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COMMENTARY

Beyond Method

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Procedures are not the same as processes. But purposive modules are probably no more than a way-station to understanding learning processes and may not be as simply represented in brain neurophysiology as many seem to assume. And finally: it is impossible (and therefore unwise) to specify in advance what the ultimate theory of learning *must* explain.

It is possible to object to many specific points in this thoughtful article by Grau and Joynes (2005), but it is impossible to disagree with its overall theme. Learning and conditioning procedures *are* surely a means to an end, where the end is to understand the myriad ways in which behavior, human and animal, depends on the environment. Conditioning methods, the standard paradigms for habituation, classical and instrumental conditioning, are not themselves processes or mechanisms, nor are the phenomena they demonstrate universal building blocks for learned behavior.

When I first learned about classical conditioning, as an undergraduate, I remember being told that Western scientists had misunderstood Pavlov. “Conditioned response” for him just meant a response “conditional upon” a history of pairing between CS and US. It was simply a descriptive term, nothing more. Pavlov assumed no “conditioning process” of the sort to which Grau and Joynes now object. It is, I suppose, rather incredible that these caveats have been ignored by so many able people for so long that now the very same lesson must apparently be learned again.

So, what is to be done? The authors propose an *ism* they call neurofunctionalism: “Functionalism because the approach focuses on the identification and comparison of operational modules designed to accomplish a particular goal, be it the abstraction of environmental relations, recognition of a food source, or spatial navigation. Neural because an integral component of the approach involves the specification of the underlying neural mechanisms” (p. 5). The functional part of this idea resembles Timberlake’s behavior systems approach, and the modular view now common in human evolutionary and cognitive psychology. The brain, it is argued, is not in any sense a general purpose learning system (so much for Hull, Skinner, Bitterman, and all!). Rather it is organized as a set of more or less isolated units that have specific niche-related functions such as spatial navigation or food recognition. The neural part of their idea is that there are may be more than one neural mechanism underlying a given function and that mechanisms that underlie “learning” need not exclude peripheral processes.

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Well almost every science begins with classification—of species, substances, elements, whatever. Learning psychology began with a classification of procedures but, as the authors point out, that has not worked well. An appropriate analogy might be to the early chemists. Their first try was air, earth, fire, and water. A good start, but not fundamental. Physical science had to wait until Mendeleev to get a classification worked—in the sense that it led onward to a deeper understand of physics and chemistry. So, will classification based on "modules" work better than the tripartite division the authors criticize? I'm not sure, because this also is not a new idea. It is hard to see where "modules" differ much from faculty psychology, or William McDougall's idea that we all have a number of what he called "instincts."

If functional modules can be identified with a particular neural substrate, of course, progress will have been made. And in a few cases, this does seem to be the case. But in others, it seems pretty clear that the relevant neural structure may be interwoven with other structures serving very different functions. But these possibilities are familiar and it is probably difficult to find anyone who would disagree with this approach.

What of "mechanism," the other half of neurofunctionalism? Mechanism is a protean term. The OED gives as a first definition: "The structure or operation of a machine or other complex system; a theory or approach relating to this." In other words, *mechanism* can refer to structure—and I believe that Grau and Joynes intend this meaning—or to the theory of operation of the system. Pure structure is nothing but neuroanatomy. To put "mechanism" into structure implies a bottom-up approach: explaining the overall operation of the system by the properties of, and interactions among, its neural elements. I have argued elsewhere (Staddon, 2001a, Chapter 1) that explanation at this level, of a complex, evolved structure like the brain, is likely to be very difficult—much more difficult than is usually supposed. The other meaning of "mechanism" refers to a theory of operation. This, I suggest, is where both psychologists and neuroscientists should look first if they wish to solve the brain-mind problem.

Now to specifics. The authors begin their otherwise splendid article with what has become an almost obligatory swipe at behaviorism—"This trichotomy, a vestige of our behaviorist past …" (p. 1, Abstract). But behaviorism is a philosophy rather than a specific set of scientific assertions (Staddon, 2001b). It does not rest on these distinctions. Moreover, as Roediger (2004; not noted as a behaviorist himself) recently pointed out: we are (to a degree) all behaviorists now!

The authors mention *operationalism* as a component of the framework that led to the prevailing view of associative learning. They are right to be skeptical. Premature emphasis on operational definitions has always put the cart before the horse. A mature science allows you to define your terms operationally, but the converse is not true. Coming up with an operational definition will not by itself advance science. Theoretical understanding comes first; operational definition afterwards.

In their discussion of habituation, the authors write: "learning that is thought to rely on an associative mechanism generally exhibits the opposite relation; given an equal number of pairings, spaced presentation produces a stronger CR than massed presentation…" (p. 12) This may not be true. The standard Groves and Thompson discussion of habituation is purely qualitative, but statements like this depend on quantitative properties. The habituation property we have termed *rate sensitivity* (Staddon, 1993; Staddon & Higa, 1996) suggests that this conclusion will depend on quantitative details: When habituation is tested after training, after a long time, the long-ITI group may show more habituation than the short-ITI group. But a short time after training, these relations may be reversed. Perhaps this just makes the authors' point: You have to understand the mechanism, the theory of operation of the system, to make sense of observations like this.

The authors attribute to learning theorists the core assumption "that learning is essential" (p. 1). But this is plainly false. Many species, particularly protists, get by with nothing beyond simple habituation. Learning is of course necessary for more complex niches, but I know of no one who thinks it essential to adaptive behavior in general.

Figure 1 is problematic, because it treats the biological and functional mechanisms as separate. This cannot be true. The functional mechanism, rules of operation, of the system is implemented by the biology, it is not something separate, or separable, from it.

As someone reared in the Skinnerian tradition, I nevertheless do not see the distinction between "operant" and "instrumental" behavior that the authors discern. I think that, as historically defined, there is no defensible distinction between operant and instrumental behavior. Pecking in pigeons is "operant" by Skinner's, or anyone's definition. Yet it is clearly "constrained" in the authors' sense (Staddon & Simmelhag, 1971).

Finally, the authors speak of what "a complete theory of learning must speak to…" (p. 15). This is such a common philosophical error that it deserves a name, perhaps the *imperative fallacy*—the idea that it is possible to specify in advance of its discovery the domain of a theory. It is as if some pre-Copernican scholar argued that a complete theory of the planets *must* specify their number, size and color, as well as their orbits and duration of year. Well, yes, in a sense. But we know now that this list is apples and oranges. Newton's laws apply to only some of these things; the others are either inexplicable or attributable to other processes entirely.

But enough carping. I thank the authors for a productive attack on a difficult and important problem.

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