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BEHAVIORAL TECHNOLOGY AND BEHAVIORAL ECOLOGY[1]

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Applied behavior analysis, as a special case of behavioral technology, is discussed from the standpoint of behavioral ecology. The ecological orientation and its emphasis upon system-like interdependencies among environment, organism, and behavior are presented. The widespread possibilities for unintended effects of simple interventions provide the context for evaluating effective behavioral technology and calling for cooperation between the technologist and ecologist. Such cooperation, in the form of mutual and cooperative research efforts, should come naturally for the technologist and ecologist, because they share some fundamental values and assumptions, and each has much to offer the other. Several areas of such cooperative effort are spelled out.

Technology is the systematic application of tested scientific principles to pragmatic, real-life tasks and problems. On these terms, applied behavior analysis, or behavior modification, is a behavioral technology *par excellence.* In fact, the basic research paradigm is also the basic treatment paradigm, and the basic research manipulation—contingency management—is also the treatment manipulation. This close coordination of the treatment model to the research process surrounds applied behavior analysis with an enviable degree of explicitness, rigor, and precision.

Once developed, technologies are usually used, and the tendency to use them increases in proportion to the precision with which they can be applied. Thus, we can anticipate phenomenal growth in the array of behavior problems, settings, age groups, and diagnostic groups to which behavior modification will be applied, partly because its precision and specificity will continue to increase, and partly because its developers and users display an unusual amount of zeal and optimism about their work. The purpose of this paper is to raise some troubling questions about this expansionist approach by outlining a larger ecological framework within which the enthusiastic proliferation of simple strategies of behavior change should be evaluated and planned. "With each decade, scientific findings translated into technology radically reshape the way we live. Technical capacity has been the ruling imperative, with no reckoning of cost, either ecological or personal. If it could be done, it has been done. Foresight has lagged far behind craftsmanship. At long last we are beginning to ask, not *can* it be done, but *should* it be done? The challenge is to our ability to anticipate second- and third-order consequences of interventions in the ecosystem before the event, not merely to rue them afterward" (Eisenberg, 1972, p. 123).

Even though its major principles are just now being formulated, the ecological perspective on behavior should offer the behavior technologist grounds for deep concern about his work. To set the stage for these arguments, a word is in order regarding behavioral ecology, some of its general implications, and some of its commonalities with applied behavior analysis.

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AN ECOLOGICAL PERSPECTIVE

Complex interrelationships and interdependencies within organism-behavior-environment systems and the behavioral, adaptive dependencies between organism and habitat are among the central interests of the ecological perspective on behavior. These are challenging issues, because full understanding of such interdependencies requires attention to complexity of a kind for which psychology is hardly prepared. These issues can be illustrated by examples and analogies, some of which fall outside of psychology.

Macro-Ecology

We know now that large-scale attempts to rid whole areas of insect-borne diseases and to release crops from the ravages of insects have created very unpleasant but unanticipated results, also on a large scale. Some 20 years ago, with the noblest and most humane intentions, the world-wide use of insecticide technology began. Many results of this world-wide experiment are now in, and we have observed (a) new, larger outbreaks of insect pests due to the killing, by the insecticides, of their natural predators, (b) explosive emergence of insect strains that are resistant to even the most advanced insecticides, and, (c) the accumulation of high concentrations of insecticides sufficient to do great harm to and threaten the survival of many top carnivores, such as birds of prey and, perhaps, even man.

The second example comes from the Aswan Dam on the Nile River (Murdoch and Connell, 1970). The reasons for building were humane and respectable—to supply water for irrigation, to prevent floods, and to manufacture electrical power. However, two sets of unpleasant and unanticipated effects resulted from the dam. One was the reduction in the sardine harvest in the Mediterranean Sea, from 18,000 tons to about 500 tons per year because the dam disrupted the cycle involving silt-nutrient seeding, plankton, and fish. A second effect has been a profound increase in both the incidence and virulence of schistosomiasis among the people of the Nile, because quiet waters (behind the dam) harbor snails that carry more virulent blood flukes than running water (the old river) and because the new stable bodies of water have attracted large numbers of people.

It is clear that our humane efforts to apply proven technology and to alleviate human suffering on a large scale can go awry in the most vexing ways. More importantly, (a) we simply do not have enough basic understanding of environmental systems, and (b) there is something pervasively wrong with our available understanding of environment-inhabitant systems and the impact of singular intrusions into those systems. In the insecticide and Aswan Dam cases, we are now quite sure that they are ecological phenomena whose complexity was not anticipated because we know now what happened—someone has discovered the principles that govern such events. Many other examples could be cited; *e.g.,* long-range effects of introducing new species of organisms into a given habitat, widespread crop diseases resulting from attempts to increase yield by reducing genetic diversity.

More Direct Examples: Micro-Ecology

There are analogies and examples that are closer to our primary level of analysis. In one case, an ornithologist with a European zoo wished to add a bird called the bearded tit to the zoo's collection.[2] Armed with all the relevant information he could find about the tit, the ornithologist went to great pains to build the right setting. Introducing a male and female to the setting, he noted that, by all behavioral criteria, the birds functioned very well. Unfortunately, soon after the birds hatched babies, they shoved the babies out of the nest, onto the

[2]I am indebted to Robert B. Lockard for this story. If the story has lost or gained anything in the present use, the fault is mine. Lockard's recent paper (1971) on the "fall of comparative pyschology" also offers strong corroboration of many of the present suggestions.

ground, where they died. This cycle, beginning with mating and ending with the babies dead on the ground, repeated itself many times.

