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A RESTRICTED VIEW OF THE INTRODUCTORY KNOWLEDGE BASE FOR SPEECH PHYSIOLOGY AND ACOUSTIC PHONETICS

(Review of
Lieberman (1977), *Speech Physiology and Acoustic Phonetics*)

by

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Introduction

An important challenge for those of us who teach at the college level involves the impartation of a base of knowledge to neophytes in our chosen fields. As an instructor of the so-called 'introductory course', one must introduce a certain bias into the course structure and content, since the actual knowledge base of most fields will dwarf the amount of information that can be (adequately) covered in a fifteen-week semester. It should not be surprising, therefore, when a textbook designed for an introductory course—that is, a text whose purpose is to impart a base of knowledge—deviates somewhat from our own conception of the FUNDAMENTALS of our field. Under these circumstances, we must evaluate, through our own biased perspective, the extent to which this different conception actually DISTORTS the knowledge base. This kind of evaluation is decidedly subjective, and when structured within the framework of a textbook review must be viewed as somewhat similar to an educated review of a literary effort: the opinions expressed therein may have definite value and be of interest to a certain audience, but they are not necessarily statements of right or wrong.

What of right *versus* wrong? A textbook which deals with the fundamentals of a field will perforce trade largely in the currency of facts. It would seem as if the evaluation of this aspect of a text is straightforward in that 'facts' are, by definition, observable and therefore subject to objective evaluative criteria. If a statement of fact in an introductory text is wrong, then it would seem to be an appropriate review tactic to 'call' the author on the error, and advise readers of the correct formulation. Unfortunately, the situation is not nearly so simple, especially if you have a Popperian inclination which leads you to believe that 'facts' are actually observables INTERPRETED through an existing theory. If this is so, then our evaluation of the factual aspect of a text must involve a subjective component and is perhaps not much

different from our evaluation of the author's general conception of the 'knowledge base'. In fact, the former and latter features of an introductory text are probably, to a large degree, highly interdependent. An author may be aware of several (interpreted) facts which apparently are contradictory, yet choose to ignore the ones which might disturb his conception of the knowledge base.

It also seems of great importance to evaluate the logistical aspects of an introductory text. In this regard, a reviewer should attend to such matters as general organization, clarity of writing, preparation of graphics, and general editing. The three former items are clearly the responsibility of the author, whereas the matter of editing involves the publisher as well as the author. Logistical concerns are of critical importance for a text designed to introduce the knowledge base of a field, since careless errors such as a mislabeled figure or erroneous mathematical equation may not be recognized as such by the neophyte. The evaluation of textbook logistics may thus proceed on a somewhat objective basis, at least where such factors as lapses in the editorial process are concerned. The more substantive logistical issues of organization and clarity of writing are, however, less susceptible to objective evaluation; for example, what one reviewer judges to be 'witty writing' may be viewed by another as banal.

At this point, the reader may suspect that I believe a textbook review is a less scientific undertaking than, say, a review of the report of an experiment. To a great extent this suspicion is correct, for, as indicated above, it seems to me that many components of an introductory textbook are not open to the kinds of objectively-based criticism which may be applicable in the case of an experimental report. With this in mind, I would hope that the reader regards this review as a personal, but hopefully principled, evaluation of Philip Lieberman's *Speech Physiology and Acoustic Phonetics*, published in 1977 by Macmillan Publishing Co., Inc. I make this request of the reader because I will make some very negative comments concerning this text, and in fact conclude that on balance the book is not a very useful companion for a student taking an introductory course in speech physiology and acoustic phonetics; yet I would not want a good portion of my comments to be represented as statements of fact concerning 'right' and 'wrong' (although there are certain aspects of the text about which I feel justified in identifying errors: see below), but rather as statements of scientific and pedagogic judgment.

Certainly an introductory text in 'speech physiology and acoustic phonetics' should be of some value to linguists, especially those working in the area of phonology. In recent years there has been a good deal of research in which instrumental techniques—that is, techniques which are designed to probe the phonetic aspects of language—have been employed in an attempt to illuminate certain aspects of phonology. For example, at

the physiological level instrumental techniques have been brought to bear on such issues as contrasts among nasals as a function of position (Ohala, 1975) and stop epenthesis in nasal-fricative sequences (Ali, Daniloff and Hammarberg, 1979); at the acoustic level, temporal measurements of the speech wave have been related to syllabification (Davidsen-Nielsen, 1974), morphophonemics (Ingrisano and Weismer, 1979), and issues in developmental phonology (Macken and Barton, 1977; Weismer, Dinnsen & Elbert, 1979); and at the perceptual level aspects of sound change (Lindblom, 1978; Janson, 1979) and phonological processes (Macari, 1978) have been investigated. It should not be unusual, therefore, for a linguistics curriculum to include a course which could make extensive use of an introductory text in speech physiology and acoustic phonetics. Moreover, when such a book is authored by an important linguist such as Philip Lieberman (hereafter, PL), it would be logical for instructors who are linguists to give the text serious consideration.[1] This review will proceed in the following manner. First, the contents of the text will be described in the fashion of a nonevaluative precis. Then, evaluative comments will be offered in two sections, one of which will deal with the knowledge base/fact aspect of the text, whereas the other will focus on logistical characteristics, as defined above. In general, I will try to use a chapter-by-chapter format within each of these sections. Finally, a concluding section will summarize preceding contents and suggest possible alternatives to the text by PL.

Precis

The preface of *Speech Physiology and Acoustic Phonetics* (hereafter, *SPAP*) states that "there is a need for a text that guides students from different backgrounds to a quantitative understanding of speech without their having to take a two or three year sequence of specialized courses" (p. v), and describes the text as providing "a step-by-step introduction that starts with simple examples that put the subject into perspective" (p. v). PL's view of this text is that it is "self-contained insofar as it has the range of information that is necessary for a student to move from unfamiliarity with the area to publishing a research paper" (p. vi).

Chapter 1 provides a highly condensed overview of the history of speech research, and foreshadows several of the contemporary issues which are described in greater detail later in the text. In this chapter PL informs the reader that his coverage of topics is somewhat selective, but at the same time states that the book can "be viewed as an introduction to the physiology of speech and acoustic phonetics" (p.2).

Chapter 2 is entitled 'A Qualitative Introduction to the Physiology of Speech', and briefly describes the respiratory, laryngeal and supralaryngeal components of the speech mechanism. These descriptions are designed to provide the reader with general notions regarding the subsystems

of the speech mechanism, and serve mainly as an overview to the physiology of speech. The chapter ends with a brief section entitled 'The Neural Processing of Speech'.

Chapter 3, entitled 'Basic Acoustics', develops the subject matter by means of examples drawn from everyday experiences. Thus the notion of periodicity is first exemplified by fluctuations in temperature throughout the twenty-four hour cycle of a day, following which characteristics of sinusoids are described in some detail. Acoustical filters are also introduced in this chapter via an 'everyday experience' type example.

Chapter 4 deals with the 'Source-Filter Theory of Speech Production', and provides basic information on the generally accepted acoustical theory of speech production, most often connected with the classic monograph by Gunnar Fant (1960). This chapter also contains an extended section on the calculation of formant frequencies, and a briefer discussion concerning the perception of source and filter characteristics of speech production.

The title of Chapter 5 is 'Speech Analysis', and an extensive discussion of the sound spectrograph and its applications are contained therein. Wave-form and computer-based analyses of speech are also discussed, but in a relatively abbreviated fashion. A brief section on tape-recording techniques is also included.

Chapter 6, 'Anatomy and Physiology of Speech Production', presents anatomical and physiological information on the three components of the speech mechanism described in Chapter 2. With respect to the respiratory system, PL focuses on the need to maintain a roughly constant subglottal pressure during speech production, and also discusses several linguistic aspects of respiratory function. The laryngeal section introduces the widely accepted theory of phonation—the MYOELASTIC-AERODYNAMIC THEORY—and then provides discussion of some of the aeromechanical aspects of this theory. Information concerning the control of fundamental frequency, voice registers, the voiced-voiceless distinction, and the effect of supralaryngeal factors on fundamental frequency, is also found in this section. The section dealing with the supralaryngeal vocal tract identifies some of the muscles which are important for the functioning of this component of the speech mechanism, and then describes a cineradiographic study of speech production. Some mention is also made of the possible ways in which supraglottal function in speech production may be modeled using electrical networks. Chapter 6 terminates with a listing of some of the muscles which are involved in speech production.

'Speech Synthesis and Speech Perception' is the topic of Chapter 7, which introduces much of the influential work performed by scientists associated with the Haskins Laboratories. A certain amount of attention

is also directed toward the contemporary notion of perceptual 'property detectors' for speech sounds, and the possibility that these property detectors are, in an evolutionary sense, 'matched' to the sound-producing capabilities of the human vocal tract.

