SOME DARWINIAN LESSONS FOR BEHAVIOR ANALYSIS:
A REVIEW OF BOWLER'S *THE ECLIPSE OF DARWINISM*¹

A. CHARLES CATANIA

UNIVERSITY COLLEGE OF NORTH WALES

Even biologists who were well disposed toward the selection mechanism found it necessary to pause and take stock of the situation. They admitted that there was considerable disagreement over the future course of biology. . . . Those who were opposed to the selection mechanism had no doubt about the overall trend. Darwinism was on the decline and would soon be eliminated altogether as a major evolutionary theory. This confidence is best illustrated by the title of a German work as translated into English, Eberhart Dennert's *At the Deathbed of Darwinism* (1903). The translation was American, symbolizing the fact that here flourished the most vocal school of neo-Lamarckism, one determined to show that selection was at best only a secondary force in evolution. Yet these reports of the death of Darwinism were exaggerated. (p. 4)¹

To the behavior analyst who has heard or read reports of the imminent demise of behaviorism (e.g., as in Kosslyn, 1983), the preceding passage may strike a disturbingly familiar note. But given the status of behavior analysis in contemporary psychology, we should be reassured to discover that the secure position of selectionism within contemporary biology is a relatively recent development. The present review explores some of the parallels between natural selection and behavior analysis suggested by the eclipse of Darwinism.

Our first guess might be that a treatment of the Darwinian eclipse would be primarily concerned with the long-standing conflict between Darwinism and its creationist opposition. In recent years this conflict seems to have grown rather than diminished; although it is usually conducted outside the standard academic and intellectual arenas, it is presumably familiar to most researchers in the behavioral sciences. The guess, however, is wrong. It is less well known that over several decades in the late 19th and early 20th centuries the Darwinian revolution faced serious challenges from within biology itself.

Looking back from the 1940s in his classic survey of the modern synthesis, Julian Huxley expressed the belief that Darwinism had emerged from a crisis. He coined the term "eclipse of Darwinism" to describe the situation at the turn of the century when many biologists had turned their backs on selection. Although the creators of the modern synthesis were aware that they were rescuing Darwinism from a crisis, there seems widespread ignorance of this fact outside the scientific community. (p. 5)

This is the history addressed by Bowler in *The Eclipse of Darwinism* (1983). To the extent that it parallels events in the history of behavior analysis, it merits our attention.

¹ Bowler, Peter J. (1983). *The eclipse of Darwinism: Anti-Darwinian evolution theories in the decades around 1900*. Baltimore: The Johns Hopkins University Press. xi + 291 pp., including bibliography and index. All page references without citations are to this book.

Preparation of this review was supported in part by NSF Grant BNS86-07517 to the University of Maryland Baltimore County, by the James McKeen Cattell Fund, and by a Fulbright Fellowship. I thank C. Fergus Lowe for some resources provided by the University College of North Wales. Philip N. Hineline served as Review Editor in the editorial processing of the manuscript. For reprints, write the author at the Department of Psychology, University of Maryland Baltimore County, 5401 Wilkens Avenue, Catonsville, Maryland 21228 USA.

SELECTIONISM AND ITS ALTERNATIVES

Natural selection here refers to Darwin's account of evolution in terms of the differential survival and reproduction of the varied members of a population; the environment selects the individuals that pass their characteristics on from one generation to the next and thereby shapes the characteristics of individuals in subsequent populations. (A discussion of the technical details and subtleties of natural se-

lection is beyond the scope of this review, but see Dawkins, 1986, for a persuasive introduction.)

Darwin's account relatively quickly supplanted that of theistic evolution, which differed from creationism in that it did not deny evolution itself but which posited variation directed by a Creator's will and thus put evolution outside the scope of scientific investigation.

By the end of the century, theistic, or designed, evolution was no longer under serious consideration by the scientific community. At least one central principle of Darwinism had been universally accepted: that evolution was to be explained solely in naturalistic terms, leaving no room for the supernatural. (p. 15)

The major intellectual threats to natural selection came from other quarters. "The fact of evolution was no longer in dispute" (p. 3). Darwin's mechanism of natural selection, however, was another matter.

