On the Clarification of Concepts

A Reply to Gallistel (2007) and Lau (2007)

Armando Machado
Universidade do Minho
University of Redlands

The authors argue that some of C. R. Gallistel’s (2007) and H. C. Lau’s (2007) solutions to the problems raised in the article by A. Machado and F. J. Silva (2007) are unlikely, logically inconsistent, or at odds with empirical evidence. In particular, Lau seems not to appreciate the necessity of clear and consistent psychological constructs before trying to map these constructs to brain structure and function. The authors conclude that conceptual analysis is a much-needed component of the scientific method.

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The less a science has advanced, the more its terminology tends to rest on an uncritical assumption of mutual understanding. (Quine, 1936, p. 90)

One of the characteristics of mature sciences is the consensus reached by its practitioners. Physicists agree on the principles of Newtonian mechanics, chemists on the atomic theory of matter, and biologists on Darwin’s theory of evolution by means of natural selection. The consensus is expressed in shared concepts, laws, models, and theories; in short, it is a consensus expressed in language. But to reach a consensus, scientific language must first be “consensible” (Ziman, 1967); that is, ideas must be formulated so clearly and unambiguously that one can clearly agree or disagree with the ideas. The set of activities that we referred to collectively as conceptual analysis helps in the clarification of ideas.

In our article (Machado & Silva, 2007, this issue), we illustrated conceptual analysis using examples from 17th-century physics and from contemporary psychology. Our aim was not to challenge the methodology or the experimental findings of the corresponding studies but to illustrate with concrete cases the many aspects of conceptual analysis and how it can be carried out in different research domains. Our ultimate aim was to propose a richer view of the scientific method, a view in which conceptual analysis receives the attention it deserves. We elaborate on these ideas in our replies to Gallistel (2007, this issue) and Lau (2007, this issue).

Reply to Gallistel (2007)

After endorsing our general message, Gallistel (2007) made three additional points, one explaining why conceptual analysis is rare, a second illustrating a conceptual analysis of the key concept of trial in theories of animal learning, and a third commenting on the specific problem with scalar expectancy theory (SET) that we identified in our target article (see Machado, 1997, for the first report of this problem). We agree with Gallistel that there are many reasons why scientists do not engage in conceptual analysis and why they might fail to publicly acknowledge problems with their own theorizing. We have discussed some of these reasons elsewhere (see Machado, Lourenço, & Silva, 2000). Because these reasons relate to potentially sensitive psychosocial issues, we appreciate Gallistel’s frank and forthright discussion of his and the late John Gibbon’s experiences related to identifying and attempting to correct the problem with SET.

We also agree with Gallistel’s (2007) analysis of the concept of trial in traditional associative learning theory. The concept is problematic for the reasons he identifies, and we will add his analysis to our list of examples of conceptual analysis. The theme of his example could be “When are ad hoc hypotheses excessively ad hoc?” Parenthetically, we note that some real-time models of classical conditioning do not rely on this problematic concept of trial (e.g., Buhusi & Schmajuk, 1999).

Finally, concerning Gallistel’s (2007) summary of the results from the time-left procedure and of the challenges faced by a behaviorist explanation of them, we note that some of the controversial issues surrounding these experiments are conceptual, for they revolve around the (debatable) distinction between reinforcement value as a determinant of choice and reinforcement delay as a determinant of schedule performance (see Staddon & Higa, 1999; see also Machado & Vasconcelos, 2006). However, Gallistel’s (2007) remark that “the pigeons [in these experiments] extract three different temporal decision criteria” (p. 682)—the two standard delays of 15 s and 240 s and their harmonic mean, 28 s—is questionable. Bisection at the harmonic mean does not imply that the animal has extracted the harmonic mean. Similarly, bisection at the geo-
metric mean in retrospective temporal discrimination tasks does not imply that the animal has extracted the geometric mean of the training durations. In fact, several models of timing predict indifference at the geometric mean (or close to it) without representing the geometric mean explicitly (e.g., Gibbon, 1981; Killeen & Fettermann, 1988; Machado, 1997).

**Reply to Lau (2007)**

We used Lau, Rogers, Haggard, and Passingham’s (2004) original study to illustrate the conceptual difficulties occasioned by the use of the vernacular in a scientific sense. It is clear from Lau’s (2007) comment that he does not think there were any problems with how he and his coauthors used the everyday concepts of intention and voluntary action. We illustrate below that he should be concerned with how these concepts were used in Lau et al.’s (2004) article because the problems we identified relate to major claims made by the authors.

Lau et al.’s (2004) first major claim, as stated in the opening sentence of their article, was that voluntary action and intention are equivalent: “A motor action is voluntary if and only if it is intended” (p. 1208). They then went on to make a second claim that intentions are neural occurrences in the pre-supplemental motor area (pre-SMA) and then a third claim that these occurrences can be attended to and even edited.

