

## DEFEATIST XENOPHOBIA OR INTEGRATIVE COMPLEXITY? A Reply to the Comments About "The Clash of Giants."

Ernst L. Moerk  
*California State University, Fresno*

**ABSTRACT:** Several of the major questions raised by the discussants of the target article, "The Clash of Giants," are focused upon in an attempt to facilitate understanding across paradigm boundaries. A brief summary of the background from which the article was derived intends to remove misunderstandings that simply stem from a lack of factual information. Terminological and conceptual problems as to the exact meaning and the sources of "emitted behavior" and the nativistic implications of this term are indicated. Whether "verbal behavior" is considered merely as an effective cause, explaining the contingencies of responding in functional analyses, or a formal cause, explaining structural linguistic learning, is considered briefly. Relationships between the concepts of rule-governed behavior, tacit knowledge, procedural knowledge, and purposiveness are further explored. Finally, the problem deriving from narrow and outdated knowledge is demonstrated by indicating how far the field of epistemology has progressed in the half century that has passed since the publication date of the epistemological guidebook relied upon by two of the discussants. Recent epistemology stresses communication across disciplinary boundaries and scientific growth based on such constructive contact. Several behavioral colleagues have fruitfully built on these newer insights and have begun building bridges where previously only chasms were seen. Skill learning, which focuses on behavioral phenomena and incorporates many cognitive conceptualizations, is again suggested as one promising bridge to integrate diverse perspectives and to make behavioral approaches fully applicable to the complex question of language acquisition.

Browsing avidly through the commentaries to my article on "The Clash of Giants..." as they are provided in this volume by my behavioral colleagues, I was at first caught by surprise. Considering the commentaries as a mirror, I certainly did not recognize my article and my conceptual approach in this mirror and got the feeling they represented more a misconstrual and warping than an accurate reflection. Certainly this attempt at communication proves the presumption that it can be difficult to reach understanding across terminological differences and with incomplete knowledge of the contrasting fields of research! This could, of course, be fully my fault. I might either not have described my conceptualizations precisely enough or I might not be aware of the implications of my conceptualizations. I certainly did not give much information about my factual research in first-language acquisition. The article might therefore have been misread through the reviewers' lack of familiarity with my long-lasting endeavors. The lack of mutual acquaintance goes even deeper. At least one respondent was not clear who this Moerk is who dares to suggest an integration of two hostile camps -- an integration that seems to be impossible to some. Let me therefore clarify in a few words my background. It

### AUTHOR'S NOTE:

All correspondence concerning this article should be addressed to the author at the Psychology Department, California State University, Fresno; Fresno, CA 93740.

## MOERK

might make communication somewhat easier.

No, I am not a linguist (as one of the respondents, Malott, surmises). I am a psychologist and have worked since my Ph.D. (1964) in the field of child development. After some clinical work and while teaching child and developmental psychology, I have published since 1972 in the field of first language acquisition. Since the empirical evidence showed overwhelmingly phenomena of teaching and learning (cf. Moerk, 1972, 1977a, 1977b, 1978, 1983, 1986, 1992), I have increasingly formulated my findings in terminology borrowed from learning psychology and behaviorism, including frequency effects, reinforcement, massed and spaced rehearsals, etc. This verbal behavior of mine earned me the epithet of a "behaviorist," intended in a less than approving sense by my cognitive and nativistic colleagues in the field. I myself saw and still see my outlook as being closer to the Piagetian-Vygotskian tradition. But I had come to the US in order to learn more about behaviorism, which was at the time (1966) not yet well represented in Europe. I am still willing to learn. I am convinced, in order to utilize the insights of other approaches, we have to understand each other. In order to understand each other, we have to listen to each other. I certainly appreciate whatever constructive feedback I received and I hope to receive more in the future from colleagues who are more expert in the writings of Skinner and of behaviorism.

Admittedly, there are many difficulties, not the least of which consists of strong emotional reactions. Thus, for example, one of the present commentaries (Hayes & Hayes, 1992) reads in the first paragraph: "Moerk's argument ignores..." "He misses..." "...his specific concerns are misplaced..." It adds, in paragraph two, the accusation of "incoherence" and "unthinking combination" and ends, in the last paragraph, with an accusation of "theoretical and philosophical incoherence" and argues that "Moerk has provided the wrong solution to the wrong problem." Confronting what appears to be so much unrestrained anger, I am reminded of Skinner's puzzled remark: "I have never been able to understand why Chomsky becomes almost pathologically angry when writing about me..." (Skinner, 1977, quoted after Andresen, 1991, p. 57). And I am sincerely sorry since I certainly hoped to stimulate the cortex and not the hypothalamus.