Bowler identifies the main alternatives to Darwinism as Lamarckism, orthogenesis, and the combination of Mendelian genetics with mutation theory. Lamarckism, an evolutionary theory older than Darwin's, assumed that characteristics acquired during an organism's lifetime could be inherited; use-inheritance was one variation of Lamarckism. Orthogenesis provided an account in which evolution was directed by forces within organisms, without reference to the demands of the environment; evolution could be likened to a developmental unfolding, one manifestation of which was supposed to be the purported recapitulation of phylogeny by ontogeny. Mendelian genetics by itself provided no mechanism for variation, and it therefore appropriated mutation theory, which held that evolution proceeded by sudden appearances of new forms, to denote spontaneous changes in genes; in modern accounts, mutations introduce into a population the variability upon which selection operates, but they were originally treated as a more direct source of new populations.

These then were the alternative accounts of evolution that competed most effectively with that of natural selection. Bowler organizes his discussion of their interactions with Darwinism around three questions that each alternative addressed either explicitly

or implicitly (pp. 8-9). One is whether evolution is orderly, so that forms advance through regular patterns of development, or irregular, so that forms separate into ever diversifying branches. Another question is whether evolution is continuous, with minute changes accumulating in each generation, or discontinuous, involving production of totally new forms. These two questions capture separate concerns that can also be identified in early debates over insight versus trial-and-error learning: whether behavior is goal-directed or selected by its consequences and whether changes in behavior are all-or-none or gradual (Köhler, 1927; Thorndike, 1911).

These questions are undoubtedly of interest. The remaining question, however, is the one that should most readily catch the eye of the behavior analyst: Is evolution controlled by environmental demands or by forces internal to organisms? The issue is critical to the Darwinian account. "Darwinism is a theory of external control, since the internal process of variation is seen as purely random and incapable of directing evolution" (p. 9). The "pure randomness" may require qualification (for example, the probability of mutation may vary with such factors as the location of a gene on a chromosome). But the point remains that the initiating causes of evolution lie in the environment in the Darwinian account, just as the initiating causes of behavior lie in the environment in the Skinnerian one.

THE ECLIPSE

A few excerpts may provide the flavor of Bowler's account while briefly outlining some of the relevant history:

Since it was Darwin who converted the scientific world to evolutionism, his theory had a head start in developing its most convincing applications. His opponents took time to develop their alternatives into full-scale rivals. They were also able to exploit the frustrations that built up as it became increasingly obvious that Darwinism could not overcome certain crucial objections. . . . Opinions gradually polarized to such an extent that mechanisms originally accepted as additions to selection were in the end treated as alternatives to it. (p. 13)

On the basis of both research and theory, Weismann (1880-1882) excluded Lamarck-

ism from a role in evolution. Although his theory later proved flawed, the exclusion had important consequences:

The original, flexible Darwinism had explicitly allowed for a Lamarckian component in addition to natural selection. Weismann's more dogmatic selectionism became known as "neo-Darwinism," and it rapidly polarized the scientific community into two mutually hostile camps. . . . Those who harbored doubts about the total efficacy of selection, or who were explicitly committed to Lamarckism, were now forced to take up a position against Weismann and hence against Darwinism. . . . [Thus] lukewarm supporters of the theory were turned into active opponents. (pp. 41-42)

This was the context in which Lamarckism and orthogenesis emerged as significant rivals of selectionism. Lamarckism was originally justified by indirect arguments:

The paucity of experimental evidence was not considered crucial. To the Darwinians, of course, the weakness of the Lamarckian arguments was obvious from the start, but their objections were at first overshadowed by the wave of support for the new theory. Only after 1900 did the search for experimental evidence become crucial in the eyes of most observers, to the detriment of the Lamarckian position. (p. 75)

Because acquired characters that were not originally heritable had to become heritable in the Lamarckian account, the criterion for heredity became a problem: "[The] chief criterion for judging if a character had become truly hereditary was whether or not it bred true according to Mendel's laws" (p. 95). The weaknesses of the Lamarckians' position had begun to tell. "After 1900 they even tried to claim that their newly acquired characters would obey Mendel's laws. . . . Once they began to use their opponents' terminology to discuss heredity, it was inevitable that their theory would not survive" (pp. 76-77).

