

WORLD VIEWS AND THEIR INFLUENCE ON PSYCHOLOGICAL THEORY AND RESEARCH: KUHN-LAKATOS-LAUDAN¹

Willis F. Overton

DEPARTMENT OF PSYCHOLOGY
TEMPLE UNIVERSITY
PHILADELPHIA, PENNSYLVANIA

I. DEMARCATIONIST STRATEGIES IN SCIENCE	194
A. POSITIVISM	195
B. CONVENTIONALISM	195
C. POPPER'S FALSIFICATION CONVENTIONALISM	197
D. SCIENTIFIC RESEARCH PROGRAMS AND RESEARCH TRADITIONS ..	198
II. ORGANISMIC AND MECHANISTIC SCIENTIFIC RESEARCH PROGRAMS ..	201
A. HISTORICAL PERSPECTIVE OF HARD CORES	201
B. ROOT METAPHOR PERSPECTIVE OF HARD CORES	205
C. POSITIVE HEURISTIC COMPONENTS	207
D. THE MEANING OF BASIC TERMS	213
III. WORLD VIEWS AND SCIENTIFIC RESEARCH PROGRAMS	216
A. WORLD VIEWS IN SCIENCE	216
B. CONTEXTUALISM AND DIALECTICS	218
C. RESEARCH PROGRAMS AND FAMILIES OF THEORIES	219
D. INCOMMENSURABILITY	221
IV. AN ORGANISMIC-MECHANISTIC COMPROMISE?	223
V. CONCLUSIONS	224
REFERENCES	225

¹Portions of earlier versions of this paper were presented at the biannual meetings of the South-eastern Conference on Human Development, Baltimore, Maryland, April, 1982, and at the biannual meetings of the Society for Research in Child Development, Detroit, Michigan, April, 1983. Special acknowledgments are expressed to Kenneth Gergen, Philip Hineine, Richard Lerner, Hayne Reese, and Ralph Rosnow for their critical reading and comments on earlier drafts.

A number of years ago, when my colleague Hayne Reese and I began to explore the relationship between empirical investigations of development and the conceptual foundations that generate these investigations (Overton & Reese, 1973; Reese & Overton, 1970), we were influenced in several ways by the writings of Thomas Kuhn (1962).

Kuhn, more than any other philosopher of science at the time, seemed to present the best articulation of our own belief that very general and abstract conceptual systems exert a categorical influence on the construction of scientific theories and, hence, on empirical investigations that derive from these theories. Following Kuhn and others (e.g., Pepper, 1942), we referred to such general systems as world views or paradigms, and we suggested that any given world view leads to a set of corollaries that acts as an abstract framework defining a research agenda. The world view and corollaries, in turn, generate a set of theories designed to explain or resolve various empirical problems. We referred to such a set of theories as a family of theories, in which the theories exhibit a genetic similarity (the world view and corollaries) but in which each theory deals somewhat differently with the diverse empirical problems it approaches. Members of a family might differ in content and they might differ over specific theoretical issues, but they exhibit a commonality and compatibility of deep structural features.

In addition to Kuhn's position with respect to the relationship between any given abstract conceptual system and empirical investigation, we were also influenced by arguments concerning the relationship between divergent, contradictory conceptual systems. Again, Kuhn's discussion of the issue of "incommensurability" seemed, at the time, to be the best articulation of our own view that incompatible abstract conceptual systems lead to incompatible forms of explanation, families of theories, and ultimately to an incompatibility in the meaning of the empirical phenomena and problems scientists attempt to resolve.

A final important influence of Kuhn's work on our own position involves the understanding of the very nature of scientific activity. Through the 1950s and into the 1960s psychology and other fields were dominated by a philosophy of science that held tightly to an empiricist's epistemology. From this epistemological position, as will be described more fully later, abstract conceptual systems of the type that Reese and I were attempting to examine had no legitimacy in science. Thus, our arguments that such systems perform an essential and necessary function in scientific activity also suffered from a very basic illegitimacy when interpreted within this empiricist framework.

In the late 1950s and early 1960s, however, a radically different version of the nature of scientific activity began to emerge and develop. This was a philosophy of science that admitted a more rationalistic epistemological orientation and maintained that even the seemingly most neutral scientific observations were

permeated by abstract conceptual features. Pepper (1942) and Hanson (1958) were early contributors to this reconstruction of the nature of scientific activity, and Kuhn was particularly influential in elaborating and disseminating significant features of it. Because this orientation toward the nature of scientific activity was compatible with our own views, and because this orientation provided our work with a rational scientific legitimacy, Reese and I were naturally influenced by many of its features.

Since the publication of Kuhn's original paper (1962), a good deal of philosophical activity, among those who begin from rationalist epistemological assumptions, has been directed toward elaborating and developing a number of the issues raised by Kuhn. With time, although the basic structure concerning the relationship between conceptual systems and empirical investigation has remained intact, significant elaborations and extensions have also occurred. The best contemporary representations of these elaborations and extensions are defined in Imre Lakatos' theory of "scientific research programs" (Lakatos, 1978a,b) and Larry Laudan's extension of this work into what he refers to as "research traditions" (Laudan, 1977).

In a similar fashion, since the publication of our original papers, Reese and I have developed and modified our positions each in our own way. If the original papers were to be rewritten today several changes would be made. For example, I would move away from various considerations of the nature of truth alluded to in those papers and toward the view presented by Laudan (1977), that a central aim of science is to solve problems, not to seek universal truths about the universe. I would also consider in a more detailed fashion the nature of the incommensurability issue.

However, the most important modification I would make in the original papers is one that I will, in fact, use as a point of focus in the present paper. This is a general reformulation of the early Overton-Reese analysis within the context of philosophy of science issues outlined by Lakatos and Laudan. I believe that the positions articulated by Lakatos and Laudan, with respect to the philosophy of science, strongly support and clarify a number of the key points I have been trying to make with respect to the field of developmental psychology. Specifically, I believe that the early work of Overton and Reese concerning the influence of world views on psychological theory and research is today best interpreted within the context of Lakatos' strategy of scientific research programs, with appropriate modifications being made to incorporate recent advances suggested by Laudan.

In the following pages I will first set the argument by describing some central components of Lakatos' position. Then I will describe and reformulate the early work of Overton and Reese within this context. Finally, I will employ my reformulation to explore some issues related to the role played by mechanistic and organismic research programs in psychological theory and research.

I. Demarcationist Strategies in Science

For both Lakatos and Laudan, the main problem for the philosophy of science is the establishment of normative criteria that demarcate science from nonscience or pseudoscience. Lakatos (1978b) is explicit in rejecting both the view of skeptics (e.g., Feyerabend), who regard scientific theories as just one family of beliefs, equal epistemologically with any other set of beliefs, and elitists (e.g., Toulmin), who claim that no universal criteria are possible and only individual scientists can judge the adequacy of scientific theories. Lakatos and Laudan argue that skepticism and elitism each lead to a view of science as nonrational or irrational and that the rationality of science is preserved only by adopting a demarcationist strategy of attempting to reconstruct universal criteria embedded in past and present scientific activities.² Such criteria then serve as standards with which to judge the progress or degeneration of evolving science.

It should be noted that both Lakatos and Laudan included Kuhn among the group of elitists that denies the possibility of universal criteria. However, this view of Kuhn's position appears to be based on a misinterpretation. Although Kuhn's analyses have focused upon the individual differences among scientists that lead to different choices among competing theories he has not denied the existence and discoverability of universal criteria. As he stated, "One must . . . deal with characteristics which vary from one scientist to another without thereby in the least jeopardizing their adherence to the canons that make science scientific. Though such canons do exist and should be discoverable . . . they are not themselves sufficient to determine the decisions of individual scientists" (Kuhn, 1977, pp. 324–325). It is through their analysis of universal criteria—providing the context within which Kuhn's individual factors operate—that the contributions of Lakatos and Laudan may be said to be an elaboration and extension of Kuhn's. The situation in this instance is analogous to some contemporary forms of linguistic theory. For those who take the competence–performance distinction (Chomsky, 1975) seriously, a complete understanding of language requires both an inquiry into universal structures (competence) and individual factors (performance) that affect the expression of competence. So, too, a complete understanding of scientific activity requires the universal criteria described by Lakatos and Laudan and the individual factors described by Kuhn.

Lakatos (1978a) described four rival demarcationist sets of criteria or rival methodologies that have served as universal standards for the acceptance or

²The assertion that science is a rational activity is epistemologically neutral. Empiricist models of rationality claim that rationality is a product of observations of nature. Rationalist models claim that rationality is a product of mental activity (reason) and observation.

rejection of theories in science. These included the methodologies of positivism, conventionalism, conventionalism with falsification, and Lakatos' own methodology of "scientific research programs." Laudan's demarcationist methodology, referred to as "research traditions," is an extension and modification of Lakatos' "scientific research programs."

A. POSITIVISM

Two primary universal criteria were set down by positivism as standards of scientific legitimacy. The first was that ultimately, all general propositions in science must be reducible to statements describing hard data, i.e., observations. The second was that general propositions in science must be formulated on the basis of inductive inference and only inductive inference from observables. Thus, positivism claimed that the only propositions admissible into the essential body of science were those that describe observations, or infallible inductive generalizations drawn from observations (Lakatos, 1978a, p. 103). To the extent that deductive inference was accepted, it was employed only to derive from proven empirical generalizations other potentially provable propositions, i.e., propositions that could be reduced to observations.

Ultimately, positivism was widely rejected as a demarcationist position. Most of the friends and all the foes of positivism came to doubt the possibility of reducing general theories to observational propositions and, more importantly, they felt that the laws of science can seldom be adequately described as inductive generalizations (Lakatos, 1978a; Pepper, 1942; Wartofsky, 1968). Although positivism waned in popularity as a general scientific methodology, its ghostly influence continued to be felt in psychology. Guided by this influence, many still distrust any proposed scientific approach that suggests that to some extent theories are determined by more abstract conceptual systems; and many still believe that ultimately, conceptual incompatibilities will be totally resolved on the basis of hard-proven veridical bits of unambiguous data.

