The term ‘organism’ enjoys a revered place within the vocabulary of behavior analysis, most notably perhaps within the title of Skinner’s seminal work (The Behavior of Organisms, 1938). The exact status of this term, however, is unclear. For instance, the term does not appear to be a technical one. Nevertheless, its widespread use in the behavior-analytic literature suggests that it is masquerading as such. The present paper appraises the scientific utility of the term ‘organism’ within the domain of behavior analysis and discusses some possible reasons for its widespread use.

Behavior analysts are interested only in special types of behavior. This behavior is not that of subatomic particles or celestial bodies. Nor is it the Brownian motion of pollen grains in water, the paths of vortexes in turbulent fluid, or the dynamics of autocatalytic chemical reactions. Although these events constitute behavioral events both within the vernacular and other sciences (Kantor, 1959), they are not considered relevant to a science of psychology. Instead, psychological enquiries are confined to “the behavior of organisms” (cf. Skinner, 1938). This should not be surprising, given that it was questions about the properties of self-contained, biologically bounded individuals (e.g., consciousness) that provided the impetus for psychological inquiry at the outset.

Interestingly, however, there exists a tension between this alleged interest in the behavior of organisms and the decided absence of the concept of the organism in the principles and core concepts of behavior analysis. For instance, the organism does not appear in the three term contingency (stimulus-response-consequence). Furthermore, behavioral principles, such as the principle of reinforcement, describe stimulus-response relations that are content free. That is, the principle of

The authors thank Mike Mogie, Alan Raynor, Nigel Franks, and Anna Franks of the School of Biology and Biochemistry, University of Bath, for their helpful suggestions regarding the nature of “life.” We are also indebted to Vicki Lee and Ed Morris for their incisive and constructive criticisms of an earlier version of the current work. Material from this manuscript was presented at the Annual Conference of the Experimental Analysis of Behavior Group, London, England, March, 1996. Requests for reprints may be sent to either author at the Behavior Analysis and Cognitive Science Unit, Department of Applied Psychology, University College Cork, Cork, Ireland (e-mail: Lemmy@ucc.ie).
reinforcement describes a type of relation which, at the behavioral level, entails a response-consequence contingency and a resulting increase in the rate of responding followed by that consequence. It does not, however, specify any particular consequence or any particular response (Glenn & Madden, 1995). Such a relation is "a generalization that [is] spatio-temporally unrestricted [and which] refers to any entities that happen to have the appropriate characteristics" (Hull, 1984, p. 145).

Despite the foregoing, many behavior analysts continue to espouse the idea that behavior is a biological property of organisms (e.g., Staddon, 1993) and that the experimental analysis of behavior represents a "mating between behavioral philosophy and the methods of classical physiology (Marr, 1984, p. 358), implying that psychologically interesting events are defined topographically and located peripherally (e.g., rat/operandum interface) or centrally (e.g., physiological states) in organisms.

One of several problems with asserting the organocentric nature of behavior analysis is that the term "organism" is not a technical one. Even within biology, the organism has escaped precise definition. For instance, in the *Penguin Dictionary of Psychology*, Reber (1995) writes: "As there is no currently agreed-upon criterial set of features for determination of that which is living, there is no rigorous definition of that which qualifies as an organism" (p. 521). Reber also notes that biologists began to make progress only when they gave up trying to define the nature of living entities (1995, p. 419). In the words of Hall (1969): "there is in fact 'no such thing' as life (nor any 'such thing' as matter). Life and matter become, from this point of view, conceptual artefacts or fictions - useful fictions, perhaps, but also troublesome ones" (p. 376).1 Interestingly, one behavior analyst, Marr (1996), has recently noted that inanimate dynamic systems, such as autocatalytic chemical reactions, often display the qualities of living organisms, such as the unpredictable, the irreducible, the irreversible, and the emergent. Marr suggested that scientists might bridge the conceptual chasm between the quick and the dead by using dynamical systems approaches which offer "the promise of bringing quantitative unity to an enormous range of phenomena, living or dead" (1996, p. 19).

Given that the concept of organism is of ambiguous import within the biological sciences, it behooves us to examine the utility of this concept within behavior analysis. Establishing the role and utility of the organism is important because behavior analysts are committed to explanations of behavior stripped bare of needless terms and concepts. In effect, if the organism proves to be superfluous within behavior analysis, then we are obliged to jettison the concept forthwith. In the following section, therefore, the role of the organism within behavior analysis is broadly considered. In subsequent sections, several possible reasons for the widespread

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1The reader should note that a definition of life in terms of the presence of deoxyribonucleic acid (DNA) is unsatisfactory. Entities that possess DNA may still be inanimate (i.e., dead). See also Footnote 3.
reference to organisms within behavior analysis will be outlined. In the final section, a behavioral model of genetic inheritance will be introduced, freeing the behavior analyst to embrace the impact of evolutionary history on current behavior, without appeal to foreign concepts (e.g., genes, organisms).

Where is the Organism?

The Journal of the Experimental Analysis of Behavior (JEAB) serves perhaps as the main forum of debate for developments in basic behavioral research. In each edition, JEAB describes itself as "intended primarily for the original publication of experiments relevant to the behavior of individual organisms." At first brush, this definition appears organocentric and to be consistent with a traditional conceptualization of the experimental analysis of behavior (cf. Skinner, 1938). Prima facie, however, the idea that behavior can be of organisms does not sit easily with radical behavioral philosophy. According to radical behaviorism, behavior emerges from histories of stimulus-response interactions coming into contact with current contingencies, not from the subjects that participate in experiments. In this vein, the authors have been advised by JEAB reviewers, on more than one occasion, to remove references to organisms or persons from submitted manuscripts whenever possible. Accordingly, we now refrain from suggesting, for instance, that subjects can produce response patterns, in preference for attributing these patterns to contingencies. The participants in our experiments now deserve mention only in regard to their roles as subjects. This is difficult to reconcile with an organocentric view of behavior analysis as a science of the behavior of organisms.

