EDITORIAL

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Editor

The history of science consists of the continual clarification through experiment and reason of the language of science. Concepts evolve by gaining properties and losing others, by becoming differentiated from other concepts, and by changing their domain of applicability. Each concept is always embedded in a system of concepts and, within a system, the interrelations among concepts—of force, mass, and acceleration, of pressure, volume, and temperature, of probability of detecting a signal, $d'$, and beta, or of response rate, reinforcement quantity, and delay—are the first targets of a scientist’s investigations. In fact, the meaning of a scientific concept is, to a large extent, given by the network of interrelations in which the concept participates.

In relatively young sciences conceptual networks are rarely explicit; large parts of them are tacitly assumed and for long periods of time may remain essentially unexamined. The familiar is often taken for granted, even when poorly understood. An important role of the philosophy of science is to invite scientists to analyze their tacit networks. In the process, ambiguity may be exposed, missing steps in arguments identified, and unwarranted assumptions revealed. Attempts to solve these problems and improve the network can then take place.

In psychology, sometimes the most surprising outcome of the analysis is the replacement of a seemingly defective network by another that is hardly better. To illustrate, Kohler believed that trial and error learning, as studied by Thorndike and others, could not account for the creative, problem-solving behavior of the chimpanzees he studied in the Canary Islands. More generally, the concept of association (between a response and a consequent stimulus or between two successive stimuli) was not powerful enough to deal with the properties of the behavioral solutions his chimps displayed without explicit training. Unfortunately, Kohler’s proposal of insight learning was not any better. In fact, according to Pavlov (1957; see also Machado & Silva, 2003) it was worse because it ignored the essential elements of the situation—the actions of the chimp—and transformed their amazing performances into mysterious abilities:

His [Kohler’s] sole fundamental, but peculiar proof is this. When the ape is given the task of taking hold of fruit suspended at a certain height, and when for the purpose of accomplishing it he needs definite instruments, for example, a stick and some boxes, all his unsuccessful efforts to get the fruit are not, according to Kohler, proof of intelligence. This is simply the method of trial and error. When the ape becomes tired, as a result of his unsuccessful efforts, he...
gives up and remains for some time in sitting posture. When he has rested he tries again and succeeds in accomplishing his task. According to Kohler the ape’s intelligence is proved by the fact that he sits for a period without doing anything... It turns out that nothing but the silent inaction of the ape proves its intelligence!... For it is the processes disregarded by Kohler that are of greatest importance. I grasped and realized this while observing the behavior of the ape. And I say that all this activity of the ape in trying now one, now another way of solving the task, is the intelligence, the reasoning in action, which you can see with your own eyes. This is a series of associations; some of them have been acquired in the past, others are formed before your eyes, and are either combined, united into a positive whole, or, on the contrary, are gradually inhibited and lead to failure. One can clearly observe the manifestation of some of the associations formed earlier in the ape, in the course of his life in the jungle, in his native surroundings. (pp. 558-560)

History repeats itself. One study (Blaisdell, Sawa, Leising, & Waldmann, 2006) concludes that associative theory cannot explain a set of empirical findings and proposes, as an alternative, the view that rats are capable of causal reasoning. This means, among other things, that rats have the ability to understand that the manipulation of an effect cannot affect its cause, that exposure to a sequence of events (e.g., a light and a tone) is the training of a causal link, that rats treat absent but expected events as possibly present but missing, or even that rats integrate individual learning relations into coherent models. The problem is that, except in the most general way, the actual details of the rat’s behavior are not derived from the new system of concepts. Moreover, the system coordinates concepts in ways not justified by experiment (e.g., on what basis do we believe that rats know that effects do not affect their causes?). Our familiarity with human discourse about causal reasoning, expectations, or coherent models blinds us to the simple fact that the new system of concepts does not shed more light on the puzzling data.

And because history repeats itself in the articles and books written by too many of today’s psychologists (Machado, Lourenço, & Silva, 2000), the importance of Paul Meehl’s work retains its full strength. Few psychologists have been more aware of the importance of analyzing scientific concepts through reasoning and experiment than the late Paul Meehl. One could illustrate this remark with several of his works, but to me one stands above the others, the criticism of Tolman’s cognitive theory of learning. In collaboration with Kenneth MacCorquodale, Paul Meehl (MacCorquodale & Meehl, 1954) exposed some of the grammatical difficulties with Tolman’s concept of expectation (e.g., the confusions between the literal and the metaphorical). In the process, Meehl showed us the power (or at least his power) of conceptual analysis. I am delighted that in this issue of Behavior and Philosophy three distinguished behavior analysts remember Paul Meehl and expand on his accomplishments.

During the last two years it has been my privilege to serve as editor of Behavior and Philosophy. Unfortunately this experience must now come to an end. I take this opportunity to express my deepest appreciation to the reviewers of the journal for their generous time and helpful comments on manuscripts. Former
Editors Max Hocutt, George Graham, and John Staddon helped significantly during the (hardest) first months of my term. Francisco Silva shed the light that helped me see things more clearly. I would also like to express my gratitude to all 36 members of our fine Editorial Board and to Bruce Waller, Sigrid Glenn, Richard Eldridge, and Henry Plotkin for their help with the review process.

My regrets for stepping down are tempered by the fact that Jack Marr will be the new editor. In terms of depth of scientific knowledge, breadth of philosophical interests, and enthusiasm for behaviorism, the journal could not be in better hands.

References


