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ON WHY THERE ARE SO FEW COMPARISONS IN COMPARATIVE PSYCHOLOGY

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ABSTRACT: The comparative study of behavior requires close attention to the ecologically unique details of the environmental challenges and adaptations (both behavioral and structural) of a systematically selected range of species. It offers an understanding of which aspects of behavior change and which remain constant across phylogenetic pathways and evolutionary challenges. The General Process View of Learning (also known as the principle of the transsituationality of reinforcement, and by several other names), however, militates against study of the details of behavioral adaptations, by insisting that particular behaviors may be regarded as arbitrary instances of universal associative principles. The history of behaviorism, and of contingency theory, in particular, is largely the history of the gradual emergence and dominance of this General Process View, and of the working out of its profoundly anticomparative implications. The increasingly wide repudiation of the General Process View is providing the basis for a renewal of comparative studies of behavior.

Comparative psychology, like psychology as a whole, has always attracted its share of commentators who are willing to pass judgment on its past record, its present health, and its future prospects. At the present time, and for quite a number of years in the past, the commentators have been split between those who find comparative psychology in robust health and those who find it in decline or worse.

Those who find comparative psychology healthy point to the amount of research in progress on the behavior of nonhuman species, to new or greatly amended behavioral principles that have come from this research during the last two decades, to the build-up of information about the adaptations of particular species, or to the use of animal data in developing general behavior theories (e.g., Dewsbury, 1984; Galef, 1987). The orientation of these positive commentators tends to be more behavioral than cognitive, more laboratory-based than field-based, and more operant than ethological. They tend to view contingency theory on a continuum ranging from tolerance to enthusiastic approval.

On the other hand, those who find comparative psychology less healthy, or altogether moribund, point to the limited number of different

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species typically studied in psychological laboratories (Porter, Johnson, & Granger, 1981), to the limited range of environments typically studied in animal experimentation (Doré & Kirouac, 1983), to the paucity of interspecies comparisons (Tolman, 1987), and to the absence of any meaningful, overarching theoretical goal of the discipline as a whole (Adler, Adler & Tobach, 1973; Lockard, 1971). These commentators are not united in their orientations, but many of them tend to be more cognitive than behavioral, more field-based than laboratory-based, and more ethological than operant. They tend to view contingency theory on a continuum ranging from tolerance to severe disapproval.

The two groups of commentators sometimes seem to be passing judgment on two quite different disciplines, and in an important sense I suspect they are. They have, I suggest, quite different conceptions of what comparative psychology should be, and thus have different criteria for assessing the current scene. At the descriptive level both groups are accurate; modern comparative psychology is characterized by both microtheoretical ferment (e.g., in the operant anlaysis of "foraging") and macrotheoretical stagnation (e.g., in the relative inattention paid to phylogenetic integration). Judgments about the health of the discipline depend on which of these is considered more important. I will not try to survey both groups of conceptions. Instead, I will present one conception which owes something to both groups, although it probably has more affinity with the "pessimistic" group. It is a relatively bald statement of what comparative psychology's mission should be, and how it started off fulfilling that mission, and how it lost sight of it, and why that was regrettable, and how it may have started to find its way again in recent years. The view is my own, but it has some significant points in common with that of Adler *et al.* (1973), Tolman (1987), and others. It also has some major divergences from those views, especially in its conclusions.

Comparative psychology, in this view, is not merely the study of the behavior of nonhuman organisms, or even the study of a specified subset of that behavior as Dewsbury (1984) has argued. It is instead the study of the evolutionary development and progression of behavior in all three of its major aspects: cognitive, affective, and conative. The subject matter of the discipline is neither rats nor pigeons nor human beings, but rather behavioral systems and the way they both change and remain constant across phylogenetic pathways and evolutionary challenges. A single species or group of species no more provides the subject matter for the field than it does for comparative anatomy, although a group of related species may comprise a specialist area in each. The understanding of human behavior is a legitimate goal (although not the only goal) of comparative psychology, just as the understanding of human anatomy is a legitimate goal of comparative anatomy. But progress toward achieving that goal in each case requires seeing the human subjects as comprising a node in a phyletic network, and cannot be achieved by regarding them purely on their own. Comparative psychology, like comparative anatomy, may well therefore be "anthropotelic," taking the understanding of humans as its goal or end point, but can never be anthropocentric.