The ornithologist tried many modifications of the setting, but none forestalled the infanticide. After many hours of direct observation of tits in the wild, the ornithologist noted three patterns of behavior that had missed everyone's attention. First, throughout most of the daylight hours in the wild, the parent tits were very active at finding and bringing food for the infants. Second, the infants, with whose food demands the parents could hardly keep pace, spent the same hours with their mouths open, apparently crying for food. The third pattern was that any inanimate object, whether eggshell, leaf, or beetle shell, was quickly shoved out of the nest by the parents. With these observations in mind, the ornithologist went back to observe his captive tits and he found that during the short time a new brood of infants lived, the parents spent only brief periods feeding them by racing between the nest and the food supply, which the ornithologist had provided in abundance. After a short period of such feeding, the infants, apparently satiated, fell asleep. The first time the infants slept for any length of time during the daylight hours, the parents shoved them (two inanimate objects, after all) out of the nest. When he made the food supply less abundant and less accessible, and thereby made the parents work much longer and harder to find food, the ornithologist found that the infants spent more daylight time awake, demanding food, and that the tits then produced many families and cared for them to maturity.

There are several important implications of this story. The first is the subtlety and elusiveness of the interdependencies among (a) some aspects of a total environment, (b) the ongoing, short-range behavior of the birds, and (c) some long-range outcome. The second is that neither the designer's good will nor the technologist's respect or concern for his subjects will themselves ensure his creating the right environment. The third implication is more complex and has to do with the criteria that are used in making evaluative inferences about intervention efforts. All the indicators in the behavior of the parent tits suggested that their captive environment was congenial and hospitable and that it fulfilled their needs. Yet, the long-range criterion of survival of the captive representatives of the species (a surprise, a shock to the ornithologist) pointed to a very different conclusion about the environment. It is easy to look in the wrong place for indicators of success in intervention. We must pick and choose our criteria with the greatest care, perhaps flying in the face of what common sense, accepted social wisdom, and even past success with our technologies tell us is humane, important, and worthwhile. The last implication points to methodology. Since it involves behavior and behavior-environment relations, the case of the bearded tit and its human analogs would be of direct interest to the psychologist. And yet, our traditional methods of research on humans hardly put us in a position to elucidate the real-life interdependencies of behaviors and environments. We say that systems concepts, complex dependencies, reciprocity, and extended time-related cycles must be entertained as descriptive and explanatory terms, but they almost never show up in the actual reports of our research. By and large, we continue to study behavior as though its important phenomena were simple, single-file, and relatively short-term.

The behavior of the predators of lemmings in Alaska is also instructive (Sears, 1969). Living and breeding under the snow, lemmings have a cyclical population record, in which high and low density alternate in fairly regular fashion. When the snow melts, they are preyed upon by a variety of animals, including the jaeger, a kind of sea hawk resembling a gull. When the lemming population is low or average, the jaegers space their nests and consume their prey in orderly fashion. But, when the lemming population is at its peak, so that food is no problem, there is a great deal of fighting over nesting space and food among the jaegers. Few of them raise normal broods and their numbers decline, but not from lack of food. Plenty is not the road to biological success among the jaegers, and their behavioral development is somehow involved in this paradox. Again, the governing principles, the interdependencies, are little understood.

Several years ago, Proshansky, Ittelson, and Rivlin attempted to increase the therapeutic effectiveness of psychiatric facilities through environmental design (Chapters 3 and 43 in Proshansky, Ittelson, and Rivlin, 1970). They focused their efforts on one ward of a state mental hospital. The ward was laid out on one long corridor, with a nurses' station at one end, near the entrance, and a solarium at the other end, with bedrooms, a bathroom, and a dayroom in between. The solarium, which was meant to be a place of relaxation and recreation, was overheated, poorly furnished, and generally unappealing, with intense sunlight pouring in through a bank of uncovered windows. It was used very little, even though there was a TV set there. Just about the only thing patients did consistently in the solarium was to stand alone for long periods of time in a state of preoccupation, detachment, and withdrawal—that singular behavior pattern in which severely disturbed persons engage so much. This isolated standing was one of the behavior patterns that the hospital staff wished to change. The psychologists changed the solarium by adding furniture, drapes, and other small accessories. Immediately, larger numbers of patients began spending longer periods of time there and the solarium took on the air of a pleasant recreational and social area. More importantly, the rate of isolated standing behavior went down so that very little of it now occurred in the solarium. The psychologists had achieved their purpose—for the solarium. However, all they had succeeded in doing was to change the *location* of the isolated standing behavior—a great deal of it now took place at the other end of the corridor, by the nurses' station. Luckily, these intervention agents did not restrict their focus to the solarium, but studied a whole environment-behavior *system,* of which the solarium was only one component. Creating the environmental conditions for reducing the level of troubling behavior in one part of the system had only shifted the behavior to another part.

