SEEKING THE NATURAL LINES OF FRACTURE: A REVIEW OF
THOMPSON AND ZEILER'S ANALYSIS AND INTEGRATION OF
BEHAVIORAL UNITS¹

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A conference was held at the University of Minnesota in the fall of 1984 to recognize Kenneth MacCorquodale's significant contributions to the experimental analysis of behavior. The events of the conference have been recorded in a volume, *Analysis and Integration of Behavioral Units*, edited by Thompson and Zeiler (1986). As stated in the opening remarks, few of the people involved in behavior analysis have spoken so eloquently for the behavior-analytic approach as has Kenneth MacCorquodale. His many students and colleagues consistently praise his scholarship and his personal example.

The conference was addressed to an area of inquiry central both to MacCorquodale's thinking and to the foundations of behavioral theory—namely, the question of the appropriate units of analysis of behavior. When Skinner first considered a general theoretical account in which behavior would serve as a scientific datum, he was concerned that behavior be treated as significant in its own right and not merely as a manifestation of underlying processes or entities. Skinner's philosophy of radical behaviorism, implicit in his solution to the problem of units of analysis, circumscribed and defined the domain of operant behavior. The substantial practical and technological advancements that followed from the development of operant theory were directly related to Skinner's unique solution of the units problem.

Critical analysis of Skinner's treatment of behavioral units should contribute importantly to progress in contemporary behavior analysis. But some critics, especially theorists trained

outside of the area of natural-science psychology, do not understand the difference between operant and S-R behavior theories, and thus confuse operant theory with the fundamentally different older behavior theories based on associationism. In a noteworthy and spirited defense of the operant analysis of verbal behavior that addressed such criticisms, MacCorquodale's arguments hinged on the units-of-analysis question (MacCorquodale, 1969, 1970). On the other hand, there are theorists sympathetic to the aims of behavior analysis who try to work within the broad theory of operant behavior, while at the same time trying to learn its strengths and weaknesses.

It is refreshing to encounter a volume that addresses theoretical problems in behavior analysis without the distortions that characterize many contemporary accounts. The contributors to the volume *Analysis and Integration of Behavioral Units* are drawn from those who have contributed substantially to behavior analysis, or are at least sympathetic to the behavior-analytic approach, and who work to enhance and improve our understanding of behavior through the behavior-analysis framework. Although the contributors may have concerns about the actual implementation of analyses with certain types of units, all consider the units problem as worthy of consideration.

CONCEPTUAL BACKGROUND OF
THE UNITS OF ANALYSIS
PROBLEM

Several thorough treatments of Skinner's approach to units of analysis have appeared over the past three decades—for example, the excellent accounts by Verplanck (1954), Staddon (1967), and Catania (1973). To provide the appropriate context for the present dis-

¹ Thompson, T., & Zeiler, M. (Eds.). (1986). *Analysis and integration of behavioral units*. Hillsdale, NJ: Erlbaum. xvii + 367 pp., including references and index.

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cussion. it is necessary only to briefly summarize the analysis of behavioral units that appears in Skinner's early works (e.g., Skinner, 1931, 1935, 1938).

Skinner's earliest concern was to construct a science of behavior that would permit the development of valid laws. At that time, the reflex served as a powerful model for those interested in the scientific approach to psychology, including Skinner. But Skinner believed that the reflex model, as traditionally conceived, accounted only for behavior of a limited type. By means of operational analysis, Skinner redefined the reflex more broadly (Skinner, 1931).

The defining characteristic of the reflex was the observed *correlation* between stimulus and response. Skinner therefore argued that the reflex was the general *form* of description of the functional relations between the events acting on the organism, and the responses of the organism to those events. In that formulation, scientific investigation of behavior involved identifying reflex units (i.e., observed correlations between stimuli and responses) and identifying changes in these correlations due to the operation of what were termed third variables (i.e., antecedent conditions that affected the observed correlation).