On the other hand, the replies by Malott (1992) and Street (1992) directly contribute to the attempted bridge building. I will focus on several of the major points and questions that arose from this constructive interchange. Street provides me with a welcome chance to search for clarification regarding one central point in Skinner. She argues a "fundamental difference" between Skinner and Chomsky in that Skinner only stresses the innate possibility of a response. Yet it seems to me that Skinner's "emitted *behavior*" is an *actuality* and not only a possibility. Did the pigeon have to learn *how* to peck or only *when* and *where* to peck? And if the pecking was "emitted behavior," where did it come from? Possibly from innate knowledge? (I discount for the present the studies of Kuo [cf. e.g., 1976] on the physiological 'preshaping' of pecking in the egg). Skinner repeatedly refers to

## XENOPHOBIA

"emitted behavior" in connection with "reflexes" (e.g. Skinner, 1937); and reflexes normally are considered as innate by (almost?) everybody. Admittedly, I have not yet found a fully detailed explanation of Skinner's concept of "emitted behavior," and several behavioral colleagues whom I asked have not yet answered my inquiry. I am therefore willing to learn and invite the specialists to provide needed clarification.

While I have been very critical of Chomsky's nativism in most of my writings from 1977 to the present (Moerk, 1977a, 1992), I wonder whether a similar relapse into unnecessary nativism is not found in Skinner also. Certainly, the question as to where specific grammatical forms of a specific language derive from cannot be solved by a reference to "emitted behavior," whether it is innate or not. But this is my criticism -- in different words -- against Chomsky's nativism.

Turning to Street's first (p. 28) quote from Skinner (1974, pp. 88-89) on verbal behavior, there is much with which I would gladly agree. Nevertheless one, perhaps central, question seems not answered. What is the best conceptual way to handle the products of verbal behavior -- such as the black marks on this paper. It would be inexact to label these products "behavior" since my pertinent behavior stopped months before the reader encounters their traces. Or to make the point clearer through an analogy: the behavior of a dancer and the choreographed notations on paper prescribing her steps are not the same. If the dancer learns the steps by following the symbolic code, then the notations stand in a causal relationship to the behavior. In other words, there is something "objective," or "reified" (in Malott's [1992] terms), that stands in multiple relations to verbal behavior, sometimes as a product, at other times as a cause. And "verbal behavior" is learned through the influence of this "something," which is, in the case of 'first language acquisition,' the spoken verbal product. We need therefore a separate 'discriminative stimulus,' to make possible careful discussions about the relationships between the variables. From my perspective, I would formulate it thus: You have to acquire language skills in order to engage in verbal behavior. Without learning Chinese (the skill), I won't be able to engage in Chinese verbal behavior.

Street's second quote (p. 28) from Skinner (1974, p. 100), has been substantiated by much of my research and my theoretical arguments. Language is learned, quite slowly for that matter, although the judgment as to speed depends upon the measurement stick used. It is certainly learned more slowly than many other nonverbal skills, such as reaching/grasping, walking, etc. I invite Street to browse through almost any of my publications and she will see that I have not "missed this pivotal distinction" (Street, p. 28).

My next and perhaps most immediately important point pertains to the relationships between structural and functional explanations. It is raised in the remarks of both Street and Malott. I have little quarrel with the assertion that contingencies control *whether* a behavior is performed at certain occasions and I have argued repeatedly against Chomsky's denial of stimulus control. Yet, if the

type of behavior is *not* based on innate knowledge and cannot simply be "emitted" fully formed -- like Athena jumping forth from the head of Zeus -- then a second task remains: the explanation of how the structure of this behavior is acquired. This is, of course, the main goal of researchers in the field of first language learning and of skill learning. Skinner's general principle of "shaping" does not seem to explain much fine-grained structural learning. Even Hayes and Hayes seem to agree with me on this point in referring to "the most fundamental problem with Skinner's view..." and "...his failure to see what is psychologically new in language" (1992, p. 46). They also refer to a "kind of fundamental action missing from Skinner's account" (p. 46) and provide valuable recent references (which I admit not having known) to learn more about new behavioral approaches to this problem. On the other hand, a statement that "the concept of operant classes...account[s] for the form and structure of the utterance" (Hayes & Hayes, 1992, p. 45) is empty until the genetic history of these forms is explained. Skinner spells out repeatedly that operants of very different forms can be grouped in the same class if they have the same result.

