, since the extreme Chomskian position concerning human uniqueness is close to that usually associated with religious dogma. Chomsky claims that human linguistic ability is unique; as with human cognitive abilities, it supposedly could not have evolved by means of Darwinian Natural Selection (Chomsky, 1980). In reviewing my book, *The Biology and Evolution of Language*, Jacques Mehler takes an extreme Chomskian position. In effect, he states that one should not study human evolution. Mehler denies the most basic aspects of modern biological thought. He distorts my book and finally casts a personal slur concerning some of my closest colleaques.

Throughout his review, Mehler disputes the biological premises that structure my work. The readers of *The Journal of the Acoustical Society of America* should know that these premises are consistent with modern biological data and theory. As Mehler himself notes, the premises that underly the study of evolution "are espoused by most evolutionary biologists." Ernst Mayr (1982), in his definitive work, *The Growth of Biological Thought*, discusses these basic principles. Mayr shows the following.

(1) All living organisms exhibit genetic variation. Diversity is the norm. Biological mechanisms that are genetically transmitted thus vary for different human beings *because* they are genetically transmitted. A universal grammar and speech perception mechanism that is part of human biologic endowment must necessarily be genetically transmitted. Hence, there must be variation in the human population with respect to these biologically determined elements.

(2) Essentialistic thinking, which yields models like the hypothetical uniform linguistic competence that Mehler expounds, is inappropriate for describing the biological endowment of living organisms. We have to work with populations and they consist of individuals who show variation.

(3) The Synthetic Theory of Evolution, which is a synthesis of Darwin's theory and modern genetics, accounts for what we know about the evolution of human beings and other species.

Mayr demonstrates that the philosophic positions and biological premises espoused by Mehler and Chomsky are out of touch with modern biological thought. Natural Selection, which Mehler pejoratively dismisses as "Panglossian," is a central concept in modern genetics and evolutionary biology. Mehler's comments concerning genetics and variation show that he is completely confused. Genes themselves can be invariant, but they are continually reshuffled. The expression of one gene depends on the total genetic environment, and mutations constantly occur. Cognitive scientists who dogmatically assert that individual subjects cannot vary with respect to any genetically determined characteristic are ignorant of basic biological principles.

Mehler distorts the overall tone and content of *The Biology and Evolution of Language*. I do not, as Mehler charges, advocate a return to "behaviorism." I agree with Chomsky and Mehler that human beings have innate biological capacities that underlie linguistic ability. Unlike Chomsky, I propose a biological framework for their evolution. Though the major premises of the book are diametrically opposed to the crypto-creationist position of Chomsky, it is *not*, as Mehler claims, full of "constant criticism" of Chomsky that "smacks more of crusade than of intellectual argument." To

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a zealot like Mehler, any data or discussion that does not echo the dogma of the master can be interpreted as "constant criticism" but Chomsky is seldom mentioned throughout the body of the book. Chomsky is not active in the study of fossils, evolutionary biology, the communications of other species, aphasia, neural modeling, or the perception and production of speech. Most of the data, analysis, and discussion in my book are not concerned with his particular claims.

Mehler, in his Chomskyite zeal, moreover distorts my position by quoting fragments of my text out of context. For example, he would have the reader believe that I consistently make "unsubstantiated claims." He presents "quotations" that illustrate this supposed deficit. His first example illustrates his method. On pp. 79–82, I discuss the work of Cassier (1944) and Bronowski (1971) with respect to the limitations of mathematical logic. Mehler quotes the concluding sentence, omitting any reference to Bronowski or Cassier. He can hardly have missed the preceding discussion, but prefers to create the false impression that I "drone on endlessly," making "unsubstantiated claims and *a priori* arguments."

Mehler's misuse of quotations can be also seen in his treatment of my discussion (pp. 32–33) of the data of Carew *et al.* (1981). These data show associative learning in mollusks; they are consistent with a distributed Hebbian neural model. Mehler quotes the last two sentences of my discussion out of context to create the false impression that I believe that "syntax" derives from "mechanisms that are already present in mollusks." Mehler's claim is patently false since one of the major points of *The Biology and Evolution of Language* is that the neural substrate for human syntax is species specific.

The thesis of *The Biology and Evolution of Language*, which the innocent reader will not find in Mehler's review, is that human linguistic ability derives from a number of innate, i.e., genetically transmitted, anatomical, and neural mechanisms. The plurality of these mechanisms is consistent with the mosaic nature of evolution. Some of these mechanisms, particularly those relating to human speech, appear to be species specific. Other aspects of human language, like rule-governed syntax, also appear to involve species specific neural mechanisms.

The evolution of human speech is probably the aspect of my book that is most germane to the readers of *The Journal of the Acoustical Society of America*. One of my major premises is that a human-like supralaryngeal respiratory system in a fossil is an *index* for the presence of the neural mechanisms that are necessary to produce human speech. The human supralaryngeal airway has a selective deficit for all vegetative functions. We are more susceptible to choking on swallowed food. Our teeth are crowded due to the reduction of the length of the body of the mandible, decreasing masticatory efficiency and increasing the likelihood of death when teeth become impacted. Our respiratory efficiency is lower due to the right-angle bend in the supralaryngeal airway. The only selective advantage of the human supralaryngeal airway is human speech, which yields a high data transmission rate that overcomes the limits of short-term memory. However, the anatomy of the vocal tract, in itself, would be useless for speech unless two sets of "matching" neural mechanisms were also present: (1) the neural mechanisms of Broca's region that control the complex articulatory mechanisms of speech production, and (2) the neural perceptual mechanisms that "decode" the speech signal.

The Biology and Evolution of Language presents comparative data that demonstrate that some of these matching neural mechanisms are not present in living species other than *Homo sapiens*, though homologues exist. The book also develops the theory that human language is the result of a mosaic of biological mechanisms. Some of these mechanisms, like the Hebbian distributed neural network that probably is the basis of the linguistic lexicon, are clearly present in living pongids. Studies of the lexical abilities of many chimpanzees and one gorilla, for example, demonstrate that they can use and acquire words. In contrast, these animals lack both the anatomical mechanisms and neural mechanisms that underlie human speech. They also do not appear to be able to use syntactic relations productively. However, homologous neural mechanisms exist in these and lower species. I see human language as a phenomenon that follows from the "normal" evolutionary processes that constitute the modern Synthetic Theory of Evolution.

In closing, I would like to respond to an offensive accusation that Mehler makes in his zeal to convince the reader that I am engaged in an anti-Chomsky "crusade." Mehler states that my

> "distaste is broad and many cognitive scientists are put on the Index which strangely means that they will not be mentioned in the index even when they are cited in the text."

Mehler specifically notes the omission of two of my closest colleagues, Alvin Liberman and Peter Eimas. The policy of the Harvard University Press regarding the inclusion of an individual's name in the index is that the name itself must appear in the body of the text; names appearing in citations are not indexed. Alvin Liberman does not appear in the index because the text notes the work of the "Haskins Laboratories group" (which I am certain is what Alvin Liberman himself would find appropriate) in the detailed discussion of pp. 142–149 and 173–180. The appropriate references are obviously cited. Peter Eimas likewise does not appear in the index for the same reason, though his work is discussed and cited on pp. 171 and 184–185. Philip Lieberman also does not appear in the index, though my work is cited throughout the text. Mehler might have noted the omission of my name before he transmitted his bizzare, insulting theory to the readers of *The Journal of the Acoustical Society of America*.

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