Behavioral analyses of imitation serve as a further example of our apparent indifference to organisms. In a demonstration of imitation as a behavioral outcome, we present a model and observe the replication of a behavioral pattern in a different spatio-temporal location. It is not a behavior-analytic concern, however, to determine what is doing the replicating. We avoid such enquires partly because they can lead to mentalism. Specifically, although organisms may form in whole or in part the loci for behavioral replication, it is mentalistic to suggest that an organism can produce imitation (or any behavior) in a literal sense. Organisms are behaviorally impotent. Thus, in analyses of imitation, the organism is relegated to the role of mere participant whose status scarcely exceeds that of the laboratory equipment that contains it.

The idea that organisms are behaviorally impotent would appear to run contrary to the view held by some biologists (e.g., Dawkins, 1986) and psychologists (e.g., Lee, in press) that organisms are active biological entities, that make efforts and produce work to keep themselves alive. In fact Dawkins (1986) distinguished between living and nonliving entities in these very terms:

> Without work, creatures would merge with their environments, which is what happens when creatures die. For example, in a dry
environment, water naturally flows from creatures, which have a higher water content than the dry environment, into the environment. Without work to maintain the water content, the creature will die. Nonliving objects do not die. Forces act on them and bring them into equilibrium with their environment (e.g., erosion). (pp. 9-10)

For the behavior analyst, however, this suggestion is problematic on two counts. Firstly, the concepts of work and effort are mentalistic and have been rejected as such by behaviorists since Watson's time (e.g., Skinner, 1993/1974). Secondly, defining life simply in terms of a sustained disequilibrium with an environment would appear to be overly simplistic, not least because liquid crystals, ubiquitously considered to be nonliving entities, can sustain such disequilibrium through the reversal of entropy (see also Marr, 1996).

A final example of our communities’ eschewing of organocentrism is captured by our conceptualization of the stimulus-response relation. Specifically, those of us who teach behavior analysis sometimes go to lengths to impart to our students that a discriminative stimulus can not be presented to an organism: Stimuli and organisms exist in different epistemological planes. Lights, shocks, and tones, however, may be presented to organisms, but each of these can become a discriminative stimulus ($S^{R+}$) only in relation to a specified response. Thus, reinforcement, for example, is administered for particular responses but it is not delivered to organisms. In effect, the organism is irrelevant to the definition of an $S^{R+}$. Stimuli of all kinds, therefore, are defined in terms of the responses to which they give rise, not in terms of their physical properties. Similarly, responses are defined in terms of the stimuli with which they bear functional relations, not in terms of their taking place in organisms. Thus, although an organism may represent the site of independent (e.g., stimuli) or dependent (e.g., responses) variables, this is not an epistemological requirement. This conceptual strategy is important because it allows us to extrapolate our principles to embrace instances of behavior that might be viewed by other scientists as originating in organisms of different kinds.

Skinner emphasized the need for general principles of behavior from the very outset.

In the broadest sense a science of behavior should be concerned with all kinds of organisms, but it is reasonable to limit oneself, at least in the beginning, to a single representative example. Through a certain anthropocentricity of interests we are likely to choose an organism as similar to man as is consistent with experimental convenience and control. (Skinner, 1938, p. 47)

The generality for which Skinner called is captured well in the principles of respondent and operant conditioning. Using these principles, we can describe virtually any behavioral phenomenon, irrespective of the species
from which our subject is selected. Even the most complicated forms of verbal behavior, for instance, can be captured within an operant paradigm (Barnes, 1994; Boelens, 1994; S. C. Hayes, 1991). Nevertheless, Skinner also maintained that no respectable student of animal behavior has ever taken the position “that the animal comes to the laboratory as a virtual tabula rasa, that species differences are insignificant” (Skinner, 1966, p. 1205). In effect, behavior analysts acknowledge that each subject brings an extended phylogenetic history to the laboratory.

Although phylogenetic history appears to impact on current behavior in a very real way, the behavior analyst must handle this observation with extreme caution. Specifically, from a behavior-analytic viewpoint, phylogenetic and ontogenic histories do not belong to organisms in a literal sense. Rather, history is construed in terms of temporally overarching relations between events which reside in the conceptual domain of ongoing behavioral interactions. As scientists with an interest in historically based accounts, we necessarily operate at this functional level of analysis. Furthermore, if we are not careful to remain at this level of scientific enquiry, we run the risk of looking directly to organisms to find the histories wherein our explanations for behavior lie. This has happened before. Woodworth's (1929) Stimulus-Organism-Response (S-O-R) paradigm, for instance, was invoked to account for the variability in behavior formerly conceptualized with a simple S-R psychology. With only the S, O, and R terms to explain variability, any unexpected variability between the S and R could be accounted for by the O (Morris, 1992). This strategy drew the organism into the spotlight of scientific enquiry, thereby giving rise to modern day cognitive psychology (Wessells, 1982).

In contrast, behavior analysts traditionally resist treating the organism as the subject of scientific enquiry (see Skinner, 1969, p. 28). It is possible to study behavior without direct concern for organisms because the radical behaviorist explains behavioral variability, not in terms of organismic variables, but in terms of context. This context does not replace the organism by intervening between stimuli and responses but rather encompasses both (Morris, 1992). Embedding the organism in context in this way has allowed us to import the biological concept of organism into our own level of analysis—the conceptual domain of ongoing stimulus-response interactions. Furthermore, our treatment of the organism as generic context bears witness to our concern with phylogeny. To this extent, the organism as generic context increases our communication with neighboring, and ultimately distant sciences, because we can treat any variable as context (Morris, 1992). At the same time, however, this strategy appears to have removed the concept of the organism, as a biological entity, almost entirely from the experimental analysis of behavior.