Chapter 8 is entitled 'Phonetic Theories', and begins with a discussion of what PL calls 'traditional "articulatory" phonetic theory'. It is suggested, based on PL's own research, that the traditional theory is inadequate, at least where vowels are concerned. PL then describes, in some detail, Kenneth Stevens' (1972) quantal theory, which accounts for acoustic stability of vowels even in the presence of a substantial degree of articulatory 'sloppiness'. A certain amount of discussion is also devoted to the basic characteristic of stop consonants and nasals. Following a review of standard phonetic classification of speech sounds, brief discussions of prosody and biological influences on speech production are offered. Chapter 8 also includes an appendix which deals with 'Acoustic Correlates of Sounds and Phonetic Features'. Finally, Chapter 9 presents some 'Old Problems and New Directions', and considers briefly primate vocalization, innate mechanisms for speech production, and speech pathologies. Approximately 210 citations from the literature are included in the bibliography, and a combined author-subject index is furnished at the end of the text.

Knowledge Base and Facts

For the most part, this text contains few actual errors of fact (but see below, discussion of Chapter 2), but its OMISSION of certain information is noteworthy. It may be recalled from the precis that PL's preface and introductory chapter identifies this text as one for students from diverse academic backgrounds and as an INTRODUCTION TO SPEECH PHYSIOLOGY AND ACOUSTIC PHONETICS. I would agree that the text serves as an introduction of sorts, but also suggest that it conceives the knowledge base for speech physiology especially, and for acoustic phonetics to a somewhat lesser degree, in such a biased fashion as to mislead a neophyte regarding the fundamental information in these areas. The text largely ignores the extensive and pertinent research on speech production performed by scientists who are or at one time were associated with the speech and hearing group at the University of Iowa, including K. Moll, H. Hollien, T. Shipp, R. McGlone, T. Hixon, R. Daniloff, F. Minifie, and R. Kent; where these investigators are mentioned in the text, it is usually in passing and none of their work will seem very important to the neophyte reader (see below for more specific comments). I suppose that PL's conception of the knowledge base is best summarized by his own statement from Chapter 1, where he claims that "The aim of speech science must be to EXPLAIN why certain sounds tend to occur and why some sounds seem to occur more often than others" (p.2, emphasis in original). I do not quarrel with this as one possible aim of speech research, but do reject it as THE aim of our research efforts. PL's conception of the knowledge base, as revealed by

the above quote, gives his text a decidedly static personality; the reader who wants to go past such descriptions and acquire fundamental knowledge concerning speech production as a dynamic, temporal phenomenon will find little of interest in this text. I do not mean to imply that the topics covered in *SPAP* are unimportant or potentially uninteresting to the student who wants to learn about speech physiology, acoustic phonetics and speech perception; indeed, if one is interested in the influence of speech research on the development and elaboration of phonetic feature systems, portions of this text will be very useful. My point is that PL does not appear to view the material presented in *SPAP* as merely a fragment of the relevant knowledge base.[2]

The constrained nature of the knowledge base as presented in *SPAP* should be painfully obvious to any speech pathologist who reads the text from cover-to-cover and then returns to Chapter 1 to be told that "An understanding of the acoustics of speech, the physiology of speech production, and the special factors that are involved in the perception of speech is a prerequisite for further study of the pathologies of speech production or the neurological impairment of either speech production or speech perception" (p.2). Speech pathologists learned long ago that static descriptions of, for example, speech production do little to further understanding of most of the speech pathologies. The speech pathologist who seeks information regarding normal speech production, acoustics, and perception which can be applied to problems in the speech and audiology clinic will find little useful in *SPAP*.

PL's 'Qualitative Introduction to the Physiology of Speech', found in Chapter 2, contains several conceptual errors which are largely confined to the description of the respiratory system. For example, the drawing of the mechanical model of the 'subglottal respiratory system' (Figure 2-2, p.6) is inaccurate because the tube representing the trachea is not depicted as being subject to the influence of pleural (intrathoracic) pressure. That pleural pressure is, in fact, exerted on the trachea is demonstrated by the tendency of the trachea to collapse when the magnitude of the pleural pressure becomes extremely great due to very forceful contraction of the expiratory muscles of the thorax and abdomen. The more difficult aspect of this mechanical model, however, is that PL chooses to describe the pleural space[3] as being governed by BOYLE'S LAW, which states that within a closed volume of gas the product of pressure and volume must equal a constant ($P \cdot V = K$). This description is extremely unfortunate since the pleural space is filled by a serous liquid (Agostoni, 1972), and liquid-filled volumes are not subject to the gas laws (such as Boyle's Law).

This section on the 'subglottal respiratory system' also includes some rather odd statements and arguments. The introductory comment informs the reader that "The lungs and associated musculature often are not discussed with regard to their activity during the production of speech" (p.5). The question is, not discussed by whom? Other texts dealing with

speech physiology (e.g., Zemlin, 1968; Minifie, Hixon and Williams, 1973; Hardcastle, 1976; Dew and Jensen, 1977) routinely discuss respiratory function for speech, and the research literature contains some very excellent and comprehensive accounts of speech breathing (see below, comments on Chapter 6). Later in the introductory paragraph, PL explains:

> The best way to approach the discussion of the subglottal respiratory system is to describe in simple terms how the lungs work. The respiratory system is put together in a manner that is not immediately obvious. Stetson (1951), for example, proposed a theory that attempted to explain the linguistic and physiologic bases of the syllable as a unit of speech. Stetson's theory is unfortunately flawed because he misinterpreted the function of the lungs during quiet respiration and speech. (p.5)

The second sentence in this sequence seems to be a reference to the anatomy of the respiratory system, but the rest of the information is concerned with physiology (function). Obviously anatomical functions bear on physiological issues, but PL fails to provide the necessary link in this case. For the neophyte reader the relevance of the anatomical statement to Stetson's explanation of function will be somewhat obscure.

Later in this section PL claims (p.8) that "most of the internal intercostal muscles are inspiratory muscles (Bouhuys, 1974)". Whereas the function of the internal intercostals historically has been a controversial matter, the inspiratory view is a decidedly minority report according to several prominent physiologists (Campbell, Agostini and Davis, 1970:163-164).

The introductory sections on the larynx and the supralaryngeal vocal tract in Chapter 2 do not present factual difficulties, but when compared to each other and the respiratory section, will not give the reader a sense of a coherent superstructure for the chapter as a whole. The respiratory section is partly in the form of an anatomy lesson, whereas neither the larynx nor supralaryngeal sections presents anatomical information in any structured fashion. Along the same lines, the respiration and larynx sections provide brief sketches of the relevant physiology for speech production, but the supralaryngeal section is little more than a general statement concerning the acoustic resonator properties of the vocal tract. The reader might note, for example, that in the supralaryngeal section no mention is made of aerodynamics, whereas such information is included as an important component of both the respiration and larynx sections. Indeed, it would be inadvisable for the reader learning the basics of speech physiology to disregard the importance of aerodynamic factors at the supralaryngeal level of the speech mechanism; unfortunately, PL fails to fill this lacuna later in the text. The closing section of Chapter 2 on 'The Neural Processing of Speech' (pp.13-14) is, AS WRITTEN, completely irrelevant in a chapter which purports to provide an introduction to the physiology of speech production. The heading of

this section might lead one to expect information concerning motor mechanisms for speech production; to the contrary, PL's focus here is on the possible evolutionary match between the output capabilities of the human vocal tract and the response tendencies of the human auditory system. There is a brief, unelaborated mention of Broca's area, but no further discussion which could shed light on the neurophysiology of speech.

The organization of Chapter 3 represents a considerable improvement over that of Chapter 2, and in general the material dealing with basic acoustics is presented in a concise fashion. A problem in this chapter, however, is that certain large inferential leaps—which might be thought to be undesirable in an introductory text—are required of the reader. There is, for example, a distinct absence of tutorial writing on the topic of air pressure, which should be an integral part of a chapter on basic acoustics. This omission becomes somewhat problematic when PL attempts to describe in Chapter 4 the distribution of air pressure in tubes. There is also a rather inadequate description of sinusoidal waveforms (pp.18-20). PL chooses to describe sinusoids primarily on the basis of their graphical appearance ("The waveforms plotted in Figure 3-2 is...SINUSOIDAL. Sinusoidal waves, which always have this smooth shape..." (p.19)), and not by explaining that a sinusoidal waveform results from the linear projection of UNIFORM CIRCULAR SPEED. Again, this is a most unfortunate omission, for when PL states that "It is usual to quantify phase differences in terms of degrees" (p.20), the origin (i.e., circular motion) of this convention will not be accessible to the neophyte reader.