In the view of the supporters of orthogenesis,

Darwinism overstressed the extent to which the environment dominated the life of the individual and hence the course of evolution. . . . acceptance of the widespread existence of nonadaptive characters was simply a reaction against the blanket utilitarianism of Darwin's theory . . . [and] against the Darwinians' ten-

dency to invent utilitarian explanations of characters they did not understand. . . . A generation of naturalists now decided to try out the possibility of explaining such characters in terms of purely biological forces. . . . The remaining Darwinians wondered why so many naturalists were determined to repudiate one of Darwin's key insights. The danger of postulating developments independent of the environment was that all too easily they could take on a purpose of their own and could lead to a reintroduction of teleology. (p. 145)

Unlike Mendelian genetics, Lamarckism and orthogenesis were outgrowths of a philosophy that

rejected the fundamental insights of Darwinism by transferring the driving force of evolution to an internal level where it partook of the teleological nature of individual growth. Had the experimental movement not been contaminated by extreme anti-Darwinian sentiments derived from this more traditionally oriented philosophy, the emergence of modern genetics might have been accompanied by a far less obvious repudiation of selection. (p. 219)

The issues were further complicated by the increasing specialization within biology: "Once again we see the lack of contact between the field and the experimental biologists, with the former making an important consolidation of the Darwinian position just when the latter were rejecting selectionism altogether" (p. 33).

The combination of Mendelian genetics with mutation theory was initially seen as a threat to Darwinism because the size and frequency of mutations was at first so overestimated that selection seemed not to be a prerequisite for the creation of new species (p. 198):

At first genetics and the mutation theory appeared to be contributing to the eclipse. . . . In part, Darwinism was saved from extinction by the fact that the new science of heredity was just as implacably opposed to the other alternatives. (p. 183)

As it turned out, the split that did occur was mended within a few decades, while the rise of genetics spelled the end of the recapitulation theory and the mechanisms of evolution based upon it. (p. 219)

In the end, orthogenesis and Lamarckism failed simply because they could not deal with the facts of evolution.

PHYLOGENIC AND ONTOGENIC PARALLELS

This review takes for granted the parallels between natural selection and the selection of behavior by its consequences (as a corollary, of course, it also assumes that the reference science for behavior analysis is biology, not physics). The common properties of the two kinds of selection have been explored in considerable detail (e.g., Catania, 1978, 1983; Skinner, 1953, p. 430; Skinner, 1981; Smith, 1986), and they are not limited merely to the similarity between evolution and shaping. For example, there exist phenomena corresponding to stimulus control and to extinction in both domains, as Skinner has pointed out in response to a critic:

He seems to suggest . . . that nothing in natural selection corresponds to stimulus control in operant conditioning. But if the long neck of the giraffe, to use an outworn example, was selected in terrains in which there was an advantage in being able to eat leaves high on trees, the trait is adaptive only when tall trees are available. [He] also suggests that there is nothing in natural selection corresponding to extinction, but I should have supposed that the legs of the whale would qualify as an example. (Skinner, 1984a, p. 508)

One feature of Bowler's account that recommends it to the behavior analyst is that the reader will enjoy identifying possible analogues of Skinnerian or behavior-analytic themes in the Darwinian account and its history. Here are a few examples.

"To the Darwinians, of course, the weakness of the Lamarckian arguments was obvious from the start, but their objections were at first overshadowed by the wave of support for the new theory" (p. 75). Is a comparison with the failure of behavior analysts to provide a prompt rebuttal to Chomsky's critical review of Skinner's *Verbal Behavior* appropriate (Chomsky, 1959; MacCorquodale, 1970; Skinner, 1957)?

Paleontologists always looked at evolution from a distance, so to speak. They could describe a change, but could not investigate its causes directly, and may thus have been inclined to raise a purely descriptive trend to the status of an explanatory "law." They accepted loose analogies with totally unrelated phenomena such as inertia or crystallization as though

they were valid contributions to evolutionary theory. (p. 161)

Should behavior analysts therefore be cautious about metaphors drawn from other phenomena (e.g., momentum, reactance, dissonance and resonance)? Or should they merely note that some metaphors of this sort are already well established (e.g., reinforcement or strengthening, shaping, and resistance to extinction)?

"Morgan argued that the Darwinians simply assumed that every character had a function. . . . If Morgan could show useless characters to be as common as adaptive ones, then selection would no longer provide a viable explanation of evolution" (pp. 202–203). Was Morgan's endeavor analogous to the pursuit of biological constraints as evidence against the role of reinforcement in behavior (e.g., Seligman & Hager, 1972)?

