

**RESPONSE TO REVIEWS OF
BIOLOGICAL FOUNDATIONS OF LANGUAGE**

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It is rare that an author has the opportunity to reply to reviews of his book. I feel particularly honored that my thoughts about language were the subject of this discussion by three authorities in the field, all highly respected by the profession and by myself.

J. W. Black's review is the most thorough one my book has had, and it is a fair representation. I would first like to make a comment which is relevant to the title of this triad of reviews; it is on my purported aim. I am definitely not offering a new theory on the origin of language. To write about biological foundations of language is not the same as to divine how language came about. A treatise on the biological foundations of the formation of schools in fish need not concern itself with the question of how schooling started, just as a study of the biology of the cell need not discuss the origin of life. In fact, questions of origin are seldom part and parcel of modern scientific inquiry. Instead, one is satisfied in discovering and describing the operation of variables and a short-range cause-and-effect sequence – never a complete regression to origins. Social and cultural investigations of man, on the other hand, may profitably (and often do) search for beginnings. They can be traced sometimes to inventions. Furthermore, to write about biological foundations does not imply an exclusion of social foundations. Many aspects of language have social origins (though they were not within the realm of my chosen topic) and, what is more, many social functions are themselves based upon biological and species-specific propensities.

Another common but totally unfounded assumption about my book is that the demonstration of species-specific traits implies an anti-Darwinian view of life, perhaps espousal of special creation; or that it is a departure from the accepted notions of epigenesis and a return to preformism; or that it is a denial of the existence of homologues in related species, or of primitive antecedents in the course of evolution. Since I have been very explicit on all of these points in my book, there is no need to say more here than that all such imputations are in error. These reviewers have not made them; I am

merely taking this opportunity publicly to reject them, since they have been made repeatedly in other reviews.

Klassen and Wepman have dealt with me kindly, and I certainly have no reasons to be unhappy about their overall judgement of my contributions. They are right in saying that I have often taken recourse to analogies in order to illustrate a point. But I believe I have been careful to make a distinction between occasional illustrations and the use of evidence. For every chapter I have taken pains to weigh the available evidence, and in most instances I have also pointed out the strengths and weaknesses of my corroborations. I do not believe that the *logic* of my arguments is 'the logic of analogy'; it is the logic of circumstantial evidence. Very often this makes for a weak argument, but it is the best we can do, since crucial experiments on children are in many cases not feasible. At one point the reviewers seem to think that they are in disagreement with me. After a comment on behavior genetics, they continue: "To say, however, that a particular behavior complex is genetically determined need not apparently be the same as saying that genes for that behavior exist." Pp. 239-244 of my book are devoted to explaining that the relation between genes and behavior is extremely circuitous. The relationship can only be understood by investigating first the role of genes in embryogenesis and development to maturation. On p. 244 I explicitly warn against "shaky assumptions about genes for language". The following page gives complete references to the scientific studies of the inheritance of certain language traits (together with appropriate cautions) and on p. 265 I reiterate: "Pedigree and twin studies suggest that genetic transmission is relevant to language facilitation. However, there is no need to assume genes for language." Apparently the reviewers are in complete agreement with me. I am somewhat surprised to see that they think the discussion of color terminology was intended as evidence of biological determination of semantics. It was not. Color terminology is interesting because one can study its referents with great precision. Therefore, one can mount experiments (described in the text) to see whether an individual's naming habits influence his color perception, his memory for color, or his conceptualization of color materials. (The experiments described indicate that in most instances language habits do not influence these cognitive processes to any appreciable extent.) Despite these small misunderstandings, I am grateful for their review.

Furth offers some comments, but even though he refers to three pages in my book, these comments can hardly be considered a book review. Since my position is thoroughly misrepresented, I must assume that his comments are merely on what he has heard about the book; they could not possibly be on what he has read. He mentions two basic disagreements. The first is with "my

radically biased epistemological point of view". He does not tell us what he has in mind, and since I did not know I had any epistemological point of view, I have nothing to say in my defense. His allegation that "cognition is quickly gone over as referring to three capacities..." is simply a misstatement of the passage (p. 331), where cognition is not even mentioned. Similarly, Furth is distorting the facts when he claims that p. 374 has "a rather superficial description of thinking in terms of categorization". No mention whatever of thinking occurs or is implied. Furth is referring here to my general summary and the paragraph is a summation of the contents of *two chapters*! Furth's exhortation to Chomsky and me to "make a more serious effort to understand the psychology of knowledge" has not fallen on deaf ears. Perhaps Furth can show us how. He hints darkly at my "theory of innate neurological mechanisms", and claims that I have used these concepts as explanations. He is wrong. On p. 221 I say, "It would be presumptuous to try to explain the nature of the innate events that control the operation of language." He obviously failed to understand the chapter from which he quotes. I wrote an entire chapter (I) to show that the dichotomy between innate versus learned behavior is completely untenable. If he had read it, he would not have offered advice on how to overcome the "dilemma of 'innate versus learned'". His remedy, incidentally, is "a biologically based understanding of the nature of human knowledge", which he thinks is opposite to my point of view (whereas Klassen and Wepman think I do propose just such a view). The criticism that I "reduce language to an innate structure without genesis" is curious! Why not read my chapter on evolution (VI) which was written exclusively to show how the concept of genesis can be understood. My statement "We assume that our potential for language has a biological history..." (p. 247) hardly confirms Furth's claims. In theoretical biology the mechanistic position is usually the antipole of a position of vitalism. The former starts with the assumption that all aspects of life are the result of the general laws of nature, i.e., those that constitute the subject matter of physics and chemistry. The vitalist position assumes forces or laws that are unique to the phenomena of life. I have confessed to the mechanistic point of view. Furth is unhappy about this, but I cannot understand his further reasoning. If Mr. Furth does not have time to read my book, I recommend to him that he read at least the summary at the end of each chapter.