Some years ago, New York City police, in work with gangs, engaged in a program of intervention whose purpose was to break up the gangs and their fighting behavior. Several troublesome and unanticipated phenomena accompanied their systematic intervention: outbreaks of vandalism, isolated drug-taking, feelings of alienation, and serious crimes of assault (Philip G. Zimbardo, *personal communication*). These phenomena beg for further research, but if the accompanying phenomena can be attributed to the intervention, then they point again to the system-like complexity of behavioral phenomena.

At CCNY in 1965, a student snack bar was closed for several months in mid-year to permit remodelling. On the basis of independent observation and tallies of seating patterns and occupancy before and after the remodelling, it was possible to ascertain that the proportions of occupancy by blacks and whites and the cross-racial seating patterns that had reached a very stable level before the closing never reinstated themselves afterwards (Zimbardo, *unpublished*).

Some General Implications

Other examples and the key aspects of the ecological perspective on behavior are presented elsewhere (Barker, 1965; 1968; 1969; Willems, 1965; 1973; *in press*). However, if we think of the implications of such phenomena and what they suggest (emphasis on suggest) about human behavior in general, and if we think about the growing pressure to apply known behavioral technologies, the following observation emerges: we have become fairly conservative and sophisticated about introducing new biotic elements and new chemicals into our ecological systems, but we display almost childish irresponsibility in our attitudes toward behavioral and behavioral-environmental systems. I am thinking here about many of our favorite sacred cows: (a) intensive psychotherapy upon single, perhaps arbitrarily selected members of social and behavioral networks, (b) poverty programs, (c) social change programs, in which simplistic measures of attitudes or values provide the criteria of change, (d) managerial and industrial consultations, in which we intrude arbitrarily into organizational-behavioral systems about which we know little, (e) educational programs, and (f) yes, even that most solidly empirical of sacred cows—contingency management in the modification of behavior. Applied behavior modification is an astonishingly simple and successful technology of behavior change. However, its precision and objectivity depend, in large part, upon its application to single dimensions of behavior, one at a time. The questions of larger and unintended effects within interpersonal and environmental contexts and over long periods of time beg for evaluation and research, because lessons learned in other areas suggest that we should always be sensitive to "other" effects of single-dimensional intrusions.

It is becoming clear in the ecological literature that "we can never do merely one thing" (Hardin, 1969), that every intervention has its price, no matter how well-intentioned the agent of intervention may be. The counter-argument often is: "Don't try to immobilize us with all that alarmist talk. We'll deal with side-effects when they come up. After all, we're not stupid!" However, when we think in terms of environment-behavior *systems*, we can see that there is a fundamental misconception embedded in that popular term, "side effects" (Hardin, 1969). This phrase means, roughly, "effects which I hadn't intended, hadn't foreseen, or don't want to think about." What we so glibly call "side effects" no more deserve the adjective "side" than does the "principal" effect—they are all aspects of the interdependencies that we need so badly to understand. But it is hard to think in terms of systems, and we eagerly warp our language to protect ourselves and our favorite approaches from the necessity of thinking in terms of interdependent systems. It is quite foreign to us to think of the physical and behavioral environment as inextricable parts of the behavioral processes of organisms and as relating to them in ways that are extremely complex.

For the student of behavior, there is much to be learned from this emerging ecological orientation, but if the lessons are learned, then there is an immediate and pervasive need for an expansion of perspective. Until a few years ago, technologists believed that most, if not all, of their developments would be useful in a rather direct and simple sense. We know now that this is not necessarily true—feasibility and even intrinsic success are not sufficient grounds for immediate application. This widening awareness—the ecological perspective—suggests that many things that *can* be done either should not be done or should be done most judiciously, and that more technology will not provide solutions to many technologically induced problems (Dubos, 1965; 1970-1971; Eisenberg, 1972). Before we can be truly effective at alleviating human suffering, we must know much more about the principles that characterize and govern the systems into which such alleviating efforts must, of necessity, intrude. Seeking that knowledge raises a host of theoretical, metatheoretical, and methodological problems.

One implication of this line of argument may well be a conservatism with regard to intervention in behavior-environment systems and the clear hint that the most adaptive form of action may sometimes be *in*action. The problem is that we know little as yet about the circumstances under which the price for action outweighs the price of inaction and *vice versa.* So, if we give the above examples and arguments a slight interpretive twist, we arrive at a second implication that is even more important. This is the clear suggestion that we need a great deal more basic research and theoretical understanding that take account of the ecological, system-like principles that permeate the phenomena of behavior and environment. There is immediate need for a systematic scientific basis to plan behavioral interventions and technologies in such a way that they will not produce unanticipated negative costs in behavior-environment systems.

THE PROBLEM OF EFFECTIVE BEHAVIORAL TECHNOLOGY

A frequent and inevitable accompaniment of progress in basic scientific understanding is the transformation of that understanding into technology. When technology is developed, there follows the understandable predilection to apply it and explore its range of application. This is just as true of behavioral technology as it is of medical procedures, pharmaceutics, cleaning agents, insecticides, electronics, food preparation, and agriculture. This tendency is even more reasonable in the case of behavior modification, because the paradigms of research and application are so closely intertwined. As the procedures of intervention become more powerful, more sophisticated, and more precise, their *intended* effects become easier to specify. However, most technological interventions also have *unintended* effects. One would think that increased power, sophistication, and precision would ensure greater ability to specify and anticipate unintended effects. But they do not. From the ecological standpoint, this is a fundamental issue, and it may call for mounting investigative efforts that have not been designed as yet.