Having briefly outlined some of the features of behavior analysis that dispute its organocentricity, let us now consider some possible reasons for the widespread reference to organisms within behavior analysis. Each of the reasons posed subsequently have in common that they represent conceptual errors leading to an implication of the organism in behavioral
phenomena. Examining each of these errors will afford us the opportunity to assess the utility of the concept of organism at the behavioral level of analysis.

1. **Mistaking the Doing for the Thing-Done**

   The first, and perhaps most obvious, reason that organisms enjoy elevated status within behavior analysis is that we derive their agency in behavior from the vernacular. In the vernacular organisms or living entities *do things* and it is through the agency of organisms that doing gets done. As outlined previously, however, organisms are in themselves behaviorally impotent from a radical-behavioral view of events. Attributing the action of “doing” to the subjects of our experiments, therefore, represents an unnecessary digression. For example, although a statement such as “the woman walks” is impressionistically usable, it is no more behaviorally sensible to make the woman *do* the walking than to let the ground *do* it (Bentley, 1941). In the present case, therefore, it is the walking itself, rather than its *doing*, that represents the event of interest to the behavior analyst. It is nothing short of mentalism to elevate the organism beyond the status of participant in such events.

   It is important to understand that although the woman in the foregoing example surely changes location as an organism, she does not do so as a person: A person is not an organism, although a person is also that. Rather, an “organism becomes a person as it acquires a repertoire of behavior under the contingencies of reinforcement to which it is exposed during its lifetime” (Skinner, 1993/1974, p. 228). Whereas the organism is bounded at the skin, the same is not true of the person with whom the organism is associated. Persons do not have skin! Thus, the question “Who walked?” in reference to the above example is not a behavior-analytic one. What is more, a behavior-analytic answer to the question “How did walking occur?” will point only to reinforcement contingencies and prevailing contexts and *not* to relative movements of parts of the body in relation to forces acting on them.

   In addition to being behaviorally impotent, many organisms are dismissed as illegitimate subjects in the experimental analysis of behavior. Whereas experimental analyses rarely employ invertebrates as subjects (e.g., protozoa, coelenterates, platyhelminthes, annelids, molluscs, anthropods), plants, fungi, monera, and protista are excluded as a rule. It appears that we exclude this latter group of organisms as subjects because events localized with respect to them do not submit to behavior analyses. In contrast, it is much easier to use behavior-analytic principles to understand events localized with respect to organisms whom we identify by the presence of a complex central nervous system (e.g., pigeons, rats).

   The foregoing suggests that the subjects of behavioral analyses are chosen not on biological grounds, but on the basis of their utility in generating events amenable to behavior-analytic concepts. Interestingly, this supports the view that our subject matter is not organismic activity (Bentley, 1941; Chein, 1972; Gutherie, 1959; L. J. Hayes, 1994; Kantor,
1947), but the act in context (Barnes & Holmes, 1991; Hayes & Brownstein, 1986; S. C. Hayes, L. J. Hayes, & Reese, 1988). Thus, although the act in context is ubiquitously associated with organisms possessing large central nervous systems, it does not appear to contravene radical behavioral philosophy to search for it elsewhere (cf. Hull, 1984). Perhaps prescientific judgments to the effect that behavior will only be observed in the presence of specific types of organisms, however, have ushered the organism center-stage within behavior analysis.

2. Mistaking Subjects for Subject Matter

Although the exclusive use of organisms as subjects in the experimental analysis of behavior may not be an epistemological requirement, it might still be reasonably argued that successful behavior analyses are unlikely to occur in the absence of an organism. Experience dictates that organisms must be present in order for behavioral events to be observed. To this extent, the concept of organism is a useful one in that it restricts analyses to spatio-temporal regions that bring prediction and control within reach. What is interesting in the present context, however, is that our interest in the organism does not normally extend beyond its mere presence in our laboratories (i.e., except in the unusual case when an operant is defined functionally in terms of some specific physiological change). More specifically, because behavior analysts define behavior functionally and not topographically, the physical activity of the organisms in our laboratories are irrelevant at our level of analysis. In operant conditioning treatments, for example, what is important is that responses are brought under the control of reinforcement contingencies. The topographical movements that mechanically result in the production of an operant (e.g., a lever press) are of no consequence to the behavior analyst (Skinner, 1953, p. 65). We are not so much interested in the activity of our subjects, therefore, as in the behavior that occurs while our subjects participate in our experiments. Indeed, many important behavioral discoveries, such as superstitious behavior and the scalloped fixed-interval (FI) pattern, were made without the activity of organisms being observed at all. In these cases, behavioral phenomena were identified through the abstracting out of patterns on a cumulative record upon which behavior had impacted. In effect, the status of the organismic subjects was of the same order as the laboratory equipment that contained them. The organism's role, therefore, appears to be one of mere participation in the generic context of an experiment that selects specific strands of behavioral history as relevant. Interestingly, this raises a conceptual dilemma which may be explored using the following thought experiment.

Imagine that you are led to an operant conditioning chamber in which there is a subject, and you are required to establish a lever pressing operant using food as a reinforcer. Imagine further that the chamber is normal in all respects, save for the fact that you can not see what is inside. However, you can infer what is happening to the operandum by examining cumulative records, and you can influence the activity of the operandum
with the delivery of food pellets. Now, let us suppose that having successfully brought lever pressing under the control of a fixed-ratio 10 schedule, you are informed that there is in fact no organism present in the conditioning chamber. Instead, lever presses have been controlled by a computer that simulates behavior under the schedule of reinforcement characterized by your food delivery pattern. The question now arises: Do the lever presses, which were apparently brought under operant control, constitute genuine instances of behavior? Or are these events rendered nonbehavioral by the absence of an organism in the conditioning chamber?

