been published, one can no longer assert that Schmidt's work cannot be replicated.

Alcock (and the others) ignore several major areas of research in which there has been considerable conceptual replication: findings relating hypnosis to ESP success; findings relating nonneuroticism to ESP success; findings relating extraversion to ESP success; findings relating avoidance of response bias to ESP success; and findings relating "hypnagogic-like states" to ESP success. Summaries of this work, plus references, can be found both in the *Handbook of Parapsychology* and in *Advances in Parapsychological Research*, Vols 1 and 2, edited by Stanley Krippner (1977–1978). Over a hundred studies are involved overall. Any evaluation of the experimental literature that does not consult these studies can hardly be regarded as a scientific evaluation. It is interesting that Alcock gives no reference in the book to any direct parapsychological research that he himself has conducted. Wouldn't such research be an important component of a scientist's evaluation?

In short, *Parapsychology: Science or Magic?* has both strengths and weaknesses. It points intelligently to many of the problems facing those who evaluate claims for anomalous communication and reminds us that efforts to label parapsychology a true full-fledged science are at best premature. However, by its omissions it also reminds us that there can be bad science (or pseudo-science if you prefer) among counteradvocates as well. Perhaps it is time for those concerned to reevaluate the appropriateness of strong advocacy or counteradvocacy positions. It may be best to abandon such emotion-laden labels as *parapsychology* and to work instead toward a sense of cooperation in evaluating a completely valid scientific question: What are the current communication strategies to which humans (and animals) have access, and how can we enhance them? Inferences about the nature of mechanisms involved can be allowed to develop as the strength of the data base permits, and the business of science as a socially useful endeavor can proceed, one hopes, more effectively.

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# Modeling the Fundamental Frequency of the Voice

William E. Cooper and
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**Fundamental Frequency in Sentence Production**
New York: Springer-Verlag, 1981.
223 pp. $24.90

*Review by*
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The subject of this book is syntactic influences on the fundamental frequency (FO) of the voice. The book contains previously unpublished data, but the major concepts are familiar from Cooper and Sorensen (1977), Sorensen and Cooper (1980), and Cooper and Paccia-Cooper (1980). The authors treat three topics: Declination, an overall downtrend in FO widely reported in declarative speech, is investigated as a mirror of syntactic planning. A quantitative description of it is attempted. Fall-rise configurations at phrase boundaries are studied as indicators of boundary strength. The authors make an effort to extend to the domain of FO the idea that syntactic boundaries can block the application of phonetic rules.

The subject matter of the book requires an interdisciplinary approach, using results and methodology from both psychology and linguistics. The authors advocate such an approach, but fail to carry it out. In linguistic theory, phonology (the system of rules dealing with speech sounds and their structure) mediates between syntactic representation and speech production. Cooper and Sorensen suggest (pp. 3–4) that their concerns are orthogonal to those of the major works on the phonology of intonation. As a result of this view, they do not profit from basic results about the structure of the phenomena they are seeking to investigate. Nor do they make their view persuasive by formulating an alternative set of issues. Rather, the book suffers from a pervasive lack of critical thought. Consideration of theoretical alternatives appears not to have guided either the design or the interpretation of experiments. Many experiments fail to control for well-known effects, even effects discussed in works cited. For example, the design of experiment 3.2.3 does not take into account a predictable difference in intonation type. Similarly, a series of experiments in chapter 4 does not control for glottal stop insertion, which strikingly depresses FO, and which would be more likely to apply in some of the sentence types compared than in others. In addition, the statistical analyses are ill conceived; examples are discussed below.

The proposal that Cooper and Sorensen view as the most significant outcome of their investigation (p. 160) is the top-line rule, which describes declination by using the first and final peak values to predict those in between. Three deficiencies in the development of this proposal make it a poor centerpiece for the book. First, the authors put forward this proposal, like others in the book, without serious discussion of alternatives. One of the main reasons to study declination is that it may reflect advance planning of