Returning to Street (p. 30) and her contrast between Skinner's avoidance of "a finely grained structural analysis" versus his "finely grained functional analysis" I would like to invite her again to browse through Moerk (1992) -- or any of the preceding reports quoted there -- and she might attest that these are among the most fine-grained functional analyses of the acquisition processes. Malott touches the same topic concerning the learning of fine-grained structure with his distinction between "topography" and "structure." He even agrees with me that "behavior analysts may show disinterest in the topography of behavior on a theoretical level" (p. 38). This was my major point. My close acquaintance with the work of Baer, Kaiser, Warren, and several other investigators in this field makes me fully agree with him that they have performed admirable *practical* work in training structure. As I see it, they have done this, however, on the basis of a modeling-imitation paradigm, which is, in principle a *perceptual* learning paradigm, as Bandura (1986) has shown so convincingly. If behaviorists have begun to turn more specifically to the explanation of perceptual learning, as Hayes and Hayes imply, this will be an important contribution to my conceptualizations since I have focused repeatedly on 'perceptual learning' in the course of language acquisition. It will, hopefully, also contribute to communication between our diverse approaches.

I am not so certain that I understand fully Malott's distinction between topography and structure and the relationship of these two terms to what the philologists and linguists call grammar. This grammar -- under whatever name -- undoubtedly is identical when the same sentence is spoken, handwritten, and typed. While Malott has taught me not to use the term "topography" for "syntactic structure," I am somewhat at a loss which term he would suggest as equivalent and appropriate since there exists certainly a phenomenon that needs description: namely the fact that "topographically" (in Malott's sense) very different structures

## XENOPHOBIA

are exactly the same, both from the linguistic and functional perspective. On the other hand, "topographically" and grammatically very different structures (*Fire!* vs. *Please call the fire department*, written or shouted) would often fall into the same operant class.

As to "rule-governed behavior," I think we have little or no disagreement, only some misunderstandings. I have criticized Chomsky for decades because he confounds descriptive and prescriptive rules, and much of my factual research on first language acquisition (cf. Moerk, 1992, Ch. 6 "The Learning of Syntax") strives to show that early language performance is *not* rule-governed in the Chomskian sense. The misunderstandings seem to be based on several quotations taken out of context (Malott, p. 36); the reader is invited to compare my text with the quotes in order not to be misled. I concur with the skeptical behaviorist that we should employ Occam's razor and only posit rule-governed behavior when we have strong independent evidence for it. Yet Malott's equation of *knowledge* with "the ability to describe verbally" (p. 36) is probably not based upon a broad consensus. First I remind him of Zuriff's (1985, p. 157) conclusion: "... although subvocal verbal behavior may play some role in thinking, the simple equation of thought and speech is not viable." Additionally, we have since Polanyi (1958) the emphasis on "tacit knowledge," which has become "procedural knowledge" in most approaches to skill learning -- and this is contrasted with "declarative knowledge" (the latter would be close to Malott's "ability to describe verbally"). This type of "tacit knowledge" and Polanyi's work is also repeatedly referred to by Skinner (e.g., 1974, p. 139).

Yet it might again be mainly a terminological (pseudo) problem. It appears to me that Malott's and the behaviorists' "contingency-controlled behavior" is somewhat equivalent to the cognitive psychologists' "procedural knowledge" (cf. also Skinner, 1969, p. 166 et passim), and the contrast is largely terminological. The Skinnerian term "contingency-shaped" has, however, stronger implications regarding the genesis of the behavior than the term "contingency-controlled." Careful conceptual analyses of these terms and their implications might be fruitful. Cognitive approaches to skill learning focus much more on the acquisition of the fine-grained structure of behavior, such as riding a bicycle or performing an effective serve in tennis, and provide therefore an epigenetic explanation. I did not find equally clear explications in the strictly behavioral literature. Yet, this might again be due to my less extensive knowledge of this literature -- a lack which friendly advice might remedy.