"The creation of hypothetical genealogical trees, which, given the lack of fossils, were completely untestable, had become a totally sterile scientific exercise" (p. 189). Are there analogies to such trees among the rivals of behavior analysis—for example in elaborations of semantic networks, which have become a prominent component of contemporary cognitive psychology (White, 1983)? Or was the problem merely that the fossil record was inadequate to the task, so that the only recourse was interpretation, as when a behavior analyst is confronted with complex behavior such as the production of a sentence but lacks access to the organism's relevant past history? "To accept that most of what happened in the past is concealed by lack of evidence is to impose a limitation that many paleontologists have been unwilling to accept" (p. 146).

REPRESENTATIONS AND HOMUNCULI

The examples from Bowler can be multiplied, and they can also be supplemented by examples from Dawkins (1976, 1982; cf. Catania, 1984a, p. 167). But although we may find the search for analogies to be intellectually challenging, their value will depend on whether they have implications for the terms and practices of behavior analysis. Perhaps the most telling illustration of such implications is provided by the concept of representations in cognitive psychology.

A major and recurrent feature of Skinnerian thinking has been its opposition to copy theories of behavior or perception. For example:

Organisms are changed by contingencies of selection, they do not store them. (Skinner, 1984e, p. 723)

Whether internal representations are copies or interpretations of images, something called "seeing them" is still required. Notions such as "convex edge," "concave edge," and "occluding edge" are a step in the right direction. They are the beginnings of an *analysis* of an image rather than a replication. (Skinner, 1984c, p. 660)

Despite Skinner's admonitions, it is not unusual to find the language of representations entering into purportedly behavioral accounts (e.g., see Terrace, 1984, on the pigeon's representation of stimuli to itself).

The question is not whether some lasting effect has been produced by a stimulus (presumably in the nervous system, although that constraint is not essential to the argument); no responsible behavior analyst would argue that an organism that has responded to a stimulus is not a changed organism. Rather, the question is about the form that the change takes, and in particular whether the change can be regarded as in some sense a copy of the stimulus. A cognitive account might argue that the physical manifestation of the stimulus as it persists in the organism is best regarded as a transformation of the stimulus; it might be further said to contain information about the stimulus. But the argument proceeds as if any change whatsoever could be interpreted as a transformation, with no hint that there are alternative accounts. This is where the Darwinian analogy is helpful.

Implicit in the formulations of both Lamarckism and some varieties of orthogenesis was the requirement of hereditary materials that constituted a representation of the organism. In the earliest preformationist versions of orthogenesis, the embryo was literally a homunculus; in later variations, it also took on ancestral forms, as ontogeny recapitulated phylogeny. As for Lamarckism, if acquired characteristics were somehow to be preserved in the germ plasm, the germ plasm had to contain in some form a plan of those parts of the organism that were to be altered in subsequent generations. In either case, the

germ plasm could be regarded as a transformation or copy.

A recipe is a sequence of procedures or instructions. It does not necessarily incorporate a description of a finished product, and although the recipe may be informative, it probably will not contain information about its origins. A blueprint, however, typically omits the details of the construction of its subject, even though it may exhibit structural correspondences with it. A blueprint is a representation but a recipe is not, and the Lamarckian and the preformationist orthogenetic accounts worked with blueprints rather than recipes.

A major achievement in contemporary biology was the reinterpretation of genetic material as a recipe for the organism's development rather than as a blueprint for its structure (see Dawkins, 1986, Chapter 11, for an elaboration of the metaphors of recipe and blueprint). The modern formulation also demanded rethinking of the sense in which the genetic material can be said to contain information, whether about evolutionary history or about the structure of the organism. The implications are profound. One is that their implicit copy theories were what made Lamarckism and at least some varieties of orthogenesis untenable alternatives to Darwinian selection: "The paleontologists' support for orthogenesis all too often foundered on exactly the same problem as Lamarckism: the impossibility of explaining an interaction between the organism and the environment in terms of the new genetics" (p. 169).