That is how I conceive comparative psychology, and I believe that that is how it was conceived by the pioneers in the field, such as Romanes, Hobhouse, Morgan, and Thorndike, working from the 1880s to the period just before the First World War. They all studied animal behavior, especially animal learning, not just to learn about the behavior or mind of the species they were investigating, and certainly not to learn about learning as a general process, unrelated to any particular species or environments, but explicitly to construct a behavioral phylogeny, so that human thought and action could be seen as further examples and developments of the patterns of adaptation they were discovering in their animal subjects. The goal of their studies was most often to understand human thought and action, so they were in this sense anthropotelic. In the beginning, especially in the work of Romanes, the descriptions of animal thought and action were based on those of humans, so they were clearly anthropomorphic, and this was a flaw that had to be overcome by Romanes' successors. But these researchers never lost sight of their goal of placing human thought and action firmly in an evolutionary context, and so they were never anthropocentric.

The collapse of this program was gradual over the first 10–20 years of the development of behaviorism after 1913. There is a nice irony in that behaviorism, which first developed as a response to conceptual problems in comparative psychology (cf. Mackenzie, 1977, Ch. 3), soon abandoned the comparative orientation of its parent discipline.

There were two main moves in this behaviorist repudiation of its own comparative background. The first was the rejection of instinct. Instinct had been a major organizing concept for comparative psychology from the beginning. However, the excessive use of the instinct doctrine in the 1920s (Tolman, 1923) encouraged behaviorist theorists to deny them as unobservable and gratuitous concepts, much as had been done with the concepts of mind and consciousness earlier. In the absence of a more sophisticated concept for describing species-typical behavioral adaptations, however, the loss of the instinct concept made many comparative questions difficult even to formulate.

The more important influence, however, was the development in behaviorist psychology of what Seligman (1970) has called the General Process View of Learning. In this view, the process of learning can be studied as easily and effectively in one species as in another, and as easily with one problem situation, response, reward (or later, reinforcer), and environment as another. As Seligman put it,

> In instrumental learning, the choice of response and reinforcer is a matter of relative indifference; that is, any emitted response and any reinforcer

> can be associated with approximately equal facility, and a set of general laws exist which describe acquisition, extinction, discriminative control, generalization, etc., for all responses and reinforcers. (Seligman, 1970, p. 407).

This principle, which Seligman also called the assumption of equivalence of associability, was earlier identified by Paul Meehl (1950), who called it the principle of the transsituationality of reinforcement. Both Seligman and Meehl concentrated on the generality of the general process view across situations, although Seligman's illustrations underlined its generality across types of organisms also. I also gave it a name somewhat later, in the specific context of Skinner's system, explicitly dividing the principle into what I called the assumptions of environmental and speciational generality (Mackenzie, 1977). More importantly, the principle had its origins in British associationist theory from the time of Hobbes (as discussed by Deese, 1965), in the notion that any idea could, by association, lead to any other idea. The principle, in one form or another, had thus been central to associative theories of learning and cognition since long before the introduction of evolutionary theory. It is also quite clearly incompatible with evolutionary theory, or with any theory which emphasizes unique patterns of adaptation for different groups of organisms. Consistent with this judgment, it was not emphasized by the early comparative theorists I mentioned, except eventually for Thorndike. Thorndike (1911) revived it from the later British associationist writers and proposed it as a major theoretical principle that any behavior could be connected with any "satisfier," as he called reinforcers. From Thorndike the principle passed into currency in later learning theory, but more as an assumption than as a theory.