B. CONVENTIONALISM

The second demarcationist position or methodology is conventionalism. Conventionalism was built upon positivism and incorporates many of its features, including a heavy reliance on inductive inference (Lakatos, 1978a, p. 106). Conventionalism, however, recognizes that not all scientific propositions can be reduced to observational statements. As a consequence, conventionalism allows for the introduction of propositions, describing nonobservables, including theoretical entities and general theoretical models such as, in psychology, the computer model of information-processing approaches (Lachman, Lachman, & But-

terfield, 1979). Although such propositions are held to be irreducible to observational statements, their primary feature is that they operate *only as convenient and conventional ways of ordering and organizing hard data*, i.e., observations. They do not influence the data base itself. Rather, they operate like pigeonholes to classify, arrange, and organize hard data into coherent units. In psychology, the framework of encoding, storage, and retrieval in memory offers a familiar example that is often interpreted from such a conventionalist perspective.

As conventions, theoretical entities or models are lightly held and readily given up when simpler ways are found of organizing hard data. As Lakatos pointed out, "For the conventionalist . . . [theoretical] discoveries are primarily inventions of new and *simpler* pigeonhole systems" (1978a, p. 107). However, the "genuine progress of science . . . takes place [still, as with the positivists] on the ground level of proven facts [i.e., hard data] and changes on the theoretical level are merely instrumental" (p. 106).

Conventionalism results in the creation of two distinct levels of scientific activity. These levels have been termed the context of justification and the context of discovery (Nickles, 1980; Overton, 1976; Reichenbach, 1938). One level (the context of justification) includes observations, experimental manipulations, and inductive generalizations, and this is the level at which genuine progress and explanations take place according to conventionalism. The other level (context of discovery) includes theoretical terms, entities, and models. These propositions may themselves be the products of the scientist's hunches, guesses, creative imagination, or metaphysical presuppositions. The origin of general propositions matters little because, according to conventionalism, this level exerts no real influence on the essential features of science, i.e., those included in the context of justification.

Within either positivistic or conventionalistic interpretations of the nature of scientific activity, the Overton-Reese analysis thus finds no real scientific legitimacy. At best, within conventionalism, the rival world models or world views described by Overton and Reese would be understood as mere convenient devices used to arrange theories and hard data. This, in fact, appears to be the interpretation that Beilin (this volume) gives to the Overton-Reese position and, in turn, Beilin seems to argue that he has found a simpler pigeonhole system, i.e., the "new functionalism." Overton and Reese, however, rejected any conventionalist interpretation. Our thesis has been that world models or world views enter into the essential body of science by providing certain metaphysical and methodological commitments that provide a set of guidelines for the construction of specific theories and the employment of specific methods of procedure. As Laudan emphasized with respect to "research traditions," I would repeat with respect to a world view: "Put simplistically, a . . . [world view] is thus a set of ontological and methodological 'do's' and 'don'ts.' To attempt what is forbid-

den by the metaphysics and methodology of a . . . [world view] is to put oneself outside the tradition and to repudiate it" (Laudan, 1977, p. 80).

C. POPPER'S FALSIFICATION CONVENTIONALISM

An important variant of the conventionalist position is Karl Popper's falsification methodology (Popper, 1959). Popper recognized that the acceptance of theories or models based only on comparison of intuitive *simplicity*, as suggested by "classical" conventionalism, can be only a matter of subjective taste and hence constitutes a very weak criterion. Popper also recognized that theories or theoretical constructs can be neither verified nor confirmed. As a result, Popper proposed that in order for a theory or theoretical construct to be accepted into the body of science it must be shown to be in principle falsifiable. That is, the scientist, if his or her own theory is to be scientific, must specify results that, if found, would disprove the theory.

This criterion of scientific acceptance, although attaining a good deal of popularity among conventionalists, has also been severely criticized because of the ambiguity over whether falsification of an observational experimental hypothesis can spread to the falsification of the theory that generated the hypothesis. Several arguments against the position that a general theory can be falsified are detailed by both Lakatos and Laudan. One central problem is that when an experiment is conducted, a complex network of theoretical propositions is required to produce the experimental prediction or hypothesis. Falsification of the experimental hypothesis, however, does not unambiguously indicate the location of the error in the network. Another problem is that when the prediction is not supported it is pragmatically difficult to exclude the possibility that the error did not reside in either the inference drawn from the theoretical network or in the manner in which the experimental measures were conducted.

The nonfalsifiability of general theories is central to the understanding of scientific progress as analyzed by Kuhn, Lakatos, and Laudan. Each of these investigators accepted the view that falsified experimental hypotheses do not directly refute a general theory, but rather that they constitute anomalous instances. For Kuhn, anomalies accumulate until they induce a "crisis" and the dominant paradigm or general theory is replaced by a new paradigm. However, as noted earlier, Kuhn offers no universal rational criteria for the change from one to another paradigm. For Lakatos and Laudan, scientific progress is judged in terms of the empirical productivity of the general system (i.e., "scientific research program" or "research tradition") and anomalies are weighed in this context.

Before turning directly to Lakatos' position, I would like to mention a final

feature of Popper's demarcationist strategy because of its relevance to the Overton-Reese analysis. Clearly, within the rules proposed by Popper, specific theories may enter the essential body of science but more general propositions, such as metaphysical propositions, may not. For Popper, metaphysical propositions may provide the external stimulus that leads the scientist to falsifiable theories, but the metaphysical propositions themselves are inherently nonfalsifiable, and hence peripheral to science per se. Thus again—as with the classical conventionalist strategy—within a falsification position world views are, at best, convenient heuristic devices but they do not perform the necessary and essential role claimed by Overton and Reese.

D. SCIENTIFIC RESEARCH PROGRAMS AND RESEARCH TRADITIONS

An important point to note is that each of the demarcationist strategies discussed to this point is heavily influenced by an empiricist epistemology. According to this epistemology, ultimately all legitimate scientific knowledge derives directly from fixed observational data free from any theoretical interpretation. The systems of Lakatos and Laudan, however, are much more strongly influenced by a rationalist epistemology.³ Following Kant, this epistemology asserts that legitimate scientific knowledge is the product of both mental activity (reason) and observation. As mentioned earlier, the contemporary expression of this perspective as it applies to the nature of scientific activity took form in the late 1950s. Gradually it became articulated and elaborated through the analysis of various philosophers of science including Hanson, Hesse, Kuhn, Feyerabend, Pepper, Toulmin, and Wartofsky. The major point of agreement among this group is that although scientific activity is directed toward the solution of empirical problems, the observations that constitute the empirical content are never free from interpretation. As Laudan stated,

In calling such inquiry situations "empirical" problems, I do not mean to suggest they are directly given by the world as veridical bits of unambiguous data. Both historical examples and recent philosophical analysis have made it clear that the world is always perceived through the "lenses" of some conceptual network or other and that such networks and the languages in which they are embedded may, for all we know, provide an ineliminable "tint" to what we perceive. More to the point *problems* of all sorts (including empirical ones) *arise within a certain context of inquiry* and are partly defined by that context. (1977, p. 15)

³Lakatos (1978a) at times referred to himself as a conventionalist. However, his ideas are more appropriately described as epistemologically rationalistic. As Laudan pointed out, Lakatos tried to make Popper's conventionalist "theory of rationality germane, and to fit his own interesting ideas into a Popperian context (where they do not really belong)" (Laudan, 1977, p. 227). Also, Lakatos himself explicitly accepted a Kantian "activist approach to the theory of knowledge" (Lakatos, 1978a, p. 38).

This rationalist orientation toward scientific activity, with its claims that bare, uninterpreted data are not possible and that conceptual systems permeate observations, is obviously compatible with the Overton-Reese analysis. However, a clear expression of the implications of this compatibility required the elaborations and extensions provided by Lakatos and Laudan, as they advanced beyond earlier investigators, particularly Kuhn.

The demarcationist strategy for scientific acceptability provided by Lakatos begins with a unit of analysis that is broader than any observational data base, and broader than any isolated theory or conjunction of theories. Lakatos called this unit a "scientific research program." It can be thought of as being composed of three levels arranged in a hierarchy. From top to bottom, in terms of decreasing levels of generality, these consist of first, a "*hard core*" and second, a "*positive heuristic*." These levels define problems and outline the construction of the third level, called a "*belt of auxiliary hypotheses*," which are embodied in a family of specific theories.

The hard core itself may consist of various types of propositions, including metaphysical propositions. The potential inclusion of metaphysical propositions is particularly important because, in contrast to positivism and either form of conventionalism, the "hard core" admits into the *essential body of science* propositions that may have no potential falsifiers. Metaphysical propositions, as they may constitute a hard core of a scientific research program, are not, then, simply idle psychological or sociological curiosities, rather they are essential components of scientific activity. They exert a formative influence on lower levels and give meaning to the theoretical concepts of specific theories. For example, Lakatos described how Cartesian metaphysics, i.e., the mechanistic theory of the universe, operated as a hard core of a scientific research program that discouraged work on scientific theories that were inconsistent with it, e.g., Newton's theory of action at a distance, while encouraging work on auxiliary hypotheses that might have saved it from counter evidence (Lakatos, 1978a, p. 48).

In addition to bringing into the essential body of science that which is an external influence in positivism and conventionalism, another characteristic of the hard core is that it is not open to falsification. That is, the hard core is irrefutable. The reason the hard core is irrefutable is open to question. Lakatos claimed it is irrefutable through a "methodological decision of its proponents" (1978a, p. 48). In contrast, Overton and Reese (Overton, 1976; Overton & Reese, 1973) claimed that this characteristic derives from the fact that the categories of the hard core represent the extension of a basic or root metaphor.

The value of the irrefutability of the hard core is that it allows for the development of the "positive heuristic" without unnecessary distraction. The positive heuristic of the research program is influenced by the hard core but it describes the long-term research policy of the program. It is more flexible than the hard

core and consists of, to quote Lakatos, "a partially articulated set of suggestions or hints on how to change, and develop the 'refutable variants' of the research programme, how to modify, and sophisticate the 'refutable' protective belt of auxiliary hypotheses" (Lakatos, 1978a, p. 50).

Taken together, the hard core and the positive heuristic of a given program constitute a *conceptual framework* that generates specific theories that within any given program constitute a family of theories. These theories in turn embody the "belt of auxiliary hypotheses," which are sets of observational hypotheses that constitute the falsifiable or refutable component of the scientific research program.