Skinner further suggested that types of units could be identified on the basis of static and dynamic laws applicable to each. The static laws were those governing single occurrences of the reflex, whereas the dynamic laws were those governing repeated reflex occurrences. In addition to the classic dynamic laws of reflex activity, Skinner included the laws of conditioning and extinction of Type S, which were meant to account for the elicited behavior studied by Pavlov. Skinner called this type of unit a *respondent*, and the dynamic laws governing such units the laws of *respondent conditioning*. Skinner called other dynamic laws appropriate to emitted or spontaneous behavior the laws of conditioning and extinction of Type R. The law of conditioning of Type R was Skinner's version of the law of effect. The consequent event that defined the unit was called a reinforcer, and the unit itself was called an *operant* (Skinner, 1938).

In addition to providing a taxonomy of reflex units, Skinner gave an account of how to discover the defining properties of the stimulus and response that yield an analytic unit.

The key to this account is to recognize that the terms "stimulus" and "response" refer to *classes* of events (Skinner, 1935). Thus, the unit of analysis must describe a correlation between these classes. Moreover, the appropriate level of analysis at which the defining properties of these classes appear must be discovered through observation of the obtained functional relations. The defining properties of these classes cannot be deduced in advance.

Skinner maintained that the correlation of classes of stimuli and of responses could be defined at levels of specification marked by the orderliness of dynamic changes. For example, after establishing a functional relation between aspects of behavior and of environment, one can progressively restrict the defining properties of both. Some of these restrictions lead to a more orderly form of dynamic function relating behavior and environment, while others do not. At some point before all the properties of the stimulus and response classes have been specified, there will be no further increase in the consistency of the relation between the classes. This point of specificity, where the dynamic changes between stimuli and responses approach an *optimal* degree of orderliness, ultimately determines the appropriate level of analysis (Skinner, 1938).

This method identifies the operant as an analytic unit rather than as a theory (see also Verplanck, 1954). The task is to identify analytic units along the so-called "natural lines of fracture" (Skinner, 1935, p. 40). Behavior analysis does not reduce behavior to predetermined simple elemental units. Instead, the unit is inferred from observation and manipulation of controlling variables. A given unit of behavior is uniquely defined at a given level of specificity, but there is no ultimate level of analysis. Many valid levels of specification exist and depend solely on observed functional relations.

Because the terms "stimulus" and "response" appear frequently within these analyses, operant theory has often been inappropriately identified as an "S-R approach" (Verplanck, 1954). Nevertheless, operant theory leaves the defining properties of the analytic units open-ended. Skinner used such terms as "optimal level of uniformity," "degree of community of functional properties," "smoothness of curves," and "levels of spec-

ification marked by orderliness of dynamic changes" to describe the means by which analytic units are discovered. He did not have a single criterion for "orderliness," nor was one possible, because the actual laws of behavior remained to be discovered (cf. Staddon, 1967). By so broadly defining the area of inquiry, Skinner made the work of the investigator more difficult, but at the same time made the potential and lasting benefits of the analysis much greater.

THEORETICAL CONCERNS WITH UNITS IN BASIC BEHAVIOR ANALYSIS

Given this summary of Skinner's approach, we may proceed to examine some of the issues addressed by the contributors to the Thompson and Zeiler volume. Most chapters,² though not all, address in a more or less direct way the units problem that originally concerned Skinner. Nevertheless, there is considerable diversity of opinion and direction in individual contributions. Although it is not possible to do justice to all issues raised by the authors, it is reasonable to summarize and examine a few theoretical issues that cut across and are interwoven in the chapters.

In his background chapter, Thompson identifies the problem of units of behavior analysis as the problem of structure in behavior. Despite its unfortunate connotations in psychology, structure is the appropriate term in that it emphasizes not only organization, for which "pattern" would suffice, but also invariances. One of the first questions to arise concerns the number of different types of analytic units we need to consider in behavior analysis.

THE OPERANT/RESPONDENT DISTINCTION AND UNIT TAXONOMIES

Although Skinner's original distinction between respondent and operant units still has some practical utility (e.g., Pear & Eldridge, 1984), his initial basis for the distinction seems to have been thoroughly discredited, as discussed by Hineline. Two different views emerge concerning how many types of units should be employed in behavior analysis.

Lubinski and Thompson believe that Skinner's original taxonomy should be expanded to include three types of units, while Hineline argues that two units are enough if the controlling variables are properly defined.