Perhaps the most intuitive answer to the foregoing question is that the observed pattern of events should be retrospectively dismissed as nonbehavioral on the grounds that the subject was not an organism. If we adopt this stance, however, we will always have to appeal to biological taxonomies to support our claims as behavior analysts. Furthermore, in certain cases (e.g., working with "lower" life forms) our behavioral observations may be subject to invalidation when changes in zoological taxonomies take place.\(^2\) In contrast, if we need not, in principle, consult zoological taxonomies in order to confirm the behavioral nature of the events in our laboratories, then behavior analysis is, ipso facto, not a science of the behavior of organisms.

The activity of the computer may also be rejected as nonbehavioral by appeal to pragmatism. Specifically, because the computer's limited programming will prevent us from bringing events localized with respect to it under increasingly complex contextual control, the computer makes for a poor psychology subject, as do plants, fungi, monera, and protista. Computers, therefore, like plants, should be excluded as subjects in the experimental analysis of behavior. Undoubtedly, in the interests of economy, behavior analyses should be limited to using subjects that bring interesting and relatively complex psychological phenomena into the laboratory. However, we should bear in mind that this approach also suggests that behavior analysis is primarily a science of psychological events (i.e., acts in context) and not one of the behavior of organisms.

The contradiction contained in a suggestion that behavior may occur with respect to inorganic entities (e.g., a computer) might be resolved by concluding that any entity with respect to whom we can reliably localize psychological events (e.g., operants) must, by definition, be an organism. Indeed, at least one philosopher (Searle, 1984) has argued that if computers can be built to be mistaken for human beings then they are themselves necessarily human. Ipso facto, psychologically interesting events can never be localized with respect to a computer, and by extension to any 'inanimate' object.

Interestingly, the concept of the organism must itself have arisen in ancient times by virtue of similar intuitive inductions based on behavioral

\(^2\)The classic debate concerning the status of viruses as living or inanimate entities highlights the definitional nature of arguments concerning life and matter (see Rayner, in press).
properties. Specifically, decisions as to whether something was alive or not must have been based on the behaviors (e.g., self-instigated motion) of a broad class of objects and the sense that these objects somehow possessed an independent "aliveness" not unlike that of the human observer (Hall, 1969, p. 365). Schrödinger (1944), for example, defined this aliveness in terms of the sustained disequilibrium between an organism and its environment (e.g., the activities of organisms apparently defy the laws of Newtonian mechanics). Such prescientific observations, in turn, formed the basis of folk zoological taxonomies that continue to influence taxonomies in modern day biology (Altran, 1993, p. 73). Indeed, with the relatively recent dismissal of the concept of vital forces, which pervaded the study of "life" since Greek times, the focus of modern biology has returned almost entirely to the study of life-as-action (Hall, 1969, p. 95).

The biological study of organisms in terms of action has radical implications for an organocentric conceptualization of behavior analysis. Specifically, because organisms have been prescientifically identified on the basis of behavioral properties, it represents somewhat of a tautology to define behavior in terms of whether it has been localized with respect to an organism. Thus, decisions as to whether an entity is living or dead, for instance, are based in an important sense on the scientist's ability to generate responding with respect to it. This is necessarily the case because an entity may possess all of the structural properties of an organism but may nevertheless be dead.3 Behavior is not so well characterized by its location with respect to organisms, therefore, as much as organisms are themselves characterized as the entities with respect to which behavioral events are localized. In the words of Hall (1969):

Named abstractions possess a notorious tendency to take on illusory concreteness. Inadvertently, we begin to operate conceptually as if something (life) were there, some unitary entity, agent, or act, that biologists should try to explain - whereas, what is there may be nothing more than an interactive ensemble of observed or inferable material changes. These are the things that seem to need to be explained. (p. 376)

All scientists look to regions of space-time (discrete or extended) in order to find phenomena of interest. A physicist may look to a rolling ball in order to study gravity or friction. Another scientist may look to vortexes in order to study turbulence, or to Uranium235 in order to study nuclear reactions. These scientists, however, do not mistake spheres, liquids, or the heavy elements, respectively, for their subject matter. In effect, all sciences have in common an interest in the behaviors characterized by interactions, although the various interactants of interest vary across

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3The conspicuous problem that the dead organism is as complexly organized but yet qualitatively different from a live organism has preoccupied many thinkers. Thus, biological analyses are necessarily concerned with action as well as structure (see Hall, 1969, p. 95, pp. 375-377).
disciplines (e.g., chemists use chemical interactants to study compound formation, physicists use atoms to study radioactive decay, etc.; see Kantor, 1959). From this perspective, behavior in psychology is not literally of organisms but of stimulus-response interactions occurring in specified contexts. The organism serves merely as a context for these interactions. Thus, behavior analysts should not mistake the organisms with respect to which psychological events are localized for the subject of scientific enquiry. Organisms are our subjects, not our subject matter.

3. Mistaking Activity for Action

A third reason why organisms may enjoy elevated status in behavior analysis may be that behavioral processes are assumed to ride on top of and depend upon physiological processes. Indeed Skinner (1953) viewed behavioral evolutionary processes as themselves the products of organic evolution and behavior itself as “the physiology of an anatomy” (1969, p. 173). Not surprisingly, therefore, the idea that behavioral phenomena are ultimately explained in terms of physiological processes is strongly implied by this suggestion that behavior analysis is a biological science (Reese, 1996, p. 62).

Although organismic activity may be reduced to its physiological components, this activity should not be mistaken for the subject matter of behavior analysis. Activities of organisms become interesting to the behavior analyst only when they have been conceptualized as historical acts in context (L. J. Hayes, 1994). Thus, the subject matter of behavior analysis is action rather than movement (Lee, 1988).