The ingenuity involved in the widespread use of various forms of applied behavior modification need not be documented here. Together with some techniques of psychopharmacology, the contingency-management paradigm probably represents the most powerful technology of behavior change available today. This admirable power and precision, which has been demonstrated hundreds of times over and which is the great strength of the approach, is also the reason for greatest concern. First, there is the problem of the metaphor of the world as a vast, programmed, learning situation and the problem of failures and partial successes. When operant technology is applied with a particular behavioral outcome in mind and the result is outright failure, marginal success, or some vexing behavioral drift over time, it is easy to assert that no larger, system-wide problem or no *theoretical* problem has arisen; that there is only the need for more technological ingenuity and for more rigorous programming and control of contingencies. I submit that there is a theoretical issue here that has to do with assumptions and predictions not borne out and with the overall adequacy of the operant view of behavior to deal with behavior-environment phenomena. As an ecologist, I would prefer that behavior analysts became involved in clarifying the profoundly complicated and theoretical nature of the simplified interfaces they arrange between organisms and environments. The behavior modifier is justified in his pride over the power of contingencies of reinforcement and in arguing eloquently for his simple solutions to many pressing social and behavioral problems. I just wish that he and others could *join* in reflecting upon and probing into the principles and laws that govern what must surely be very complicated systems in which organisms, behavior, environmental events, *and* the technologist's programmed intervention are all implicated. I say this partly because such a strategy has been very fruitful in other areas of science, and partly because we also know that, depending upon the time, the place, the organism, the state of the organism, and many other as yet unknown properties of natural settings, the behavior analyst has degrees of success that range from extremely high down to zero.

Clearly, the theoretical issue and the implications of partial success boil down to a matter of preference. The devotee of behavior modification can certainly disregard those questions and go about his work productively. However, the second reason why this behavioral technology gives me pause is less a matter of personal preference and more a matter of unavoidable professional and practical importance. This is the problem of the possible implications of success.

What we admire so much about the behavior modification approach is the rigor of the paradigm and the explicitness with which a successful outcome can be specified and evaluated. What worries me is the distinct possibility of unanticipated accompaniments of success. In part, I am arguing by analogy from such phenomena as the insecticide problem, but there are examples from behavioral research that are also suggestive.

Recently, I heard a description of some attempts to modify the behavior of parents of troubled children. One subject-mother was observed to nag (emit commands) at rates of up to 100 or more per hour, and the child complied at a very low rate. The rate of mother's commands was reduced to an average of 15 per hour, and, correspondingly, the proportion of compliance on the part of the child went up. This was the outcome that had been designated as successful. However, the investigator went on to report difficulty in dealing with this case. As the study progressed and as the shaping of nagging succeeded, the mother's rate of eating went up, she gained weight, and she reported frequent anxiety and tension. Finally, she abandoned the child and left town. These events were seen by the investigator as only an unfortunate and vexing interruption of the treatment program.

In the Probation Department of Los Angeles, some explicit use has been made of token systems and other behavior modification techniques in dealing with deviant behavior among adolescent boys.[3] The probation officers were successful in reducing the rates of petty vandalism, such as stealing hubcaps and items from stores. However, as the petty vandalism went down, the rates of more serious offenses, such as stealing cars and destroying property, went up. It goes without saying that such phenomena beg for further research before any definitive statements can be made about their causal linkages. However, that is precisely my point: the phenomena beg for research, but they beg for research (a) that *admits the possibility* of unanticipated complexities, (b) that uses models that lead us to look for them and define them as real phenomena, and (c) that adds procedures that allow their detection and measurement when they occur.

Arguing against a simplistic interpretation of operant behavior control, Wahler (*unpublished*) raises the possibility of *indirect* stimulus control; *i.e.,* that one set of behaviors can be maintained or affected by reinforcers applied directly to other behaviors. He argues that these phenomena can occur because of unaccounted and unmeasured covariations among naturally occurring behaviors. One commonly accepted form of complex covariation is chaining, wherein a sequence of behaviors is maintained by a single reinforcer following the last response. More interesting for present purposes are other covariations that do not readily fit the chaining model because they do not always occur in the same temporal order (*e.g.,* Buell, Stoddard, Harris, and Baer, 1968). Wahler, Sperling, Thomas, Teeter, and Luper (1970) showed that parents' successful efforts to reduce nonspeech deviant behaviors by their children also led to reductions in stuttering and that this "side effect" was not due to differential reinforcement of stuttering and fluent speech. Wahler uses the term *response class* to denote naturally occurring, covarying, functional units of behavior that arise by means of processes that are unknown at present, and he argues: "Not only are developmental and maintenance features of the response class unknown, but predictions about which behaviors will become so organized are equally vague. When a clinical investigator restricts his operations to one child behavior, he has no way of knowing what other behaviors emitted by the child will be affected by that operation. Unless his baseline observations encompass multiple behaviors, including a correlational analysis of these observations, the complete outcome of his intervention pro-

[3]Personal communication from Mr. Caldwell M. Prejean, who was a probation officer in Los Angeles for three years.

cedures cannot be predicted in most cases. . . . Simply stated, these guidelines first require the investigator to monitor more than a single troublesome behavior presented by the child. However, rules concerning what other behaviors to record are necessarily vague at this point." It is from such phenomena that unanticipated effects of interventions are made. At present, we know almost nothing about the properties of situations, places, persons, and interactions that affect and maintain such complex covariations in nonlaboratory behavior.