For Lakatos, falsification, i.e., the occurrence of anomalies, constitutes a local and minor criterion of scientific progress. The major criterion of progress of a scientific research program is that it predicts novel or unexpected phenomena with some degree of success. Furthermore, the anticipation of novel events should be guided by a coherent, preplanned positive heuristic rather than via patched-up ad hoc auxiliary hypotheses. Laudan expanded this position by maintaining that science aims "to maximize the scope of solved empirical problems, while minimizing the scope of anomalous and conceptual problems" (1977, p. 66). Thus, scientific progress is measured ultimately by a pragmatic criterion and not by the realist truth criterion of positivism and conventionalism. For Lakatos and for Laudan (1977) the advance of science is best described in terms of problem solving rather than in terms of making observational discoveries (see Nickles, 1980, p. 47).

With respect to the falsification of observational hypotheses, Lakatos claimed that the scientist must note them as they occur "but as long as his research programme sustains its momentum, he may freely put them aside. . . . Only when the driving force of the positive heuristic weakens, may more attention be given to anomalies" (1978a, p. 111). Laudan, in his extension of the general strategy, suggested ways in which anomalies can and should be graded in terms of their cognitive importance (1977, pp. 36–40).

In summary, Lakatos has defined several alternative demarcationist strategies and has put forward his own methodology of scientific research programs as the most adequate normative criteria of science and scientific progress. Lakatos' methodology builds upon but extends several features of Kuhn's analysis, as Laudan's similarly builds upon Lakatos'. I would maintain at this point that my own views with respect to the relationship of deep-seated ontological and methodological commitments to theories and research practices are currently most reasonably interpreted within the framework of research programs or research traditions. I believe that this interpretation both highlights the scientific legitimacy of the early Overton–Reese analysis and supports and clarifies a number of key components of that position.

II. Organismic and Mechanistic Scientific Research Programs

In essence, both Overton and Reese have maintained that two rival programs have continued to exert a strong influence in several scientific domains,⁴ including psychology generally and developmental psychology specifically. These rival programs were termed the mechanistic and organismic world views or world models, but for the purposes of clarity, they will now be referred to as scientific research programs.

The thrust of the Overton–Reese analysis has been to articulate the hard core of each research program, to describe the positive heuristic that the hard core generates, and to demonstrate how the hard core and positive heuristic of a program result in a family of theories and testable auxiliary hypotheses. Consistent with both Lakatos and Laudan, we have also maintained that the hard cores of the rival programs are not open to experimental test, i.e., they are irrefutable, the hard cores represent an essential feature of each group's scientific activity, and the hard cores, through their positive heuristic, have an effect on the way the developmental psychologist does his or her research and understands basic terms in the field. The Overton–Reese analysis of the organismic and mechanistic research programs entails both a historical perspective, describing formative philosophical issues that came to represent the hard core ontological commitment of each program, and a parallel perspective describing the categories of each hard core as elaborations of root metaphors (see Table I and Overton, 1976, 1982a; Overton & Reese, 1981).⁵

A. HISTORICAL PERSPECTIVE OF HARD CORES

From an historical perspective, the two issues that have framed the hard cores of these rival programs are the question of the categorical nature of Being or Becoming, and the question of accidental or necessary organization. The issue of

⁴For an analysis of the influence of these programs with respect to classical physics and quantum theory, see Heisenberg (1958), especially Chapter V.

⁵The recognition of the role played by metaphor in the generation and elaboration of the hard core of a research program or tradition facilitates the solution to a problem that existed for both Lakatos and Laudan. Both asserted that "certain elements of a research tradition are sacrosanct, and thus cannot be rejected without repudiation of the tradition itself" (Laudan, 1977, p. 99). Lakatos suggested those elements derive from trial and error but once established they do not change. Laudan insisted that the elements change with time although he offered no solution to how this occurs. Recognition of the metaphorical base of the hard core suggests that the origin of the elements is not a trial and error process and that change is constrained by a systematic elaboration of the metaphor (see Overton, 1976; Pepper, 1942).

TABLE I
Origin and Nature of Scientific Research Programs

Metaphor		
Living organism (e.g., a plant)		Machine (e.g., a watch)
Research programs		
Organismic	Hard cores	Mechanistic
World models		
<ol style="list-style-type: none"> 1. Organization 2. Activity 3. Change (dialectic) 4. Accidental factors (a minor focus) 	} Hard core assumptions }	<ol style="list-style-type: none"> 1. Uniformity 2. Stability 3. Fixity 4. Accidental factors (exclusive focus)
↓	Epistemology	↓
Constructivism-rationalism Knower actively con- structs the known	} Hard core assumptions }	Realism-empiricism Knower comes to reflect or acquire a copy of reality (the known)
↕	Models of humans	↕
Active organism <ol style="list-style-type: none"> 1. Inherent organization and psychological functions 2. Inherently active 3. Qualitative change of organization 	} Hard core assumptions }	Responsive organism <ol style="list-style-type: none"> 1. Uniformity, organiza- tion as appearance 2. Inherently at rest 3. Quantitative change
↓	Positive heuristics	↓
<ol style="list-style-type: none"> 1. <i>Holism</i> Understanding in context of the organic whole 2. <i>Structure-function analysis</i> Establishing the organiza- tion of a system ex- plains behavior (formal explanation) Establishing contingent factors explains rate of behavior (contingent explanation) 		<ol style="list-style-type: none"> 1. <i>Elementarism</i> Understanding through reduction to elements 2. <i>Antecedent-consequent analysis</i> Establishing contingent factors explains behav- ior (contingent expla- nation)

TABLE I (Continued)

3. <i>Necessary change</i> Establishing the order of organizational change explains development (formal explanation) Contingent factors explain rate of development (contingent explanation)	3. <i>Accidental change</i> Establishing the contingent factors explains development (contingent explanation)
4. <i>Discontinuity-continuity</i> Emergent systemic properties and levels of organization	4. <i>Strict continuity</i> Strict additivity
5. <i>Reciprocal causality</i>	5. <i>Unidirectional causality</i>
6. <i>Organized complexity</i>	6. <i>Linear causality</i>
↓	↓
Families of theories	
Examples Contemporary structuralist theories: Piaget, Werner, Chomsky, Kohlberg, G. Allport, G. Kelly Humanistic theories: Rychlak Gestalt theories Ego development theories: Erikson, Kegan, Bowlby Bronfenbrenner's ecological perspective	Examples Behavioristic and neobehavioristic theories Operant and classical conditioning theories Observation learning theories Mediational learning theories Information-processing theories: Skinner, Bijou and Baer, Berlyne, Spiker, Bandura, Gewirtz, H. Kendler, Gibson

Being and Becoming entails the question of whether we represent the basic nature of objects and events as ultimately stable and fixed (Being) or as ultimately active and changing (Becoming). The history of the Being position can be traced to the early writings of Thales, Anaximenes, and Democritus, and from there to Locke, Berkeley, and Hume. The history of the Becoming position originates in the writings of Anaximander and Heraclitus and leads from there to Leibniz, Vico, Kant, and Hegel (see Overton, 1982a). Representation in terms of the Being position requires that any apparent activity or change be explained by reducing it to stable and fixed elements and then discovering accidental or contingent factors that generated the original complex. Representation in terms of the Becoming position accepts activity and change as necessary. Here the

primary task of explanation entails the discovery of the organization, form, or pattern of activity and change.

The distinction between the necessary and the accidental is derived from Aristotle. Activity or change that is accidental is that which is caused by fortuitous or contingent events. Necessary activity and change are free of causal events and are natural to the entity being considered. For example, a plant might be understood as going through a sequence of changes that is as necessary to the essence of the plant as are any other intrinsic features. At the same time, the plant has a history in the sense that accidental events such as favorable or unfavorable nutrients or good or bad weather may occur. The issue of accidental versus necessary organization is similar in the sense that one position claims that it is best to represent objects and events as ultimately uniform and organization as the product of accidental factors whereas the other position claims the best representation is found in considering objects and events as exhibiting necessary organization. The theme of necessary organization was first elaborated by Plato and Aristotle and later developed by rationalist philosophers (e.g., Kant). The theme of uniformity was developed within the historical tradition of philosophical empiricism (e.g., Bacon, Newton, Locke, Hume) (see Overton, 1982a).

The *hard core* of the *organismic* program is expressed in an ontological commitment to a Kantian–Hegelian philosophy of Becoming, wherein activity, change, and organization are understood as natural and necessary features of the cosmos and not simply as the product of contingent accidental factors. Accidental factors can here affect activity, organization, and change, but they cannot explain them. The *organismic positive heuristic* encourages its practitioners to work within a holistic–analytic framework, to consider change and organization as necessary and consequently open to a structure–function analysis, and to represent change as both continuous and discontinuous. The positive heuristic also establishes that both formal and contingent explanations are legitimate and that each serves a different role in providing general explanations (see Overton & Reese, 1981; Rychlak, 1977). Finally, the positive heuristic has led to a family of theories (see Table I) and numerous auxiliary hypotheses such as the set of hypotheses that maintains that the child's conceptual understanding is a necessary reflection of the child's level or stage of structural development.

The *hard core* of the *mechanistic* program is expressed in an ontological commitment to a Lockean–Humean philosophy of Being, wherein stability, fixity, and uniformity are considered basic, and change and organization are understood as the result of contingent or accidental factors only. The *positive heuristic* or research policy that this hard core generates—or what Overton and Reese earlier referred to as corollary model issues—encourages the practitioners of this program to work within a framework of elementaristic or reductionistic analysis, to consider all change and organization as the product of contingent antecedent factors, and to represent all change as strictly additive or continuous

in nature. The positive heuristic also establishes that all explanations will be contingent explanations based on efficient or material factors (see Overton & Reese, 1981). Finally, the positive heuristic has, in turn, led to a family of theories (see Table I) and numerous auxiliary hypotheses, such as the set of hypotheses that asserts that the child's conceptual understanding (organization) is a direct product of learning (contingent antecedent factors).