There are several very positive features in Chapter 3. PL provides a very nice analogy to propagation of a wave in a fluid medium (p.21), and his discussion of filters is original and intelligible. The latter material could have been strengthened even further by the inclusion of detailed information on BANDWIDTH, a concept which is of some importance in Chapter 4 and 5.

Chapter 4 is a reasonable introduction to the source-filter theory of speech production, especially because it provides a nice description of the way in which the separate source and filter characteristics combine to yield an acoustic output from the vocal tract. PL largely confines himself, however, to a discussion of the acoustic theory of speech production as it relates to vowels. It is well known, of course, that the analytical theory for vowel acoustics is substantially more sophisticated than the available theory for consonant acoustics (Fant, 1960). This discrepancy between the development of a quantitative theory for vowel *versus* consonant acoustics is accounted for largely on the basis of two related aspects of consonant production. First, consonants often have one of their sources located above the larynx (as compared to vowels, which almost always have their source located at the larynx),

which means the source may be surrounded by resonators; this arrangement makes it difficult to specify precisely the spectra of sources (Stevens, 1971),[4] and so impedes development of a quantitative theory of consonant acoustics. The second problem concerns the propagation of high frequencies through the vocal tract which is characteristic of many consonants. In the case of vowels, where the important acoustic information is usually associated with relatively low frequencies (below 4000 Hz), the pressure wave propagates through the vocal tract essentially as a series of longitudinal waves. For the high frequency consonants, however, transverse as well as longitudinal pressure waves may be propagated in the vocal tract, a situation that is decidedly less tractable to accurate mathematical analysis as compared to the case of longitudinal waves only. PL might have at least identified these considerations for his readers, and then proceeded with his concentration on the vowel theory. As this chapter stands, the reader receives virtually no information regarding the theoretical aspects of consonant acoustics (other than the general principles of source-filter theory, which apply to both vowels and consonants), which, although not as precise as those for vowels, are still worthy of mention.

There are several additional problems with the presentation of source-filter theory which may present difficulties for the neophyte reader. One of the problems encountered in teaching speech acoustics is the adequate explanation of why there are often differences between CALCULATED resonances associated with a given vocal-tract configuration (those resonances which are derived from the analytical theory) and the actual peaks (i.e., formants) in a measured spectrum. The reason for this is that energy in the source spectrum may or may not coincide with the frequency location of a CALCULATED resonance. If there is energy at the calculated resonance, then the MEASURED spectrum will show a peak at the frequency of the calculated resonance; if, on the other hand, the source spectrum does not contain energy at the calculated resonance, the measured spectrum will show a peak that is somewhat displaced from the frequency location of this calculated resonance. PL attempts to describe this phenomenon at the bottom of page 36, but fails to distinguish adequately between the theoretical (calculated from analytical theory) and measured peaks in the spectrum. His discussion, especially as cast within the framework of perceptual considerations, is confusing and in my opinion lacks the kind of precision necessary for a clear exposition of the issue.

A similar problem exists with the description of tube acoustics (pp. 40-43). For the most part this is an excellent section, especially since PL gives a more detailed statement of the factors which govern acoustic vibration in tubes than is found in comparable texts. The difficulty arises when PL deals with the relationship between tube length and tube resonance. Following a proper statement of the quarter-wavelength rule for a tube closed at one end, PL writes:

> If the glottal source were exciting the tube at the closed end, then it would generate a wave pressure wave[5] at the frequency corresponding to the wavelength with minimum input energy... it [the first formant frequency] is the lowest frequency at which maximum sound energy would be generated by a source at the closed, glottal end of the tube. (p.42)

The concern here is with the wording of these statements, which I believe could lead the neophyte reader to conclude that the source ADJUSTS its frequency to match the vibratory constraints associated with a particular tube (i.e., a particular vocal-tract configuration). I am certain that PL does not mean for the reader to reach such a conclusion, but again it seems to me a case of a somewhat imprecise exposition which potentially could mislead the reader.

Chapter 4 has one organizational flaw, that being the inclusion of a section dealing with 'The Perception of Fundamental and Formant Frequencies' (pp. 36-39). In the present opinion, the information contained therein is misplaced, even though PL attempts to use this section to point out the potential difference between a calculated and measured spectrum (see comments, above). In addition, this chapter makes no mention of Helmholtz resonators, which is unfortunate, since the vocal tract responds as such a resonator in several cases (e.g., the front cavity resonance of /u/).

Chapter 5 is the Dr. Jekyll/Mr. Hyde installment of the text, because it is, in terms of the information presented, an excellent account and description of the techniques available for acoustic analysis of speech. It is also, however, the most deficient chapter in the text, for it fails to present any information whatsoever on the instrumental means by which SPEECH PHYSIOLOGY might be studied. I would remind the reader of this review to regard the current critical remarks relative to PL's own description of *SPAP*—it is, according to him, "...an introduction to the physiology of speech and acoustic phonetics" (p.2). Nowhere does PL indicate that he will focus largely on acoustic matters, and he in fact mentions several of the physiological techniques in his preface, where we are told that "the implications of quantitative studies involving techniques like cineradiography, sound spectroscopy, dichotic perception, electromyography, computer modeling, etc., are often unintelligible to the nonspecialist; this book should make these results accessible by providing a knowledge of relevant theories and TECHNIQUES and ALSO OF THE DEFICIENCIES OF THESE theories and TECHNIQUES." (p. v, emphasis added here). The information presented in Chapter 5 certainly would make the principles underlying spectrographic analysis of speech intelligible to any serious reader—indeed, PL's account of the sound spectrograph as a tool for speech analysis is by far the best this writer has encountered in the literature. Contrary to PL's prefatory statement of what the reader should expect from the text, however, the principles and deficiencies of

such techniques as cineradiography and electromyography will remain obscure for the neophyte, no matter how carefully or often the text is read. The relevant technical information simply is not included in the chapter dealing with 'Speech Analysis' (where it belongs), and when dealt with in other chapters is presented in only the sketchiest form. For example, some technical information concerning cineradiography can be found in Chapter 6 (pp. 101-102), but it is of the most general type (such as noting that the subject's head is stabilized for filming, or that radiopaque pellets are secured to the tongue to act as reference points). There is no mention of the fact that tongue-movement data derived from cineradiograms is critically dependent on whether aspects of tongue motion are referred to a maxillary or mandibular reference point (Kent, 1972); there is no discussion on the effect of frame rate (film speed) on the temporal resolution of cineradiographic analysis; and there is no mention of other X-ray techniques, such as tomography or laminagraphy (Hollien and Colton, 1969), which can be used to view a dimension of the speech mechanism which cannot be appreciated in a lateral (saggital) X-ray view of the vocal tract. Similarly, a discussion of the use of, and problems associated with, electromyography (EMG) belongs in the 'Speech Analysis' chapter, especially because of the widespread use of EMG in speech research, as well as PL's extensive description of electromyographic data in Chapter 6. The only technical information on EMG is found in Chapter 6, and it amounts to no more than a general statement of the advantages of averaged and integrated EMG signals relative to the raw EMG signal (p.86). It would have been appropriate to inform student readers that no matter how an EMG signal is conditioned, the relationship between the MAGNITUDE of the signal and the force of muscle contraction is exceedingly complex (Bigland and Lippold, 1954), especially under those conditions of muscle activity which characterize speech production.[6] Given this information, the student would be wary of those speech researchers who regard EMG magnitude as an index of muscular contractile force (see Weismer and Ingrisano, 1979:531-532 for relevant discussion). Moreover, an adequate discussion of the technical aspects of electromyography would allow that electrode insertion to specific muscles often involves a substantial degree of guesswork. PL does inform his readers of the complexity of electrode insertion (p.85), but does not describe, for example, the potential effects of anatomic variability on the use of textbook anatomy to 'locate' a particular muscle with a needle electrode; nor does he describe the different types of electrode which have been used in speech research. Thus the student who is interested in EMG studies of speech production will, having read *SPAP*, still be unable to evaluate critically the relevant literature.