Another clear example comes from the work of Sajwaj, Twardosz, and Burke (1972), who found various "side effects" of manipulating single behaviors in a preschool boy. For example, using ignoring by the teacher to reduce the child's initiated speech to the teacher in one setting of the preschool led to systematic changes in other behaviors by the child in the same setting and in another setting as well. Some of the "side effects" were desirable (increasing speech initiated to children, cooperative play), while some were undesirable (decreasing task-appropriate behavior, increasing disruptive behavior), and some were neutral (use of girls' toys). The investigators were able to show that the covarying effects were not due to differential contingent attention by the teacher applied directly to those behaviors, but were somehow (as yet, mysteriously) a function of modifying another single dimension of behavior. Sajwaj *et al.* consider it distinctly possible that modifying one behavior modifies the properties of the larger setting, thus changing the system of contingencies and the opportunity for reinforcers to contact behaviors. We know almost nothing as yet about governing principles in such systems or about the ways in which behaviors and behaviors and environments form clusters or covarying units in the everyday world. Such system-like ecological phenomena must be elucidated if we are ever to substitute rational anticipation and planning in the use of operant approaches, where we now fight only brush fires on unintended results.

COMMONALITIES AND SOME IMPLICATIONS

The ecological and behavior modification points of view seem to operate from quite different values and assumptions. Differences, strongly stated, may stimulate research and thus progress, but the explicit phrasing of commonalities is a step toward another form of scientific progress—the common formulation of principles and procedural styles (see Krantz, 1971).

Shared Values

Contrary to some widely held views, the ecological perspective is not a single, unified body of theory, nor is it defined by any particular method (Willems, 1973). Rather, it is an orientation, a set, a perspective, that leads the investigator to do his research, ask his questions, and view his phenomena in certain distinctive ways. Behavioral ecology as an enterprise has some distinctive features, even though it is not *a* model or *a* theory (Menzel, 1969; Willems, 1965; 1973). Here are several values that behavioral ecology and the behavior modification movement hold in common.[4]

(*1*) *Empiricism and objectivity.* In general, the ecologist and the applied behavior analyst are socialized to place a great deal of emphasis upon empirical data, especially if they must choose between complex, speculative theories and an empirical base. For both, the ratio of empirical data to theory is higher than it is for many other subdisciplines, and both prefer to base their generalizations on extensive data sets. However, if we add the criteria of explicitness and rigor, then, at least in psychology, it is quite clear to me that the behavior analyst has it over the behavioral ecologist. The behavioral ecologist admires and aspires to the behavior analyst's objectivity, but does not often achieve it. Both are known to say, "Let's look at the facts",

[4]It should not be implied from this listing of shared values that other professionals would not share some or all of them.

but there may be some intriguing differences in the facts they seek.

(2) *Environment as selective.* Both emphasize a transactional view of behavior, *i.e.,* that the organism's functioning is mediated by behavior-environment interaction. This transactional credo, which is foreign to many scientists, is so dear to both that Skinner might as well be speaking for both when he says: "The environment is obviously important, but its role has remained obscure. It does not push or pull, it *selects,* and this function is difficult to discover and analyze. . . . the selective role of the environment in shaping and maintaining the behavior of the individual is only beginning to be recognized and studied" (1971, p. 25). The implications of this view are widespread (see Platt, 1972), but two that are accepted by the behavioral ecologist and behavior analyst are (a) that behavior is largely controlled by the environmental setting in which it occurs and (b) that changing environmental variables results in the modification of behavior.

(3) *Importance of site specificity.* One offshoot of the transactional character of behavior and its responsiveness to environmental selection is the phenomenon of site specificity of behavior. Both the behavioral ecologist and the behavior analyst assume site specificity, know it from their own work, and urge other professionals to recognize the strong linkages between place and behavior (see Barker, 1968; King, 1970; and Sells, 1969 for the ecologist's argument). "The correlation between site and activity is often so high that an experienced ecological psychologist (or behavior analyst)[5] can direct a person to a particular site in order to observe an animal exhibiting a given pattern of behavior" (King, 1970, p. 4). Furthermore, the behavior analyst offers the behavioral ecologist a very promising model for understanding site specificity. Whether the governing principles must always be labelled *contingency control* or *stimulus control* is a separate issue.

(4) *Baseline data on behavior.* Both the behavioral ecologist and behavior analyst tend to focus upon what organisms *do,* defined quite physicalistically, in relation to the environment and tend to deemphasize what organisms feel, think, and say about their behavior and environment. A common value follows from this behavioral emphasis; *i.e.,* both attach fundamental importance to gathering extensive, reliable, relatively atheoretical data as a starting point for their work. Both tend to begin by obtaining unusually extensive observations of their phenomena of interest because they insist upon documenting the frequency and distribution of behavior and upon understanding the descriptive character of the behavior with which they are working. Their activities diverge sharply after such baseline observation, but this commonality is worth noting (see also Bijou, Peterson, and Ault, 1968).