B. ROOT METAPHOR PERSPECTIVE OF HARD CORES

A perspective that is parallel to the historical focuses upon the metaphor that generates the categories of the hard core of the rival programs (see Table I and footnote 5). The basic metaphor for the organismic program is a living organism such as a plant, and the metaphor for the mechanistic program is a simple machine such as a wind-up watch.

If we consider a plant from a relatively naive, common sense perspective it seems to have certain characteristics. For one, the plant has a unique form, structure, or organization that is not characteristic of any of its parts, e.g., its cells, nor a product of the parts simply added together. The organization is a feature of the whole organic unity. Furthermore, the meaning of a part is never complete except in relation to the whole. That is, the parts have no independent existence outside the organization in which they function. A second characteristic is that the plant has an inherent activity in the sense that it draws some things into itself (e.g., carbon dioxide) from the environment, rejects others (e.g., dust), gives back to the environment (e.g., oxygen) and is generally self-regulating or self-maintaining. Finally, the plant changes. This change appears to be as intrinsic to the plant as is its structure. Furthermore, the change is both directional, rather than random, and irreversible. The plant also goes through phases in its directional change and during these phases the structure of the plant changes qualitatively. In addition to these characteristics, the plant may also require nourishment by external factors and it may be subjected to harmful accidental factors acting upon it, but both sets of factors affect rate of activity and rate of change and not the inherent characteristics of the plant.

Given this image of the plant, it is not difficult to see how the basic categories of necessary organization, activity, and change were metaphorically drawn out to form the organismic hard core. Nor is it difficult to see how the focus of explanation is on organizational or formal features and how accidental factors came to be understood as efficient causes that primarily affect rate of activity and rate of change.

If we now consider a simple machine—again from a relatively naive perspective—a contrasting set of characteristics becomes apparent. First, the machine exhibits a uniformity of its parts. The apparent organization that seems to make the thing a watch can be broken down or reduced to these uniform parts. Al-

though the parts operate together, they maintain their identities either in or out of the watch. Thus, the whole is an additive sum of its parts. Second, a machine is inherently at rest. Any activity is ultimately a product of external factors (e.g., winding the watch sets the gears in motion). Finally, a machine does not change. A machine does not go through phases in which its features become transformed. Any apparent changes (the hands on the watch move around the dial) are quantitative in nature and they, like activity, are the product of external factors.

It is easy to see how this machine image leads to the mechanistic hard core with its basic categories of uniformity, stability, and fixity and how accidental factors form the explanatory focus. Beyond generating different research program hard cores of such great scope that they may include virtually any domain of study (i.e., world models), the metaphors also provide guidelines for introducing greater specificity. For example, with respect to a specific focus on the investigation of man, the metaphors provide the categories for both the relationship between man and knowledge (i.e., epistemological models) and for the very nature of man. Thus, given the image that the knower (man), like any other component of the world, is active and possesses an inherent organization, the general organismic categories lead directly to rationalism and the position that the knower actively constructs the known. In contrast, the mechanistic categories of stability and uniformity lead to realism-empiricism and the position that the knower comes to reflect or acquire a copy of reality, and this is the known. Furthermore, a constructivist position concerning knowledge is possible only if one starts from the assumption that man possesses an inherent activity and an inherent organization such that the organization is applied to the world of experience in the creation of knowledge. On the other hand, a realist position is possible only if one basically views man as a kind of neutral mirror (or as Locke's blank slate) that reflects an external reality.

More broadly, when the general categories of the organismic hard core are extended to create a model of man, there results what has been called the *active organism* model (Overton, 1976). This model presents the following representation of man: Man, too, like other components of the universe, exhibits a necessary organization. Properties of this organization include psychological functions, such as representation, perception, and attention. Man, too, is inherently active; man, not the environment, is the source of acts. Finally, psychological growth or development in the sense of necessary, directional, qualitative change of organization and hence change also in the form of psychological functions, i.e., change in the way we represent things, the way we perceive, etc., is a reality, not an appearance.

In contrast, the extension of the mechanistic categories results in a model of man called the *responsive organism* model (Baer, 1976). Here man, like the machine, exhibits a uniformity of structure (Locke's blank slate). Man possesses

no necessary organization or inherent psychological functions. If we seem to detect such organization or functions, they are to be treated as appearances that are ultimately understood as the product of external factors. Also, man, like the machine, is understood to be inherently at rest, and activity is the product of external factors. Finally, psychological growth or development, like organization, is to be treated as appearance. Only behaviors change, and these do so in a quantitative manner.

These models, although providing some specificity in terms of the articulation of a limited domain of study, i.e., man, are still quite abstract. They do not constitute psychological theories, nor are they specifically psychological concepts. However, these often implicit categories do form the hard cores of rival programs and they do provide guidelines for the development of the research policies of these programs (i.e., the positive heuristics), the psychological theories of the programs (i.e., families of theories), and specific psychological concepts. In the following section the positive heuristic features of the rival programs will be considered.

C. POSITIVE HEURISTIC COMPONENTS

1. *Holism versus Elementarism*

The positive heuristic feature of holism derives directly from the organismic categories of considering organization as an active organic unity in which the whole is characterized by systemic properties that do not describe the parts. Holism asserts that parts of an organic unity get their very meaning from these systemic properties. Consider, for example, any of the human biological systems, such as the visual system. It should first be noted that vision is not a property of any of the parts of the system; the retina does not have vision, nor does the optic nerve, nor does the occipital lobe of the brain, etc. Vision is a property of the total, intact, active organization. Furthermore, the parts themselves derive their meaning from the context of this whole system in which they operate. The optic nerve is differentiated from other tissues precisely because it functions as part of the visual system.

In psychology, behavior represents the ultimate parts and, according to this holistic assumption of organicism, specific behaviors derive their meaning and hence can be understood only in the *context* of the system of which they are a part. In essence, holism warns that it is inappropriate to reduce systems, e.g., "thinking," "language," or "perception," to simple behaviors or to assume that the simple behaviors are the sum and substance of the system. This position does not argue that analysis is unimportant. It argues that the best research approach is to analyze in terms of the function or end that the behavior serves in

the system. This position leads to the next organismic corollary, structure-function analysis, which will be described shortly.

Before leaving holism it should be mentioned that in addition to influencing the research approach, any theory constructed within the organismic approach will in some way translate this feature and the other heuristic features into its theoretical concepts. The same may be said concerning the mechanistic approach. With respect to holism, an example of this translation is found in Piaget's statement concerning "structures" in his theory. "Wholeness is a defining mark of structures . . . all structuralists . . . are at one in recognizing the contrast between structures and aggregates, the former being wholes, the latter composites formed of elements that are independent of the complexes into which they enter" (1974, p. 6).

The corollary of elementarism derives directly from the mechanistic categories of uniformity and stability wherein apparent organization is understood as an aggregate composed of elements that maintain their identities in or out of the complex. Elementarism asserts that elements or parts constitute the real. As already mentioned, in psychology, behaviors represent the ultimate parts. Elementarism then argues that the best research approach is to reduce any complex phenomenon to these elements. This reduction is the first step in what has been traditionally called mechanical explanation.

2. *Structure-Function Analysis versus Antecedent-Consequence Analysis*

As mentioned above, the best research approach according to the organismic position is to begin from the definable functions of any system or subsystem. Following the specification of function, the investigator seeks to discover (i.e., rationally represent) the organization or structures that serve the function. This approach is familiar in organismically oriented biology. The biologist acts *as if* each organ or structure has a function. He defines the function and inquires into the operation of the structure in serving this function. For psychology, the approach is similar but the structures are psychological and not physical or physiological. Psychological structures refer to the relatively stable organizational properties or patterns of specific behavioral systems. In essence then, having defined the function, the investigator seeks to discover (rationally) the organization. That is, the investigator seeks to establish the formal explanation.

Here we may take Chomsky's (1975) work on language as an example both of the research approach and the theoretical incorporation of this feature. In its general features Chomsky's approach begins with a definition of the function of language as communication and representation. (Note that this excludes some behaviors that on the basis of material identity would be judged as language,

e.g., the “language” of the bees and the mimicry of certain birds do not have a representational function.) Given this defining context, Chomsky then examines specific behaviors (written and spoken language acts) and draws inferences in the form of a relatively small number of rules that he believes adequately characterize both the behavior he has examined and all other language behaviors. These rules or structures, he would assert, capture the essential organization of language. Furthermore, the rules describe the psychological organization of the language producer, i.e., man, because man must in some sense (not consciously) possess the rules in order to demonstrate the behavior.

Several features of this approach might be questioned. First, it might be asked whether the approach isn’t overly speculative because the rules are actually made up by the investigator. The answer is yes, it is speculative or, as stated earlier, it is a rational construction but the speculation is kept in check by the fact that later empirical tests will be made to determine whether the rules are in fact a good representation of the organization of behavior. Second, there is also a question of whether this approach doesn’t demonstrate circular reasoning because the rules are inferred from the behavior and the rules are said to determine the behavior. The answer is that any vicious circularity is avoided if the investigator can show that the rules determine many other behaviors (e.g., other sentences) beyond the few that were used to make the inferences about the rules. In fact, demonstrating this and also demonstrating that the rules would not lead to impossible behaviors (e.g., groups of words that are not acceptable as sentences in a language) is one way of testing whether the rules are a good representation. Third, one might wonder whether the approach isn’t descriptive rather than explanatory because the rules describe the behaviors. The answer is that if there were a different rule for each behavior it would be descriptive. Explanatory power in the formal explanatory sense comes from establishing a small set of rules that accounts for a wide range of behaviors.

A final question is whether experimental methods play any role in this approach. It was just pointed out that empirical tests are employed to decide whether the proposed organizations or structure, or rules are good or poor explanatory representations. Usually these tests employ correlational methods to decide whether different sets of behaviors exhibit sufficient coherence to warrant the inference that they are reasonably represented by the proposed organization. However, this still leaves open the question of whether actual experiments are conducted within this approach. An experiment, i.e., the manipulation of an independent variable and the observation of the effect of the manipulation on the dependent variable, is the method for establishing efficient explanations. But inherent organization is not the simple product of efficient factors, it is the formal explanation. Thus, with respect to the momentary organization that we are discussing, the approach does not use experiments to establish the organization.

