Letters

Language Unexplained

The relationship between language skills and biological development shows that language learning is dependent on the effects of physical maturation. It is also true that normal children universally learn the language that their parents speak. Children acquire the rules of grammar (syntax) and vocabulary necessary to communicate adequately within their social setting. Because of this Lenneberg has postulated that man has a unique, biologically endowed system for language learning ("On explaining language," 9 May, p. 635).

It might also be postulated that man has a highly complex information gathering, storing, and retrieval system which can be used for problem solution and problem generation. It may be conceived of as the common property of man, monkey, dolphin, and so forth, to generate learning sets (*1*) which can operate in many sense and response modes. Language generation and decoding are therefore examples of mankind's ability to solve complex problems, use rules, and respond to complex perceptual patterns which he shares with other organisms.

It is unfortunate that a biological approach to language learning should ignore a large body of literature which demonstrates that more experience with language skills can improve intelligence scores and academic performance and that environments deficient in early language experience can be detrimental to later language development (*2*).

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References

1. H. F. Harlow, *Psychol. Rev.* **56**, 51 (1949).
2. W. Fowler, *Psychol. Bull.* **59**, 116 (1962).

Dixon's is the predominant view of American psychology on language. It maintains that language learning is merely another example of the acquisition of learning sets, and the implication here is that the generation of learning sets is a well understood "property," constant over the animal kingdom, and easily measured (as if it were weight or water content). However, I take issue with this assumption. A rhesus monkey forms a learning set when faced with one series of tests but appears as unintelligent as a guinea pig when faced with another series. The formation of learning sets has a prerequisite: the task must be suitable to the specific abilities of the species that we are testing. If our aim is to show growth of "general problem-solving ability" in different species, we must adjust the setting to each type of animal; that is, we must use different types of problems, different sorts of stimuli, accept different types of responses, and use different criteria for learning. My claim for language is that it constitutes a task in which a specific problem is posed, involving specific stimuli, calling for specific responses, and using specific criteria for accomplishment. Only man has the abilities to cope with these particular specificities. If we wish, we may give the same label to this accomplishment as to those observed in animals put into special apparatus; but this will not alter the objective differences between language in man and discrimination behavior in other animals. Consider also that there is no single criterion that can be used to identify species. This means that evolution is characterized by constant change of direction. There is not a single property that changes regularly in magnitude as we move from one species to the next (not even complexity, however defined). Because of this constant change, I doubt that the "generation of learning sets" is a monolithic property common to man and all animals.

No doubt one can suppress language as one can suppress any other aspect of ontogeny; nor is there any argument that the performance on IQ tests improves with improved language abilities. This is necessarily so because IQ tests use language as a vehicle. Biology is not concerned with either of these phenomena. It is concerned exclusively with the underlying capacities for language, and this is the reason that the literature Dixon refers to has not been mentioned in my article.

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Talk Time

Verbal communications are shaped by many events and may fluctuate qualitatively or quantitatively on the basis of time. Only those directors of federal projects who are located in the eastern time zone have temporal correspondence with Washington, D.C. Theoretically, there is a "temporal gap" between geographically remote project directors and officials in Washington who hold important decision-making powers. Assuming a standard 8-hour day, 40-hour week, and 12:00 to 1:00 lunch period, the West Coast project director has but 3 hours daily "talk time," and of 240 working days per year, he loses 150 of the available "talk" days! Certain questions arise: (i) Does time differential (TD) magnify alienation between local and Washington-based agency officials? (ii) Does it create a geographic selection factor relative to awarding of grants? (iii) Does the unavailability of top Washington officials contribute to deviations from established guidelines (possibly a positive event)? (iv) Does TD preclude development of effective nationwide systems of information exchange? (v) Is much time now lost waiting for "time windows"? (vi) Since distance is irrelevant in the jet age, would time zones be an effective way of organizing regions for government operations? (vii) Does TD encourage travel, hence absenteeism?

In view of the foregoing, some schedule changes appear in order. In Wash-

ington, agency officials with decision powers (directors and associate directors) could stagger lunch and one could report to work an hour later to extend the day. This expedient alone recaptures 60 percent of the talk time lost by West Coast personnel and completely "covers" certain zones. At the present time it is a moot question whether the service which is provided to grantees suffers by this temporal disjunction and whether the gap actually influences communication with Washington.

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When Found, Make a Note of . . .

Since 1961 I have doggedly kept exhaustive notebooks as an aid to memory. In them is everything, written down as it occurs: good ideas, bad ideas, plans for experiments, data from experiments, notes of what I read, and what I think. Each entry is dated with the exact time of the entry. Recently I have analyzed the time course of entries in the notebooks. I am astonished to discover regularities. For example, over the years the modal time between entries holds at 2 minutes, half of all entries are made within 1 hour of the previous entry, with about 1200 entries each year.

Many scientists keep comprehensive notes, but do others keep them so that an exact analysis of "ideas" by topics over time might be made? In the *Théorie Analytique de la Chaleur*, Fourier (*1*) noted that "primary causes are unknown to us; but are subject to simple and constant laws, which may be discovered by observation." Exact time notebooks permit a measurement of the scientific process through Fourier analysis.

I welcome communication from any of you who keep dated notes that would lend themselves to such an endeavor.

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Reference

1. J. Fourier, *The Analytical Theory of Heat*, Freeman, Transl. (Dover, New York, 1955).

McElroy Incident

The editors of *Science* saw fit to publish against my express wishes, emphasized to the author, Philip Boffey, but not cited by him, an article entitled "W. D. McElroy: An old incident embarrasses new NSF director" (25 July, p. 379). I trust you will afford me the courtesy of permitting me to deplore in print what I consider an uncalled for, inappropriate, and tasteless action on the part of *Science*.

The incident discussed was settled, I believe to the satisfaction of all concerned parties, 5 years ago. I accepted McElroy's apologies for his oversight then, and as Boffey acknowledges, I naturally still accept them. Boffey writes of concerned "scientists in England, who are close to the field of research involved" who are requesting "clarification of the matter." I hope that these persons, whose identity has not been divulged to me, together with Daniel S. Greenberg, the foreign editor of *Science* in London, will consider this letter adequate and final "clarification" of an incident that, in its restatement, does little credit to them, to Boffey, to the editors of *Science* or, indeed, to the advancement of science.

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Philip Boffey has given an accurate account of the McElroy-Smith episode insofar as I can remember, but I don't understand what possible point there was in resurrecting the business. The claim that "a few scientists in England, who are close to the field of research involved" are agitating for clarification sounds like nonsense because it is precisely we who are in the field who know that McElroy has never published a word of original research on firefly cytology and could not conceivably have been trying to represent himself as an expert in electron microscopy. As I told my good friend David Smith at the time, the incident was so clearly a bit of stupid and reprehensible, but honest, carelessness by a very busy guy that it could not possibly diminish Smith's own high standing among his informed colleagues.

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