In whatever form, the principle is profoundly anticomparative. It militates against any detailed comparative analysis of the problem situation and adaptive behavior of different classes of organisms, by stipulating that they must be fundamentally the same, the result of associative or S-R bonds formed according to unvarying universal laws. Inevitably, therefore, it is the laws that will be of interest rather than the specific adaptations, or attempts to link the adaptations (in any way other than as instantiations of the general laws) across species. Acceptance of the principle as an assumption therefore made comparative analysis of behavior unnecessary (or irrelevant) for understanding behavior, just as the rejection of the instinct doctrine made such analysis difficult and methodologically suspect. If any behavior will do, it makes sense to concentrate on the most convenient ones. The result was the retreat from comparative studies, and the concentration of experimental effort on a restricted behavioral repertoire in a small number of species (preeminently rats and pigeons) that has been widely documented and decried from Beach (1950) to Grosset and Poling (1982).

Acceptance of the principle also made it quite obscure as to why anyone would want to study animal learning. The two major justifications advanced during the 1940s and 1950s for the detailed study of learning in rats and pigeons were: (a) to study the process of learning for its own sake, and (b) to determine laws of learning which could subsequently be applied to the behavior of humans. The first amounts to the study of behavior without any behaving organisms, and underlines the antievolutionary (and often antibiological) character of later behaviorist learning theory. The second restricts the study of animal behavior to those features of it which may be helpful in explaining human behavior, and thereby incorporates precisely that anthropocentrism which was absent from the earlier comparative psychology. That fault, at least, was a minor one however, because the application of the "laws of learning" to human behavior was always more of a hope for the future than a focus for the present.

Fortunately, the assumption of the equivalence of associability, and with it the whole of the general process view of learning, is not only wrong but widely seen to be wrong. It has been shown to be wrong both in learning theory in general (Seligman, 1970) and in the system where it was most highly developed, that of Skinner's operant psychology (Mackenzie 1977, 1984). It is now fairly generally accepted that responses and reinforcers cannot be connected indifferently, that in each species of organism some behaviors can be potentiated best by certain reinforcers and other behaviors by others; so that in the pigeon, key-pecking can be reinforced very well by food, and wing extension can be reinforced very well by shock avoidance, but neither response can be reinforced nearly as well by the other reinforcer. There seems almost to be some intrinsic connection between pecking and food, and between extending the wings and avoidance of danger, and of course there is. The breakdown of the principle of the equivalence of associability, in other words, is not adventitious; it does not just happen. Instead, it happens in a consistent and meaningful way, and to understand the way it happens it is necessary to understand the details of the organism's behavioral adaptation to its environment.

In short, the principle that was central to the behaviorist dismissal of the comparative study of behavioral adaptations can be seen to have failed, precisely because those adaptations impinge at every stage even upon the restricted behavior studied in the behaviorist laboratory. They impinge by placing constraints on that behavior, constraints that prevent the behavior from consistently following general laws of learning and that can be taken account of within the learning theory only by introducing them as *ad hoc* supplementary principles ("instinctive drift," according to Breland & Breland, 1961; a "dimension of preparedness," according to Seligman, 1970). Such supplementary principles, however, rather than strengthening the learning theory accounts, merely lessen their applicability; they acknowledge the need for an analysis of behavioral adaptations in the natural environment as the basis for understanding behavior in the operant chamber, but do not provide it. Meehl (1950) was correct in rewriting the empirical law of effect to read "All reinforcers are transsituational"; and the failure of the assumption of the equivalence of associability amounts, quite simply, to the refutation of the law of effect.

Finally, I come to my optimistic conclusion. The refutation of the general process view of learning at the hands of Seligman and other writers has helped spark a revival of interest in comparative studies of animal adaptation and even animal minds. Animal cognition has emerged as a thriving field in the past 10-15 years, with the application to animal studies of some of the models and insights of the human cognitive laboratory (Roitblat, 1982; Roitblat, Bever, & Terrace, 1984). Other authors are again explicitly trying to understand behavioral adaptations in the context of broad descriptive evolutionary trends (Gottlieb, 1984, 1987). The important point is that it is specifically as part of the collapse of some of the central assumptions of associative learning theories that these signs of a rebirth of comparative psychology may be seen. If the influence of these theories continues to wane, and if the general process view of learning which underlay them gradually becomes recognized as intellectually bankrupt, then we may look forward once again to seeing widespread comparisons in comparative psychology.

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