Lubinski and Thompson suggest three basic types of response classes, which they call elicited, emitted, and evoked. Elicited behavior seems to include all forms of behavior dependent on antecedent events, such as classic reflexes, kineses, or fixed-action patterns. Emitted behavior continues to be the domain of operant behavior, defined by consequent events. The new type, which they call evoked, is introduced to encompass schedule-induced or adjunctive behavior, as well as instances of polydipsia, interim behavior, or displacement activities.

This taxonomy has a surface plausibility based on Skinner's criterion of establishing differences in dynamic laws, but its utility in organizing observations of behavior is unclear. The taxonomy may inappropriately group kinds of behavior together without considering important differences within the group. One example is that of treating classic ethological displacement behavior as of the same behavior type as schedule-induced polydipsia. There seems sufficient evidence that displacement usually results from conflict between two more highly probable response classes, such as fighting and fleeing, and thus may be a more nearly static phenomenon than polydipsia, which relies on a specific history of exposure to temporally organized reinforcers, or other inducing events. This difference may be missed if a global label, such as "evoked," is used to describe both types of behavior. Another example is the case of so-called orientation mechanisms, such as kineses, taxes, and tropisms. The well developed theory applied to these types of behavior is lost by merely calling all instances "elicited" (see Staddon, 1983).

Hineline's proposal is more radical, in that he argues that there need be only two broadly defined types of unit, elicited and emitted. Emitted behavior again follows the convention of being defined by its consequences. On the other hand, Hineline takes the unusual step of defining induced behavior by means of exclusion—that is, it is behavior other than that shown to be sensitive to consequences, with elicited behavior being a special case

² Chapters are identified by authors' names; see Appendix for chapter titles.

within that category. Although this proposal may seem to mix too many heterogeneous types of behavior, there is at least one aspect of the proposal worthy of consideration.

The notion of only two types of units is made possible by liberalizing our usual view of behavioral processes in a fashion consistent with Skinner's original approach. That is, if we regard our units as extended over time, rather than as discrete events, then induced behavior can be treated as resulting from sequential patterns of events whose occurrence is independent of behavior. The analogy drawn by Hinson is with the physical concept of resonance. For instance, adjunctive responses such as those in polydipsia can be viewed as functioning in a resonance relation with respect to food deliveries arranged according to some schedule. The virtue of this suggestion is that it predicts an optimal resonance frequency between induced and inducing events, which is in accordance with the observed bitonic relation between schedule-induced behavior and frequency of presentation of the reinforcer. Although only a tentative suggestion at the moment, the resonance concept does suggest more flexible approaches to schedules, illustrated by an adjusting schedule described by Hinson.

BIOLOGICAL CONCERNS IN THE DEFINITION OF RESPONSE UNITS

Well beyond the refinement of taxonomies, Zeiler suggests an even greater departure from convention as he considers response units in the context of temporal differentiation of behavior. Zeiler summarizes and analyzes an extensive literature and contrasts two approaches: using temporally differentiated behavior as an analytic unit and interpreting temporal differentiation as time estimation. At the end of the review, he finds both approaches wanting and argues instead for optimal foraging as an integrative principle.

In some ways, optimal foraging theory departs noticeably from traditional behavior analysis. As described by Zeiler, one of the key assumptions of optimal foraging theory is that natural selection is a maximizing process in which the relevant dimension maximized is Darwinian fitness. Optimality occurs with respect to relevant biological variables such as energy expenditure or energy cost. Further, behavior is held to be optimal only

with respect to some set of constraints. From this point of view, a reinforcement schedule can be viewed as modeling a foraging environment. The experimenter's task is to find the aspects of the environment with respect to which behavior is optimal (see also Krebs & Davies, 1978; Pyke, Pulliam, & Charnov, 1977).

It is difficult not to be impressed with reports of workers in the area of optimal foraging. The ingenuity of their analyses is often matched only by the orderliness and parsimony of their obtained results (e.g., Charnov, 1976). Also, the clear and sensible description of behavior in terms of adaptive function is admirable. Nonetheless, one may easily forget that even in evolutionary biology there is still vigorous debate about the units of natural selection. Gene-frequency models that specifically exclude maximizing assumptions are at least as popular, if not more popular, than optimality models (Gould & Lewontin, 1979).