(5) *Environmental measurement.* Closely related to the baseline observation of behavior is the emphasis of both groups upon explicit documentation, measurement, and recording of the environment of behavior. The fact that they conceptualize the environment quite differently is less interesting than the fact that both actually *carry out* environmental measurement as part of their work; *i.e.,* that both usually engage in observation of the objective environment of behavior.

(6) *Commonplaceness.* Perusal of the writings of the behavioral ecologist and behavior analyst indicates that, to an unusual degree, both accept common, ordinary, everyday behaviors as primary phenomena to be described, counted, understood, explained (ecologist), and manipulated (modifier). Both proceed on the basis that "science advances by relentless examination of the commonplace; that some of its greatest discoveries have been made through fascination with what other men have regarded as not worthy of note" (Henry, 1971, p. xix). Thus, the naturalistic observation of the ecologist and the baseline observation of the applied behavior analyst bulge with reference to such ordinary

[5]My addition.

phenomena as location of persons, eating, talking, resting, fighting, reading, holding and handling objects, participating in activities, making mistakes, *etc.* The fact that each tends to do different things with commonplace events should not becloud the fact that both attach fundamental scientific importance to the common, mundane behavior repertoires of persons, rather than to highly abstract, rarified concepts.

(7) *Common sense and principles of behavior.* Beyond the emphasis on commonplace events is another shared value. Everyone has some intuition regarding his behavior and the behavior of others. Conventional social wisdom and commonsense principles of how things work and what is right, good, and humane permeate what people say about behavior and behavior change. Beyond the value placed on common behaviors as primary data, I detect that both the behavioral ecologist and behavior analyst display an unusual openness to accepting, pursuing, and discovering explanations and governing principles that are counter-intuitive and violate common sense. Furthermore, once the principles have been found to hold, it is probably true that both show an unusual degree of willingness to promote pragmatic programs that violate conventional social wisdom or commonly held views of what is humane. The phenomena of punishment (Baer, 1971; Birnbrauer, Burchard, and Burchard, 1970), contingent love (Baer, 1969), and amount of effort (bearded tit example above) illustrate what I mean.

(8) *Intervention and its effects.* Finally, the behavioral ecologist and behavior analyst share a common value—maybe even a converging fate—at the level of studying the effects of intervention. Even the most narrowly focused behavior modifier is (or should be) interested, case by case, in creating the conditions for shaping and maintaining a particular level of functioning in a behavior-environment system. We know little as yet about the extent to which he does that successfully in the long run. We do know what he does extremely well—achieving categorical outcomes in behavior by means of dimensional management of contingencies. He says, "Show me the specific troublesome behavior", and then applies a highly specific remedy to it. This approach satisfies a fundamental principle of applied ecology: control measures that are as specific as possible to the particular troubling events are always preferable (see Odum, 1963, pp. 106-107). However, another fundamental principle of applied ecology and an objective of ecological research is: we must improve our ability to predict the results of intrusions in ecological systems, and thereby more intelligently prescribe or avoid the removal of vital behaviors or the inadvertant addition of dysfunctional ones (Odum, 1963, p. 27). Thus, far more than other strategies of behavior change, the work of the behavior modifier fulfills one ecological principle. The work of the behavioral ecologist emphasizes the second. This should be seen as a logical and productive opportunity, rather than a diversion of basic values. Logic would dictate that the behavioral ecologist and the behavior modifier link efforts to fulfill both principles.

Interdependent Effort

The behavior modifier and behavioral ecologist are both deeply concerned with interfaces between behavior and environment. Now, both must formulate and promote a new kind of interface—the interface between their separate and mutual concerns, skills, and efforts. The time has come to demand of the behavior modifier that he provide information and insight regarding the ways in which the effects of his interventions ramify across other phenomena that may extend widely into physical, social, and behavioral space and across time. He need not do this alone, of course, but he must open up his domain to disconcerting questions and outside interests. Analogously, the time has come to demand of the behavioral ecologist that he open up his domain, that he articulate his questions and principles in such a way that they can be evaluated in the arena of real behavior problems and real attempts to apply behavioral technology, and that he accept the results of replicated experiments and intervention attempts as valuable and crucial data (Willems, 1973).

Not only is it scientifically critical that the behavior analyst and ecologist engage in new forms of cooperative effort, joint-event research, or piggybacking (Eisenberg, 1972; Kranzberg, 1972), but the practical products will be worth the investment of effort and ingenuity.

The ecologist and the behavior modifier work differently, they work with different objectives, they tend to use their data very differently, *but* they have much to offer each other. The strength of the one is that he restricts himself to an intervention and what he considers to be its direct, immediate characteristics. The strength of the other is that he focuses on the *context* of intervention, on the characteristics of context and person, and what he suspects might be the indirect and unintended effects of intervention; *i.e.,* ecological indicators of the functioning of an interdependent system. Just as the lion tamer and the ethologist both contribute much to the understanding of behavior control, so should the behavior modifier and the ecologist jointly be able to contribute much to the understanding of behavior control in such a way that costs and benefits can be balanced.

SOME PROBLEM AREAS

Exhortations and propaganda are of little use unless their implications for concrete work can be spelled out. The general arguments about behavioral ecology and behavioral technology suggest some points of sharp disagreement and some concrete areas for action. I shall attempt to spell out several such areas in terms of a brief list of issues whose pursuit should benefit both movements. The intent is that each issue be made into an empirical problem that can be developed with mutually acceptable data.