This might be the best place to touch upon Malott's skepticism concerning purposivism in contingency-shaped behavior. Let me begin by quoting "the master": "...operant behavior is the very field of purpose and intention" (Skinner, 1974, p. 55). Morris (1988) agrees with Skinner: "...behavior analysis is focally concerned with the field of purpose and intention" (p. 294) and Day (1980) suggests a close similarity of this approach to Brentano's "act psychology." As to "expectation of reinforcement" (Malott, pp. 35, 36) I might quote Kimble (1971): "A strong argument can be made

## MOERK

that cognitions [expectancies] and stimulus-response connections are formed in any learning situation ..." (p. 75). Admittedly, Kimble publishes in a book on "neobehaviorism," but radical behaviorists accept 'cognition' (not only in humans it seems to me if I read Skinner correctly -- and against the assertion of Malott). While those points are not central to the present discussion of the proposals made in the target article, they certainly are important for the definition of the nature and use of language -- whether it is a tool to express cognitions or a tool to achieve purposes, or both. Guidance by specialists would be greatly appreciated.

And yes, I gladly agree with Malott to add reinforcement to frequency effects. I have done this already in 1976 and 1978 (though the editor of the latter journal required me to change the label "reinforcements" to "consequences" in order not to offend the sensibilities of his readers). Since then, I have explored reinforcement quite consistently (cf. Moerk, 1990). Whether these reinforcements are necessary for learning or fulfill mainly a motivating function is a well-known controversy in learning psychology (remember Guthrie and Tolman) and is not central for language acquisition. Certainly children need to be motivated to engage in verbal behavior, as Whitehurst and associates (Whitehurst & Valdez-Mechacha, 1988) have shown so clearly. And they need to engage in verbal behavior in order to train their verbal skills (cf. Moerk, 1986, 1992).

Having consistently stressed imitation as one central aspect of language acquisition (Moerk, 1977b, 1991; Moerk & Moerk, 1979), I am familiar with and have quoted the seminal work of Aronfreed (1969), Baer & Sherman (1964), and of Gewirtz and Stingle (1968). Considering the work of Bandura (1969, 1986) and my own findings in language learning, I am compellingly aware of much delayed imitation, very delayed imitation indeed. This I see as perceptual learning in one of its purest forms and as long-term storage of items without immediate performance and with often quite changed performance after longer intervals (cf. Moerk, 1991). I would eagerly welcome a fully behavioral explanation of delayed imitation and of imitation changed and improved during the delay, if this explanation can (or should) be provided without any terms from the "cognitive model" that Malott criticizes in my article.

Finally, a remark that has much wider pertinence than for the present exchange of conceptualizations and wider than for the field of first language acquisition. As the old adage says: "A little knowledge is a dangerous thing." It occurs with increasing frequency that a psychologist discovers one or two philosophers of science. Mostly it is the by now notorious Kuhn (1962), and sometimes the even more dated Pepper (1942). Being impressed by the lucid and critical approach from an unknown field, the psychologist is smitten and takes to the discovered author as if he were a prophet of the final truth. Yet, much has happened in epistemology in the 50 years since Pepper (1942) and in the 30 years since Kuhn (1962). I can mention only a few of the outstanding names (Bunge, 1991, 1992; Lakatos, 1978; Laudan, 1977; Toulmin, 1972) and some central concepts

## XENOPHOBIA

that pertain to the topic. There is, of course, also Sir Karl Popper (e.g., 1983) who is continuing his "unending quest" (Popper, 1976) and the psychologist D. Campbell (1974), both having made important and progressive contributions that are too little known to many psychologists. Despite diversities in terminologies and in minor emphases, all these authors have in common that they have surpassed the defeatist belief in "incommensurability" found in Pepper and Kuhn. In contrast, they focus upon "progressive research programs" (Lakatos) across which scientists *can* communicate indeed. And this communication leads at least to partial understanding, to "corroboration" (Popper) of findings, and to "intellectual strategies that are fruitful" (Toulmin). Applied to the present discussion: We are not condemned by outdated epistemological suggestions to give up on all communication across "paradigms" (or "root metaphors," if you will). Of course, General Systems Analysis (Bertalanffy, 1968) has been propounding such overarching approaches for decades.