One can interpret optimality analysis as a new approach that adopts the technical achievements of operant theory, such as reinforcement schedules, while divorcing itself from the explanatory apparatus of operants, reinforcers, and discriminative stimuli. Instead, one would introduce new explanatory or integrative concepts such as search rules, strategies, or heuristics. On the other hand, one can use optimality analysis as a *means* of identifying the important controlling variables that define functional operant units. In the latter case, we maintain the fundamentals of operant analysis without giving up the potential benefits of optimality analysis. In either case, it is important to remember that to analyze behavior with respect to some maximizing criterion does not tell us *how* that outcome is achieved. That is, maximizing is an outcome, not a behavioral process or mechanism (Staddon & Hinson, 1983). It is not the animal who is the optimizer or the satisfier, but the experimenter may well be.

LEVELS OF ANALYSIS AND BEHAVIORAL UNITS

The appropriate *level* of analysis of behavioral units has been a persistent problem in behavior theory. Meazzini and Ricci trace some of the history of the problem of molecular versus molar levels of analysis. They share with many others the conclusion that the

problem cannot be resolved definitively. Even so clear an example as Guthrie's distinction between movement and act was unable to survive. Consistent with Skinner's analysis, the conclusion is that analytic units may be defined at broader or narrower scales of analysis. Neither the molar nor the molecular level is more fundamentally important, because order may emerge at many levels. The question of the *appropriate* level of analysis most often depends on our aims and on our ability to manipulate properly the relevant controlling variables (Morris, Higgins, & Bickel, 1982).

Controversy over levels of analysis probably will continue as researchers try to determine empirically the "optimal degree of specificity" with respect to behavioral units. Over the last two decades this has been manifested in the controversy between matching and maximizing theories of choice (Baum, 1981; Herrnstein, 1970; Shimp, 1966). Despite the sometimes divisive nature of the conflict, this debate has been beneficial in telling us more about the time scale over which behavior is integrated, and about the relative importance of rate and probability variables in the control of behavior (Hinson & Staddon, 1983; Peele, Casey, & Silberberg, 1984; Silberberg & Ziriax, 1985; Vaughan, 1981). Moreover, the study of levels of analysis has encouraged new quantitative analyses that are more consistent with the original aims of operant theory. Rather than developing self-perpetuating models that tell us little about behavior, the newer quantitative approaches typically describe and organize behavioral observations to identify higher-order invariances that appear in diverse situations (Nevin, 1984). Although no *ultimate* level of analysis will emerge, our understanding of behavior at various levels of analysis will continue to improve.

A less direct issue related to the levels-of-analysis question is addressed by Schnaitter in comparing explanations based on mechanistic, mentalistic, and behavioristic approaches. In his analysis, Schnaitter summarizes some modern mentalistic approaches as kinds of functional accounts of the machinery of behavior. This assessment seems particularly accurate with regard to the new "faculty psychologists" who try to combine cognitive, functionalist, and artificial-intelligence approaches (e.g., Fodor, 1983). Part

of the rational justification for the new cognitive approach, pointed out by Schnaitter, is the erroneous belief that behavior is stimulus-independent, or underdetermined.

This misunderstanding of behavior analysis follows from the assumption that the functional description of behavior is equivalent to specifying a sequence of states of a finite-state machine. The claim made is that the behavior of the machine (organism) cannot be understood (predicted) without reference to the internal state of the machine. The error is one result of the assumption that there is one fundamental level at which behavior is integrated over time, with the current behavior (or state) of the machine somehow temporally independent of prior behavior. Clearly, an operant can be understood without reference to an internal state, but only if reference is made to an environmental history. The simple fact that contingencies extend over greater or lesser amounts of time, implicit in the concept of the operant, is overlooked by those who use machine intelligence as their model of behavior. Problems arising from failure to consider the extended nature of contingencies seem to occur repeatedly in artificial intelligence. Until machine designers get away from the idea that behavior must be subdivided into discrete machine states that are connected by a sophisticated rule-guided process, it appears the goals of artificial intelligence are likely never to be realized (Dreyfus, 1979; Dreyfus & Dreyfus, 1986).