And that is where the behavioral parallel comes in: "In cognitive psychology these days we have far too many homunculi and their little friends. Mental representations are a fine example. These have an embryological analogue in the notion that the germ somehow contains a 'blueprint' of the soma" (Ghiselin, 1984, p. 687). What, then, are the implications for cognitive structures? Consider Piaget's schemas (e.g., Duckworth, 1979), which illustrate one kind of representation in psychology; it is probably relevant that Piaget's biology has its roots in orthogenetic rather than in Darwinian thinking (Vonèche & Bovet, 1982, pp. 87-88). But it might be argued that representations are an issue for psychology whether or not their origins can be traced to Lamarckian or orthogenetic ante-

cedents in biology; in either case, we may continue to entertain the possibility that organisms make copies of the environment and even that they later re-present those copies to themselves. If biological precedent is anything to go by, however, the odds are that we can spend our time more profitably exploring analytic alternatives that do not demand formal correspondences between initiating events and their end products.

IDEOLOGIES IN SCIENCE

Bowler's history of the eclipse of Darwinism is about the sides taken by biologists concerned with the problem of evolution. Their decisions were sometimes influenced by data but were more often determined by other variables that Bowler discusses in terms of beliefs. For example, with respect to the new synthesis, "The arguments supporting this position are eminently logical—however, we are not dealing with logic here, but with deep-seated beliefs that exert a far stronger control over the imagination than mere logic" (p. 112).

In contemporary behavior analysis, questions about beliefs are typically translated into questions about verbal behavior: To what extent were those decisions rule-governed, or determined by verbal antecedents, rather than contingency-shaped, or determined by their consequences (Skinner, 1969)? The distinction is important because rule-governed behavior may be relatively insensitive to contingencies or may be sensitive to contingencies in different ways than contingency-shaped behavior (e.g., Shimoff, Matthews, & Catania, 1986). Although scientific behavior includes practices that maintain close correspondences between verbal reports of events and the events themselves, those practices do not guarantee immunity to the insensitivities to contingencies to which rule-governed behavior may be prone.

Bowler is keenly aware of the political dynamics of science. For example:

If the early supporters take up dogmatic stands on technical issues and fall out with one another in public, their chances of success will be slim even if there is a core of truth in their basic idea. In contrast, if they take up a flexible position that will admit a number of partially divergent views, their chances of attracting support will be increased. Even those scientists who have reservations about some parts of

the theory might join the group to exploit the more successful areas of application. The early form of Darwinism was certainly flexible in this way, and its later eclipse may well have been encouraged by an increasing dogmatism that alienated all those biologists who retained doubts about selection. (pp. 14–15)

Clark (1984, p. 58) similarly noted, "If politics is the art of the possible, Darwin knew that the art of survival consisted of not offending more readers than was absolutely necessary."

Political arguments were marshaled in support of the various sides of the Darwinian controversies. These arguments were undoubtedly influential, but there were many uncertainties in the derivation of particular political philosophies from a given evolutionary position. To some, orthogenesis seemed consistent with progress, but to others it came to imply an inevitable decline that generated

an outlook on humanity's destiny far more hopeless than anything attributed to Darwinism. . . . Paradoxically, in an effort to maintain the autonomy of internal biological forces, the supporters of orthogenesis had created a nightmare. Far better . . . to abandon the inevitability of progress and accept that it could only occur through our own exertions. (p. 180)

Lamarckism, on the other hand, was viewed by many as consistent with democratic institutions, especially in its American variations. But some saw it differently:

Few Americans would have followed Eimer in his support for a totalitarian "beehive" society, but the fact that such an implication could be drawn from a world view with strong conceptual links to orthodox Lamarckism reveals the impossibility of identifying any scientific position with a particular philosophy or ideology. (p. 91)

[Lamarckism] rested upon a profoundly conservative intellectual foundation. Considering that it also supported the theory of racial senility and a hierarchical view of organic relationships that was a major force in the establishment of race theory, modern Lamarckians should be grateful that it was eliminated. . . . Opponents of the modern selection theory who dismiss it as an expression of blind materialism or capitalist ideology should pause for a moment to think that if it were not for the triumph of the modern synthesis, their own alternatives might still be burdened with equally distasteful implications. (p. 221)

It would be foolhardy to assume that ideology has not similarly played a role within psychology. Schwartz (1986), who targeted both natural selection and behavior analysis for attack in a critique that grouped these disciplines together with the economics of Adam Smith, provides a cautionary example. It can hardly be coincidental that his recent program of research has been addressed to the shortcomings of reinforcement. It had been designed to show that reinforcers produce stereotyped behavior and oppose variability (Schwartz, 1982a, 1982b). But his conclusion that the variability of the sequence of a pigeon's pecks on an array of keys cannot be shaped (Schwartz, 1982a) has been refuted by an experiment showing the opposite when artifactual constraints on the required sequence are removed (Page & Neuringer, 1985), and his demonstration of an allegedly similar effect in human behavior (Schwartz, 1982b) is flawed because it failed to take into account the possibility that the behavior was rule-governed rather than contingency-shaped (e.g., Shimoff et al., 1986). The Darwinian eclipse included cases in which science was undermined by ideology (e.g., p. 214); was this an analogous one in behavior analysis?

