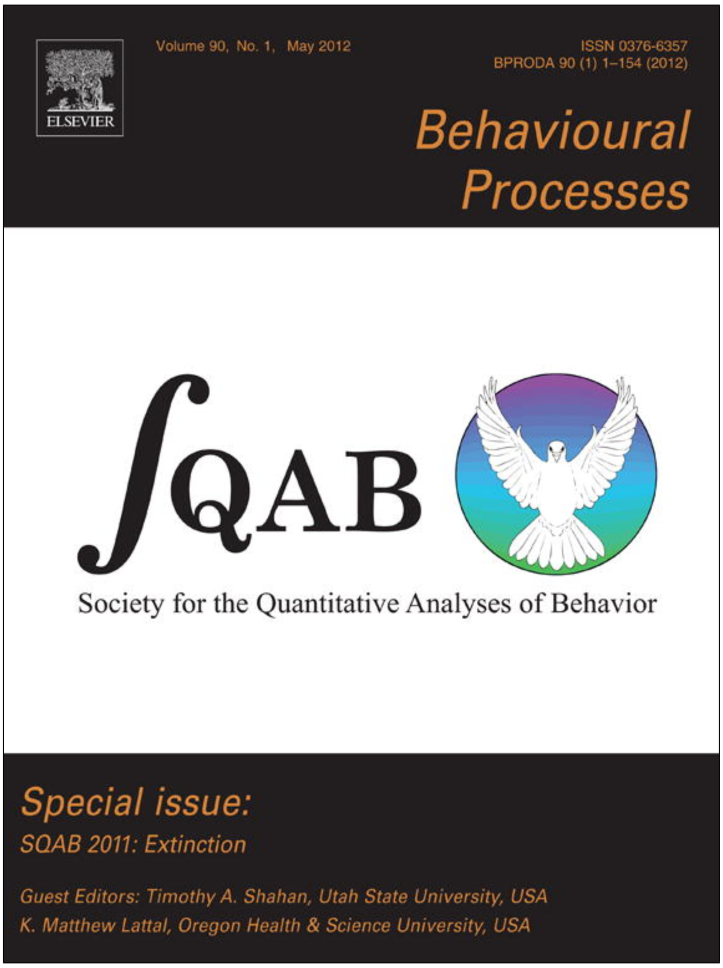
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On the evils of group averaging: Commentary on Nevin's "Resistance to extinction and behavioral momentum"

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ABSTRACT

Except under unusually favorable circumstances, one can infer from functions obtained by averaging across the subjects neither the form of the function that describes the behavior of the individual subject nor the central tendencies of descriptive parameter values. We should restore the cumulative record to the place of honor as our means of visualizing behavioral change, and we should base our conclusions on analyses that measure where the change occurs in these response-by-response records of the behavior of individual subjects. When that is done, we may find that the extinction of responding to a continuously reinforced stimulus is faster than the extinction of responding to a partially reinforced stimulus in a within-subject design because the latter is signaled extinction.

Nevin's paper seeks to describe the course of extinction, beginning with an equation suggested by an analogy between resistance to change in rate of responding and physical momentum. His focus on a descriptive approach is consistent with the behaviorist theoretical canon that the science aims to discover the most economical and precise description of behavioral phenomena. My own approach reflects my training as a behavioral neuroscientist and the influence of the German behavioral physiology (*Verhaltensphysiologie*) tradition on my intellectual formation. In the behavioral physiology tradition, behavioral experimentation aims at inferring underlying physiological mechanism. Because I was deeply influenced by the cognitive revolution, I take the mechanisms that we are attempting to infer to be computational mechanisms.

Accurate description is a pre-requisite to valid inference, so there is no conflict between the behaviorist's descriptive approach and the behavioral physiologist's inference-oriented approach. However, in the search for accurate description, we must ask, Accurate description of what? I assume that what both the behaviorist and the behavioral physiologist want is accurate description of the change in behavior of individual animals. When we stop reinforcing a behavior, the frequency of that behavior declines. What is the mathematical form of this decline? Skinner (1976) and I are united in our belief in the value of cumulative records for visualizing behavioral change. Like him, I deplore the disappearance of cumulative records from behaviorist papers. I want to know what extinction under Nevin's conditions looks like in the individual subject. The central tendency in group data is of no interest if it does not indicate properties of the individual.

