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This paper reviews Philip Lieberman's "Intonation, Perception, and Language," (Research Monograph No. 38) Cambridge, Massachusetts, M.I.T. Press, 1967. The review is also scheduled to appear in the "Journal of Linguistics." (JD)

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INTONATION, SCIENTISM, AND "ARCHETYPALITY"¹

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Editor's Note: the following is a review of Philip Lieberman Intonation, perception, and language. (Research Monograph No. 38.) Cambridge, Mass.: M.I.T. Press, 1967, pp. xiii + 210.

The author thinks that adults produce and perceive intonation (both in English and universally) in terms of certain "archetypal" patterns observable in the cries of newborn infants. Armed with this insight, he believes he has succeeded in isolating the grammatically relevant prosodic distinctions of English (capping the hapless near-misses of his predecessors) and he proceeds to postulate "archetypal" physiological correlates for each of two binary features. Further, he presents experimental evidence which, he claims, "quantitatively supports our hypotheses regarding both the nature and the primacy of the archetypal correlates [p. 61]." For good measure he includes a review of the myoelastic-aerodynamic theory, a procrustean reinterpretation of Hadding-Koch's and Studdert-Kennedy's (1964) results, and a sketchy, quasi-transformational treatment of questions. These parerga aside, the substantive merit of his work rests on (a) the adequacy of the prosodic model; (b) the credibility of evidence adduced for the primacy of the physiological correlates posited; and (c) the explanatory power of the notion "archetypality." Unfortunately, the prosodic model, acceptable as far as it goes, does not go far enough; the evidence is a physiological four-flush; and the empirical content, if any, of the nebulous notion "archetypality"--a term regrettably redolent with primordial and Jungian connotations--is a question which goes begging throughout this book and in further pronouncements (Lieberman, 1968).

Lieberman's two binary prosodic features are: choice of rising (marked) or falling (unmarked) pitch pattern at the end of each breath-group--whence [+BG] and [-BG] respectively--and assignment to occasional syllables of a single degree of extra prominence (called [P_s]; read presumably "pressure,

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(sub-)subglottal)." Beyond giving awkward new names to familiar prosodemic distinctions, Lieberman sallies forth, clad in archetypal whole cloth, in quest of the currently consecrated linguistic grails: universals; child language acquisition; the nature of mental processes. He contemptuously dismisses earlier notations for intonation, which though "sufficient to describe all of the observable phenomena...do not reflect the mechanisms that people use when they deal with intonation as a linguistic signal...[nor] the underlying structure [p. 1]." Access into the penetralia of linguistic structure and phonetic mechanisms has, it seems, been vouchsafed to him, and mere description is uninteresting--"we are, instead, trying to see how the human mind functions [and] to show that intonation...must be the product of an innate, rather than an acquired, mechanism. Thus it should have a similar status in all languages." He modestly admits, however, that his study is "not definitive in the sense of having exhaustively analyzed the intonational system of every language on earth [p.2]."

Lieberman's expository style is turbid and diffuse; solecisms abound; the non-inclusive we (for 'I') intrudes ubiquitously and annoyingly. Some of the sentences must have been written in his sleep--e.g. : "The increase in the durations of stressed syllables in languages where length is not a phonologic [sic] feature may follow from a match to the limitations of auditory perception [p. 30]." He has a penchant for pontifications comprising equal portions of the specious and the obvious: "The subglottal air pressure has to fall abruptly at the end of the expiration since the air pressure in the lungs must be below atmospheric pressure during inspiration [p. 25]." That there must be at least one zero-crossing in the lung pressure function between exhaling and inhaling is surely a universal--a trivial one--for all pulmonates. But it does not follow, nor is it true (even in Lieberman's own figures), that the pressure drop must be abrupt, nor coincident with the end of outward air flow. A still worse fault is his couching of hypothetical assumptions in the language of factual assertion--as: "The tension of the laryngeal muscles is constant during the production of the normal breath-group [p. 53]."

The postulated physiological correlates can be summarized succinctly: the pitch obtrusion of [P_s] and the pitch fall of [-BG] are archetypally produced, the hypothesis holds, by changes in subglottal pressure--a peak of pressure for the former; a falling off of pressure for the latter. Only the marked