Although action may comprise movement, the former always extends beyond the latter. For example, walking is not merely the movement of legs. Nor can walking be localized peripherally at the point of contact between one's legs and the ground. Rather, the ground is explicitly implicated, as much as the legs, in the act of walking (Bentley, 1941; L. J. Hayes, 1994). Acts are not visible in physiology, therefore, but rather extend across space and time and include context. Psychological acts constitute field events that involve extraorganismic factors (Kantor, 1947, p. 139). Thus, behavior is to be regarded as an interaction. Paying heed to the role of the extraorganismic factors that define action, protects against the misattribution of behavior to organisms or persons (Dewey, 1957, p. 14-17).

Kantor (1959) protected against the organocentric locution of acts by conceptualizing behavior as the action of a whole organism in a whole environment. According to this view, parts of the organism considered separately from the whole do not participate in psychological events (L. J. Hayes, 1994, p. 151). Because all of the physiological changes that occur during responding are of equal importance, one physiological change can not be said to mediate another. Kantorians adopt this approach in the name of conceptual consistency (e.g., avoiding the problem of mediation), but at the cost of a loss in experimental control that may eventually lead to solipsism. Specifically, pragmatics require that in the experimental analysis
of behavior highly specific quantifiable changes be identified as responses to experimental manipulations. The lever press and key peck represent the most typical of such specified responses; however, an operant has no fixed boundaries. Thus, a muscle twitch constitutes as legitimate a response to experimental manipulations as does cooking a four-course meal (S. C. Hayes et al., 1988). Moreover, discrete physiological changes may also serve as stimuli (Skinner, 1953, p. 258). In effect, environments and organisms are sometimes necessarily partialized for experimental purposes.

Interestingly, the partitioning of behavioral sequences into stimuli and responses for analytic purposes also calls into question the organocentric nature of behavior analysis. More specifically, it appears that behavioral analyses are not so much concerned with the effects of environments on organisms as with the effects of stimuli on responses, wherever these may lie (cf. Hull, 1984). Any event can be either a stimulus or response, and these events are not defined by their location with respect to organisms, but rather in terms of their functional relation to each other. It is important to understand, in the present context, therefore, that the S-R formulation makes redundant the loose organism-environment interaction paradigm by which behavior analysis has formerly been characterized. Accordingly, the analysis of persons is one of stimulus-response, rather than organism-environment interactions. The organism and the individual, therefore, are not synonymous from a behaviorist’s point of view. Although all persons are also organisms, not all organisms are persons (Skinner, 1993/1974; Woodger, 1956, pp. 117-118).

Because organismic activity and behavior lie in distinct conceptual domains, behavioral explanations should not refer to physiological events. In Skinner's (1950) words: A behavioral explanation is not useful if it “appeals to events taking place somewhere else, at some other level of observation, described in different terms and measured, if at all, in different dimensions” (p. 193). Pharmacological effects on behavior, for example, couched in terms of underlying physiological mechanisms, can not be contacted directly by the behavior analyst. Rather such effects must be framed in terms of a more general set of environmental principles regulating behavior (Thompson, 1984). Moreover, because behavior is now widely recognized to impart direction and magnitude to drug effects (Barrett, Glowa, & Nader, 1989), the behavioral effects of drugs are considered to be a function of environmental contingencies (Blackman, 1993).

The reader would be forgiven for thinking that we have forgotten Skinner’s prediction that physiologists will one day provide an account of what is happening inside the behaving organism. In so doing, they will fill temporal gaps in historical behavioral accounts, thereby making the picture of human action more nearly complete (Skinner, 1993/1974). It is important to emphasize, however, that the temporal gap-filling, which Skinner predicted, will not represent an embarrassment for behavioral accounts. Legitimate behavioral accounts often appeal to temporally
overarching stimulus-response relations (Barnes & Holmes, 1991). This is one of the features of the contextualistic approach to science that behavior analysis typically adopts (Hayes & Brownstein, 1986; S. C. Hayes et al., 1988, Lee, 1988). Filling temporal gaps in overarching historical behavioral accounts will surely illuminate the myriad of processes that occur while an organism is behaving. However, it can shed no light on explanations delivered at the behavioral level (Reese, 1996). Behavior analysts will be deaf to the foreign terms and concepts in which the physiologist's temporal gap-filling will be couched. Nevertheless, physiological analyses may be of use to the behavior analyst insofar as they may reveal contexts in which functional behavioral relations may be more or less likely to obtain (e.g., the absence of an adrenal gland may reduce the probability of hostile behavior occurring under given conditions). When physiological information is used in this way, however, it is no longer physiological. It has now been transformed into generic context. A complete account of human action, therefore, can not be of direct relevance to a behavior analysis, although it may have behavioral implications.

The residence of behavior analysis in the domain of actions (i.e., transdermal events defined with respect to context) rather than activity (i.e., mere movement), calls into question the relevance of another nontechnical concept germaine to behavior analysis; the concept of the locus. From the outset, Skinner viewed the organism as the locus of behavior wherein contingencies come together to produce musculo-skeletal movements in the central nervous system and striate skeletal muscle (1953, p. 45; 1969, p. 173). The organism, from this perspective represents a unique location for the confluence of variables. This view is clearly organocentric and does not sit well with the idea, outlined earlier, that behavior occurs in a region wider than the skin (Bentley, 1941; Dewey, 1957; L. J. Hayes, 1994; Kantor, 1959; Lee, 1988). According to the latter view, the locus of behavior in the Pavlovian case, for example, was not the dog but the region of seen-food. If, in contrast, one adopts the perspective that organisms form the loci of psychological acts, then interesting conceptual dilemmas arise.