It should come as no surprise that, just as the interpretation of genetic data was influenced by ideological considerations (e.g., p. 214), the interpretation of behavioral research can be similarly biased. Some may argue that all science is ideology at some level, but perhaps this holds only for certain varieties of rule-governed behavior; if so, the verbal practices of scientific behavior may sometimes override control by ideologies. It is difficult to imagine how Darwin's position could otherwise have survived its eclipse. "Great is the power of misrepresentation, but the history of science shows that this power does not long endure" (Charles Darwin, quoted in Clark, 1984, p. 151).

QUO VADIS?

Perhaps the most tantalizing aspect of Bower's account of the eclipse of Darwinism is the question of whether it may help us to predict the course of behavior analysis in the decades around the turn of the next century. Let us start by assuming that the Darwinian analogy still holds and we are in the midst

of an eclipse of behavior analysis. What kinds of events will lead to the reemergence? When it begins to happen, how will we know?

Some guesses about the future seem fairly safe. For example, just as no later biologist displaced Darwin from his primary position in evolutionary thought, we may be confident that Skinner will remain central in the history of behavior analysis. But a critical feature of the Darwinian history was the development of the discipline of Mendelian genetics, its adoption of mutation theory, and its merger with selectionism in the modern synthesis. Is there an emerging discipline toward which behavior analysis should look? As for the synthesis (not to be confused with the synthesis of complex behavior), what will be its nature, and how will it alter behavioral thinking?

It is tempting to speculate that the critical discipline will be some variety of neuroscience. Progress in that area has been most evident with respect to relatively simple processes such as sensitization (e.g., Kandel & Schwartz, 1982). That direction, however, seems to follow more from an associationist than from a selectionist account of behavior. Neuroscience seems not yet to have given its attention to a mechanism for the selection of behavior by its consequences. But perhaps the accumulating evidence for growth and reorganization within the nervous system will turn out to be consistent with a selectionist account of the development and maintenance of neural systems (e.g., Fox, 1984; Yates, 1986).

It is relevant to the analogy that the early geneticists had no biochemical evidence about genes; their conclusions were based only on the data of reproduction. Skinner has compared the relation between behavior analysis and neuroscience with that between genetics and biochemistry:

It is the function of a science of behavior at the present time to give neurologists their assignments, as it was the function of genetics prior to the discovery of DNA to give modern geneticists their assignment with respect to the gene. I look forward to a comparable development in behavior, though I do not expect to live to see it. (Skinner, 1984a, p. 507)

Neurology will eventually give behavioral science what DNA has given genetics, but it has not done so yet, nor will the "reduction" of behavioral facts to neurological facts be helpful until the behavioral facts are correct.

The phylogeny and ontogeny of behavior are subjects in their own right, as was genetics prior to the discovery of the structure of DNA. (Skinner, 1984d, p. 707) (See also Skinner, 1984b, p. 607; 1984d, p. 703; 1984e, p. 722.)

Yet there may be other opportunities for behavior analysis that are more important than those provided by neuroscience. In particular, it may be important to recall the Darwinian problem that was resolved by the modern synthesis: the source of the variability upon which selection operated. Experimental analyses of rates and magnitudes of mutations made possible a detailed account of selection as the creative process that shaped that variability into new forms in evolution.

If we look for analogous problems in behavior analysis, we should expect to find them where the most telling attacks upon behavior analysis have been directed, and that has been in the area of verbal behavior rather than in neurology. The crux of Chomsky's (1959) assault was also about creative processes: productivity in verbal behavior. For the purposes of our analogy, it should be clear that the ideological origins of Chomsky's arguments are more relevant than their soundness.