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(terminal rising) breath-group, on this view, archetypally employs changes in laryngeal adjustment--an increase in larynx tension being needed to raise voice fundamental frequency (f_0) despite falling subglottal pressure at ends of breath-groups. All this makes for a tidy system; however, insofar as it constitutes an empirical claim it is factually incorrect. Indeed there is good evidence that pitch control for intonation and accentuation is vested primarily in the larynx, with subglottal pressure playing a quite ancillary role (Ohala & Hirano, 1967; Vanderslice, 1967). Furthermore, Ladefoged (1962) and Ohman and Lindqvist (1966) have shown that f_0 is a far weaker function of transglottal pressure drop than Lieberman would have us believe; he dismisses their results, speculating that "some sort of feedback control...may function during singing [p. 97n]." But Ohman and Lindqvist used spoken, not sung, sentences, and Ladefoged anticipated and refuted the 'feedback' objection. This factual discord would have warned an experimenter less infatuated with preconceptions to re-examine his logic. Lieberman had made no attempt to monitor, even qualitatively, his Ss' laryngeal action; he merely "assumed that the tension of the laryngeal muscles was unchanged" everywhere but at terminal rises (pp. 95-96). This assumption, and not any experimental evidence, was his basis for postulating a strong effect of pressure on pitch--which he then uses to argue that the (rather rough) correlation of his f_0 curves with subglottal pressure manifests a causal relation. He concludes with vicious circularity that "this assumption thus seems to be reasonably valid [p. 97]."

Even if f_0 were a strong function of pressure, there would remain an incurable contradiction in Lieberman's hypothesis. The pitch obtrusions for accents and emphases can be either upward or downward (Bolinger, 1958); if upward obtrusions really were caused by momentary subglottal pressure peaks, then downward obtrusions ought by the same reasoning to result from momentary minima (albeit [+P]_s) would be a perverse name for such a feature). That this is not only counterintuitive but counterfactual can be seen in Lieberman's own data. Where the disparity is too obvious to ignore, as in figure 4.24, he offers this: "f₀ does not rise on Joe, perhaps because Speaker 2 characteristically starts his breath-group with a lower laryngeal tension [pp. 83-84]." But the f_0 curve from which lower tension ostensibly was inferred flatly contradicts this; it starts out above 100 Hz on the unaccented word Did, then drops a major third (to about 80 Hz) for the beginning of the emphatic JOE--the very word

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for which there is a strong subglottal pressure peak. Other instances of anomalous phase or polarity in the pitch changes "re" pressure (cf. figures 4.14, 4.22, 4.26) go unexplained except for two "unmarked breath-groups" where terminal pitch admittedly "seems to fall through some laryngeal adjustment [p. 86]."

The author's determination in forcing the facts to fit his theory is matched by his dogmatism in enunciating naive notions about language behavior. He asserts ex cathedra that if a main clause and a relative are uttered as two falling breath-groups, "the listener will treat the speech signal as though two sentences were uttered, the simple declarative sentence I saw the boy and the interrogative sentence Who fell down the stairs? [p. 169]." No doubt the use of two falling intonations would be unusual in pronouncing these as a single sentence, but there remains a criterial difference of accentuation: interrogative who is canonically accentable; relative who, canonically weak (cf. Vanderslice, 1968, pp. 53-72). In his chapter on questions--a hodge-podge of diachronic development, cross-language comparison, information theory, and transformational derivation--he tells us that "the sentence Joe ate the soup? [presumably pronounced with terminal rise] implies that the speaker has heard that Joe ate the soup but he can hardly believe it." Four sentences later he says "the reply to the question You're going to drive down that rutted road? is either a confirmatory silence or an explanation....The speaker already knows that you are going to drive down the road [p. 136]." Just how the identical question form conveys doubt verging on disbelief in one case and positive certainty in another is not revealed to the reader.

Several of the more indefensible views advanced in this book now seem to be disowned by its author (Lieberman, 1968). But far from admitting mistakes pointed out by unacknowledged critics, he patronizingly explains that in saying A he really meant B, so it was really B that he said. One recalls Weinreich's (1967) castigation of a colleague for "the 'absorbent' quality of his theorizing in relation to suggestions made by his critics [p. 285]," and for his "mysterious power to change his theory without changing it [so as] to guarantee the perennial correctness of his approach, abstracted from any particular formulation of it [p. 286]." Thus whereas in the book at hand Lieberman claimed his data showed that adult speakers "indeed employed the archetypal articulatory gestures [p. 106];" now he says--as if it had all along been his position--that "the archetypal form [is] manifested most clearly at

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the articulatory level in the vocalizations of extremely young children [whereas] adult speakers often use alternate articulatory maneuvers [1968, p. C-4-4]". This reformulation makes "archetypality" safe from empirically disconfirmation. But still the gestures that adults use "often" (in fact REGULARLY) are called "alternate". Justification for such nomenclature defies imagination, yet Lieberman continues to use these tainted terms--and to claim that, whatever gestures people may use to PRODUCE intonation, they nevertheless PERCEIVE it in terms of his archetypal maneuvers. No empirical basis whatever exists for believing this (or any of the other motor-theory clap-trap) despite an airy reference, sans citations, to "recent experimental evidence [1968, p. C-4-4]."