Nevin's figures give group averages based on session-level counts of responses. It is impossible to infer from these curves the form of curves that describe the extinction behavior of the individual subjects, as Estes pointed out long ago (Estes, 1956, pp. 134–135):

> "Just as any mean score for a group of organisms could have arisen from sampling any of an infinite variety of populations of scores, so also could any given mean curve have arisen from any of an infinite variety of populations of individual curves. Therefore no 'inductive' inference from mean curve to individual curve is possible, and the uncritical use of mean curves even for such purposes as determining the effect of an experimental treatment upon rate of learning or rate of extinction is attended by considerable risk.....we can no longer expect averaged data to yield any direct answer to the question, 'What is the form of the individual function?" (See also Hayes, 1953).

Estes explains and illustrates the fact that one cannot, except under unusually favorable circumstances, infer either the form of the individual functions or the distribution of individual parameter values. The latter points is sometimes driven home by the memorable if somewhat vulgar observation that the average human has one breast and one testicle.

It is important to keep Estes' cautions in mind when considering Nevin's estimates of the numbers of responses or numbers of trials to extinction following training with continuous versus varying degrees of partial reinforcement. One cannot validly derive from the group average data what the mean or median of the individual trials- or responses-to-extinction distribution in fact was. This is all the more true when working with data that at the subject level are pooled across sessions or substantial blocks of time (that is, counts of the number of responses each subject made in a session or in successive lengthy blocks of time).

The way to obtain valid estimates of omitted reinforcements to extinction after different degrees of partial reinforcement is to use response-by-response or trial-by-trial data from each subject to estimate for each subject the trials to extinction. What one wants is an estimate of the point of downward inflection in the slope of

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the cumulative record (cf. Gallistel et al., 2004; Papachristos and Gallistel, 2006). The point at which the cumulative record becomes flatter is the point at which the animal gives evidence of responding to the decrease in the probability or rate of reinforcement. From estimates of those inflection points, one may obtain estimates of the omitted reinforcements to extinction of the responding to each stimulus. Because continuous versus partial reinforcement is a within-subject manipulation in these experiments, one should then compute the ratio of the two estimates for each subject. In my experience, the resulting ratios are likely to have a radically non-normal distribution. Therefore, the median ratio is a better measure of the central tendency, but it would be much more informative to give the cumulative distribution of these ratios. To summarize, what one wants from each subject's data is: (1) an estimate of the omitted reinforcements to extinction of responding to the continuously reinforced stimulus; (2) an estimate of the omitted reinforcements to extinction of responding to the partially reinforced stimulus; (3) the ratio of these two estimates; (4) the cumulative distribution of the ratios (for the across-subjects summary); (5) A Bayesian calculation of the relative likelihood of the hypothesis that the central tendency of this distribution is 1 versus the hypothesis that the central tendency is greater than 1 (with a plausible limit on how much greater, see Gallistel, 2009).

This warning about the misleading effects of group averaging on learning and extinction curves has broad application, as group average learning and extinction curves are the rule rather than the exception in the animal learning and behavioral neuroscience literatures (and in psychological textbooks). Group averaging is a disease from which the experimental study of learning has not yet recovered, despite the disease's having been diagnosed more than half a century ago. The disease is prevalent even among psychologists working in the operant tradition, despite Skinner's having prescribed the cure, which is to describe accurately the behavior of each individual subject and generalize only when there emerges from these descriptions a truth that is repeatedly demonstrable in the behavior of the individual subject.

That said, I would guess that when the within-subject experiment is properly done and analyzed, it will emerge that the within-subject effect of partial reinforcement on extinction differs quantitatively from the between-subject effect in the direction that Nevin suggests. I would guess that the omitted number of reinforcements to extinction after continuous reinforcement of one stimulus tends to be greater than the omitted number of reinforcements to extinction after partial reinforcement of a another stimulus, *at least when the non-reinforcement of both stimuli commences simultaneously*. Why? Because we know that signaled extinction is faster than unsignaled extinction. (There are fairly obvious rationalist explanations for why this should be the case–see my own paper in this issue.) If, as I think likely, subjects perceive changes in stochastic parameters and estimate the time in the past at which the change occurred, then the perception that the rate of reinforcement for the continuously reinforced stimulus has declined precipitously constitutes a signal that is coincident with the (harder-to-detect) decline in the rate of reinforcement of the partially reinforced stimulus. Thus, the extinction of responding to the partially reinforced stimulus is signaled extinction, whereas the extinction of responding to the continuously reinforced stimulus is unsignaled extinction. The experience of the latter is what signals the former.

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C.R. Gallistel*
Rutger's University, Rutgers Center for Cognitive Science, 152 Frelinghuysen Rd, Piscataway, NJ 08854-8020, United States

* Tel.: +1 732 445 8086.
E-mail address: galliste@ruccs.rutgers.edu

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