Experiments *are* used in this approach but their role is that of determining how various situational (efficient) or biological (material) factors influence the way in which the organization is demonstrated in overt behavior (Overton & Newman, 1982). For example, we may know that an individual possesses the necessary structures for language and then we may want to investigate what kind of situational factors will increase the rate at which he or she actually speaks the language. Here, an experiment would be appropriate.

The mechanistic corollary of antecedent-consequent analysis derives from the hard core categories of uniformity and stability and it represents the second and key step in mechanical explanation. Once the phenomenon of interest, e.g., language, has been reduced to its elements, i.e., sounds, the rest of explanation consists primarily of establishing the material and efficient features or antecedents of those behaviors or consequences. This, of course, is done by performing experiments in which the assumed antecedent is the independent variable and the consequence is the dependent variable. Explanation is complete when the regularity between antecedent and consequent is reliable to the point that we inductively understand it to be scientific law.

3. *Necessary versus Accidental Change*

The foregoing positive heuristic features do not in themselves address the problem of change, which is the primary focus of developmental psychology. Rather, they establish the necessary features of general approaches to psychology and provide the basis for considerations of development. A number of theories have, in fact, been constructed on the basis of only these two sets of corollaries and these exhibit little interest in issues of development per se. Chomsky's work is an example of an organismic theory that ignores the issue of change and various contemporary cognitive information-processing approaches (see Lachman *et al.*, 1979) typify nondevelopmental mechanistic approaches.

As already discussed, the organismic model asserts that change and, hence, development are necessary, intrinsic, directional rather than random, and irreversible. What this means as a research approach is that the investigator must define the end or function of change, hence establishing the direction, and then discover (rationally) the organization or rules according to which the change occurs. Notice that the approach here is identical to that taken with respect to a structure-function analysis. But here the focus is upon the organization of change rather than upon momentary organization. Here also the momentary structure-function organizations are the part systems that change in accordance with the rules of the overall organization of change. Piaget's equilibration process, Werner's orthogenetic principle, and Erikson's epigenetic ground plan are all examples of such developmental rules. For Piaget, there is a necessary sequence of structures that proceeds from biological structures to sensorimotor structures to concrete operational structures to formal operational structures, all

in accordance with the rules described by the equilibration process, always moving toward the end of intellectual maturity.

One may again question the speculative nature of having the investigator involved in yet more rational construction of rules. But here, as with the momentary structure-function analysis, the speculation is later empirically assessed by conducting investigations to determine whether the rule of change is a good representation of the changes of the structures of behavior. Both *cross-sectional* and *longitudinal* investigations can be directed toward examining whether the sequence is as universal as, e.g., Piaget claims, and correlational techniques can be employed to assess the changes in behavioral coherence across the sequence.

Experiments can also be conducted within the context of the rationally constructed rule of necessary change. For example, one might do an experiment to determine whether a group of children exposed to a poor educational environment is slowed down in the rate in which changes of organization are shown. Or training studies might select variables under the hypothesis that certain factors will speed up the rate of change. But it is most important to be aware that the *variables* chosen (efficient or material factors) for these or any other *experiments can never be used to explain the change itself* (Overton & Newman, 1982). They explain rate of change, the rule explains the development, and the rule cannot be directly tested by experimentation.

The mechanistic model asserts that fixity is basic and as a consequence change is produced by extrinsic accidental factors. Thus, the mechanistic group would claim that rules of change, like momentary organization, can be reduced to specific behaviors and full explanation is found by observing the antecedent causes that produce behavioral change. Thus, from this perspective, cross-sectional and longitudinal research is purely descriptive whereas the experiment provides the full explanation of change. To find that education has an effect on behavior or that training has an effect explains development from the mechanistic perspective.

4. *Discontinuity-Continuity versus Strict Continuity*

The final positive heuristic features to be considered here (see Overton & Reese, 1973, for others) involve the issue of whether change entails qualitative as well as quantitative features. As a model issue the debate is over whether, with change, later types of organization exhibit novel features that cannot be reduced to earlier organization and ultimately to behavior per se (discontinuity), or whether they are reducible (continuity). As a model issue it is completely irrelevant whether observed behavioral change is gradual or abrupt, small or large (Overton & Reese, 1981; Reese & Overton, 1970).

As already discussed, from the organismic model and its holistic corollary, any momentary organic system has systemic properties that are characteristic of organization as a whole, and not of any of the individual parts. From a psycho-

logical point of view, functions such as representation, symbolization, thinking, and language are all examples of systemic properties of active behavioral systems, just as in our earlier example the function vision was a systemic property of a particular biological system. Furthermore, as just described with respect to organismic change and earlier with respect to Hegel's contribution to this view, less advanced momentary structure-function relationships become transformed and integrated with later ones and, thus, as each new organization progresses further novel systemic properties emerge. The effect of this is that the later levels of organization cannot be *completely* analyzed into or reduced to earlier levels or ultimately to simple behavior. Language, for example, is a system of signs and symbols. Certainly early speech sounds may be significant precursors to later language, but from the organismic perspective, language can never be completely analyzed into these simple elements. Similarly, as discussed earlier, Piaget's later levels of organization, i.e., structures, cannot be completely reduced to the behaviors of infancy any more than a single level can be completely reduced to contemporary specific behaviors. The impossibility of complete reduction across the progressive levels of organization is called *discontinuity*.

Discontinuity entails qualitative change, i.e., the novel systemic properties, but this does not mean that it denies that any quantitative change is possible. Recall that earlier it was stated that from an organismic position analysis of the parts of any system is appropriate as long as it occurs in the context of the whole. Similarly, analysis of the behaviors and systems that are precursors to later levels of organization is appropriate as long as it is explicitly recognized that they are precursors to and not the ultimate elements of the later organization. Thus, for example, from this perspective it is appropriate to analyze the quantitative nature of the movement from early speech sounds to mature language, while recognizing that early speech sounds are precursors to language rather than elements because they do not serve the representational function of language. The organismic position, therefore, maintains that development involves both discontinuity and continuity.

As with other corollaries, discontinuity between levels of organization is translated into any theory constructed within an organismic framework. The specific theoretical concept that is used to demonstrate a model commitment to discontinuous levels of organization is "stage." Stage, then, in any organismic theory refers to a level of organization that exhibits novel or emergent systemic properties that are not completely reducible to specific behaviors. Because "stage" reflects model assumptions it is not in itself open to empirical tests of truth or falsity. If one is working within an organismic framework it is simply meaningless to ask the question, are there really stages of development? This, in fact, is a mechanistic question because it implies stages are things to be observed rather than formal explanatory constructs. However, although "stage" is not open to empirical testing, the specific representation of a stage (i.e., the particular struc-

tural description) that is provided by the theorist can be empirically assessed. That is, the organismically oriented investigator cannot ask, are there really stages, but he or she can ask, are the specific stages such as those that Piaget, Werner, or Erikson describe good representations? The specific procedures for this assessment were presented earlier under the structure-function and necessary change corollaries.

The mechanistic corollary of strict continuity derives from the position described earlier of reductionism and accidental change. Simply stated, the mechanistic position is that all change is to be understood as completely additive. Later, apparent organization is completely reducible to earlier behavior. On occasion the mechanistic group will use the concept "stage" but here, as in other cases, I will present shortly that the term has a different meaning than the one deriving from the organismic framework. For the mechanistic group, "stage" is at best a descriptive device used to give a summary statement about a group of behaviors. Like organization, "stage" is not necessary to explanation, and ultimately "stage" will be eliminated when the organization is fully reduced to behaviors and the effective antecedent causes of the behaviors have been discovered.

In summary to this point, the primary contribution of the Overton-Reese analysis has been to articulate what was earlier implicit. The goal of such an analysis is to demonstrate the continuing influence of these often implicit ontological and methodological commitments on day-to-day research strategies. In doing this, we expected that recognition of the hard cores and positive heuristics of rival programs will provide more coherent and explicitly defined research policies. In addition, by making the implicit explicit, an analysis of this type provides a more well-defined basis for the critical appraisal of the adequacy and progress of rival programs as they go about their task of solving both empirical and conceptual problems.

D. THE MEANING OF BASIC TERMS

A related consequence of the type of analysis conducted by Overton and Reese concerns the clarification of the nature of the meaning of basic terms as they are used in psychology generally, or developmental psychology specifically. Both positivist and conventionalist methodologies assume that there is a neutral observational language independent of any deep-level ontological and methodological commitments. Basic terms are defined either through operational definitions or, in the case of conventionalism, they are often left relatively undefined and simply operate as pigeonholes used to order experimental findings. The difficulties with the positivist's position have been well documented; the difficulty with the conventionalist's position is that in avoiding the issue of meaning it leads to a worst-case scenario of vagueness, ambiguity, and degenerate eclecticism.

Consider, for example, Beilin's analysis (this volume) of early, middle, and

new “functionalism.” The primary basic term employed in this historical analysis is “function.” However, the meaning of this term is left undefined. New functionalists consist of a group “dominated ideologically by information processors at one pole, and by Vygotskyans at the other” (Beilin, this volume, p. 254). Piaget’s theory is included. Middle functionalists included Woodworth and non-Hullian learning theories. “‘Early’ and ‘middle era’ functionalists made much of their rejection of mentalistic language . . . although they never went as far as Watson or Skinner in their positivism” (this volume, p. 254). Beilin, taking a conventionalist perspective, appears to group everyone—excepting avowed positivists—who employs the term “function” under the rubric “functionalists.” The effect of this procedure is to blur significant distinctions in the way the basic term is applied and, thus, to ignore significant distinctions in basic research strategies.

From the perspective of a research program analysis as described by Overton and Reese, the meaning of basic terms is generated to a significant extent by the hard core and positive heuristic of a program. The deep-level hard core and positive heuristic of a program form, as Lakatos and Laudan have pointed out, the conceptual foundation of the program and this foundation generates meanings for the surface-level terms in a field. The point of overwhelming importance here is that in different research programs the same surface-level term may have different meanings and hence different implications for methods and procedures employed to investigate problems (Overton, 1982a). [An example from physics is that basic terms such as “time,” “length,” and “velocity” all change in meaning depending on whether they are interpreted within a classical Newtonian model or a relativity model (Feyerabend, 1971).]