Consider, for instance, a person who uses a stick to press an operandum in an operant conditioning experiment. Is the locus of behavior in this case strictly the organism bounded within their skin? Or does the locus of behavior also include the stick? It would appear that if a locus is defined as the region wherein contingencies come together, then the stick must form part of that locus. This conclusion then extends itself to include many other instances of behavior. In Bentley's (1927) words:

[we have] in our body-coverings, hand-tools and weapons . . . 'outside' attachments which might well - save for our arbitrary delimitation at the rind - be functionally partitioned with the organism, quite as much as hair, claws and teeth, instead of with the environment. (p. 56)
In effect, although the organism serves as a locus of movement, attempting to constrain the coming together of psychological contingencies to that same region hinders progress by creating unnecessary intellectual digressions (Lee, 1988).

Perhaps our persistence in relating action to organisms arises from the view that a psychological analysis is simply less worthy than a physiological one. Interestingly, however, it was probably little more than an accident that happenings within the skin were studied as anything other than aspects of behavior in the first instance. Early behaviorist technology was not sophisticated enough to detect the subtle physiological changes that in time came to be known as private or mediating events for their location inside the skin rather than for their inaccessibility to observers. Consequently, intradermal activity is now often considered to underlie behavior rather than constitute it (L. J. Hayes, 1994). It is perhaps not surprising, therefore, that some behavior analysts may look directly to organisms to find behavior when the physiology that is sometimes said to mediate action is located within the skin of organisms.

4. Making Too Much of a Metaphor

A fourth possible reason for behavior analysts’ concern with the organism may be that we share a selectionist approach to our subject matter with evolutionary biologists and organic evolutionary theorists (see Catania, 1992; Chiesa, 1992; Palmer & Donahoe, 1992; Skinner, 1953, 1981, 1984, 1986). Using the Darwinian metaphor, evolutionary biologists conceptualize species as the products of selection by the reproductive capacity of individual organisms. In a similar way, behavior analysts conceptualize behavior as the product of selection of operants by the consequences of individual responses. This parallel between organic and behavioral evolution lends support to the idea that behavior analysis is a biological science (Glenn & Madden, 1995). Indeed, some authors have suggested that behavior analysis should abandon efforts to model itself on physics in favor of embracing the evolutionary biological model of science (e.g., Glenn & Madden, 1995, p. 238; see also Marr, 1996, p. 19). For instance, Glenn and Madden (1995) suggested that because evolutionary biologists have already identified the genome as their unit of replication, giving rise to retention of organismic characteristics across generations, it behooves behavior analysts to identify the unit of replication and retention at the behavioral level. Such a unit would bind operant class members to each other at the neural level and represent the mechanism by which response traits are replicated across class members (see also Donahoe, Burgos, & Palmer, 1993).

The view that selection by reinforcement, as the key insight into complex behavior, depends upon supplementation by knowledge of synaptic plasticity and neural networks (Donahoe, 1996, p. 73) creates conceptual confusion regarding the relationship between levels of scientific inquiry. Specifically, this view is essentially reductionistic in suggesting that a behavioral principle might be directly validated by information gathered
at the neural level. In fact, if the concepts from one level of analysis could speak directly to the concepts of another, then these would not be separate levels of analysis (L. J. Hayes, 1994). More specifically, an operant is an analytic concept and it exists in the conceptual domain of reinforcers and histories of stimulus-response interactions. It does not exist in a “thing-based” domain such as that in which neurones reside. To this extent, it is a fiction that operants have neural substrates that may speak directly to the behavior analyst. The organismic movements that accompany behavior may, of course, have neural substrates. As outlined earlier, however, this activity is of no interest to the behavior analyst. Activity is not behavior, and the operants that comprise behavior have no boundaries in the physical world.

Interestingly, few scientists now consider the organism to be the unit of replication at the biological level (see Brandon & Burian, 1984). Whereas some biologists consider the genome to be the unit of replication (e.g., Dawkins, 1982), others favor species as this unit (Brandon & Burian, 1984). The organism, however, rates as a poor third contender for the unit of biological selection (see Goodwin, 1994). From this viewpoint, the organism serves merely as a summary term for the collection of ways in which the genotype expresses itself. The various forms that organisms take, therefore, are rather incidental to evolutionary theory, although there could be no evolution without individual variation. In a similar manner, the operant is a unit of interest in behavior analysis. Individual responses, however, merely characterize the multitude of ways in which the operant expresses itself. The various forms (i.e., topographies) that responses take, therefore, are rather incidental to a behavior analysis, although there could be no behavioral selection without individual response variation. In effect, it appears that neither evolutionary biologists nor behavior analysts are concerned with selection processes occurring with respect to organisms. Common use of the Darwinian metaphor, therefore, does not denote a shared scientific interest in organismic phenomena.

5. Mistaking Biological Limits for Behavioral Boundaries

A fifth reason that the concept of organism might be germane to the experimental analysis of behavior may be that organic structure is thought to limit behavior. In particular, the genetic inheritance of organisms is sometimes considered to impact upon behavior in a real and direct sense. In Donahoe’s (1996) words:

In any science, phenomena are encountered at the border between the level of analysis of that science and its neighbouring sciences in which information from both sciences becomes crucial in understanding . . . At such times, understanding suffers unless the experimental analysis of the contingencies of reinforcement (and the products of such contingencies) is supplemented (not supplanted) by knowledge of the products of other contingencies (e.g., the contingencies of natural selection) at both the behavioral and other levels. (p. 72)
The idea that contingencies from other levels of analysis can directly place constraints on behavior, however, is misleading. The phenotype is the product of natural selection that determines, with respect to variations in current environments, the forms of the genome (i.e., the organism). To this extent, one might argue that organismic activity is bounded by genetics, as in the case of elephants, for instance, who can not fly, apparently for an absence of wings. Although the activity of wing-flapping may be precluded by a genetic "handicap" on the parts of elephants, however, the act of flying can not be bounded in this way. Behavioral acts are never organismic activity although they may consist of that. The limitations on topography imposed by genes, therefore, are not synonymous with limits to behavior. Nevertheless, efforts to produce flight using elephants as subjects are unlikely to succeed. From a behavior-analytic view of events, however, this can not be simply because of an absence of wings. Statements concerning biological limits to behavior (vis-a-vis limits to topography) can apply only within given ranges of biological and environmental variation. The behavior analyst, therefore, can not say what outcomes might be observed if environments are deliberately arranged (S. C. Hayes, 1993). Because statements of biological limits are based only on environments that have already been observed, it is always premature to refer to biological limits to explain the absence of behavior. In S. C. Hayes' (1993) words:

