

istics of speech production. Furthermore, in the case of fall-rise patterns examined in Chapter 3, the results indicate that the simple binary distinction noted by the reviewers provides no basis for accounting for the data, including incremental magnitudes of falling and rising patterns that appear to be contingent on the specific strength of each phrase crossing the boundary (summarized on p. 34 in the book). It may be that the binary distinction noted on the basis of impressionistic observation is relevant at a gross level of description, but the acoustic data indicate the need for a more elaborate theoretical account, such as the pragmatically based one proposed in the book. The reviewers' claim that their binary distinction might be beclouded in the data by differences in phrase length, speech rate, and style is largely irrelevant because most of the sentence materials used in the fall-rise studies were structurally unambiguous. Proper statistics were used to match-pair data from the same set of speakers whose individual variations in rate and style across versions of each ambiguity can be assumed to be systematic.

Item 3. The reviewers claim that "in consideration of theoretical alternatives, the authors do not have guided either the design or the interpretation of experiments" (p. 690). The decidedly linguistic orientation of the reviewers notwithstanding, it is surprising that they do not acknowledge the discussion of *psychological* issues that motivate the design and interpretation of the various experiments. The issues are appreciated in the more comprehensive review of the book by Abbeduto (1982).

Item 4. The reviewers claim that the regression line used in testing the declination model's predictive adequacy was founded on a "completely misguided" belief in the elimination of extraneous factors. The reviewers attempt a *reductio* by arguing that "any nonvertical line through the mean of a set of points yields a mean deviation of zero" (p. 691), illustrating cases in which such lines bear no relation to a set of data points resembling the outline of an iguana. The real message, of course, is that the mean signed deviation must be used with proper care, with some consideration of the data's pattern and variance, reported in the book's figures and tables (variance for each of several speakers is necessarily due to the influence of the speaker's characteristics and other factors). Even the simple-

minded modelers among us would doubtless observe that an iguana template would provide a better fit to the data shown by the reviewers than would any of the lines in their illustration.

Item 5. The reviewers compare our model of declination with two others, but nowhere do they indicate the predictive power of these alternative models for fitting the intermediate peaks for which our model was designed. Such a comparison would be fruitful in distinguishing the predictive merits of the competing models; but for our purposes, the very close fit between data and our model was sufficient for testing issues that motivated the modeling attempt in the first place, such as the distinction between declination resetting and local fall-rise patterns. The model is clearly falsifiable, and any attempt to compare it with alternatives must reckon with the model's ability to handle different speaking rates, sentence types, and other factors studied in Chapter 2.

Item 6. The reviewers claim that "many experiments fail to control for well-known effects" (p. 690), and they cite two instances. In both cases, the inability to provide proper control is discussed in the book itself. The control difficulty arises because English often does not permit, or seems not to permit, an experimental design that involves a single independent variable, making it necessary in some cases to conduct a number of systematically related experiments in the hope of finding a common denominator of interpretation. It is the experimentalist's challenge to construct language materials that are as well controlled as the language permits, and on this score the materials in this book compare favorably with previous literature. In the case of the experiments of Chapter 4, the reviewers correctly note the influence of glottal stop insertion but fail to acknowledge the cleanly designed experiment on stress blocking in the same chapter. As stated, the authors based their main conclusions in this chapter on the stress blocking study because of its appropriate control.

Item 7. The reviewers claim that "references to the literature tend to err in the direction of asserting support" (p. 691) for the authors' claims, and they cite two examples. In the case of the reference to Streeter's study, they note that the authors claim that the F0 configurations of Streeter's stimuli were not reported and the reviewers state that they were. In fact,

Streeter provides no description whatsoever of the acoustic properties of F0 configurations for the natural base utterances in Experiment 1 and only a general description of the configurations of utterances in Experiment 2. In the case of the reference to Lea's work on speech recognition by machine, they are correct in noting that stressed syllables provide the most reliable information; but the authors never claimed otherwise. Rather, they claimed that recognition is "more reliably" represented at phrase boundaries than within phrases.