Verbal behavior may have created problems for behavior analysis because it is part of a different domain. It involves not only the selection of behavior by consequences over the lifetime of an individual organism but also the transfer of behavior from one organism to another. This domain is a third variety of selection (Skinner, 1981), and along with verbal behavior it includes such topics as observational learning, imitation, and the survival of cultural practices (Catania, 1984b, p. 713). The study of this new domain has already begun, and new phenomena are emerging from it (Catania, 1986). These include varieties of stimulus control with novel properties (equivalence classes: Sidman, Cresson, & Willson-Morris, 1974; Sidman & Tailby, 1982) and characteristics of rule-governed behavior that can be tracked developmentally (e.g., Lowe, Beasty, & Bentall, 1983). In conjunction with analyses of the structure of nonverbal behavior (e.g., Catania & Cerutti, 1986), explorations of this new domain may already be putting the question of productivity in verbal behavior to rest.

The third kind of selection is also critical to the future of behavior analysis in a different

sense, because behavior analysis will survive only if it is sufficiently well defined and cumulative that it can be passed on from one generation of students to another. Behavior analysis seems well prepared to do so, given the coherence of its taxonomy of behavior, but that does not appear to be the case for some of its rivals or for psychology as a whole. A discipline so loosely defined that its practitioners cannot agree on its major features can hardly be expected to pass on a consistent set of principles to future generations of students.

This review has suggested that the analysis of verbal behavior will play a more critical role than will synthesis with neuroscience in bringing the eclipse of behavior analysis to an end. But even at the risk of failing to provide closure, it would be premature to try to spell out the details here. Further projections of future progress must be left to the reader, because the reader may well contribute to it. If the path is similar to the one already traveled by Darwinism, Bowler's account of the Darwinian eclipse may provide some useful sign-posts along the way. The language of eclipse is of course metaphor, but it is far better metaphor than that of the deathbed with which we began. "The very term 'eclipse' suggests a temporary diminution of previous brightness" (p. 12). For behavior analysis, as it once was for natural selection, the eclipse appears partial rather than total, and perhaps the maximum occultation has already passed. If so, the prospects are bright.

REFERENCES

- Bowler, P. J. (1983). *The eclipse of Darwinism: Anti-Darwinian evolution theories in the decades around 1900*. Baltimore: Johns Hopkins University Press.
- Catania, A. C. (1978). The psychology of learning: Some lessons from the Darwinian revolution. *Annals of the New York Academy of Sciences*, 309, 18-28.
- Catania, A. C. (1983). Behavior analysis and behavior synthesis in the extrapolation from animal to human behavior. In G. C. L. Davey (Ed.), *Animal models of human behavior* (pp. 51-69). Chichester, England: Wiley.
- Catania, A. C. (1984a). Conceivable book reviews. *Journal of the Experimental Analysis of Behavior*, 42, 165-169.
- Catania, A. C. (1984b). Problems of selection and phylogeny, terms and methods of behaviorism. *Behavioral and Brain Sciences*, 7, 713-717.
- Catania, A. C. (1986). On the difference between verbal