And the Kuhnian argument represented a peculiar defeatist attitude all along. We have been communicating quite effectively across hundreds and thousands of languages world-wide and we have learned to communicate across ideological systems. Should scientists, speaking the same language and sharing the same culture, in a domain that deals with a quite clearly circumscribed problem and that can be supported by good empirical evidence, that is, language acquisition or the acquisition of verbal behavioral skills, be so rigid or narrow-minded that "meaningful discourse among world views [is] impossible"? (Hayes & Hayes, p. 44) In contrast, diverse conceptualizations of the same objective phenomena could alert us to features and dimensions that individual conceptualizations might have missed. They could suggest weaknesses or idiosyncracies caused by the embeddedness in specified "language games" and they could help in overcoming the linguistic determinism exerted on our conceptualizations, as postulated by Sapir (1931) and Whorf (1956) and their followers. That is, different metaphors could function as the "aliment" (in the Piagetian sense since his terminology best describes the contingencies of the phenomena in question) to shake up our rigidities and produce the "disequilibrium" that leads to restructuring and new insights. It is for me one of the attractions of the present state of behaviorism that I see this openness and this threshold to creativity.

I remind the reader again of Staats' (1983) brave attempt at designing a "bridging theory that resolves schisms, integrates diverse concepts, principles, phenomena, findings, theories, and methodology" (p. 299). Staats is joined in this endeavor by the epistemologists mentioned above, and, as far as I see it, Zuriff's (1985) excellent work, providing "a conceptual reconstruction" of behaviorism, is open-minded enough to allow such bridge building, even to invite it. I find similar potential in Morris (1988) and in Marr (1984). Most leaders in the field seem to exhibit this creativity and movement toward bridge-building -- much more than in

## MOERK

the nativistic cognitive field -- and I greatly applaud this. It would be a real loss, if it would be shortcut by reliance on outdated epistemologies.

Since my first attempt to communicate across research communities with somewhat diverse linguistic habits was not fully effective, let me try to summarize the main message I intended in the original article: No, we are not so different that we need to hate each other. There are quite some central pillars, "core principles" (in Lakatos' terms) or "core commitments" (in Laudan's) that are shared by the two conflicting systems. They have been overlooked often since different schools had acquired different distinctive cues. One of the commonalities is the functional pillar, even if it is labelled "pragmatics" by linguists. Another is a structural aspect, whether we simply call it "behavior" (which is presumed to be structured and not a sequence of random jerks) or "syntax," "language structure," or what have you. I suspect furthermore a shaky element in both approaches that consists of a nativistic label, whether called a "language organ" or "emitted behavior," which interferes with fine-grained exploration of the learning processes. And there are, of course, these learning processes in the course of language acquisition, which Skinner only adumbrates with the label "shaping" and Chomsky has to admit grudgingly since specific languages are certainly learned.

There are also complementarities between the two approaches as discussed in the target article. While mainstream linguistics has become almost fully static since Saussure (1949) and has been focused minimally on short-term changes in the acquisition process, learning psychology, of whatever provenience, is certainly intent on exploring such short-term changes. Since research on language acquisition must centrally be concerned with the influence of the linguistic environment, behaviorism with its emphasis upon stimulus control and the context of learning provides welcome means to study the impact of the environmental stimuli. Yet behaviorism has very little detailed terminology to describe linguistic structure or "topography" (but Malott convinced me this might not be the correct translation) of the environmental language stimuli. Linguists have focused upon this structure for thousands of years. While I find it promising to explain language learning based on Gibson's (1979) research on perceptual learning (cf. Moerk, 1986) and the environmental affordances he emphasizes, there is much room for behavioral research on stimulus equivalence and other formulations.

Yes, there are differences, difficulties, and hurdles interfering with communication. But neither epistemology, nor the history of humanity, or of religion or ideology decrees that we have to give up in a defeatist manner when confronting such differences. Cultural progress has mostly been based on culture contact not on isolationism and xenophobia. We can compare research programs as to their "progressivity" (Lakatos, 1978) or their "problem solving capacity" (Laudan, 1977). This includes contributions to the teaching of language, wherein behaviorism has made impressive progress. And yes, we need to search for



## XENOPHOBIA

conceptual liabilities that might weaken research programs, such as labels that are used in an explanatory fashion but are factually empty.