speech production. However, one work cited (Fujisaki & Sudo, 1970) generates declination without such planning by using exponential functions. Although the output of the topline rule resembles an exponential, Cooper and Sorensen presuppose advance planning without argument. All models considered are close relatives of the model adopted and use the final peak value in predicting earlier ones. A second reason to study declination, as the authors observe, is so that it may be factored out in future studies of other influences on FO. For example, they note that unequal stress can cause medial peaks to fall above or below the predicted declination line. It is by comparing the peak heights to the predicted declination that one might hope to model such effects quantitatively. This concern, which is a central one, cannot be addressed by a rule that uses particular peaks to predict others, as the topline rule does. The peaks used as anchor points are themselves variable due to stress. So, the topline rule confounds stress effects on the anchor points with declination. To separate these influences, it is necessary to posit an implicit declination function and then solve for stress and declination effects simultaneously. This is the approach taken in Fujisaki and Sudo (1970) and Liberman and Pierrehumbert (1979).

The third and most serious problem with the topline rule arises because of the statistic used in fitting the model to the data. The error metric used is the mean *signed* deviation. The authors use this metric rather than the mean squared or mean absolute deviation because they believe it permits them to capture the trend of the data while eliminating extraneous effects due to factors like vowel quality and stress. This belief is completely misguided. Any nonvertical line through the mean of a set of points yields a mean signed deviation of zero. As our figure shows, such a line need not bear any relation to the trend of the data. Similar results obtain for curves of other shapes that are fit under appropriate transforms. Thus, the small nonzero error of Cooper and Sorensen's model cannot be taken to mean that the model captures the main features of the data. Comparisons between small nonzero errors for alternative formulations, like those on pages 45 and 48, are meaningless.

Inadequate data analysis also makes it difficult to interpret the results on fall-rise patterns in chapter 3. The consensus

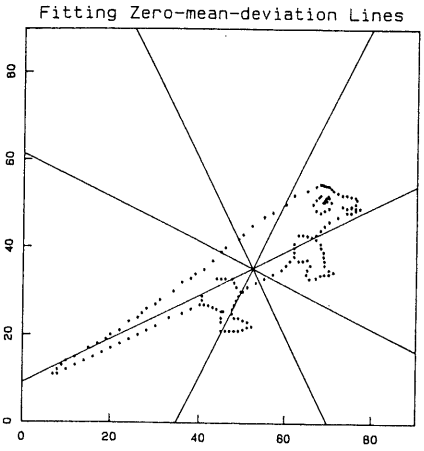


**Figure 1.** All of the lines shown fit the data points with a mean signed deviation of O. Mean absolute deviations range from 5.1 to 42.

of the linguistic sources cited is that the basic fall-rise patterns fall into two categories. (Some variants that were produced in Cooper and Sorensen's experiments, for example, the vocative pattern in experiment 3.2.3, would fall into other categories.) One pattern, which has a fall to the bottom of the speaker's range, occurs at the boundary between one intonation phrase and the next. The other, which involves a less extreme fall, occurs within a single intonation phrase. Works presenting theoretical analyses of this contrast include Halliday (1967), Crystal (1969), and Liberman (1979). The relationship between intonational phrasing, which is an aspect of the phonological representation, and the syntactic phrasing is observed to be complex. Intonation phrase boundaries, which may or may not be marked by a pause, occur more readily at stronger syntactic boundaries than at weaker ones. However, an intonation phrase need not be a syntactic constituent, as Selkirk (1978) and others have noted. At a given syntactic boundary, an intonational boundary is typically optional. Its probability of occurring is influenced by phrase length, speech rate and style, and the information structure of the discourse. To summarize, then, linguistic theory posits two categories that occur with different probability in different circumstances.

Cooper and Sorensen present a different picture, although they do not note that it differs from that developed by linguists. Under their account, the depth of the valley in fall-rise contours varies continuously, reflecting syntactic boundary strength. This is surely the most striking theoretical claim in the book. Unfortunately, the experimental data presented to support Cooper and Sorensen's position are compiled in a way that cannot discriminate between it and the alternative. Measurements at designated locations are averaged and compared using *t* tests. This procedure would give a reasonable picture of data that varied continuously, but it would also make data that fall into two categories with varying probability look as if they varied continuously.