Another issue of levels of analysis raised by Schnaitter, though less directly, is the relation between the machinery of behavior and behavior itself. This has been especially important for radical behaviorists who are eager to avoid any hint of reductionistic explanation. But in avoiding reductionism, some investigators fail to realize that the study of the nervous system is part of the study of behavior. One of the most cogent advocates of this position is Kennedy (e.g., 1954, 1967), an experimental biologist who presents arguments strikingly similar to those made originally by Skinner (1938). The study of neural mechanisms involves the same analytic methods as those for the study of the reflex preparation. The functions of structures in the nervous system are also understood by changing the levels of specification of stimuli and responses (see Kennedy, 1967). And, just as

there are different levels of analysis in overt behavior, so too are there different levels of analysis of equal validity in the nervous system. As there is no ultimate level of analysis in overt behavior, one cannot expect to understand all levels of nervous integration by reference, for instance, only to single-unit recordings. Failure to follow the functional approach, which is necessary to explain behavior at *whatever* level of integration, leads to such mistakes as attributing the organization of behavior to a parallel, counterpart, internal organization consisting of mental entities, instincts, or neural motivational centers (Kennedy, 1954).

OPERANT UNITS IN THE ANALYSIS OF COMPLEX AND SYMBOLIC BEHAVIOR

The application of operant analysis to symbolic behavior such as language has developed more slowly than other areas of inquiry. Skinner's hypothesis on the functional organization of language has provided the primary impetus for studies of symbolic behavior (Skinner, 1945, 1957). More recently, several investigators have developed methods to experimentally manipulate symbolic behavior.

OPERANT ANALYSIS OF VERBAL/ABSTRACT BEHAVIOR

One example of the operant analysis of symbolic behavior is reported by Catania and Cerutti, who attempted to produce something comparable to productivity in language (Catania, 1984) in the nonverbal behavior of pigeons. Specifically, one source of novelty in verbal or nonverbal behavior may be a novel combination of stimuli that control different aspects of behavior. In their experiment, Catania and Cerutti separately established different response properties under the control of different stimulus properties. For instance, response rate (i.e., fast or slow) and response position (i.e., left or right and up or down) were shown to be brought under the control of the separable dimensions of a stimulus array comprising color, form, and number. Within the limited context of their experiment, Catania and Cerutti show that the verbal property of productivity could be demonstrated in nonverbal behavior. Their pigeons' behavior could be described as a generalized

behavior class. Novel behavior was produced by combination of separate classes of stimuli controlling separate aspects of behavior, although significantly, novelty in this case was not simply produced by shaping. In addition, they illustrate the difference between relaxation and elimination of contingencies in establishing these classes. Finally, they provide a reference experiment showing that structural aspects of behavior do not distinguish verbal from nonverbal behavior.

Sidman addresses the operant analysis of abstract behavior by systematically increasing the size and complexity of the unit of analysis, going from a two-term contingency to a five-term contingency. The interesting aspect of this analysis is that it is easier to recognize the potential complexity of behavior at each level of integration. For example, Sidman points out that the move from the two-term to the three-term contingency provides not only for discriminative stimulus control, but also for conditioned generalized reinforcers. This level of control provides for a great deal of independence from specific situational determinants of behavior. The further step from the three-term to the four-term contingency provides for conditional stimulus control. Conditional stimulus control is, in a sense, the unit of control-by-context, which is clearly important in much of human behavior, and which appears to be difficult to account for in formal models. In addition, the four-term contingency provides for the existence of equivalence relations, which are demonstrated by the properties of reflexivity, symmetry, and transitivity in the relations between stimulus instances.