There are other techniques for the study of speech physiology (such as motion transduction, or palatography) which might have been mentioned in *SPAP*, if only to alert the reader to their existence. These omissions, however, are relatively unimportant due to the relatively recent or marginal use of the techniques. There is one remarkable omission, and that is the nearly complete absence of technical information concerning

aerodynamic measures. PL does describe the manner in which air pressure is scaled (p.73), but offers no discussion of techniques for sensing air pressures or flows. The technique of tracheal puncture is mentioned (p.85), but the principle underlying its application is not described; and, there is no discussion of a well-known technique for estimating subglottal pressure (by means of an esophageal balloon: see Bouhuys, Proctor and Mead, 1966). The single statement concerning flow measurements (p.79) only identifies the relevant equipment, but fails to offer any explanatory discussion. I suppose the minimum amount of space devoted to these aerodynamic measures, as compared to the extensive discussion of acoustic techniques, says a great deal about PL's orientation to speech research. Again, I emphasize that this imbalance is a problem only because PL does not define the limitations of his text, and may therefore suggest to the neophyte that the relative space devoted to topics is somehow proportional to the importance or relevance of these topics.

There is, in Chapter 6, a brief discussion of lung volume measurement by means of plethysmography (pp. 78-79), but the description of the technique is largely inadequate. Volume-pressure plethysmography is a technique in which a subject sits inside an airtight box, such that all body parts below the neck are encased in the sealed chamber. As the subject breathes, the volume of the lungs will change; since the chest wall (chest wall = thorax, diaphragm and abdomen) is essentially incompressible, the lung volume changes will be reflected by equivalent volume displacements of the body surface (Mead, 1960). When such volume displacements of the thorax and abdomen occur within the plethysmograph, the magnitude of air pressure within the sealed chamber will change according to the magnitude and direction of the body surface movement. Now, if a wire-mesh screen is mounted in a wall of the plethysmograph, pressure changes within the chamber will result in the displacement of air through the screen. Such a device is typically configured as a flowmeter, since it can be shown that the pressure differential across the screen (i.e., the difference in pressure between the inside of the plethysmograph and the atmosphere) is proportional to the magnitude of airflow through the screen. When the airflow from plethysmograph to atmosphere (or vice versa) is integrated (summed with respect to time), one obtains an accurate measure of lung volume change. PL's description of the technique is all too vague, as it fails to specify the relationship between changes in plethysmograph pressure and flow through the wire-mesh screen. In addition, PL is mistaken when he claims that, "The... plethysmograph...will not respond to rapid changes in air flow" (p.79). It had been recognized for several years prior to the publication of *SPAP* that this limitation of early plethysmographic systems could be eliminated through rather simple electronic means (Hixon, 1972:71).

It is in the next chapter (6) where PL's conception of the appropriate knowledge base and view of the facts departs radically from my own. PL's account of respiratory function for speech production is re-

markable for its failure to alert the reader to the outstanding work in this area done by Thomas Hixon and his colleagues (Hixon, Goldman and Mead, 1973; Hixon, Mead and Goldman, 1976). The description of how a speaker balances muscular and nonmuscular forces in the respiratory system (pp. 74-75) to maintain a roughly constant subglottal pressure during speech production is awkward and lacking focus. Consider the following passage from Chapter 6 of *SPAP*:

> The internal intercostal muscle monitored [here PL is referring to his figure 6-2] is an expiratory muscle, i.e. it contracts to deflate the lungs and force air out of the respiratory system. The speaker brings this muscle into play to keep the pulmonary air pressure steady as the elastic recoil force falls below the level necessary to maintain the initial level. The 'scheduling' of these different muscles will depend on the length of the sentence that the speaker intends to produce, and the speaker's posture. If you are standing erect, the contents of your stomach will pull downwards [*sic*] and tend to expand the lung volume. If you are flat on your back or in an intermediate position, things will be different and a different pattern of muscular activity will be necessary to maintain the same pulmonary air pressure (Mead *et al.*, 1968). (p.75)

The neophyte reader might have several very basic questions to ask after reading this passage. In the second sentence, for example, the 'initial level' below which the elastic recoil force is said to fall is unspecified; what PL means by 'initial level' is the roughly constant subglottal pressure, but this in no way will be obvious to the neophyte. The next sentence, which describes the factors responsible for the scheduling of respiratory muscular events during speech production, fails to mention the most important determinant of that schedule—namely, the magnitude of the roughly constant subglottal pressure (Hixon, 1973). And, it is unclear to me how PL knows that this scheduling is dependent on the "length of the sentence that the speaker intends to produce...", as I am unaware of any published data which address this issue.[7] Continuing through this passage, the reader will learn that the upright posture results in a downward pull on the abdominal contents (PL does not indicate that this pull is a gravitational effect), but in the supine position "...things will be different...". The industrious neophyte may want to know what 'things' are different, and so may consult the citation of Mead *et al.* (1968) which PL appends to this statement about posture effects on speech breathing. Unfortunately, consultation of the citation will not reveal the identity of these 'things', for Mead *et al.* (1968) mention not a word concerning posture effects on speech breathing.

Later, when PL discusses respiratory control as a linguistic phenomenon, he appears to be somewhat concerned with an explanation of why speech takes place on expiration, rather than inspiration. In this regard he states:

> The physiology of the respiratory system that allows the inspiratory muscles to hold back the elastic recoil force of the lungs probably is the reason that speech takes place on the expiratory phase and the duration of the expiratory phase is the linguistically conditioned variable. (p.76)

This is perhaps an interesting hypothesis, which the neophyte might take to suggest that respiratory control during much of our speech production (i.e., 'conversational' type speech) often involves use of the inspiratory muscles. The hypothesis pales, however, when confronted with the relevant contemporary research which shows the use of inspiratory muscles during conversational speech to be a rare, rather than typical, occurrence (Hixon *et al.*, 1976:334). It is more appropriate to assume that speech takes place on expiration because under these conditions the respiratory system can operate in its most compliant lung volume range (i.e., the range in which the respiratory structures are least stiff, and therefore easiest to displace by the application of muscular and/or nonmuscular forces), and at the same time take advantage of positive pressures generated by nonmuscular (elastic) forces; the net result of these considerations is that speech can be produced with a minimum of muscular effort. If speech were to be produced on inspiration, the respiratory system would have to expend a substantially greater muscular effort to produce a satisfactory output. This would be so whether or not speech was taking place within the most compliant lung volume range.

In his discussion of speech breathing, PL includes some comments concerning possible feedback regulation of respiratory muscle function (pp. 76-77). These comments are placed within the framework of the well-known 'gamma-loop' reflex system (Matthews, 1964; Granit, 1970), the relevance of which has been of some interest to both respiratory physiologists (e.g., Sears, 1971) and speech physiologists (e.g., Abbs, 1973; Hughes and Abbs, 1977). My objection to the inclusion of this material is largely due to the minimal amount of space devoted to a topic which I believe must be developed somewhat more carefully to be intelligible to neophytes. Moreover, PL's account of the gamma-loop is another example from this text of non-rigorous tutorial writing. Thus PL's description of the critical peripheral organ of the gamma-loop—the muscle spindle—implies that it consists of a single muscle fiber; a more precise description would have informed the reader that muscle spindles may contain from two to twelve muscle fibers as well as non-muscular structures (Matthews, 1964). The reader would also take PL's discussion as suggesting that muscle spindles are sensitive to the FORCE of muscle contraction; on the contrary, the primary sensitivity of the muscle spindle is to the length of a muscle and/or rate at which the muscle changes length (Matthews, 1972:156-157).

One final feature of the section on speech breathing physiology should be mentioned here: PL notes that "...the diaphragm performs no

active role in either speech or singing..." (p.77). If this statement is concerned only with the respiratory task of maintaining a roughly constant subglottal pressure throughout an utterance (see top of p.78), it is a reasonable claim. This approach strikes me, however, as an overly constrained view of respiratory function for speech production. My argument here is based on the fact that the inspirations which separate consecutive utterances (i.e., 'speech inspirations') are associated with muscular mechanisms somewhat different from those of inspirations which only serve a vegetative purpose. Specifically, speech inspirations involve diaphragmatic contraction only, whereas vegetative inspirations are produced by combined activity of the diaphragm and external intercostals; and it appears that the respiratory system is adjusted in certain ways during the EXPIRATORY PHASE of speech with the specific purpose of making these 'speech inspirations' more efficient (see Hixon *et al.* (1976) for the relevant experiments, and especially p.350 for the interpretative comments). In other words, 'speech inspirations' do not resemble inspirations in general, and therefore should be included as an important component of respiratory function for speech production. In this sense the diaphragm plays a very active role in speech.