In the earlier section (II,A) on the nature of the mechanistic and organismic research programs, I briefly alluded to the fact that the very meaning of the term “concept” changes depending upon whether it is generated by one or the other program. Here, I will try to demonstrate the point by considering two basic terms that have been widely used in psychology and that play important roles in Beilin’s analysis. These are the terms “structure” and “function.”

For both Wundt and Titchener, “structure” was a basic term in their “structuralist” school of psychology. However—at least in the sphere of experimental psychology—both Wundt and Titchener were committed to working within a mechanistic research program and, as earlier noted, such a commitment entails a further commitment to an elementaristic analysis. As a consequence, “structure” was defined within this system as the elements of consciousness, and a research strategy was employed in an attempt to reduce the complexity of thought to these elements. “Structure” is also a basic term for Piaget and Chomsky. But here, “structure” is defined as the abstract organization or form of knowledge and language. This definition reflects an organismic research program commitment to the assumption of necessary organization, and research strategies, quite incompatible with those of Wundt and Titchener’s, have been

developed to explore the nature of such structures. Without the benefit of a deep-level research program analysis one might easily but improperly conclude that the research approaches of Wundt, Titchener, Piaget, and Chomsky were for all significant purposes identical because each sought to identify "structures."

The situation is similar, if more complex, with respect to the term "function." One meaning of "function" is the natural, proper, or characteristic action of anything, such as an organ of the body (Random House Dictionary of the English Language, 1967). Within the confines of this definition, a reciprocal relationship exists between the thing, e.g., the organ, and its function. Given the organ stomach we can ask about its function, or given the function of digestion we can explore the organ. The relationship here is one of structure and function, and except for purposes of abstraction one cannot be analyzed without reference to the other.

This meaning of "function" is exactly that which is generated by the organismic program, with its hard core commitment to an interrelationship between necessary organization (structure) and activity (function) and with its consequent research policy of requiring that the exploration of structures proceed within the context of their specified functions, i.e., a structure-function analysis (see Table I). This meaning generated by the organismic research program is, in turn, exactly that employed by both Piaget and Chomsky as the one considers the function of intelligence and explores the structures that serve it and the other considers the function of language and explores the structures that serve it. The fact that at one time or another the functional or structural side of the equation is relatively more elaborated does not necessarily lead to difficulty in determining whether Piaget's system is primarily functionalist or structuralist (Beilin, this volume, p. 254). To suggest otherwise misses the point of the program-generated deep-level meaning of these basic terms. Similarly, to ignore Chomsky's consideration of function is to ignore what Papert called "Chomsky's 'organistic' tendency to see mental functions as . . . organized into organs of the mind" (Papert, 1980, p. 92).

A second meaning of "function" drops the characteristic feature of the activity as well as the object and focuses simply on the verb form, to act (Random House Dictionary of the English Language, 1967). If "function" is taken to mean a commitment to activity in general, then one need have no concern with organization or structure. Clearly here, such a meaning would not be generated by an organismic research program. Furthermore, because the commitment to activity is left vague, an ambiguity arises: Is the activity involved to be understood as origin or as outcome?

This vague definition of function best describes the meaning it held for Beilin's (this volume) "middle functionalism" group, including Harvey Carr and Robert S. Woodworth. However, in the views of these investigators, the meaning of the term acquired additional connotations from the mechanistic research program. For both Carr and Woodworth the activity or function was the

product of other forces. That is, deriving from the mechanistic program, the organism was considered inherently stable and activity or function was the product of antecedent stimuli or drives. In fact, in his original statement of the drive concept, Woodworth (1918) was explicit in making this commitment: "The drive is the power applied to making the mechanism go. . . . The mechanism without the power is inactive, dead, lacking in disposable energy" (p. 37). Rychlak (1977) provided a more elaborate historical analysis of the manner in which this brand of functionalism, as well as later mediational theorists, continued to demonstrate mechanistic hard core commitments to a Lockean-Humean set of philosophical assumptions. The fact of significance here, however, is that again the meaning of the basic term is generated by the research program. Ignoring this deep-level analysis of meaning and grouping according to the surface-level labels has the effect of tearing apart the integrative coherence of any general research program, and leads to conceptual and methodological confusion.

A third meaning of "function" refers to a factor related to or dependent on other factors. This meaning has been applied by those favoring an operant or experimental analysis of behavior approach (e.g., Bijou & Baer, 1963; Gewirtz, 1969; Skinner, 1974) as a method of analyzing antecedent-consequent relationships without recourse to causal statements. Thus, proposals from this group for a functional analysis of behavior reflect a mechanistic research program commitment to an analysis of antecedents and consequences.

III. World Views and Scientific Research Programs

In the foregoing general discussion I have attempted to highlight the fact that the philosophical progression from Kuhn to Lakatos and Laudan establishes a rational context for an elaboration of the early Overton-Reese analysis of the role of world views as they impact on scientific activity in psychology. As both Lakatos and Laudan argued, world views can and do form a necessary and rational dimension to scientific research programs and traditions. From this perspective, they are not, as Beilin and other conventionalists would maintain, merely an added "psychological and sociological dimension" (Beilin, this volume, p. 256).

A. WORLD VIEWS IN SCIENCE

Several other issues concerning the role of world views as they enter scientific research programs and traditions require clarification, particularly as they relate to my understanding of the impact of world views on psychology. A first issue concerns the question of whether the organismic and mechanistic world views

provide the only possible basis for research programs in psychology. The answer here is simply no. Both Overton and Reese have maintained that these world views appear to have had the greatest impact on psychology, but it was never asserted that these are the only possible sources of research programs. However, the burden of proof in establishing alternative research programs lies with their proponents. To simply assert that it appears that some part of some investigator's work is more formistic or contextualistic than mechanistic or organismic does not satisfy the demands of this task. The task necessarily requires the explicit articulation of a coherent set of assumptions forming a hard core; the demonstration that the hard core influences the formulation of an explicit research policy or positive heuristic; evidence that the hard core and positive heuristic influence the formulation of a family of theories; and an analysis of the ways in which the hard core and positive heuristic have a determining effect on the meaning of key terms in a field and on day-to-day research strategies.

A closely related issue involves the question of whether any of the *currently available* world views, aside from organicism and mechanism, can, in fact, form the basis for a *scientifically* viable research program. This is a complex and arguable issue, but for myself and not speaking for Reese, I would again submit that the answer is no. Consider first the very nature of science. Although significant disagreements have occurred and do exist about the specific nature of scientific activity, agreement has been uniform that a central aim of science is to establish an "organized or systematic body of knowledge" (Wartofsky, 1968, p. 23). That is, science aims at introducing order and organization into the vagueness and ambiguity of common sense perceptions and understandings. Any viable approach to science must therefore provide some means of *integration* of disparate and seemingly divergent data sources. For positivists and conventionalists the means of integration consist of careful observation, inductions, and ever simpler pigeonhole systems. From a rationalist perspective, in contrast, the primary vehicle for integration resides in the deep-level assumptions of the research program or research tradition's hard core and positive heuristic.

Any scientifically viable research program, then, must include the means for achieving integration as a part of its basic assumptions. To the extent that a research program is based upon or incorporates the assumptions of a world view into its hard core, *the world view must be integrative in nature*. But here is exactly where the problem arises. For of the currently available world views, only organicism and mechanism are integrative; others, in fact, reject the idea of integration.

If we take as the class of currently available world views those that have been relatively well defined and articulated, then this class is composed of formism, contextualism, mechanism, and organicism (Pepper, 1942). But as Pepper pointed out, only mechanism and organicism are integrative in nature; formism and contextualism are dispersive. "That is to say, the categories of formism and contextualism are such that . . . facts are taken one by one from whatever source

they come and are so left. The universe has for these theories the general effect of facts rather loosely scattered about. . . . The cosmos for these theories is not in the end highly systematic" (Pepper, 1942, pp. 142–143). And specifically with reference to contextualism, "disorder is a categorical feature of contextualism" (p. 234). Thus, for whatever else their value, formism and contextualism as world views cannot form the basis for scientifically viable research programs—unless science were to abandon its attempt to establish an organized and systematic body of knowledge, which is unlikely. Therefore, although mechanism and organism do not form the only possible basis for scientific research programs, they do seem from my point of view to be the only *currently available* world views to serve this purpose.

B. CONTEXTUALISM AND DIALECTICS

Because the proposal has been made (e.g., Lerner, 1982; Moshman, 1982; Reese, 1976; Riegel, 1976, 1979; Sigel, 1981) that, for purposes of understanding development, contextualism may be a viable alternative to organicism, a few additional points should be made about this option. Two underlying assumptions appear to be implicated in making such a suggestion. The first assumption is that change must be understood as a dialectic process and such an understanding requires contextualism. Although I have agreed, and indeed have argued that development is a dialectic process (e.g., see Overton & Reese, 1981, pp. 108–109), the plain and simple fact of the matter is that the dialectic is a category of organicism. As Pepper specifically stated, "The early organicists, notably Hegel, thought that there was one and only one course of progress from maximum fragmentariness to ultimate integration. . . . Thesis-antithesis-synthesis is the ever recurring form" (Pepper, 1942, p. 293). Furthermore, as Pepper also clearly stated, although later organicists rejected the rigid picture of a single path of development, they maintained the central category of the dialectic (pp. 294–295). Therefore, any assumption that dialectic change requires contextualism is mistaken.

The second assumption underlying the use of a contextualist world view, with respect to the understanding of development, begins with the recognition that change is a category of contextualism. This recognition then seems to lead to the assumption that change is not a category of organicism. However, the assumption that change is not a category of organicism is based on a subtle misinterpretation of organicism. Organicism, as a world view, is composed of two sets of categories. The first of these consists of the progressive categories in which events proceed from fragments through a dialectic process *toward* an ultimate organic whole. Quite clearly, this set of progressive categories constitutes the change or developmental feature of the system. In contrast, the second set of categories describes the ideal absolute state that would occur if all contradictions were actually resolved and the dialectic ceased. Here, indeed, change or devel-

opment would cease. In modern interpretations, the ideal or the absolute is an end toward which events progress, it is not an end achieved. Only if an interpretation is made that the absolute is an end achieved, can organicism be viewed as a nonchange or nondevelopmental position.