Both Sidman's and Catania and Cerutti's chapters provide demonstrations of the application of operant analysis to significant experimental problems, showing that the definition of the operant is sufficiently open-ended for the analysis of symbolic behavior, without appeal to a different interpretive domain. At the same time, implications for other areas of research interest in symbolic behavior were not addressed. One such area is the line of research concerned with language or language-like behavior in infra-humans (Premack, 1971; Terrace, Petitto, Sanders, & Bever, 1979). Much of the recent controversy surrounding this work deals with the methodological soundness and interpretive accuracy

of the results obtained (Umiker-Sebeok & Sebeok, 1981). A crucial concern and major difficulty is with demonstrating syntax in the manipulation of symbol systems. Many studies start with a system of few or unknown syntactical properties, and proceed to make inferences on the basis of informal or poorly defined measures of performance (Seidenberg & Petitto, 1981). Other studies that attempt rigorous measurement are easily criticized for other methodological flaws. The general implication of Catania and Cerutti's work is that syntax is not the dividing line between verbal and nonverbal behavior. Thus, the emphasis placed on existence proofs for syntax in symbol usage by animals may be misplaced. It would probably be better to try to examine or create some aspect of verbal behavior, such as productivity, by means of experimental analysis than to try to prove its existence statistically.

Another related area is that of defining the stimulus in complex perceptual concept learning. It has been repeatedly demonstrated that behavior can be controlled by seemingly abstract concepts such as person, fish, or leaf. Nevertheless, the basis for definition of the stimulus is still disputed. One intriguing suggestion is that discrimination is organized on the basis of recurrent events and natural objects, that is, that the natural lines of fracture are the invariances represented by objects in the natural world (Herrnstein, 1982). An alternative view, however, is that apparently complex discrimination actually may be based on much simpler analyses of visual features (Cerella, 1982; Vaughan & Greene, 1984). Sidman's description of conditional stimulus-control procedures is a good candidate for the evaluation of these conflicting views. Trying to establish equivalence relations on the basis of stimulus exemplars or specific features believed to determine discrimination would be a decisive step toward resolving these conflicting positions. If, for instance, reflexivity can be demonstrated to hold between each of the members of a presumed natural category, such as each of a number of instances of leaves, the case for the existence of such organization is that much stronger.

OPERANT ANALYSIS OF HUMAN BEHAVIOR

The conceptual apparatus of behavior analysis was originally offered as sufficiently gen-

eral to apply to all forms of behavior without theoretical translation (Verplanck, 1955). Unfortunately, so-called explanations of human behavior have often been based on superficially plausible, but logically flimsy, analogies or "just-like stories." It is convenient to tell an undergraduate that the behavior of a one-armed bandit player is "just like" the behavior of a pigeon under the control of a variable-ratio schedule. But pigeon analogies quickly fall short if we try to explain anything other than highly specific, repetitive response patterns. Several contributors to this volume analyze problems related to units of analysis for human behavior.

Harzem describes some general problems that may arise in the behavior-analytic approach to human behavior. He points out that operant analysis typically examines simple acts instead of the more intrinsically interesting sustained patterns of action. Simple acts are easier to identify or record, making the corresponding control by imposed schedules much more precise. This has encouraged the use of simple acts such as button pushing in experimental studies to serve as surrogates for more complex, less easily controlled activities. But we may ask why better methods have not been developed to study complex behavior directly in experimental settings. And, as a corollary, we may ask why complex behavior has seemed so resistant to ordinary experimental analysis.

Part of the answer, according to Harzem, lies not in the behavioral phenomena but in our ordinary language. This involves what Harzem refers to as the "language trap." Simply put, "trap" occurs because we speak as if interesting forms of human behavior exist in unitary and consistent forms, when in fact they do not. In order to overcome the "language trap," Harzem suggests either relating simple acts to more complex acts by means of theory, or directly observing complex behavior in its ordinary environment.

Similar concerns are raised by Baer in his treatment of frequency measures of behavior units. Frequency has been the measure of choice in laboratory studies of operant behavior for both theoretical and practical reasons. And as Baer points out, it is a seemingly inevitable choice. But for those interested in practical applications, the free operant may not represent the most useful model. Much

behavior observed in applied settings does not have the frequency range of well chosen operants observed in the laboratory. Hence, measurement and control are not based on frequency.