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On finding the iguana

Preplanning is one of the most interesting psychological issues in Cooper and Sorensen's book, and this is why we scrutinized their proposal so critically. The effect of phrase length on the height of the first F0 peak is one of two types of preplanning in their model; this first type of preplanning effect seems to represent an optional, pragmatically conditioned choice of pitch range because it has not been observed for some subjects and reading conditions (Grosjean, 1982; Sternberg, Wright, Knoll, & Monsell, 1980) and because wide variation in initial peak height is possible for any given phrase length. Our remarks (in the review) concerned the second type of preplanning effect, a length-dependent difference in the time course of declination for a given choice of first peak height.

Summary statistics showing a decrease in slope of declination with an increase in utterance length appear on the surface to support the existence of the second type of preplanning. Such an apparent slope difference, however, can arise without preplanning if the downtrend under investigation is an exponential decay or if there are special effects at the edges of the phrase. Our concern about this point is not just abstract mathematical nitpicking—Anderson (1979) treats the "downsteps" observed in many African tone languages as abstract exponentials, and Fujisaki, Hirose, and Ohta (1979) model Japanese F0 contours using an exponen-

tially decaying "utterance component." Without a serious examination of such alternative models, it is not legitimate to claim that control of the time course of declination is planned in a way that depends on phrase length.

Numerical analysis is blind and unproductive without recognition of category distinctions; Cooper makes our point nicely when he observes that an iguana template, not a line, should have been fit to the data points in our figure. Linguistic descriptions, precisely because they are usually qualitative in nature, are a reasonable source for hypotheses about the categorization of intonational phenomena. For example, linguists with otherwise very different accounts of English intonation have noted a qualitative distinction between phrase-final and phrase-medial regions due to the (possible) presence of a "boundary tone" (Lieberman, 1979; Pierrehumbert, 1980), "phrase accent" (Pierrehumbert), "nuclear tone" (Crystal, 1969; Halliday, 1967; O'Connor & Arnold, 1961), "cadence" or "end-glide" (Vanderslice & Ladefoged, 1972), and so forth. This distinction can mediate a more complex pattern in averaged data than Cooper suggests.

The readings of the ambiguous sentences in the experiments differed in syntax and information structure and would carry different probabilities for the occurrence of an intonation phrase boundary at the measurement site. Under this account of the patterns observed in the means, "red paint" and "white paint" are sampled with varying probabilities and then mixed together (by taking the mean) to yield "pink paint" of various saturations. Under Cooper and Sorensen's model, the original paint samples are claimed to come in all the various saturations of pink. By reporting only mean F0 values at the measurement sites, Cooper and Sorensen make it impossible to compare the two accounts. Linguistic descriptions of intonation would also predict the results of the stress blocking experiment in Chapter 3.

We stand by our assertion that using the mean signed deviation in fitting and evaluating models has no validity. We cannot imagine any circumstances in which this method would be useful because it does not bring out any useful properties of the data.

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On modeling fundamental voice frequency

In grappling with "On Finding the Iguana" let us start with the tail and work our way forward. First, the book shows that our model is applicable not only to pooled data but also to the individual utterances of each of twenty speakers (Table 2.7). Study of this representative table and raw data for utterances in this and other experiments revealed no systematic bimodal characteristics, aside from those already reported, that would give credence to the reviewers' red-white analogy.

Second, the need to compare the predictive power of our model with that of others (agreed on in my original reply) does not detract from the utility of our model in addressing issues, for the first time on a large scale and controlled basis, involving the domain of declination. Such issues include, for example, the distinguishing of declination for a single main clause from the separate forms of declination that accompany parentheticals (Figure 2.32) and distinct main clauses

(Figure 2.43). Examination of these other figures in the book shows that model is both falsifiable and useful differentiating tool. For these issues use of the mean signed error was a priate in assessing the fit between r and data.

Third, regarding preplanning, the results obtained in the two studies by the reviewers might be attributed as Pierrehumbert herself (1980, p. among others has noted, to the highly artificial speaking circumstances present in those studies than in the by others and ourselves showing positive results. The data on slope patterns of declination must await comparative modeling to assess their bearing on preplanning, but the results for utterance-initial F0 peaks stand in support of some of such planning, as originally proposed by William E. Coe at Harvard University.

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