Beyond these two flawed reasons for proposing contextualism as an alternative to organicism, a certain ambiguity exists with respect to contextualism that seems to heighten its appeal to many. That is, as discussed by Pepper, contextualism consistently shows a tendency to lose its identity and to become a part of mechanism or of organicism. On the one hand, when contextualism combines with mechanism the concept "context" simply functions as yet another antecedent variable in the sense of asking the question, "How did the situation or context affect the behavior"? Thus, by adopting the facade of contextualism, mechanism gains the appearance of greater scope while maintaining the reality of virtually no basic change in assumptions.

On the other hand, as Pepper stated, "contextualism and organicism are so nearly allied that they may almost be called the same theory, the one with a dispersive, the other with an integrative plan" (p. 147). When contextualism combines with organicism, the integrative plan takes precedence and the category "context," as well as other contextualist categories, serve to specify and articulate the nature of the organic whole or holism. Here, "context" is not one antecedent event among others; "context" is the organic functioning whole that gives meaning to its parts and that can never be analyzed into simply the sum of its parts. From this interpretative framework I have myself had occasion to discuss contextualist assumptions (see Overton & Reese, 1981).

It should be emphatically stressed that taking advantage of contextualism's identity problems by both mechanists and organicists does not in any sense lead to a synthesis of the three positions. In fact, taking advantage of contextualism's identity problems has a strong tendency to lead to conceptual confusion because doing this usually leaves unclear whether an author is making an argument from a mechanistic-contextualistic viewpoint or from an organismic-contextualistic position. If, as Laudan argued, one of the major aims of science is to resolve conceptual problems as well as empirical problems, this value would be enhanced if investigators would drop the surface-level concept "contextualism"—given that, as argued earlier, such a position as a world view cannot serve as the basis for a scientific research program—and explicitly acknowledge when they mean mechanistic-contextualistic and when they mean organismic-contextualistic.

C. RESEARCH PROGRAMS AND FAMILIES OF THEORIES

The next general issue involves a problem of the relationship of the ontological and methodological commitments of a scientific research program and the family of theories entailed by the program. More specifically, the question concerns the

extent and manner in which theories can disagree among themselves and still constitute a family of theories. A part of the answer to this question was already given in an earlier quote from Laudan: "to attempt what is forbidden by the metaphysics and methodology of a research tradition [research program] is to put oneself outside that tradition and to repudiate it" (1977, p. 80). That is, to the extent that theories are formulated in a manner that is consistent with or, at a minimum, does not violate the strictures of the hard core and positive heuristic of a research program, they constitute a family of theories regardless of other mutual inconsistencies (Laudan, 1977, p. 81).

Because Overton and Reese have maintained that Piaget's and Chomsky's theories are two members of the organismic family and Beilin (this volume) argued that each implicates a different research program, these theories will be used as illustrative. First, both Piaget and Chomsky explicitly accept and incorporate the organismic hard core assumptions of the integrated relationship of necessary organization and activity. As Piattelli-Palmarini pointed out with respect to the debate between Piaget and Chomsky, both accept the assumptions that: "(1) nothing is knowable unless cognitive organization of some kind is there from the start; and (2) nothing is knowable unless the subject acts in one way or another on the surrounding world" (1980, p. 54). Furthermore, each theory also incorporates the organismic positive heuristic features of holism and structure-function analysis and places primary emphasis on formal explanation.

Here then, one has every reason to assert and none to deny that Piaget and Chomsky's theories constitute two members of the organismic family of theories. The primary conflict between Piaget and Chomsky concerns the specific nature and extent of the initial organization or fixed nucleus and not the hard core assumption of organization itself. Although Piaget's constructivist position and Chomsky's innateness position constitute a significant theoretical debate (Piattelli-Palmarini, 1980) with respect to the nature and extent of the organization, they do not implicate different scientific research programs. In fact, by the end of the debate, several potential compromises, each of which was compatible with an organismic research program, were offered and developed by participants in the debate (e.g., Cellier, 1980; Changeux, 1980).

Another feature of the relationship between Piaget and Chomsky's theories as they implicate the organismic research tradition concerns necessary change. The hard core and positive heuristic components of this feature are strongly emphasized within Piaget's theory but they are relatively undefined within Chomsky's. In considering this feature of necessary change, note first that Chomsky did not reject it. That is, Chomsky did not violate the strictures of the organismic program and put himself outside this tradition by, for example, asserting that all change is ultimately a product of direct experience (mechanistic commitment). Rather, this feature is simply not extensively articulated in the theory.

One might deal with Chomsky's failure to articulate necessary change in his

theory by simply noting that Chomsky is not very interested in development. Or it might be said that Chomsky's theory, like Gestalt theory, or George Kelly's personality theory, represents a kind of truncated organicism. However, although such statements are not incorrect, they miss an important component of the nature of scientific research programs, i.e., the manner in which empirical problems serve to initiate an elaboration of a program's positive heuristic.

Scientific research programs function to solve empirical problems. But empirical problems are not all of one sort; they vary in both their nature and generality. Thus, when an investigator confronts a particular empirical domain, the features of the positive heuristic of a program will be differentially elaborated depending upon the kinds of empirical problems being addressed. The elaboration of the positive heuristic depends to a large extent upon the problems or puzzles the investigator is confronted with *at any particular time* with respect to his or her field of investigation (Overton, 1982b).

From this perspective one can see how Piaget, in focusing on the intellectual domain and being faced with seemingly major transformations that occur in this domain between infancy and adulthood, was led to elaborate the necessary change feature of the organismic positive heuristic as well as the holism and structure–function features. Chomsky, however, in focusing on the domain of language, was faced with the relatively early sudden expression of universal components of syntax. As a consequence, the empirical problems that this issue posed required an elaboration of the holistic and structure–function features, but not the necessary change feature of the organismic program's positive heuristic. This difference in the elaboration of the positive heuristic does not, however, mean that Piaget and Chomsky pursue rival research programs. Different puzzles or problems facing different investigators will lead to the differential elaboration of the various features of the positive heuristic within any family of theories.

D. INCOMMENSURABILITY

A final general issue is that of incommensurability. In the original Reese and Overton (1970) paper we accepted Pepper and Kuhn's position on this issue and maintained that "different world hypotheses cannot be compared evaluatively with one another, because of the basic lack of communication. . . . Only internal evaluation is possible" (p. 122). At this point I would reject this radical form of the incommensurability thesis and agree with a thesis that is more compatible with the arguments by Lakatos and Laudan. The primary problem with Pepper and Kuhn's position is that, if accepted without qualification, then no rational standard of scientific progress is possible. That is, if *only* internal evaluation is possible, scientific acceptability and scientific progress can be defined only in terms of psychological and sociological criteria (Lakatos, 1978b, elitism criteria). In this context "rival" research programs or traditions are not even rivals

and the choice of one or another program is determined by psychological and sociological factors that influence one or another group of investigators.

Laudan proposed that the scientific progressiveness (or regressiveness) of a specific research program or tradition be characterized in terms of whether, in time, it has expanded its domain of explained problems, and has minimized the number and importance of its conceptual problems and anomalies. This rational criterion then allows for a progressiveness ranking among rival research programs or traditions at any given time. Thus, it is possible "to be able to compare the progressiveness of different research traditions [programs], *even if those research traditions are utterly incommensurable in terms of the substantive claims they make about the world!*" (Laudan, 1977, p. 146).

The revised form of the incommensurability thesis is consistent with the evolution of my own understanding of rational standards of scientific acceptability and scientific progress and the nature of rival scientific programs. However, it still leaves the important issue of "incommensurability" unresolved. Incommensurability involves both issues of standards of evaluation and issues of the lack of communication between rival world views and their constituent research programs and theories (Kuhn, 1970, pp. 148–150). The problem originally arose from the rationalist rejection of the idea of the existence of a neutral observational language and the acceptance of the position that every theory has its own observational language. Thus, to the extent that a research program or tradition "is a set of general assumptions about the entities and processes in a domain of study" (Laudan, 1977, p. 81) and to the extent that these assumptions influence the definition of basic terms, rival programs do exhibit incommensurability.

However, the incommensurability argument has both a strong and weak form. The strong form is that virtually all features of the observational language are determined at all times by each research program. If this were the case, communication would indeed be impossible. The weaker argument, although accepting that no pure observations are possible, begins from a position that a wide variation exists in the extent to which a specific observation is dependent on any given program at any given time. For example, the observation "that is a human being" usually entails little specific dependence on any given psychological theory (note, however, that this is not the case in paleoanthropology). In contrast, the observation "that is a concept" or "that is a psychological structure" entails maximum dependence on psychological theories. From this perspective, when two programs are at a point of incommensurability (e.g., "What is a concept, a symbol, a representation, a structure?") the proponents can withdraw, not to a pure observational language but to an observational language whose theoretical assumptions are not immediately at issue (Barbour, 1974).

The weaker incommensurability argument best characterizes my own contemporary position. That is, at any given time an incommensurability may, in fact, exist between the organismic and mechanistic research programs with respect to

the meaning of “development,” “structure,” “function,” “representation,” “intelligence,” “language,” “symbol,” “concept,” “attachment,” etc. This component of incommensurability is eliminated by appealing to a *currently* more neutral language. Thus, for example, using this technique facilitated recognition that the mechanistic program led Wundt and Titchener to understand structure as “elements of consciousness” and that the organismic program led Piaget and Chomsky to understand structure as “abstract forms of knowing.” Here, communication has been restored and incommensurability eliminated. However, and this is a point of central importance, the elimination of incommensurability in turn makes salient the incompatible and competitive nature of the rival research programs (see Lakatos, 1978a, p. 91).

IV. An Organismic–Mechanistic Compromise?

In this paper I have tried to demonstrate ways in which my own understanding concerning the functioning of rival scientific research programs in psychology has evolved along lines that are consistent with the philosophical evolution from Kuhn to Lakatos and Laudan. In closing, I would like to address very briefly a question that is often asked with respect to the rival mechanistic and organismic programs: Is a compromise possible? For myself, I tend to mistrust attempts at such compromise. The reason is that virtually all potential compromises destroy the core integrity of one or the other of the programs and, as Laudan pointed out, “it is precisely that integrity which stimulates, defines, and delimits what can count as a solution to many of the most important scientific problems” (1977, p. 80).