The more common measure of behavior in applied settings is the time filled by the specified behavior patterns. Continuous-time recording of behavior during specified intervals is also a norm. Obviously, any contingencies applied in these settings must necessarily be based on time devoted to the activity. For instance, a positive-reinforcement contingency would be designed to increase the amount of time filled with the specified behavior. A further consideration is getting observers to agree on when the behavior in question occurs. People observing behavior in social contexts seem to have difficulty using discrete measures accurately, and a rate measure is therefore ruled out from the beginning.

The questions raised by Harzem and Baer are important ones if behavior analysis is to achieve its promise in the domain of human affairs. One virtue of operant analysis is its direct application to meaningful human actions, for the relations that define an operant are its meaning. It is important to note, however, that Harzem's language trap is not a new phenomenon. In fact, it seems to be an instance of the general problem noted by Skinner (1938) of trying to properly describe and explain behavior with a language infused with "metaphor and implication." When facing this problem, an odd dilemma arises. The terms of ordinary language seem quite suitable for everyday purposes, yet these terms are obviously imprecise and unsuitable for scientific analysis of behavior. Precision of terms often is increased by simplification, which in turn reduces the inherent interest in the phenomena. As difficult as it is, the only sensible approach seems to be direct experimental analysis of human behavior. The alternative of trying to relate representative acts studied in the laboratory by means of theory to ordinary human behavior is still unrealized (Miller, 1983).

Despite the complexity of the issues involved in the experimental analysis of human behavior, one aspect of the problem can be examined in the present context. That aspect is the relative utility or practicality of measures based on number of discrete responses versus measures based on time. Although response

rate has been an important and useful dimension along which to measure behavior, we need not be inviolably wed to it. Two derivatives of basic experimental analysis support this view. One is that time-based measures of behavior provide a more nearly optimal specification of behavioral allocation at the molar level (time matching, de Villiers, 1977) or at the molecular level (melioration, Herrnstein & Vaughan, 1980) than do number-based measures. The other is that several attempts to explain reinforcer relativity focus on time as the common dimension along which the reinforcing value of behavior should be arranged (e.g., Premack, 1965). The message is that the relative efficacy of response-count and response-time measures should be given greater emphasis in future research.

TRAIT LANGUAGE AND RESPONSE CLASSES IN HUMAN BEHAVIOR

In much more global and systematic terms, Lubinski and Thompson address the question of the appropriate natural units in the continuous stream of human behavior. For their purposes, identification of one or a few functional units is clearly inadequate. Instead, they want to develop a general classification that will deal with behavioral integration of large numbers of behavioral units, which in their terms are traits or trait classes.

After laying some necessary groundwork, Lubinski and Thompson suggest four basic levels of behavioral integration: (1) fundamental response classes; (2) behavioral combinations; (3) traits, or response families; and (4) trait clusters. Although the first two levels are unremarkable, the second two are not ordinarily used by behavior analysts. Lubinski and Thompson argue that trait concepts are necessary to deal with the composition of larger and more diverse behavioral classes. That is, they argue that large and small functional units appear to be regulated according to different principles. Moreover, particular units may exist in states of strength that are not always observable. Trait clusters are meant to deal with an even higher level of integration, in that trait clusters represent two or more traits that covary for individuals. Examples of this form of functional organization are general intelligence (*g*), and the clinically observed covariation of sexual arousal and neurotic avoidance.

Like Baer in his chapter, Lubinski and

Thompson note that response-rate measures are often inappropriate for measuring human response tendencies, especially in cases of psychopathology. For example, the mere frequency with which a response occurs may not be as important as the kind of response, however infrequently it occurs (e.g., behavior with salient social consequences like sexual assault). Lubinski and Thompson suggest that psychological tests that discriminate response patterns or significant groups, at least those based on external structure, may serve as an alternative to probability estimation of troublesome behavior classes.

Along the same lines, Meehl discusses the use of trait language by behavior analysts. He lays out a scheme for classifying traits and considers which uses should be inoffensive to or consistent with behavior analysis. He divides traits into three types: (1) semantic pseudo-traits; (2) surface traits; and (3) surface quasi-traits. The first type have content similarity but lack empirical covariation. The second are true response families that possess both content similarity and covariation. The third type are response families that lack content similarity. In this scheme, surface traits and quasi-traits are attributed to latent sources that can be broken down into four sorts: (1) historical events inferred but not actually observed; (2) current schedules that are presumed to be operative but are difficult to identify; (3) parameters of functions relating traits to controlling variables, which would probably be specific to an individual; and (4) postulated internal entities, events, states, structures, or dispositions, which are mostly nontraits.