- and nonverbal behavior. *Analysis of Verbal Behavior*, 4, 2-9.
- Catania, A. C., & Cerutti, D. T. (1986). Some nonverbal properties of verbal behavior. In T. Thompson & M. D. Zeiler (Eds.), *Analysis and integration of behavioral units* (pp. 185-211). Hillsdale, NJ: Erlbaum.
- Chomsky, N. (1959). Review of B. F. Skinner's *Verbal Behavior*. *Language*, 35, 26-58.
- Clark, R. W. (1984). *The survival of Charles Darwin: A biography of a man and an idea*. New York: Random House.
- Dawkins, R. (1976). *The selfish gene*. Oxford: Oxford University Press.
- Dawkins, R. (1982). *The extended phenotype*. San Francisco: Freeman.
- Dawkins, R. (1986). *The blind watchmaker*. Harlow, Essex: Longman.
- Duckworth, E. (1979). Either we're too early and they can't learn it or we're too late and they know it already: The dilemma of 'applying Piaget.' *Harvard Educational Review*, 49, 297-312.
- Fox, J. L. (1984). The brain's dynamic way of keeping in touch. *Science*, 225, 820-821.
- Ghiselin, M. T. (1984). B. F. Skinner versus Dr. Pangloss. *Behavioral and Brain Sciences*, 7, 687-688.
- Kandel, E. R., & Schwartz, J. H. (1982). Molecular biology of learning: Modulation of transmitter release. *Science*, 218, 433-443.
- Köhler, W. (1927). *The mentality of apes* (2nd rev. ed., Ella Winter, Trans.). London: Routledge & Kegan Paul. (Original work published 1917)
- Kosslyn, S. M. (1983). *Ghosts in the mind's machine*. New York: Norton.
- Lowe, C. F., Beasty, A., & Bentall, R. P. (1983). The role of verbal behavior in human learning: Infant performance on fixed-interval schedules. *Journal of the Experimental Analysis of Behavior*, 39, 157-164.
- MacCorquodale, K. (1970). On Chomsky's review of Skinner's *Verbal Behavior*. *Journal of the Experimental Analysis of Behavior*, 13, 83-99.
- Page, S., & Neuringer, A. (1985). Variability is an operant. *Journal of Experimental Psychology: Animal Behavior Processes*, 11, 429-452.
- Schwartz, B. (1982a). Failure to produce response variability with reinforcement. *Journal of the Experimental Analysis of Behavior*, 37, 171-181.
- Schwartz, B. (1982b). Reinforcement-induced stereotypy: How not to teach people to discover rules. *Journal of Experimental Psychology: General*, 111, 23-59.
- Schwartz, B. (1986). *The battle for human nature*. New York: Norton.
- Seligman, M. E. P., & Hager, J. L. (1972). *Biological boundaries of learning*. New York: Appleton-Century-Crofts.
- Shimoff, E., Matthews, B. A., & Catania, A. C. (1986). Human operant performance: Sensitivity and pseudosensitivity to contingencies. *Journal of the Experimental Analysis of Behavior*, 46, 149-157.
- Sidman, M., Cresson, O., Jr., & Willson-Morris, M. (1974). Acquisition of matching to sample via mediated transfer. *Journal of the Experimental Analysis of Behavior*, 22, 261-273.
- Sidman, M., & Tailby, W. (1982). Conditional discrimination versus matching to sample: An expansion of the testing paradigm. *Journal of the Experimental Analysis of Behavior*, 37, 5-22.
- Skinner, B. F. (1953). *Science and human behavior*. New York: Macmillan.
- Skinner, B. F. (1957). *Verbal behavior*. New York: Appleton-Century-Crofts.
- Skinner, B. F. (1969). An operant analysis of problem solving. In B. F. Skinner, *Contingencies of reinforcement* (pp. 133-157). New York: Appleton-Century-Crofts.
- Skinner, B. F. (1981). Selection by consequences. *Science*, 213, 501-504.
- Skinner, B. F. (1984a). Some consequences of selection. *Behavioral and Brain Sciences*, 7, 502-510.
- Skinner, B. F. (1984b). Contingencies and rules. *Behavioral and Brain Sciences*, 7, 607-613.
- Skinner, B. F. (1984c). Representations and misrepresentations. *Behavioral and Brain Sciences*, 7, 655-667.
- Skinner, B. F. (1984d). Phylogenic and ontogenic environments. *Behavioral and Brain Sciences*, 7, 701-711.
- Skinner, B. F. (1984e). Reply to Harnad. *Behavioral and Brain Sciences*, 7, 721-724.
- Smith, T. L. (1986). Biology as allegory: A review of Elliott Sober's *The Nature of Selection*. *Journal of the Experimental Analysis of Behavior*, 46, 105-112.
- Terrace, H. S. (1984). A behavioral theory of mind? *Behavioral and Brain Sciences*, 7, 569-571.
- Thorndike, E. L. (1911). *Animal intelligence*. New York: Macmillan.
- Vonèche, J., & Bovet, M. (1982). Training research and cognitive development: What do Piagetians want to accomplish? In S. Modgil & C. Modgil (Eds.), *Jean Piaget: Consensus and controversy* (pp. 83-94). New York: Praeger.
- Weismann, A. (1880-1882). *Studies in the theory of descent* (Vols. 1-2; R. Meldola, Trans.). London: Sampson Low.
- White, M. J. (1983). Prominent publications in cognitive psychology. *Memory & Cognition*, 11, 423-427.
- Yates, F. E. (Ed.). (1986). *Self-organizing systems: The emergence of order*. New York: Plenum Press.