Finally, there exists a sturdy bridge that entails much potential to lead us further in the discovery of the learning process. This is the bridge of 'skill learning,' which deals with behavior and its acquisition, without relying on undocumented innate knowledge or undefined emitted behaviors. It deals centrally with structures of behavior and the patterns that have to be stored to emit the structures smoothly and automatically. It employs cognitive concepts, a fact that might stimulate behaviorists to provide substitute formulations. But it seems neither behaviorists nor linguists have tried to travel this bridge and unveil new vistas when they arrive at the other shore.

### REFERENCES

- Andresen, J. (1991). Skinner and Chomsky 30 years later. Or: the return of the repressed. *The Behavior Analyst*, 14, 49-60.
- Aronfreed, J. (1969). The problem of imitation. In L. P. Lipsitt & H. W. Reese (Eds.), *Advances in child development* (Vol. 4, pp. 210-314). New York: Academic Press.
- Baer, D. M., & Sherman, J. M. (1964). Reinforcement control of generalized imitation in young children. *Journal of Experimental Child Psychology*, 1, 37-49. [http://dx.doi.org/10.1016/0022-0965\(64\)90005-0](http://dx.doi.org/10.1016/0022-0965(64)90005-0)
- Bandura, A. (1969). *Principles of behavior modification*. New York: Holt, Rinehart & Winston.
- Bandura, A. (1986). *Social foundations of thought and action: A social cognitive theory*. Englewood Cliffs, NJ: Prentice-Hall.
- Bertalanffy, L. V. (1968). *General systems theory: Foundations, development, applications*. New York: George Braziller.
- Bunge, M. (1991). A critical examination of the new sociology of science: Part 1. *Philosophy of the Social Sciences*, 21, 524-560. <http://dx.doi.org/10.1177/004839319102100406>
- Bunge, M. (1992). A critical examination of the new sociology of science: Part 2. *Philosophy of the Social Sciences*, 22, 46-76. <http://dx.doi.org/10.1177/004839319202200103>
- Campbell, D. T. (1974). Evolutionary epistemology. In P. A. Schilpp (Ed.), *The library of living philosophers Vol. 14. The philosophy of Karl Popper* (pp. 413-463). La Salle, IL: Open Court.
- Day, W. F. (1980). The historical antecedents of contemporary behaviorism. In R. W. Rieber & K. Salzinger, (Eds.), *Psychology: Theoretical-historical perspectives* (pp. 203-262). New York: Academic Press.
- Gewirtz, J. L., & Stingle, K. G. (1968). The learning of generalized imitation as the basis for identification. *Psychological Review*, 75, 374-397. <http://dx.doi.org/10.1037/h0026378>
- Gibson, J. J. (1979). *The ecological approach to visual perception*. Boston: Houghton Mifflin.
- Hayes, S. C., & Hayes, L. J. (1992). Mixing metaphors: Skinner, Chomsky, and the analysis of verbal events. *Behavior and Social Issues*, 2, 43-46.
- Kimble, G. A. (1971). Cognitive inhibition in classical conditioning. In H. H. Kendler & J. T. Spence (Eds.), *Essays in neobehaviorism* (pp. 69-87). New York: Appleton-Century-Crofts.
- Kuhn, T. S. (1962). *The structure of scientific revolutions*. Chicago: University of Chicago Press.