Having made this disclaimer, I will briefly sketch my own version of a “compromise.” In essence, it is a “compromise” that leaves the organismic program intact and destroys the integrity of the mechanistic program. As a starting point for this compromise, consider the fact that in the past investigators have tended to approach the explanation of behavior and development from two extremes of a bipolar dimension. One group—organismic in nature—being impressed with the constancy and universality of behavior and development, has offered formal explanations in terms of universal structures (both synchronic and diachronic) and has treated contingent explanation as relatively unimportant. The second group—mechanistic in nature—being impressed with the variability and individuality of behavior and development, has proposed that universal structures are, at best, temporary heuristics and that ultimately all phenomena will be explained by contingent explanation. The proposed compromise is one that requires that the organismic group maintain its focus on formal explanation but increase its emphasis on contingent explanation (easily done because the organismic program admits both types). It also requires that the mechanistic group

abandon its hard core and its positive heuristic features of elementarism and strict continuity. The compromise leaves the mechanistic group to focus its techniques and research strategies on contingent explanation but always in the context of organismic structures and stages of structures, i.e., contingent factors explain rate of behavior and rate of change (see Table I) (see also Moshman, 1982) and deviations from the normal course of development (Overton, 1982a; Overton & Newman, 1982; Overton & Reese, 1981).

I have elaborated this compromise in other contexts where I have referred to it as a competence-activation-utilization approach or a competence-moderator-performance approach (Overton, 1982b; Overton & Newman, 1982). In essence it elevates formal and contingent explanation to a more equal partnership in which each plays a distinct and mutually supportive role in the general explanatory effort. Formal explanation operating within the hard core and positive heuristic of the organismic program yields the abstract idealized models of competence and the development of competence. It provides the laws of the universal, the regular, the normal, the necessary. Contingent explanation focuses on moderators and provides answers to questions of how competence and the development of competence is accelerated, retarded, or deflected from its normal course; how the activation or utilization of competence is facilitated or hindered by specific psychological processes and situational factors.

With the "compromise," theories such as Piaget's, Werner's, or Chomsky's could be wed to truncated forms of theories such as Skinner's or Bandura's, i.e., truncated in the sense that they are shorn of mechanistic hard core assumptions and elementaristic and strict continuity features. Here, for example, contingencies of reinforcement might well account for why one group of formal operational adolescent children (competence) utilizes this knowledge in situations whereas another group does not. Or, given that the nature of imitation is limited by cognitive level, Bandura's emphasis on the status of the model could account for differential amounts of imitation within any level. Similarly, given that a theory like Piaget's equilibration model (a diachronic competence) would account for the normal course of development, various deprivation or enrichment theories could explain the retardation or acceleration of this normal course. It is, of course, questionable whether this general proposal is in fact a compromise or simply a further elaboration of the organismic positive heuristic. However, when viewed as an elaboration it offers the added advantage, noted by Laudan (1977, p. 94), of suggesting ways in which modified theories can be taken over by an alternative research program or research tradition.

V. Conclusions

In summary, then, my own views with respect to the early Overton-Reese analysis have evolved in ways that are consistent with the philosophical evolu-

tion from Kuhn to Lakatos and Laudan. This evolution has served to highlight, extend, and modify a number of points. Today the mechanistic and organismic positions are best interpreted as scientific research programs or traditions including hard cores, positive heuristics, and families of theories. These programs bring metaphysical and methodological commitments into the essential body of science and in so doing they exert a determining influence on theories, basic terms, and research strategies. Of the currently available world views, only mechanism and organicism seem adequate to form constituent parts of scientific research programs. The Lakatos and Laudan analyses provide a more adequate interpretation for the rival relationship between the mechanistic and organismic programs. These analyses also provide a more adequate rational explanation for comparing these programs in the context of scientific progress. Compromises between the two programs must be suspect because, in fact, they tend to destroy the integrity of one or the other program.

REFERENCES

- Baer, D. The organism as host. *Human Development*, 1976, **19**, 87–98.
- Barbour, I. G. *Myths, models and paradigms*. New York: Harper, 1974.
- Bijou, S. W., & Baer, D. M. A systematic and empirical theory. In *Child development* (Vol. 1). New York: Appleton, 1963.
- Cellerier, G. Cognitive strategies in problem solving. In M. Piatelli-Palmarini (Ed.), *Language and learning: The debate between Jean Piaget and Noam Chomsky*. Cambridge, Massachusetts: Harvard Univ. Press, 1980.
- Changeux, J. Genetic determinism and epigenesis of the neuronal network: Is there a biological compromise between Chomsky and Piaget? In M. Piatelli-Palmarini (Ed.), *Language and learning: The debate between Jean Piaget and Noam Chomsky*. Cambridge, Massachusetts: Harvard Univ. Press, 1980.
- Chomsky, N. *Reflection on language*. New York: Pantheon, 1975.
- Feyerabend, P. K. Problems of empiricism, part 2. In R. Colodny (Ed.), *The nature and function of scientific theory*. Pittsburgh, Pennsylvania: Univ. of Pittsburgh Press, 1971.
- Gewirtz, J. Levels of conceptual analysis in environment-infant interaction research. *Merrill-Palmer Quarterly*, 1969, **15**, 7–47.
- Hanson, N. R. *Patterns of discovery*. London and New York: Cambridge Univ. Press, 1958.
- Heisenberg, W. *Physics and philosophy: The revolution in modern science*. New York: Harper, 1958.
- Kuhn, T. S. *The structure of scientific revolutions*. Chicago, Illinois: Univ. of Chicago Press, 1962.
- Kuhn, T. S. *The structure of scientific revolutions* (2nd ed., enlarged). Chicago, Illinois: Univ. of Chicago Press, 1970.
- Kuhn, T. S. *The essential tension*. Chicago, Illinois: Univ. of Chicago Press, 1977.
- Lachman, R., Lachman, J. L., & Butterfield, E. C. *Cognitive psychology and information processing*. Hillsdale, New Jersey: Erlbaum, 1979.
- Lakatos, I. *The methodology of scientific research programmes: Philosophical papers* (Vol. 1). London and New York: Cambridge Univ. Press, 1978. (a)
- Lakatos, I. *Mathematics, science and epistemology: Philosophical papers* (Vol. 2). London and New York: Cambridge Univ. Press, 1978. (b)
- Laudan, L. *Progress and its problems: Towards a theory of scientific growth*. Berkeley, California: Univ. of California Press, 1977.

- Lerner, R. M. *Individual and context in developmental psychology: Conceptual and theoretical issues*. Papers presented at a conference on individual development and social change, Max-Planck-Institut für Bildungsforschung, Berlin, 1982.
- Moshman, D. Exogenous, endogenous and dialectical constructivism. *Developmental Review*, 1982, **2**, 371-384.
- Nickles, T. Introductory essay: Scientific discovery and the future of philosophy of science. In T. Nickles (Ed.), *Scientific discovery, logic and rationality*. Boston, Massachusetts: Reidel, 1980.
- Overton, W. The active organism in structuralism. *Human Development*, 1976, **19**, 71-86.
- Overton, W. F. *Historical and contemporary perspectives of development*. Unpublished manuscript, 1982. (a)
- Overton, W. F. *Scientific methodologies and the competence-moderator-performance issue*. Invited address presented at the annual symposium of the Jean Piaget Society, Philadelphia, 1982. (b)
- Overton, W. F., & Newman, J. Cognitive development: A competence-activation/utilization approach. In T. Field, A. Houston, H. Quay, L. Troll, & G. Finley (Eds.), *Review of human development*. New York: Wiley, 1982.
- Overton, W. F., & Reese, H. W. Models of development: Methodological implications. In J. R. Nesselrode & H. W. Reese (Eds.), *Life-span developmental psychology: Methodological issues*. New York: Academic Press, 1973.
- Overton, W., & Reese, H. Conceptual prerequisites for an understanding of stability-change and continuity-discontinuity. *International Journal of Behavioral Development*, 1981, **4**, 99-123.
- Papert, S. The role of artificial intelligence in psychology. In M. Piatelli-Palmarini (Ed.), *Language and learning: The debate between Jean Piaget and Noam Chomsky*. Cambridge, Massachusetts: Harvard Univ. Press, 1980.
- Pepper, S. *World hypotheses*. Berkeley, California: Univ. of California Press, 1942.
- Piaget, J. *Structuralism*. New York: Basic Books, 1974.
- Piatelli-Palmarini, M. (Ed.). *Language and learning: The debate between Jean Piaget and Noam Chomsky*. Cambridge, Massachusetts: Harvard Univ. Press, 1980.
- Popper, K. R. *The logic of scientific discovery*. London: Hutchinson, 1959.
- Reese, H. W. Memory development in childhood: Life-span perspectives. In H. W. Reese (Ed.), *Advances in child development and behavior* (Vol. 11). New York: Academic Press, 1976.
- Reese, H. W., & Overton, W. R. Models of development and theories of development. In L. R. Goulet & P. B. Baltes (Eds.), *Life-span developmental psychology: Research and theory*. New York: Academic Press, 1970.
- Reichenbach, H. *Experience and prediction*. Chicago, Illinois: Univ. of Chicago Press, 1938.
- Riegel, K. F. The dialectics of human development. *American Psychologist*, 1976, **31**, 689-700.
- Riegel, K. F. *Psychology mon amour*. Boston, Massachusetts: Houghton-Mifflin, 1979.
- Rychlak, J. *The psychology of rigorous humanism*. New York: Wiley, 1977.
- Sigel, I. E. Child development research in learning and cognition in the 1980s: Continuities and discontinuities from the 1970s. *Merrill-Palmer Quarterly*, 1981, **27**, 347-371.
- Skinner, B. F. *About behaviorism*. New York: Knopf, 1974.
- Wartofsky, M. *Conceptual foundations of scientific thought*. New York: Macmillan, 1968.
- Woodworth, R. S. *Dynamic psychology*. New York: Columbia Univ. Press, 1918.