The section on laryngeal physiology in Chapter 6 is not nearly as problematic as the material on respiration, and in some instances (e.g. the discussion of the forces of phonation) the presentation is concise and appealing. The section does suffer from the absence of information on the two-mass (Ishizaka and Flanagan, 1972; Ishizaka and Matsudaira, 1972) or multiple-mass (Titze, 1973, 1974) models of the vocal folds which recently have had such influence on theories of vocal-fold vibration.[8] A related observation is that PL does not provide a discussion of the vertical phase difference associated with vocal cord vibration in the chest register; this omission becomes problematic when PL describes modifications of glottal spectrum due to changes in voice register (p. 89). Specifically, PL correctly notes that phonation in the falsetto register produces less harmonic energy when compared to chest register phonation, but does not tie this in with the absence of vertical phase difference in falsetto.

Although I do not wish to become too deeply involved in what is a rather long-standing controversy, I feel obligated to point out that PL's treatment of factors which control the fundamental frequency of phonation (pp. 85-89) is a distinctly biased one. His conclusion, based largely on Atkinson's dissertation (1973) and his own work (Lieberman, 1967), is that in declarative-type utterances "...the primary determinant of f_0 variation is the subglottal air pressure" (p.89). This view, and the functional implications which may be derived from it, have not been supported by other research findings (Hollien, 1960, 1962; Shipp and McGlone, 1971; Hixon, Mead and Klatt, 1971), which demonstrate the activity of the laryngeal muscles to be the important factor in effecting changes in fundamental frequency.[9] PL does acknowledge the importance of laryngeal muscle activity in adjusting fundamental frequency for certain specific

types of phonation, but, as the above quote indicates, leans heavily on a competing explanation which has failed to receive adequate empirical support.

The larynx section also contains a curious discussion of segmental gestures, particularly as they relate to the voiced-voiceless distinction. PL writes:

> The distinction inherent in the contrast +*voicing* versus -*voicing* (or *voiced* versus *voiceless*) is binary. We are saying that sounds can either be voiced or unvoiced and that there is no intermediate inbetween state. The binary distinction has a physiologic basis insofar as the larynx can either be adjusted to yield phonation when air begins to flow through it or alternately not yield phonation. The physiologic or functional contrast is thus inherently binary. However, the muscular maneuvers that are necessary to adjust the larynx so phonation will take place are not binary. (p. 93).

But later (p. 94) the reader is told:

> The 'binary' quality of voicing as a phonetic contrast thus rests in the acoustic consequences of the total speech mechanism.

Aside from the reader's inevitable confusion over whether the binary nature of the voicing distinction rests at the physiological or acoustical level—or both—one may begin to question exactly how PL chooses to define 'physiology'. After reading the passage on page 93, it seems as if the reader would have to reach the conclusion that the adjustment of the larynx (whether or not the vocal cords are vibrating) is within the domain of physiology, but the muscular functions which effect such adjustments are not. Perhaps PL would argue that the key word is 'functional' (see his definition of physiology on p.3 of *SPAP*), but it still is not clear why muscular maneuvers which are components of some 'general' function are not to be considered as 'physiological'. Quite apart from these considerations of internal consistency and definitional adequacy, however, I suppose one might criticize the substance of PL's discussion of the voicing distinction as being far too simplistic; there are certainly other considerations relevant to this topic which are appropriate for an introductory text (for example, see Catford, 1977:199-200).

The concluding material in the larynx section deals with effects of the supralaryngeal vocal tract on fundamental frequency. This information is presented in a straightforward fashion, and seems to be a very postive feature of Chapter 6. It should be pointed out, though, that PL's description of the manner in which the vocal tract geometry affects fundamental frequency is only one explanation among several which have been advanced in the literature (see Hombert *et al.*, 1979:41-45).

The section on the physiology of the supralaryngeal vocal tract is

in general very good, especially in those parts dealing with the modeling of articulatory behavior (pp. 103-110). The concerns I expressed earlier regarding the essentially static nature of this text were mostly related to PL's exposition of human articulatory behavior. Thus, the minimal attention devoted to the dynamic aspects of supralaryngeal behavior (p.103 and p.120 in Chapter 7) is unfortunate in a text which purports to offer an introduction to speech physiology. Especially noteworthy is the absence of any reference to the extensive cineradiographic work of Kent (Kent, 1972; Kent and Moll, 1972; Kent and Netsell, 1971), especially as regards the dynamic patterns of velar activity during speech (Kent, Carney and Severeid, 1974).[10]

A very negative feature of Chapter 6 is the Table at the end of the chapter (pp. 111-114) which lists some of the muscles and associated functions which may be relevant to speech physiology. PL does not claim this to be a comprehensive list, but the choice of which muscles to EXCLUDE from this compendium appears to be totally unmotivated.[11] For example, none of the respiratory muscles are included, nor is their exclusion mentioned or justified. The stylopharyngeus muscle is not included among muscles of the pharynx (the orientation of this muscle is such that it may help expand the pharynx, especially to help maintain transglottal flow for voiced stops: see Kent and Moll, 1969); the hyoglossus and palatoglossus, two muscles which can play a role in vertical movements of the tongue mass, are not included among extrinsic muscles of the tongue; and the levator and depressor anguli orii, zygomaticus major and minor, mentalis and buccinator muscles, which can be of importance for changing lip configuration, are omitted from the list of lip and face muscles.

Chapter 7 presents information on speech synthesis and speech perception, largely according to the tradition established over the past three decades by investigators associated with the Haskins Laboratories. Certainly, the work done at Haskins Laboratories must be included in any text which intends to present the fundamentals of speech perception to the beginning student; after all, the influence of this work on the experimental and theoretical efforts in this area has been, and continues to be, profound. PL's development of the main themes running through the Haskins tradition, and of the importance of these themes to our understanding of how we perceive speech, is a creditable effort and should prepare the neophyte for further encounters with this line of inquiry. I believe, however, that PL could have strengthened this material even further by commenting on some of the research in speech perception which is not necessarily of the Haskins genre. As is well known, the Haskins approach to speech perception research has involved an almost exclusive use of synthetic (non-human) speech stimuli.[12] Synthetic speech stimuli are, of course, valuable tools in the arsenal of the investigator who wishes to manipulate specific characteristics of the speech signal while controlling others. In my opinion, though, one must be cautious when interpreting an experiment in which a typically occurring characteristic in natural speech stimuli is 'controlled' in the synthetic representation

by ELIMINATION. Thus when synthetic, two formant stimuli *sans* burst are identified consistently as stop-vowel syllables, and the place of the stops can be shown to be a function of the second formant transition, it may be reasonable to conclude that there is some cue-value associated with that transition. This does not mean, though, that the eliminated factor (i.e., the burst) has less or no cue-value for the perception of stop place-of-articulation.[13] In this regard, PL's representation of the Haskins' ideology (see especially p.120) is not balanced by a discussion of those natural-speech experiments which have shown stop-bursts *sans* transitions to be powerful cues for identification of place-of-articulation (Winitz, Scheib and Reeds, 1972). Moreover, there is no discussion of the several experiments which, using natural speech stimuli, have dealt with the perceptual effects of coarticulation (Ali, Gallagher, Goldstein and Daniloff, 1971; Lehiste and Shockey, 1972; Kuehn and Moll, 1972). PL does discuss one experiment in which natural speech stimuli were used, that being the classic study of consonant confusions reported by Miller and Nicely (1955). The description of this study, though, is a rather terse and—I believe—somewhat misrepresentative account of this most influential work.[14] Specifically, PL chooses to illustrate the findings of Miller and Nicely (1955) with a selected observation, which is as follows: "Sounds were confused in consistent patterns. The sounds [p], [t], and [k] for example, as a class were confused with the sounds [b], [d] and [g]" (p.136). The fact is, however, that Miller and Nicely (1955: 349-50) found voicing confusions to be relatively infrequent, especially as compared to within voicing category PLACE CONFUSIONS (i.e., [t] and [k] were more likely confusions for [p] as compared to [b], [d] and [g]). Perhaps a better way to summarize the Miller and Nicely work would have been to describe those articulatory features which were more or less likely to be confused when consonant identification errors were made (see Miller and Nicely, 1955:349-350).[15]

In retrospect, it is unfortunate that PL did not devote more space to consideration of the perception of natural speech stimuli. The results of recent research (Stevens and Blumstein, 1978; Blumstein and Stevens, 1979) have shown that a major tenet of the Haskins *Zeitgeist*—that there are no acoustic invariants associated with the several places of stop articulation (see *SPAP*, p.121)—will have to be discarded. The problem has been, I believe, one of looking in the wrong place for too long a time: Stevens and Blumstein were able to turn their attention away from the synthetic speech model which often employed the second formant transition as the exclusive domain of important place cues, and ask questions which made use of ALL the information in naturally occurring stop-vowel and vowel-stop syllables. Their findings are compelling, and seem to bear out Fant's (1967:640) statement that "...The pertinent problem is to find what transforms we should apply to the speech wave data in order to extract the information bearing elements that operate in speech perception. Attempts to avoid the search for auditory relevant sound characteristics by an uncritical acceptance of the view that perception is merely a reconstruction of the production does not appear very fruitful."