In his comment on our article, Lau (2007) considered our identification of an infinite regress problem “interesting . . . but hardly a challenge” (p. 686) to the claim that the pre-SMA represents intention. If this claim were the authors’ only claim, then the infinite regress problem would indeed have little bearing on it. But Lau et al. (2004) made three major interrelated claims. One of the goals of conceptual analysis is to check the consistency of interrelated ideas and claims. The infinite regress problem is relevant when one tries to understand the interrelation of Lau et al.’s first claim that voluntary action and intention are equivalent and their second claim that the pre-SMA represents intention. In one instance, if intentions are brain occurrences (i.e., specific activations of the pre-SMA), then they must be caused by something. If that “something” is itself intentional, then one has the start of an infinite regress, and the conceptual problem we identified is relevant. In another instance, if intentions are brain occurrences that are caused by something that is not intentional, then there is an apparent contradiction—namely, a voluntary, intentional behavior that is caused by an involuntary, unintentional occurrence. The infinite regress problem is not simply interesting; it highlights a problem with the interrelation of Lau et al.’s claims that voluntary action and intention are equivalent and that the pre-SMA represents intention.

In addition to checking the consistency of interrelated ideas and claims, conceptual analysis probes the meaning of terms and assumptions. In our article, we claimed that Lau et al. (2004) assumed that intentions cause behavior. In his comment on our article, Lau (2007) remarked that he and his collaborators never made this assumption. We had interpreted sentences and phrases such as “[the subjects] reported the time at which they first felt the urge to move” (Lau et al., 2004, p. 1208) and “[attending to intentions could be] one mechanism by which effective conscious control of actions becomes possible” (Lau et al., 2004, p. 1210) as showing that, in the authors’ view, intentions are causally related to behavior. In his comment, Lau (2007) said we misinterpreted his view about the relationship between intentions and behavior, and then he attempted to clarify that view by engaging in further conceptual analysis: “Intentional behavior might really only be voluntary in a very limited sense, that is, it gives the impression to the agent that it is voluntary” (p. 687). Left unexplained is how to reconcile the view that intention and voluntary action may not have much in common with the view expressed in the opening sentence in Lau et al. (2004) that proclaimed the equivalence of voluntary and intentional behavior. This seems logically inconsistent.

In his comment, Lau (2007) also claimed that volition is an illusion (“We have elsewhere reported data in support of this idea,” p. 687), a claim that is consistent with a noncausal view of intentions. Left unexplained, however, is how to reconcile this illusory and noncausal view of intentions with the very arguments advanced by Lau et al. (2004) for believing that the pre-SMA represents intention: (a) The pre-SMA is involved in the generation of free choice, (b) lesions to this area in macaque monkeys abolish spontaneous actions, and (c) direct electrical stimulation to this region elicits a reported sense of “urge” to move in human patients suffering from epilepsy. These three sets of empirical findings suggest a causal, not an illusory, role of the pre-SMA in generating motor actions. If the pre-SMA represents intention and a lesion to this area of the brain abolishes spontaneous action, how is it that intentions are not causally related to spontaneous action? This is not a rhetorical question. Lau’s noncausal view of intentions is incompatible with the empirical evidence he provided.

The source of the aforementioned conceptual problems seems to be the uncritical use of everyday concepts in scientific discourse. The vernacular concepts of intention and voluntary behavior are not linked to inner feelings or urges (as Lau, 2007, linked them). In everyday life, one learns to attribute intentions to other people and to classify their behavior as voluntary or involuntary without perceiving someone’s brain occurrences, inner urges, and feelings. Having removed the vernacular concepts of intention and voluntary behavior from their familiar domain (i.e., behavioral patterns and the environmental circumstances in which they occur) and inserted them in a new domain (i.e., brain occurrences, inner feelings and urges), Lau (2007) needs to explain the sense in which his view of voluntary behavior—“voluntary behavior is considered voluntary because we feel this to be so” (p. 686)—is anything like the everyday concept of voluntary behavior. It is not sufficient to claim that, because the subjects produced consistent results across a variety of studies, “there is no reason to think that the subjects were not in fact attending to their intentions as instructed” (p. 687). This claim confuses reliability, which was not at issue, with validity, which was.
Nowhere in Lau et al.’s (2004) original article was the concept of intention demarcated from its everyday usages, restricted to a specific set of cases, defined technically, or framed theoretically. In his comment, Lau (2007) wrote as if the concept of intention poses no difficulties or requires no explications: “Our study . . . used brain imaging to tackle a question of functional neuroanatomy—we wanted to investigate *where* in the brain intention is represented.” (p. 687). As we mentioned in our article, this is a case in which familiarity with a concept seems to have bred contempt for its analysis. Everyone knows what an intention is; what we now want to know is where intentions are located in the brain. But mapping psychological constructs to brain structure and function requires that the constructs and the claims related to them be clear and consistent. Conceptual analysis is the process by which this clarity and consistency are checked. And it is needed because, as Lau pointed out, “good thinking is not easy” (p. 688).

**REFERENCES**