Stripped of this nebulous nonsense, Lieberman's two-bit prosodic analysis can be judged in its own right. True enough, most sentences have one or more salient syllables, and most discourses have some sentences (e.g., yes-no questions) with rising intonation and many more with falling. But in each case important further distinctions are overlooked. Among English accentual phenomena there is a clear systemic contrast between normal accents and emphatic ones. Failure to recognize this obfuscates the treatment by Gunter (1966) of such neutral sentences as Bob has acquired a MOTORcycle, which he is unable to distinguish from contrastive ones, as The man can SEE the boy. We need either another binary feature (e.g., [+EMPHASIS], implying [+ACCENT] redundantly, proposed by the reviewer--Vanderslice, 1968, p. 64ff) or a ternary one to capture the difference. Similarly for intonation, the binary categorization into falling and rising tones is too coarse. In particular, the fall-rise contour must be recognized as systemically distinct; and perhaps likewise a sustained pattern (though contrasts between the latter two are marginal). These can be handled by providing two distinct features (Vanderslice, 1968, pp. 65 & 80ff): one for the pitch subsidence of falling and fall-rise contours, which starts characteristically on the nuclear (rightmost) accented syllable, [CADENCE]; and another for the terminal rise of fall-rise and rising contours, which occurs at the end of the sense group--[ENDGLIDE]. A sense group with neither feature assigned would of course have a sustained intonation. The difference in onset point governing these two features is easily overlooked in citation utterances, where nuclear accent is normally near the end. It manifests itself clearly in discourse segments of greater than sentence length, where cadences spanning two seconds or more may occur. This is ten times longer than the

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"last 150-200 msec of phonation" which Lieberman allows for the falling pitch of unmarked breath-groups (p. 53 et passim--an ill-conceived restriction, happily now dropped though not overtly retracted). Such long cadences furnish another reason, besides the regularly co-occurring laryngeal readjustment, for doubting that the falling pitch of cadences in English can be meaningfully associated with the amount of air left in the lungs (pp. 54, 71, 98-99) or with pitch patterns in the cries of neonates (pp. 41-47, 104).

Such an analysis--roughly equivalent to splitting each of Lieberman's features to provide two degrees of prominence by pitch obtrusion and two quasi-independent intonational pitch features--seems to account fully yet economically for the within-sense-group, grammar-expounding prosodic patterns of at least American English. Lieberman conflates the fall-rise contour with the rising and the sustained patterns under the rubric of marked breath-group (pp. 123n, 168, 191) and like Gunter he recognizes only one degree of prominence. He relates this to the 'stress cycle' of Chomsky and Halle (1968) by a factor \underline{X}_i depending on rate of speech, context, etc.: in rapid discourse, \underline{X}_i might equal 0, and no vowels would be marked [\underline{P}_s] as the result of linguistic stress. In careful speech, \underline{X}_i probably equals 1 [p. 159]." This suggests gradience, but since the factor functions only as an integer (truncated, not rounded), in a system where less is more (smaller integers designate stronger stresses), it follows that at the "careful" end of the style-rate continuum the "stress cycle" assigns just one prominence per sentence, to the syllable finally getting a "primary stress," and that in all faster or more careless modes of speech it assigns none at all. Possibly Lieberman really means this; he stipulated that "emphasis is prominence that is not predicted by the stress cycle [p. 146]" and in his extensional usage "prominence" maps strictly into emphases: the captions to figures 4.10-4.33 (his main data) note occurrences of [\underline{P}_s] in all and only the instances where the subject read an italicised word. Peaks of subglottal pressure appearing with other syllables, whether elsewhere in the same sentence--higher peaks than for the italic syllable in 4.15 and 4.20--or in sentences with no italic indication of emphasis (4.18, 4.22, 4.23), are ignored. Thus the truly archetypal correlate of prominence seems to be not physiological, nor acoustic, nor perceptual, but typographic.

One good thing about this book is that it disseminates (pp. 125-128) the results of an experiment (Lieberman, 1965) showing that competent transcription

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of suprasegmentals in the tradition of Trager and Smith (1951) depends crucially on intelligibility of the segmentals. Lieberman misapprehends the Trager-Smith system so his comments are largely ignoratio elenchi. But his evidence demonstrating the lack of factual basis for a multiplicity of stress levels is welcome. It is only regrettable (the game of 'flog the taxonomists' palls) that he did not see fit to extend his strictures to the fashionable phonology of transformationalism, where the Trager-mythical levels of 'stress' are still enshrined.

Footnote

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