The ambitious task of Chapter 8 is to provide an introduction to the nature of phonetic theories, and to give the reader an awareness of both the traditional and contemporary highlights of these theoretical efforts. There is a very provocative and readable discussion concerning the deficiencies of traditional vowel theory (pp. 139-145); also treated at length in Lieberman, 1976). PL follows this by presenting a more contemporary perspective on this problem, that associated with K. N. Stevens' 'quantal theory' (Stevens, 1972). As pointed out by PL, the traditional vowel theory is structured almost exclusively within an articulatory framework, but the facts of articulation are not always consistent with the theory (see *SPAP*, Figures 8-2 and 8-3, pp.141, 142). The quantal theory provides a more unified explanation because it attempts to include within its scope acoustic and perceptual considerations as well as articulatory factors. The quantal theory is also discussed relative to consonant theory, but this section is not quite as coherent as the preceding information on vowels. The discussion of consonants, however, should nonetheless serve the reader as a stimulating source of information.

On the whole, there is less to criticize about the content of Chapter 8 than is the case for the preceding chapters. There are certain features of this chapter, however, for which critical discussion seems appropriate. For example, on p.145 PL introduces the notion of distinctive features and exemplifies the concept by demonstrating the contrastive nature of voicing in English obstruents. It is not at all clear why PL uses a morphophonemic example rather than a minimal pair to make his point; for the neophyte, it would seem as if the notion of 'phoneme' could be better communicated with a simpler illustration. It is also very curious, I think, that in the discussion of the voicing distinction for stops (pp. 158-161), the term VOICE-ONSET TIME (VOT) (Lisker and Abramson, 1964) is never used; rather, PL refers to this phonetic interval by invoking a feature called PHONATION ONSET. Since the term VOICE-ONSET TIME has been used so extensively in the literature, it should at least have been defined in this discussion of stop voicing. Somewhat later in the chapter, in a section concerned with prosodic features, PL makes some comments on possible biological determinants of normal speech prosody. An important component of this argument is that fundamental frequency variations throughout utterances are largely a function of transglottal pressure changes (which, as pointed out previously, is a decidedly minority report); a related postulate here is that normal prosody is simply a by-product of 'natural breathing patterns' ('natural' subglottal pressure variations), since "the form of the normal breath-group...is a condition of minimum departure from the constraints of vegetative breathing" (p. 170). This last notion seems to be based on the observation that during both vegetative and speech breathing the subglottal pressure must change from positive to negative at the end of the expiratory phase. This correlation-type observation would seem to have minimal theoretical appeal, especially since the change from positive to negative pressure (i.e., the need to change from expiration to inspiration) is REQUIRED in both cases —that is, the demand for the pressure change cannot be shown to be the

result of some causal link ACROSS the two types of breathing (i.e., vegetative and speech). Moreover, as noted above in the critical discussion of Chapter 6, it is not obvious that respiratory function for speech is merely a minimal departure from breathing for vegetative purposes; rather, the respiratory system appears to employ certain adjustments during speech production which are quite different from vegetative breathing (Hixon *et al.*, 1976).

PL later qualifies this discussion of the 'naturalness' of the unmarked breath group (p.173) by noting that different languages and speakers may typically use prosody which deviates from that which would be determined by the biological constraint of 'natural' breathing patterns. He says:

> ...biological constraints *structure* the possible forms of human language although they do not *determine the form* of language. The relation that holds between certain biological constraints and the possible form of sound pattern may be very strong without being entirely deterministic... There is no reason why a structuring principle must be universally manifested. (p.173).

But these thoughts raise difficult questions about how much adherence to a hypothesized 'biological structuring principle' is necessary to impute some significance to the hypothesis. In other words, how many departures from the expected phenomena are required before the hypothesized principle is rejected for its lack of explanatory power?

In terms of somewhat broader concerns, Chapter 8 could have been improved by including more information on fricatives, affricates, diphthongs and semivowels. The chapter is decidedly biased toward a description of stops (consistent with Haskins orientation), but we also have a substantial amount of data bearing on other manners of articulation, which an introductory text should communicate to the reader.

To conclude this critique of the content aspect of *SPAP* (I have no comments on Chapter 9), I feel obliged to make several brief comments concerning PL's view of the role of theory in scientific endeavors. Perhaps it is a consequence of my own unsure feelings as to exactly what 'science' is, or more likely the result of my not-too-successful attempts to comprehend those writers who make the philosophy of science their business, but I must profess confusion with regard to PL's characterization of 'simplicity' *vis-à-vis* theories (*SPAP*, 162-165). Following a brief discussion of the typical role of parsimony in constructing feature systems, PL states (p.165):

> Simplicity metrics...have virtually no value in testing scientific theories. There is no logical procedure that we can use to decide which of two different theories is 'best'. We have

> to test contending theories against the data. The 'best' theory is the theory that relates data that were hitherto seemingly unrelated and that points the way to new relationships that we otherwise would not have thought of... when we have two different theories...there is no logical or formal procedure that can decide which theory is correct.

Certainly PL is entitled to his opinions in these matters; his message to the reader, however, is hardly in the form characteristic of 'opinion' and fails to provide equal time to a view apparently shared by at least several philosophers of science. It is useful to examine the above-cited passage to determine both its relation to the specific linguistic problem (i.e., constructing a feature system) and how PL's general position contrasts with an alternate view. First, the specific linguistic problem is that of the form of a feature system—that is, the form of a theory—and not one of how the theory is to be evaluated against experience, or data. When PL states that simplicity metrics have little value in TESTING scientific theories, he is operating at the level of observations which may or may not confirm a given theory.[16] In this regard, PL seems to go past Chomsky and Halle's (1968:296-297) thoughts concerning simplicity (which PL discusses briefly prior to the passage quoted above: see *SPAP*, p.162), which by my reading do not concern the CONFIRMATION of linguistic theories. Rather, Chomsky and Halle are discussing the FORM of linguistic theories, and allow that simplicity may be a desirable form for such a theory to assume. They do not suggest, it seems to me, that these theories may be confirmed or refuted based on relative levels of parsimony; indeed, Chomsky and Halle (1968:296) explicitly describe how any notion of simplicity WILL GENERATE CERTAIN ASSUMPTIONS which when tested against data will show the correctness or incorrectness of the particular notion. But this is much different than claiming that SIMPLICITY ITSELF is the indicator of theoretical correctness, which seems to be PL's conception of the Chomsky-Halle position.

In a more general sense, the homiletic character of this passage from *SPAP* is unfortunate, because it may suggest to the neophyte reader outright rejection of a principle which for certain scholars is quite useful. For example, since PL is convinced that theories can only be tested against data, it might have been useful to inform the reader that the relative simplicity of a theory may affect its testability. Thus Popper (1965:61) has argued that simpler theories are more testable than relatively complex ones, and so are more desirable. Indeed, the notion of theoretical simplicity has appealed to many philosophers of science (Kaplan, 1964:316-318), and was for Albert Einstein—perhaps the greatest scientific theoretician—the guiding principle of all his theoretical efforts (Holton, 1979:324-326). This is not to say that the application of simplicity criteria to theoretical efforts is without problems, for the notion is often vague (Popper, 1965:241); simplicity is not, however, the useless notion PL would have his readers believe it is.

Logistics

A. Organization

I have very little to say concerning the organization of *SPAP*, since I believe that the ordering of the general topics (i.e., the chapter headings) is a defensible one. My own preference would be to have complete sections on speech physiology preceding the information on acoustics. In addition, the text would have been more appealing if a chapter dealing with speech-related neurology was included. Given contemporary interest in developmental aspects of speech communication and aphasiology, a neurology chapter would seem to be an essential component of any text which intends to introduce students to speech physiology and acoustic phonetics.

B. Clarity of Writing

In evaluating the writing found in *SPAP*, I have been concerned not only with stylistic considerations, but also with what might be termed 'expository clarity'. The evaluation of this latter aspect of the text includes, for example, the degree to which a particular exposition is supported by previously discussed information.