## MOERK

- Kuo, Z. Y. (1976). *The dynamics of behavior development: An epigenetic view*. New York: Plenum Press.
- Lakatos, I. (1978). *The methodology of scientific research programs*. Cambridge: Cambridge University Press.
- Laudan, L. (1977). *Progress and its problems: Toward a theory of scientific growth*. Berkeley: University of California Press.
- Malott, R. W. (1992). Language, rule-governed behavior, and cognitivism: On Moerk's integration of Skinner and Chomsky's approaches to language. *Behavior and Social Issues*, 2, 33-41.
- Marr, M. J. (1984). Conceptual approaches and issues. *Journal of the Experimental Analysis of Behavior*, 42, 353-362. <http://dx.doi.org/10.1901/jeab.1984.42-353>
- Moerk, E. L. (1972). Principles of dyadic interaction in language learning. *Merrill-Palmer Quarterly*, 18, 229-257.
- Moerk, E. L. (1976). Motivational variables in language acquisition. *Child Study Journal*, 6, 55-84.
- Moerk, E. L. (1977a). *Pragmatic and semantic aspects of early language development*. Baltimore, MD: University Park Press.
- Moerk, E. L. (1977b). Processes and products of imitation: Additional evidence that imitation is progressive. *Journal of Psycholinguistic Research*, 6, 187-202.
- Moerk, E. L. (1978). Determiners and consequences of verbal behavior of young children and their mothers. *Developmental Psychology*, 14, 537-545. <http://dx.doi.org/10.1037/0012-1649.14.5.537>
- Moerk, E. L. (1983). *The mother of Eve as a first language teacher*. Norwood, NJ: Ablex Publishing.
- Moerk, E. L. (1986). Environmental factors in early language acquisition. In G. J. Whitehurst (Ed.), *Annals of child development* (Vol. 3, pp. 191-235). Greenwich, CT: JAI Press.
- Moerk, E. L. (1990). Three-term contingency patterns in mother-child verbal interactions during first-language acquisition. *Journal of the Experimental Analysis of Behavior*, 54, 293-305. <http://dx.doi.org/10.1901/jeab.1990.54-293>
- Moerk, E. L. (1991). Positive evidence for negative evidence. *First Language*, 11, 219-251. <http://dx.doi.org/10.1177/014272379101103202>
- Moerk, E. L. (1992). *A first language taught and learned*. Baltimore, MD: Paul Brookes Publisher.
- Moerk, E. L., & Moerk C. (1979). Quotations, imitations, and generalizations: Factual and methodological analyses. *International Journal of Behavioral Development*, 2, 43-72. <http://dx.doi.org/10.1177/016502547900200103>
- Morris, E. K. (1988). Contextualism: The world view of behavior analysis. *Journal of Experimental Child Psychology*, 46, 289-323. [http://dx.doi.org/10.1016/0022-0965\(88\)90063-X](http://dx.doi.org/10.1016/0022-0965(88)90063-X)
- Pepper, S. C. (1942). *World hypotheses: A study in evidence*. Berkeley: University of California Press.
- Polanyi, M. (1958). *Personal knowledge*. Chicago, IL: University of Chicago Press.
- Popper, K. R. (1976). *Unended quest*. La Salle, IL: Open Court.
- Popper, K. R. (1983). *Realism and the aim of science*. From the *Postscript to the logic of scientific discovery*. W. W. Bartley III (Ed.), Totowa, NJ: Rowman and Littlefield.
- Sapir, E. (1921). *Language: An introduction to the study of speech*. New York: Harcourt, Brace & World Inc.
- Saussure, F. (1949). *Cours de linguistique generale*. Paris: Payot.
- Skinner, B. F. (1937). Two types of conditioned reflex: A reply to Konowsky and Miller. *Journal of General Psychology*, 16, 272-279. <http://dx.doi.org/10.1080/00221309.1937.9917951>

## XENOPHOBIA

Skinner, B. F. (1969). *Contingencies of reinforcement: A theoretical analysis*. New York: Appleton-Century-Crofts.

Skinner, B. F. (1974). *About behaviorism*. New York: Alfred A. Knopf.

Skinner, B. F. (1977). Letter to Stephen Murray, as quoted in Andresen (1991).

Staats, A. W. (1983). *Psychology's crisis of disunity: Philosophy and method for a unified science*. New York: Praeger.

Street, E. M. (1992). The clash of giants goes beyond terminological differences: A reply to Moerk. *Behavior and Social Issues*, 2, 27-31.

Toulmin, S. (1972). *Human understanding: The collective use and evolution of concepts*. Princeton: Princeton University Press.

Whitehurst, G. J., & Valdez-Mechacha, M. C. (1988). What is the role of reinforcement in early language acquisition? *Child Development*, 59, 430-440. <http://dx.doi.org/10.2307/1130322>

Whorf, B. L. (1956). Science and linguistics. In J. B. Carroll (Ed.). *Language thought and reality: Selected writings of Benjamin Lee Whorf* (pp. 207-219). Cambridge, MA: MIT Press.

Zuriff, G. E. (1985). *Behaviorism: A conceptual reconstruction*. New York: Columbia University Press.