References to the literature tend to err in the direction of asserting support for Cooper and Sorensen's claims. For example, on page 173 the authors state that the FO configurations that disambiguated phrasing in Streeter (1978) are not reported, and suggest that these were probably the fall-rise configurations they investigated in chapter 3. In fact, Streeter does describe the FO configurations in her study, and they are not the same as those studied by Cooper and Sorensen. Similarly, the authors report on page 178 that syntactic information derived from FO by a procedure described in Lea (1979) is used in a speech recognition system to guide the segmental analysis toward the phrase boundaries. This is where they believe the form of phonetic segments to be least variable. According to Lea (1979) and Medress (1979 pp. 445–460), however, the system treats stressed syllables, not syllables near phrase boundaries, as particularly reliable. Errors like this make the book less than useful as a reference or as an introduction to the field.

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# Mechanisms of Intersensory Perception

Richard D. Walk and Herbert L. Pick, Jr. (Eds.)
**Intersensory Perception and Sensory Integration. Perception and Perceptual Development: A Critical Review Series, Vol. 2**
New York: Plenum Press, 1981.
436 pp. $32.50

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This volume is concerned with intermodal relationships in perception. Of the three main parts, the first, with chapters by Bushnell, Butterworth, Abravanel, and Jones, deals with developmental aspects of intermodal perception. The second part, with chapters by Lackner, Cohen, Fraisse, and Cutting and Proffit, concerns event perception and intersensory integration in normal adults. The third part, with chapters by Millar, O'Connor and Hermelin, Cratty, and Samuel, deals with intermodal perception in special populations, particularly those with sensory or cognitive deficits. The volume provides a wealth of behavioral data concerning how information arriving via one sensory mode affects the processing of information arriving via a different sensory mode. The reader is left in no doubt that important and powerful intersensory interactions indeed occur.

The first section of the book examines the development of intermodal relations in normal children. Infants form the focus for Bushnell and Butterworth, and older children for Abravanel and Jones. A major question considered is whether intersensory equivalences are innate or acquired through learning. These authors argue that this way of posing the question is rather simplistic, and they suggest that there is no built-in equivalence of intersensory information, but rather innate mechanisms to aid in finding such equivalences. Another major question dealt with is whether performance on intramodal tasks improves with age faster than performance on intermodal tasks.

The second part of the book deals with higher order integration. Lackner describes and interprets his interesting contributions to the study of sensory-motor control, and adaption to sensory rearrangement. Cohen discusses some of the physiological and anatomical underpinnings of proprioception. An important point made by both these authors is that in localizing visual objects, we use information about our postural orientation. Fraisse describes his classical work on rhythm and its intersensory properties. Cutting and Proffit describe their elegant research on the perception of movement.

In the third part of the book, sensory integration in special populations is analyzed. Chapters by Millar and by Hermelin and O'Connor deal with the blind, the deaf, the autistic, and the mentally subnormal. Two other chapters extend the theme in different directions. Cratty discusses the effects of additional perceptual experience in special populations, particularly the mentally deficient. Samuel examines perception in athletes, ballet dancers, and artists. Some interesting observations emerge in these chapters. For example, the spatial schema as it is built up when haptic information is the main source (as in the case of the congenitally blind) is rather different from the space of vision. Visual space relies a great deal on reference cues, in contrast to the space of touch. Further, the visual system generalizes between larger and smaller versions of the same shape, so it may come as a surprise to the sighted to learn that braille readers have difficulty in generalizing to slightly larger braille letters.

### Three hypotheses

The main theoretical issue the volume addresses concerns the locus of intersensory interactions within the processing system. Three general hypotheses are entertained. On the first, information is initially encoded in modality-specific form, and intersensory integration results from interactions between modality-specific systems. On the second, vision is regarded as the primary modality, with information arriving via other modalities being translated into visual form. On the third, information arriving via any modality is translated into supramodal form, and intersensory integration results from activity within the supramodal system.

Although these competing hypotheses are often invoked, the behavioral evidence does little to decide between them. This may be expected because the hypotheses as stated are too general to allow for testable predictions. For example, it is unclear how we might distinguish behaviorally between interactions that occur within a supramodal system, and those that result from highly effective intermodal connections. Since most of the authors are critical of theories that stress the role of specific sensory systems in higher level processing, it may be useful to examine the properties required of a supramodal system in which sophisticated perceptual processing is assumed to occur.