Stylistically, *SPAP* is usually satisfactory, but too often unsatisfactory. Some of this may very well be the residue of poor copy editing (see below, point #4), but PL should be held primarily responsible for poorly constructed sentences, some examples of which are as follows:

> The vocal cords consist of muscles, ligament on a cartilaginous support. (p.11)
>
> In a near vacuum, as in outer space or the moon, where there aren't any gas molecules, sound cannot be transmitted. (p.21)
>
> A tube shaped like a slightly flared trumpet would have similar slightly higher formant frequencies... (p.43)
>
> The frequency of F1, the first formant is 200Hz, the fundamental frequency is 125Hz, so no acoustic energy occurs at F1. (p.59)
>
> The two graphs in Figure 6-1 illustrate the air pressure contours that result. (p.73: It is not at all clear from context what these contours would result FROM.)
>
> In the diagram of Figure 6-3 the pathway with the arrow labeled $\rightarrow\gamma$ transmits the electrical signal from the signal to the spinal cord. (p.77)
>
> If a speaker had not stressed this word, the subglottal air pressure would have about 8 to 10cm H_2O. (p.85: almost certainly an editing error).

> This can be done by introducing an opening that stays open throughout the glottal cycle... (p.91)
>
> The binary oral versus nasal sound contrast cavity thus seemingly could be directly related to the activity of this particular muscle. (p.100: hopefully an editing error).
>
> The ipsilateral connections of both ears, which transmit signals from the left ear to the left hemisphere... (p.124)
>
> The noise and filtering made the listeners' task more difficult and it threw into relief effects that might not otherwise be evident. (p.136)

One of the problems associated with the citation of individual sentences as evidence of stylistic deficiency is, of course, that the relevant exemplars are taken out of context. For the specific sentences reproduced above, I invite the readers of this review to consult *SPAP* and decide for themselves if context improves on the constructions.

There are other stylistic oddities in *SPAP* which I will not pursue in detail here. It is sufficient to note for purposes of illustration that PL has an irritating habit of using deictic pronouns for which the referents are by no means obvious (see p.26, l.9; p.90, l.14; p.96, l.7; p.151, l.38; p.162, l.4); moreover, the overall image of the text is degraded by such awkward occurrences as splitting of a compound ('further more', p.127, ll.6-7) or the invention of a compound ('offpitch', p.33, l.12).

The stylistic problems noted so far may certainly be distracting, and to some degree make the reader's task unnecessarily tedious, but the intelligent neophyte probably has the technical means at his or her disposal to overcome most of these difficulties. Where EXPOSITORY clarity is concerned, however, the neophyte may not be able to compensate for lapses on the part of an author. For example, PL often includes in certain expositions terms which have not been previously defined (and, in fact, are never defined or discussed in *SPAP*). The casual inclusion of undefined technical terms into tutorial discussions would seem by definition to be inappropriate in an introductory text. The following terms in *SPAP* are not supported by a previous definition (i.e., to that point in the text), and several are never defined: air turbulence (31), damped resonances (33), bandwidth (34), dynamic range (48), medial compression (81), external thyroarytenoid muscles (84) and laminar flow (92).

The logic of certain extended expositions in *SPAP* is also bound to confuse readers, mostly as a result of non-rigorous writing (see above, comments on 'Knowledge Base and Facts'). Whereas space does not permit reproduction of these confusing passages, I should support this contention with at least several references to the text. The discussion of the dynamic-range limitations of the sound spectrograph (p.55), for example,

is not well-constructed, because the analogy to photographic film is not kept sufficiently separate from the acoustic considerations. The section on 'Registers of Phonation' (pp. 89-90) contains a very unfortunate confounding of voice-quality comparisons WITHIN and ACROSS speakers (see especially p.89, first paragraph in section). And, finally, the brief discussion of the original goal of the Haskins' synthesis efforts is stated haphazardly, especially in the first paragraph where PL attempts to draw a distinction between print-reading devices which would operate on old *versus* new material (pp. 117-118).

C. Graphics

SPAP contains approximately one hundred figures, of which approximately 70% are original[17] with the remainder drawn from various sources in the literature. On the whole, the selection of particular figures seems to be well-motivated, and the relationship between text and figures is often complementary. There are, however, several instances throughout *SPAP* of poorly constructed figures or of failure to meld textual material with graphic displays; these are described here in chapter-by-chapter format:

Chapter 2

Figure 2-1 (p.4), which is captioned 'The three physiologic components of human speech production', fails to include the abdomen as part of the subglottal system; if this figure had been captioned 'The respiratory airways', it would be satisfactory as drawn.

Figure 2-2 (p.6) has been commented on previously in this review (p. 86). I add here that whereas the text says that Ppl is labeled in Figure 2-2, no such indication is found in that figure.

Figure 2-3 (p.8) is an example of a graphic which is poorly integrated with the text. The text claims that this figure shows "some of the muscles that are active in the regulation of respiration..." (p.8). The diaphragm is then described, but is not pictured in Figure 2-3; moreover, the two abdominal muscles shown in the figure (transverse abdominis and rectus abdominis) are not mentioned in the text.

Chapter 4

Figure 4-8 (p.40) is described in the text as a sketch of the vocal-tract configuration for schwa, but is captioned simply 'The adult human vocal tract'. The vocal-tract configuration shown does not look very much like the idealized configuration often shown for [ə] (Fant, 1960:85) and in fact looks more [ɪ]-like.[18] It is not clear why PL chose to use this drawing to exemplify the 'uniform-tube' concept, but the identical drawing

(with a different caption) turns up later in *SPAP* as Figure 6-12 (p.95) to identify principle landmarks of the supralaryngeal speech mechanism, which seems to be the appropriate use of this sketch.

Chapter 6

Figure 6-2 (p.74) is referenced incorrectly; the citation given is Draper, Ladefoged and Whitteridge in the 1960 volume of the *British Medical Journal*, but this figure actually derives from an article in the 1959 volume of the *Journal of Speech and Hearing Research* (p.17) by the same authors. The date included in the figure caption is not a misprint, since the entry in the bibliography (p.190 of *SPAP*) is the incorrect reference.

Figure 6-13, captioned 'The pharyngeal constrictor muscles', is very poorly integrated with the text. First, the figure shows muscles in addition to the pharyngeal constrictors, including the mylohyoid, hyoglossus, buccinator, tensor veli palatini, levator palatini, stylopharyngeus, and styloglossus. Two of the constrictors, the superior and medial, are in the text identified by abbreviated labels (SC and MC) which do not appear in the figure. On the other hand, two muscles which are identified ONLY by abbreviated labels on the figure (LP and MH) are not identified fully in the text. To make matters worse, the stylopharyngeus muscle is discussed in the text (p.97), but not identified in the figure. It might be argued, I suppose, that PL informs the reader that "all of the muscles that we will sketch and discuss are noted in Table 6-1, which is keyed to these diagrams" (p.97). The problems with Table 6-1, however, have been discussed previously (p.97 of this review), and it is pertinent to note that the stylopharyngeus, which is mentioned in the text and sketched (unlabeled) in Figure 6-13, is nonetheless missing from Table 6-1.[19]

Figure 6-14 (p.97) is captioned 'The "strap" muscles that connect the mandible, skull, larynx, and breastbone', but in fact shows no muscular connections between mandible and skull, mandible and larynx, or skull and larynx.

In Figure 6-19 (p.104), the reader is shown a mechanical model for fricative production in which a cylindrical tube is contained within a wooden sphere. The neophyte may wonder why the tube concept (introduced earlier in *SPAP*) is now incorporated into the sphere, but PL offers essentially no information concerning this figure. The cylindrical tube in the model represents the vocal tract, whereas the sphere approximates the radial dimensions of a man's head. The sphere is useful in this model since it permits a relatively accurate assessment of the acoustic impedance to sound radiation from the mouth (Flanagan, 1972:36-37). It should be pointed out that whereas 6-19 is not referenced in the caption (and so may appear to be an original figure), it is actually a minimally modified

reproduction from an MIT progress report by Heinz (1958, cited in Flanagan, 1972:209).

D. Editing

SPAP is not a well-edited text, both in terms of errors which do and do not make a difference to the neophyte reader. The errors which do not make a difference—that is, misspelled or omitted words—are sprinkled throughout the text and are at least twenty in number. The editing errors which are likely to confuse readers mainly involve mathematical formulas, but there also seems to be a problem with phonetic symbols. The former problem is first encountered on page 24, where a set of expressions appears in the following form:

$$f_1 = \frac{1}{T_1} = \frac{\frac{1}{T_0}}{2} = \frac{2}{T_0} = 2f_0$$

The term following the second 'equals' sign is incorrect, and should be $\frac{1}{T_0/2}$.

On p.51, an expression for fundamental frequency (F_0) is again mishandled; PL writes, "The average fundamental period in that interval therefore is 0.07 sec., i.e., 70msec. The 'average' fundamental frequency for this interval of speech is therefore equal to 140Hz." If the average period (T_0) were in fact 70msec., the average fundamental frequency would be not 140Hz, but

$$F_0 = \frac{1}{.070} = 14.28\text{Hz}$$

At the bottom of p.51 the formula for the period is used and yields the correct value (7msec.), but amazingly the original error is repeated in the formula at the top of p.52, which reads:

$$F_0 = \frac{1}{T} = \frac{1}{70/1000} = 140\text{Hz}$$

To correct this, the '70' should be replaced by a '7'.