(1) *Long time periods.* In keeping with some of the characteristics of behavior-environment systems and the kinds of behavioral dimensions with which he often works, the behavioral ecologist, like his counterparts in other areas of ecological science, pays attention to unusually long time periods. Time is not a functioning variable for him, but many of the phenomena to which he is attuned evolve over long periods. This concern is quite different from the more traditional developmentalist's concern with early and remote antecedents, because the ecologist always aspires to fill in the functional account of the sequence. We know by now from other areas (*e.g.,* crop diseases, insecticides) that empirical monitoring of very long sequences can be both scientifically illuminating and pragmatically critical. Somehow, the behavior modifier must become willing to participate in such long-range concerns. The fact that the behavioral sciences cannot match the sophistication of ecological biochemistry is no excuse to wait.

The most ready and reasonable response by the behavior modifier, a function of his view of behavior and his view of the world, will probably be: "Whatever happens before or after my technological intervention, whether good, bad, or indifferent, is a function of chaotic or unfortunate programs of contingency or, at the least, programs of contingency that are out of my explicit control. *Ergo,* those occurrences are none of my business, by definition." He should make them his business, because we do not know when that response is an evasion of direct responsibility. Some behavioral interventions might unwittingly disrupt desirable things or set undesirable things in motion that become clear only over long periods of time.

(2) *"Other" data.* Closely related is a dilemma that surrounds effective technologies of all kinds and, therefore, applied behavior analysis, whose great strength comes from its concreteness, specificity, and narrowness of focus. The dilemma derives from the fact that the more narrow and specific the technological application becomes, the greater the array of phenomena its practitioners tend to disregard. So, while the behavior analyst would tend to assert

that we should not bother him with arguments about unintended effects and "other" data—that he should take them all with so many grains of salt—the ecologist would argue that "other" data are worth gathering or may even be the most important.

The behavior modifier seems to be operating from strength when he says that the other-data argument comes from *absence* of data, from behavioral rates as yet unknown. My rejoinder is threefold. First, scientific progress sometimes follows from simply raising new questions—"I wonder what would happen if . . ." Second, behavior analysts themselves sometimes acknowledge the problem of unintended effects. Third, behavior modifiers themselves also argue from the absence of data, *e.g.,* "We observed no ill effects. Therefore, our intervention procedure must be clean". Reporting that there were no unintended effects measured within a circumscribed, preset category system is just as unconvincing as the high proportion of studies that do not allude to unintended effects at all. Furthermore, the prescription to engage in multidimensional observation in behavior modification studies is not enough. We need to formulate and carry out new forms of research, because, as Wahler (*unpublished*) points out, there is, at present, no *a priori* basis for choosing behaviors to monitor. It is embarrassing to be unable to spell out what kinds of behavior to monitor. We do not know enough yet about behavior-environment systems, but finding out will be just as important to the technologist as to the ecologist.

Altogether new, system-wide domains of data seem indicated because (a) successful modifications may produce unintended effects in the repertoire of the target person; (b) failures or marginal successes may be governed by variables that have not even been contemplated as yet; (c) with varying degrees of success on the target person, there may be unintended effects in the larger social or environmental network; (d) success may be temporary for reasons that are little understood; and (e) success may be situation-specific for reasons that lie beyond simple contingency principles.

For purposes of illustration, here is a classification of some of the kinds of unintended effects that may occur in behaviors that are not manipulated directly by the behavior modifier: (a) desirable, neutral, or undesirable behaviors may be affected; (b) the behaviors may increase or decrease; (c) the target subjects, other persons, or both may be affected; (d) effects may occur in the setting where the manipulation occurred, other settings, or both; and (e) effects may occur immediately, somewhat later, or much later. Cross-classifying these outcomes ($3 \times 2 \times 3 \times 3 \times 3$) results in 162 possible kinds of "side effects" from this classification alone, *e.g.,* decrease in a desirable behavior by target subject, occurring somewhat later in the same setting. This analysis illustrates the magnitude and complexity of the other-data problem. The classification should be expanded by taking account of kinds of behaviors. Then, extensive research should be conducted to ascertain which *kinds* of unintended effects occur most frequently and *why* they occur so that practitioners can begin to predict such effects and plan their interventions with these effects in mind.

(3) *When to rearrange?* There has been an expansion in behavior modification circles from traditional forms of reinforcement and punishment, such as candy and slaps, toward more subtle behavioral forms, such as smiles, frowns, attention, timeouts, and eye contacts. Not only are these commodities readily available, but they can be very powerful when delivered in strictly contingent fashion. However, what happens to the personal and interpersonal system in the long run with so many effective principles of behavior change available for application? ". . . adult practitioners must train themselves to inhibit spontaneous expressions of emotion in favor of expressions arranged to produce some desired behavior in the child. And assuming that the child 'learns' to produce that which may never even be openly demanded of him, he may also eventually learn that smiles and

frowns are not genuine signs of reflexive emotions, but rather are signals indicating how well he is conforming to authority . . . the legitimate emotional meaning of facial expressions may become corrupted for both adults and children" (Rappoport, 1972, Preface). The work of the Brelands (Breland and Breland, 1966) pointed out long ago that what organisms can be *made* to do in the short run may be very arbitrary and dysfunctional in the *long* run (see Lockard, 1971, for a similar argument). Ethological and cross-cultural evidence suggests that various forms of individual and interpersonal behavior occur in relatively invariant, reciprocal units that have adaptive value or, perhaps, even survival value. Also, the behavior-modification work of investigators such as Sajwaj *et al.* (1972) and Wahler (*unpublished*) suggests that we have little understanding of the covariations among behaviors between and within persons, between events, and over time. If that is so, then we should work hard to discover their full array and their functional value and not only rush to disregard them by rearranging them.