The major questions of interest are to what degree traits are legitimate descriptors of behavior, and to what practical use they can be put in behavior analysis. Behavioral-unit descriptors are ordinarily justified to the extent we are able to employ them in a variety of functional relations (cf. Thompson & Lubinski, 1986). But terms that are theoretically rich and seem to reflect the diversity of human experience may be empirically unreliable.

Trait terms seem worthwhile to those who use them, but may be of less interest as theoretical entities. For example, Meehl argues that what he calls surface traits are response families. That is, they are characterized by shared content and empirical covariation. But the salient question concerns how empirical

covariation is defined—it is certainly not on the basis of direct measurement of behavior. And even when a trait characterization provides important information, it may be inadequate evidence for the existence of the so-called trait. As an obvious example, while educated males consistently score a high 5 on the MMPI, this hardly justifies the trait label "Femininity."

Taking a page from Meehl's book, this point can be illustrated in another way by an anecdotal account of a friend's induction as a draftee into the army during the sixties. Having exhausted his deferment possibilities, he appeared at an induction center before a sergeant who wanted to know about the inductee's education. The inductee recounted that he was graduated *summa cum laude* from Harvard, followed by graduate study at the University of Pennsylvania, and eventually by a PhD from Columbia. At the end of this recitation, the sergeant reached for an inked rubber stamp and stamped the inductee's file "LITERATE" in bold, red letters.

The lesson is that for certain purposes even a crude division among exemplars may be appropriate, and even obvious, to those who employ it. But this does not provide compelling evidence for a general practice. It would be unfair to be too critical of those whose practical daily concerns are with the improvement of human welfare. And to the extent that certain theoretical terms provide an easier or better way to deal with serious human problems, they should be employed without reservation. Nevertheless, the burden of proof still remains with the practitioner to show that the terms are valid beyond the limits of specific practice. Skinner's arguments against explanatory schemes that employ dispositional terms such as traits, abilities, or needs still remain valid (e.g., Skinner, 1956).

In most cases, the major problem seems to be one of measurement. Theoretical terms used by clinicians or personality theorists are seldom grounded firmly in direct measurement of behavior. Instead, they achieve rigor by being defined operationally in terms of psychological tests and complex statistical inference. Psychological testing is often proposed as the only practical form of measurement, as it is impossible for the therapist properly to observe an individual client in a natural environment on a daily basis over sufficiently long periods of time. But other options based

on ordinary principles of behavioral analysis are available. For example, one might try to teach people to be better observers of behavior. This includes teaching better observation and recording of the behavior of others who may have problems potentially requiring intervention, or teaching the observing and recording of one's own behavior to increase the quality of self-management programs (Brigham, 1982; McConnell, Biglan, & Severson, 1984). Even a simple improvement in observation, like counting instances of behavior, can have a dramatic impact on the efficacy of self-reports.

CONCLUSION

Analysis and Integration of Behavioral Units is a diverse collection of articles, and would be difficult to summarize adequately. I have examined only limited aspects of the questions addressed by the contributors, specifically those related to Skinner's original analysis of units of behavior. The book also extends into areas not ordinarily considered by behavior analysts, such as Marr's chapter on mathematics and verbal behavior, and Falk's chapter on the formation of ritualistic patterns of behavior. Perhaps the best way to describe the book is to say that it reflects the diversity of interests that exist within behavior analysis. Also, even a cursory examination of the book reveals that the units-of-analysis question remains current and pertinent. This volume also marks a symbolic transition point. As the book was going to press, Kenneth MacCorquodale died in his home at the age of sixty-six. Those who share Kenneth MacCorquodale's interest in the conceptual framework of operant behavior should examine the book, not only for the answers provided, but also for the questions raised.

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APPENDIX

Analysis and Integration of Behavioral Units
Edited by Travis Thompson and Michael D. Zeiler

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