

Intrinsic characteristics of activity: 459 a comment on Kent's psychobiology

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SMITH, MARY REGINA. *Intrinsic characteristics of activity: a comment on Kent's psychobiology*. *Am. J. Physiol.* 246 (Regulatory Integrative Comp. Physiol. 15): R895-R896, 1984.—Kent has made seven statements that should inspire responses from linguists, kinesiologists, and developmentalists, among others. His goal of reforming our current theoretical perspective on the development of speech will be accomplished, in part, by their corrections, elaborations, and further speculations. Not all of his statements have the same status or consequence for an evolving theory of development. Those on the anatomic and neurological differences between infants and adults are much less controversial than those on the initial separateness of the perception and production systems or those on the relation of rhythmic behavior to phonology. Across all the statements, two major topics merit discussion: 1) the relevant descriptive terms of the vocal behavior of infants and 2) the relation of vocal activity to other behavior.

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TWO CONFLICTING TENDENCIES confront Kent (4). 1) He does not want to use phonetic descriptions of the infant vocalizations, yet he does want to describe what the infant is doing. 2) He expects to account for the development of speech with at least a component of the new theory concerned with the maturational characteristics independent of the particular language that is being learned, yet he is basically opposed to the separation of the organism from the environment in the search for answers to these questions. Both conflicts can be collapsed into the question of the intrinsic characteristics of the activity (3).

Kent cautions against applying (prematurely) adult phonetic terms to infant vocalizations and against ignoring the similarities between vocal and other activities. He has missed an opportunity to clarify the differences between (and different uses of) the terms speech and language and between the terms phonetics and phonology. As a result, some readers will be unable to evaluate or appreciate his criticisms of the applications of phonetics, abstract models of grammar, and environmental factors to a theory of speech development. Since his purpose is clearly larger than clarification, I will touch on these terms only in the context of discussing the major themes in this work.

With respect to the first caution, Kent bases his case

primarily on the existence of anatomic differences between infants and adults without discussing the issues of how or why phonetic descriptions succeed or fail. He concludes that vocal tracts (or systems) undergoing change cannot be guided (controlled) by the same factors employed in skilled adult behavior. However, such change per se cannot be the limiting factor in the application of phonetics, since changes of the vocal tracts of adolescent males and very old speakers are well described in phonetic terms, as are the efforts of speakers with certain pathologies. The anatomic and neurological differences between infants and adults do not invalidate the use of phonetic features, especially those concerning manner and place of articulation. What does support the caution is the issue of the intention of the speaker.

Phonetic terms are applicable within the framework of linguistic intent. Phonetic features are a short-hand system for describing the linguistically important regularities, usually in terms of the articulatory, acoustic, or perceptual characteristics that distinguish utterances. It is, of course, most difficult to discover the intentions in unskilled performances, but imputing them to the speaker seems unavoidable. When we should do so depends on just what the functional regularities are. I would join Kent's appeal for a better descriptive system with an appeal for more appropriate articulatory and acoustic geometries with which the resemblance of early and late vocal efforts may be discussed. In short, we expect to improve our phonetic descriptors not discard them.

Kent notes that reduplicated babbling is a common but not universal period in normal development. Reduplicated babbling, as an example of rhythmic activity that is coextensive with other rhythmic activities, is not the epitome of coordination, even though the infant may show a rather high degree of acoustic precision and, by hypothesis, articulatory precision (see Ref. 5 for an example of the degree of precision in the productions of older children). It is unlikely, however, that precision of this sort is the objective in production (3). I do not share Kent's concern with indexing rhythmic behavior to temporal periodicities.

The importance of rhythmic behavior for development merits comment. First, the organization that produces rhythmic behavior need not be presumed to be different from that underlying arrhythmic behavior. Rather, a system may be parameterized such that both rhythmic and arrhythmic behavior result from different tunings.

This notion of parameterization of the system is now prominent in contemporary linguistic models of the grammar to help account for the role of experience in setting the child on the course of becoming a user of a particular language (1). Kent's dismissal of Chomsky's grammars as heteroorganizational is not only cryptic but incorrect.

Further the lack of rhythmic behavior does not signal an unskilled performance. Specifically, reduplicated babbling must give way to greater differentiation in the speech stream. The issue of intention emerges again. Reduplication or any other sort of regularity in adult speech may actually reflect a lack of control, as in echolalia. We may not wish to withhold our application of (improved) phonetic terms until the evidence of reduplicated babbling, since its absence does not mitigate against the existence of controlled and coordinated activity.

With respect to the rhythmic stereotypes, Thelen (6) has demonstrated that the "disappearance" of a regularity is strongly linked to environmental factors (where the environment includes the performer). For example, the decline of the stepping response in 6-wk olds with the retention of the kicking response appears to be due to the fact that gravity and the increased mass of the legs cannot be countered by the immature leg muscles. When the effect of gravity is reduced, as in a water bath, a stepping response reappears. Thus the appearance and disappearance (or relative frequency of the behaviors) of certain "stages" may have explanations that Kent had not envisioned but are nevertheless in the spirit of his preference for process models. Such findings, however, make the conventional approach to stages attractive, since the evidence of the discontinuities can lead to more attention on the environmental factors. Currently, similar explanations for the decline of reduplicated babbling are not available, nor are they enthusiastically sought. Such babbling gives way to more relevant linguistic variation. This is an important limitation on linking the general stereotypes to speech development.

Studies of voluntary rhythmic activities in adults are beginning to show the sorts of characteristics important for describing the activity. For example, Desmedt and Godaux (2) remark on their study of finger movement, "The motor commands are patterned in terms of movements rather than of muscles. . . ." As analogous findings are forthcoming in the study of speech, we may be able

to apply systematic phonetic terms to the output of immature vocal tracts, even though those vocal tracts will not show the same organizations of masses, muscles, and joints as those of an adult.

Kent's three most provocative statements concern the initial separateness of the production and perception systems, the relative sizes of units of phonological contrast for children, and the interaction of emerging phonology with emerging motor control. Given the disposition to keep the organism and environment together in the picture, it is difficult to accept that the organism itself begins with isolated or functionally separate systems. It is more consistent to explore the acting-perceiving system as a whole. The perceptual capacities of infants have not been studied with respect to the nesting of higher-order acoustic structure, so it may appear that the perceptual capacities are greater than the production skills and that the two systems are separate initially. The apparent mismatch of the two systems may be removed as we come to understand the acoustic structure of the speech signals better and come to appreciate the gestures that the infants make.

With respect to the differences in size of the units of phonological contrast, we need to see how the signal is structured. Infants do treat the signals as structured; the signals have the structure the infants detect, but the signals have additional nested structure the infants appear not to be sensitive to. Learning the language appears to be a process of refined tuning not of supplementation or extraction.

The interaction of phonological development with motor control is the most exciting area of discussion. Recent work with children afflicted with cerebral palsy indicates that a mode of production, even a computer synthesizer, is perhaps a necessary component to the process of discovering the intended structure of the speech signals (4a). It is too early to see the full implications of such a finding, but I share Kent's enthusiasm for the project.

Kent has not taken the easy route of simply announcing that more research is needed; he has suggested guidelines for the continued effort that merit our attention if not our agreement. Serious investigators will be challenged to make explicit both the antecedents and entailments of Kent's remarks.

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