(4) *Diagnostic observation.* We marvel at the observational skill, astuteness, and hunch-generating skill displayed by many students of behavior. What is chaos to others yields functional and critical dimensions of behavior to them. Within this context, I marvel at the least publicized and least explicit part of most behavior modification studies—the observational process by which the investigator views complicated behavior systems, selects certain dimensions for study, and bets successfully that they will be amenable to contingency control. In fact, since the systematic application of contingency programs is explicitly designed to be simple and straightforward, demanding mostly self-control and tenacity by the manager, I would even hypothesize that it is the accuracy of the initial observing-selecting-betting process that distinguishes between successful and unsuccessful behavior modification studies in many cases. Knowing that many studies fail, I marvel, for example, at the unpublicized ingenuity of a behavioral technologist who observes the behavioral scene in the ward of a residential treatment center for retardates, picks four or five behavioral dimensions from the chaos, babble, and rubble, subjects the dimensions to a multiple contingency program, and ends up being successful. His skill and ingenuity in picking crucial behaviors and deciding upon category systems needs to be made more public and explicit and it needs to be subjected to study. It is, after all, diagnosis *par excellence,* and both the behavior modifier and ecologist could benefit greatly from more explicit rules for analyzing the critical aspects of behavior-environment systems and diagnosing the specific ways in which they function suboptimally.

(5) *Setting-behavior linkages.* Just as behavior and its behavioral consequences can be shown to form selective, interdependent systems, so do behavior and its physical context link up in almost inextricable ways. Furthermore, all of these elements concatenate in ways that we understand poorly at present, and which need to be investigated. For example, we might expand our investigations to consider the ways in which particular behavioral and educational outcomes in a classroom evolve as a combined function of (a) shape, distribution, and crowding of furniture, (b) mutual delivery of interpersonal reinforcers and punishers, (c) proxemics, and (d) classroom activity format. The designing of functional environments awaits such information, which should be of interest to both the behavioral technologist and ecologist.

(6) *Outcomes versus antecedent conditions.* Applied behavior modification involves more than a technology. It participates in a theoretical (yes, theoretical) movement whose view of behavior rests on assumptions of environmentalism, instrumentality, and contingency control. One of the pitfalls here is that information gleaned from interventions into troubling behavior may lead to misleading inferences back to the general model of behavior. The potential error lies in building and confirming a model

of behavior on the basis of what works in treatment, or inferring causes from effectiveness of treatment. Does the fact that aspirin alleviates headaches mean that the deficiency of aspirin causes headaches (Bernard Rimland, *personal communication*)? Likewise, does the fact that reinforcement contingencies alleviate troubling behavior mean that the troubling behaviors are caused by fouled-up contingencies of reinforcement? It is illogical to infer too much about etiology from the nature of effective treatment (Davison, 1969). It is entirely possible that there are fundamental differences between the conditions under which an organism comes to behave in a certain way and the conditions under which he can be *made* to behave in that way or another way. The issue here is that we need to understand both sets of conditions far better than we do now (see Lockard, 1971). Troubling behavior certainly suggests that something is wrong, but it does not necessarily suggest that the problem resides in the sequential microstructure of behavior and its consequences. We need more wisdom about such matters and they are matters of concern for both the technologist and the ecologist. More complete *contextual* understanding of troubling behavior can only lead to increased predictability and success in intervention.

CONCLUDING COMMENTS

Partly by analogy and partly from some general ecological concerns, I have argued that the behavioral technologist in general and the behavior modifier in particular are implicated in problems that extend far beyond their elegantly simple though successful working models and procedures, but also that the technologist and ecologist have much to offer each other by way of orienting questions, modes of analysis, and cooperative effort.

Sometimes in jest, sometimes with irony, the physician is told, "You can *bury* your mistakes." However, this semi-humorous comment presupposes that either the physician or someone else, or both, recognize when the physician has made a mistake, *i.e.,* that they have enough complete technical information to judge when a medical technique has been misapplied. If the arguments of this paper are tenable, then the practitioners and consumers of applied behavior analysis do not have enough perspective as yet even to *judge* whether a mistake has been made, much less to bury it. Within the larger context of behavioral ecology, self-defined successes may actually be failures, wherein unintended harm follows from short-term or narrowly circumscribed good. Perhaps, the ecologist and behavior modifier together can acquire the kind of complete contextual ability to judge between good and harm that follows from a mutual, scientifically defensible acceptance of the complexities of everyday human behavior and, thereby, develop the capability to anticipate the effects of intervention.

Skinner (1971) argued that: "The task of a scientific analysis is to explain how the behavior of a person as a physical system is related to the conditions under which the human species evolved and the conditions under which the individual lives (p. 14). . . . A scientific analysis naturally moves in the direction of clarifying all kinds of controlling conditions (p. 21)." Living up to this credo will require all the investigative ingenuity and cooperation we can muster.

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