Finally, Chapter 8 contains some curious usage of phonetic symbols which I assume are editing, as opposed to conceptual, mistakes. On p.139 [e] is stated to be the vowel sound in *bet* (I assume PL meant [ɛ]); on p. 152, one of the quantal vowels is said to be [m]—it should be [u]; and, on p.154, the following statement appears: "...some sounds like the sounds [h] and [w] in the words *horse* and *cow* are often classified as semivowels or semiconsonants."

Conclusion

The reader of this review has certainly determined at this point that, on balance, the present evaluation of *SPAP* is not a positive one. I stress the 'on balance' qualification of this negative evaluation, since there are some very good sections within the text. A book review, however, must focus in part on the degree to which material within the text satisfies the author's mission. PL is very explicit in describing *SPAP* as an introductory text, yet my reading of the text leads me to conclude that *SPAP* is not consistently characterized by the tutorial rigor and care in preparation which should be found in an effective introductory text.

The best, self-contained alternative to *SPAP* is probably Minifie, Hixon and Williams (1973), although this multiauthored text is uneven in quality and depth of coverage, and does not contain material on speech perception. Another alternative is Dew and Jensen (1977), which covers most of the material found in *SPAP* and is characterized by a tutorial rigor (i.e., clarity of writing) not found in *SPAP*. Although both Dew and Jensen (1977) and Minifie *et al.* (1973) are not as sophisticated as *SPAP* where theoretical matters are concerned, and both are deficient in certain respects (as any text must be), I believe either is preferable to *SPAP* as a text for an introductory course in speech physiology and acoustic phonetics.

One final comment: on the copyright page of *SPAP*, it is stated that certain illustrations have been reprinted from PL's *On the Origins of Language* (1975). This statement should perhaps have been expanded to say that *SPAP* also reprints, virtually verbatim, a certain portion of the text from *Origins*. Chapters 3 and 4 of *SPAP* are only slight modifications of Chapters 4 and 5 of *Origins*, and much of the information in Chapters 6 and 7 of *Origins* is found in nearly identical form on various pages of *SPAP*.

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I am grateful to Tom Hixon for many fruitful discussions regarding the information in *SPAP* on respiratory function for speech production.

FOOTNOTES

[1]In other words, a speech physiology and acoustic phonetics text by a linguist would initially be more attractive to the linguist instructor than would books which deal with similar subject matter but are written by authors who are speech pathologists and/or audiologists, such as the texts by Zemlin (1968) and Minifie, Hixon and Williams (1973).

[2]One might accuse me of taking on a straw man here, since it could be argued that when PL states that "the aim of speech science must be to EXPLAIN why certain sounds tend to occur..," and so forth, he identifies the constraints on the material to be presented. But nowhere does PL suggest that this material constitutes an unorthodox INTRODUCTION to speech physiology and acoustic phonetics (which, in my opinion, it most certainly does), and nowhere does he state that the text is largely biased toward those who are interested in phonetic feature systems (see, for example, the odd discussion of the possible feature representation for the functioning of the levator palatini muscle on p.100 of *SPAP*). I don't take PL's apodictic declaration of the purpose of speech research ("The aim of speech science MUST be...") as a signal to the neophyte that the presented material isolates a portion of the knowledge base.

[3]The pleural space is that region which separates the membranous lining of the lungs (visceral pleura) from the membranous lining of the interior thoracic wall (parietal pleura). These two membranes are actually intimately connected, and the region between them is what anatomists choose to call a 'potential', as compared to 'actual', space.

[4]This is a special problem in the case of fricatives, where the spectrum may be a function of constriction shape, and thus point of articulation.

[5]I assume the expression 'wave pressure wave' is an editorial error, and that PL meant 'pressure wave'.

[6]During isometric contraction (i.e., where the muscle does not change length but does change tension), there is a direct relationship between the magnitude of an integrated EMG signal and the force exerted by the contracting muscle; under conditions where muscles are changing length (isotonic contraction), however, such a direct relationship does not exist, but depends on the velocity with which the muscle changes length as well as the direction of length change (i.e., shortening *vs.* lengthening). During speech production it can be assumed that the muscles in use are changing length, so it is inappropriate to assume a direct relationship between EMG magnitude and force of contraction (for an opposite, but incorrect view, see Gay and Harris, 1971:241).

[7]PL's claim of a relationship between respiratory function for speech production and the length of an utterance reappears on p.76, where it is stated that the "...amount of air that a speaker takes into his lungs is usually proportional to the length of the sentence that will be uttered." Again, it is not obvious which body of data supports this claim (PL offers no citation in this context); further, it would have been useful for the reader to know what is meant by "...length of a sentence...", as there are several criteria, not all of which need be positively correlated, which may be taken as indices of 'sentence length' (such as number of syllables in an utterance, number of phonemes per utterance, or simply the temporal extent of an utterance).

[8]Broad (1979) provides an excellent review of such models of the vocal cords and their functional implications.

[9]See Shipp, Doherty and Morrissey (1979) for the most recent statement of this position.

[10] I believe this is an especially unfortunate omission on PL's part, because he does include a somewhat detailed discussion of the nasalization gesture (p.100). This discussion, however, focusing on whether or not one can describe the nasalization gesture by the binary scheme ±LEVATOR PALATINI TENSIONING, seems to me little more than the construction and subsequent destruction of a proverbial straw man.

[11]At the least, the reader will have no idea why certain muscles were not included.

[12]One notable exception is Dorman, Studdert-Kennedy and Raphael (1976).

[13] I should point out that my stated concerns for the lack of isomorphism between the perception of synthetic and natural speech are merely inelegant paraphrases of Fant (1970:54), who said:

> The Haskins Laboratories systematic studies of the perception of simple stylized formant patterns have contributed greatly to our knowledge of the perceptual significance of formant data. However, the potential risk when working with simplified synthetic stimuli is that they may become insufficient carriers of phonemic cues and that the conclusions drawn from such experiments will be valid for the particular synthesizer only and not for human speech.

[14]At least the following studies, all of which have certainly contributed to our understanding of speech perception and/or memory for speech sounds, have been influenced directly by the Miller and Nicely (1955) work: Conrad (1964); Wickelgren (1965: cited by PL in *SPAP*); Wic-

kelgren (1966); Singh and Black (1966); Klatt (1968); Graham and House (1971); Singh, Woods and Becker (1972); Wang and Bilger (1973). This partial listing of relevant works does not include those which have dealt with hearing-impaired listeners.

[15]Other aspects of PL's description of Miller and Nicely (1955) do not agree with the original report. PL says that listeners in the Miller and Nicely study "...identified short words..." (p.135); in fact, the speech stimuli were CV nonsense syllables in which V = /a/ and C was varied. PL also states: "The stimuli that Miller and Nicely used were derived from tape recordings that were made by real speakers..." (p.136); actually, all stimuli were delivered by live voice (Miller and Nicely, 1955:339).

[16]PL apparently believes that data will decide which of several theories is CORRECT; an opposing view held by several philosophers of science (Popper, 1965) is that scientific advances are gained by refutation, rather than by confirmation, of existing theories. Readers may be confused by the requirement of theory confirmation implied on p.165, compared with the later statement that understanding is advanced by theory refutation (p.188).

[17]By 'original' I mean originated (presumably) by PL. The fact is that many of the 'original' figures in *SPAP* (approximately 35-40%) appeared previously in PL's *On the Origins of Language*. More is said concerning the overlap between *Origins* and *SPAP* in the conclusion of this review.

[18]For example, compare PL's drawing of the neutral vocal-tract configuration in his 1975 work (p.61) to Figure 4-8 in *SPAP*. This comparison is somewhat odd, perhaps, since PL's 1975 work says: "The vowel [ʌ] (the first vowel in the word *about*) is perhaps the 'simplest' and most basic vowel sound" (p.61), whereas *SPAP* says: "The vowel [ə], called schwa (the first vowel in the word *about*), is perhaps the 'simplest' and most basic vowel sound" (p.39).

[19]Similar problems are associated with the palatoglossus, external pterygoid and hyoglossus muscles, which are pictured and labeled in figures but which are not included in Table 6-1. And this does not include all those muscles sketched but unlabeled which are omitted from Table 6-1. The drawing in the lower right hand panel of Figure 6-16 is a good example of such unlabeled muscles.

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