There presumably are such limits, but we cannot know when we have reached them. Extraordinary environments may still exist, and we will never know until we find them . . . Limits are not something we find based on the presence of something - it is something we infer based on the absence of something. (p. 319)

From a behavior-analytic point of view, behavior is limited only by the failure of functional relations to obtain historically, either across or within the lifetimes of organisms (see Bijou & Baer, 1967, p. 111). Interestingly, the time frame within which necessary functional relations fail to obtain may stretch back into the distant ancestry of any given organism. The births of the organisms in our laboratories, therefore, do not demarcate any particularly special time frame for behavioral explanations (e.g., Breland & Breland, 1961). The lifetimes of our particular subjects are important to us only because it is within this time frame that extended histories (i.e., of the individual and the species) can come into contact with current contingencies. In this sense, all behavior appears to be ontogenic (cf. L. J. Hayes, 1994). Perhaps for this reason, behavior is sometimes directly attributed to the organism rather than to the history associated with it.

It is perhaps understandable that behavior is sometimes misattributed to organisms when, from a biological perspective, some of the history coming to bear in every action resides within the organism in a very real sense (i.e., in their genes). Furthermore, the impact of this resident evolutionary history on current behavior is often very powerful. For
instance, it is relatively easy to establish a key peck using food as a
reinforcer and a pigeon as subject, whereas it is extremely difficult to
establish a wing-flap as an operant using the same reinforcer and subject
(Hineline & Rachlin, 1969; MacPhail, 1968; Schwartz, 1973). This effect is
widely attributed to the fact that the evolutionary history of birds favors the
foraging-like activity of key-pecking over wing-flapping in the acquisition of
food. In contrast a wing-flap is easier to establish as an avoidance
response to shock than a key-peck (Hineline & Rachlin, 1969). In this
case, wing-flapping is apparently favored over key-pecking for escape
purposes in the evolutionary history of birds.

As mentioned earlier, however, the view that history of any kind can
reside literally within organisms is at odds with a purely behavioral
perspective, in which history is conceptualized in terms of temporally
overarching functional relations. How then are we to pay respect to the
obvious influence of evolutionary history while remaining at our own level
of analysis? If we wish to increase our communication with neighboring
sciences by working with variables identified at other levels of analysis, it
appears that we need to “behavioralize” the concept of genetic
endowment (i.e., define this concept purely at a behavioral level of
analysis). In this way, we can import evolutionary variables to the
behavioral level of analysis, embedding them in generic context, as we
have done with the organism (cf. Morris, 1992). The following section,
therefore, explores one course this “behavioralization” of genetic
endowment might follow.

Bifurcations in the Behavioral Stream

Behavioral explanations that are couched in terms borrowed from
other levels of analysis are of no direct use to the behavior analyst
(Skinner, 1950). The impact of such variables as evolutionary history on
current behavior, however, is not in question. What is in question are the
terms in which this impact should be framed. Unless variables identified at
other levels of analysis are framed behaviorally, we have no way of dealing
directly with the phenomena to which they pertain. In order to take
advantage of the research findings of other scientists, therefore, we must
find ways to “behavioralize” their terms and concepts. Perhaps the most
pressing concept requiring “behavioralization” in the current context is that
of the genetic inheritance of behavioral traits. More specifically, if
behavioral explanations are to appeal directly to the impact of biological
variables on current behavior then the concept of genetic inheritance must
be imported to the behavioral level of analysis. As a starting point in
developing a behavioral metaphor for genetic inheritance, we might do well
to look to the familiar concept of the behavioral stream.

The behavioral stream is a historically extended and ongoing
succession of events. These events are not discrete, although, for
experimental purposes, the stream is partitioned into stimulus-response
interactions upon which no ontological status is conferred. Epistemologically,
no two behavioral streams may coalesce or overlap. Each represents an
intangible accumulation of acts in context that characterizes a single individual. This stream, rather than the organism with respect to which it is localized, represents the behavior analyst's subject matter; it is within the behavioral stream that stimulus-response interactions reside.

Although a single behavioral pattern can not be common to two ongoing behavioral streams, some behavioral patterns are clearly replicated across successive behavioral streams. In other words, the characteristics of behavioral streams localized with respect to particular organisms appear to be replicated in streams localized with respect to their offspring (see Oyama, 1989, p. 26). A mother's evolutionary history, for example, is captured to some extent in the behavioral stream that characterizes her infant. This appears to be what we mean as behavior analysts when we say that evolutionary history is present in current behavior (i.e., that current events are functionally related to events in the biological ancestry of our subject). Thus, the concept of the behavioral stream, when extended in this way, appears to capture the phenomenon of behavioral trait inheritance, entirely at the behavioral level of analysis. That is, strictly in terms of the replication of behavioral patterns across successive behavioral streams.

Although behavioral streams do not coalesce or overlap, the replication of behavioral patterns across lifetimes may occur by virtue of a bifurcation in the behavioral stream. A stream bifurcates when it forms a tributary. This behavioral tributary is unique in many respects, but it also represents the extension of a lineage of previously bifurcated behavioral streams (see Fig. 1).

**Behavioral Tributaries**

Soon after conception, a mother and an embryo are inseparable biologically. During this period, only one behavioral stream can be localized with respect to the mother's general location. In time, however, the embryo will develop into a fetus that will eventually become discriminable as an independent organism. Of course, the point at which an organism is considered to be independent in biological terms will vary from case to case and depend on available technology, such as artificial incubators which help to sustain life. From a behavioral perspective, however, this biological independence does not in itself bestow personhood upon the organism. Rather, the organism becomes a person only when psychologically interesting events are localized with respect to it (Skinner, 1993/1974). It is the bifurcation of a behavioral stream, therefore, rather than the birth of an organism, that signifies the onset of personhood (see Fig. 1). Furthermore, there are no limits placed on when bifurcations may occur. A stream may bifurcate long before the fetus is considered an independent organism in medical terms. Or it may bifurcate soon after birth (especially when this is premature). In any case, the onset of personhood will be determined experimentally using the fetus or neonate as subject. In effect, a lineage of bifurcated behavioral streams is not synonymous with a genealogy. The lifetime of a behavioral stream typically differs from that of the organism with respect to which it is localized.
Figure 1. Behavioral bifurcations localized with respect to members of a biological lineage (i.e., grandparents, parents, and a child). A bifurcation in a behavioral stream does not coincide with the birth of an organism. A behavioral lineage therefore runs parallel to, but is not synonymous with, a genealogy.

The bifurcation metaphor allows us to situate current events historically without direct appeal to concepts, such as genes or organisms, developed at other levels of scientific analysis. The extended behavioral stream (i.e., across lifetimes), rather than genetics or organisms, facilitates behavioral replication. Thus, patterns in the behavioral streams localized with respect to a parent may be replicated in the streams localized with respect to all of his or her offspring. These replicated patterns are inherited behavioral traits to the extent that they are functionally related to, among other events, occurrences in the behavioral streams characterizing the parent. It is not a behavioral concern, however, to determine whether these past occurrences are ontogenic or phylogenic. Past occurrences are interesting at the behavioral level simply because they can be functionally related to an ongoing stream of acts in context. Thus, the bifurcation metaphor circumvents the contradictions contained in a phylogeny/ontogeny distinction (see Oyama, 1989).

It is important to understand that events in the behavioral streams characterizing siblings will not be functionally co-related because these can not be historically situated with respect to one another. Furthermore, while bearing common strands reaching into the distant past, the streams characterizing a parent and its child will also contain unique behavioral occurrences. These particular events emerge from the coming-into-contact of partially common histories with unique environments.

A Descriptive Model

The bifurcating stream is a behavioral level metaphor for the phenomenon of genetic endowment as it bears upon psychological events. The fact that genetic inheritance may “skip generations,” therefore, does not undermine this behavioral model. Furthermore, the fact that many behavioral patterns are not replicated at all, and that others will be functionally related to events occurring in streams localized with respect to very distant ancestors, is of no consequence: The current model is not
prescriptive. The extent to which features of one behavioral stream are replicated in another, therefore, is determined experimentally. Behavioral replication can not be a matter of inheritance of any literal kind.

Staying close to the idea that the subject matter of behavior analysis is the ongoing stream of behavioral events guards against unnecessary intellectual digressions concerning the relative impact of ontogenic and phylogenetic histories, as outlined earlier. The phylogeny/ontogeny dichotomy, formerly delineated by the births of organisms, is of no consequence in the realm of behavioral streams. The histories that characterize phylogeny and ontogeny are of the same "type" within the current model. That is, they are both construed functionally in terms of temporally overarching relations with current events. The births of organisms, therefore, do not demarcate any special time frame for behavior analyses. Nevertheless, past history (i.e., the extended behavioral stream) can come into contact with current contingencies only within a behavioral tributary. In effect, the bifurcation metaphor guides us safely toward an analysis of the historical act wholly at the behavioral level of analysis.

The impact of evolutionary history on ongoing events is important to the behavior analyst (Skinner, 1966). Unravelling behavioral streams without treating them as synonymous with organisms, however, is surely difficult. Isolating an organism is the easiest way to isolate a behavioral stream. Delineations between behavioral streams, therefore, appear to be embodied in organisms. Perhaps, this has enhanced the attractiveness of the concept of organism within behavior analysis. The extended behavioral stream, however, should now properly take over the role formerly assigned to the organism. Behavioral tributaries rather than organisms, should now properly be considered to bind responses to histories and characterize uniqueness in personhood. From this perspective, discrete psychological events occurring in disparate spatio-temporal locations are of the same individual to the extent that they intangibly accumulate in the same behavioral tributary. This tributary binds acts to form persons and it is the extended behavioral stream that binds persons to form behavioral lineages.

**Conclusion**

The bifurcation metaphor serves the purpose of reducing the tendency to borrow haphazardly foreign concepts (e.g., organism, genes) without considering the usefulness of developing concepts from within our own level of analysis. In particular, behavioral bifurcations allow us to deal directly with variables identified at other levels of analysis (e.g., genes). To this extent, our capacity to work in synergy with neighboring scientists is enhanced. This is preferable to the confusion that results from the haphazard mixing of concepts borrowed from different conceptual domains (e.g., action and organisms). In the same manner, the concept of the organism becomes more manageable at the behavioral level when it has been implicitly embedded in context (cf. Morris, 1992). When foreign
concepts are "behavioralized" in this way, however, they are no longer foreign concepts and we should not expect their authors to recognize them. In effect, the role of the organism as a biological entity at the behavioral level is diminished, perhaps entirely, by the bifurcation metaphor. Doubtless, the present analysis will seem heretical to some on these grounds alone. Nevertheless, the current endeavor towards consistent system building within behavior analysis is pragmatically guided by the promise of increased communication with neighboring and ultimately distant sciences.

References


