Neural Quantum Controversy in Sensory Psychology

The article by the late S. S. Stevens entitled "A neural quantum in sensory discrimination" (*1*) attempts to resolve a long-standing controversy in experimental psychology on the basic processes underlying sensory discrimination. Simply stated, the question is whether, under a particular set of conditions, sensation is changed in a continuous or a discrete manner as the corresponding physical stimulus is continuously altered. Evidence on the issue is typically obtained in experiments on differential thresholds. Stevens (*1*) contends that "some 140" functions have been obtained in vision and hearing which show steplike discontinuities when the percentage of correct responses is plotted against the size of the stimulus difference; purportedly, the functions support the theory of the neural quantum in sensory discrimination (*2*). Unfortunately, as I will point out, Stevens' conclusion is at best equivocal, and the question remains unsettled on both technical and empirical grounds.

Statistical tests. Given the assumptions of the neural quantum (NQ) theory and a two-quantum criterion of judgment, the theory predicts: (i) a linear relationship between the percentage of detections and the size of increment added to a standard stimulus; (ii) a two-to-one ratio between the value of the smallest increment which is always detected and the largest increment which is never detected; and (iii) a slope characterizing the poikilitic (psychometric) function which is inversely proportional to the intercept of the function on the x-axis.

Of the three predictions, most experimenters have evaluated the linearity of the psychometric function and the two-to-one ratio. It is surprising, therefore, to find that in Stevens' article "the many NQ functions . . . were all fitted by eye" (*1*, p. 752) with no attempt at a statistical analysis. This appears most inappropriate since at least my own data on pitch discrimination (*3*), given in Stevens' figure 10, were all fitted with a linear function by the method of least squares and evaluated by chi-square tests of goodness of fit. Contrary to Stevens' assertion based on visual inspection, these findings did not appear to support the NQ theory.

At a more general level, Stevens contends that "a statistical test that pits the one-parameter straight line against the two-parameter ogive is unfair . . ." (*1*, p. 753). The argument stems from the fact that in NQ theory the slope of the poikilitic function is precisely related to its midpoint value for a given criterion of judgment, whereas the ogive (phi-function of gamma as an alternative explanation of sensory discrimination) does not have this restriction. It would appear, therefore, that departures from NQ predictions based on one degree of freedom should be more readily detected than those of the classical (phi-gamma) theory. Accordingly, rather than abandoning statistical inference, NQ investigators should be encouraged to evaluate experimental data by appropriate techniques.

In an earlier review of NQ theory, I stressed the need for "the development of a more satisfactory technique for statistically testing the goodness of fit of the quantal and phi-gamma hypotheses to a set of experimental data" (*10*, p. 392). Such developments have not occurred, although new tests of normality have been devised (*4*). Nevertheless, the techniques of probit analysis (*5*) and chi-square are available, despite their inherent weaknesses. A detailed criticism of the difficulties encountered by Stevens, Morgan, and Volkmann (*2*) in the application of the chi-square test to NQ is provided by Lewis and Burke (*6*).

In considering my data on loudness discrimination which did not support NQ theory (*3*), Stevens (*1*) offered the data of Neisser (*7*) as "the typical finding" for this function. This is highly provocative, since Neisser reported that "the majority of these functions do not have the rectilinear shape predicted by the quantal hypothesis, nor any other specific shape . . . most of them are jumbles of points with no clear character except a roughly monotonic increase" (*7*, p. 516). While the mean intercept ratio approximated the predicted value of 2.00, linearity of the poikilitic function was mainly absent. Thus, Neisser's (*7*) data do not unequivocally support NQ theory.

Table 1. Comparison of data by Corso (five observers) (*3*) and Jerger (ten observers) (*8*) on differential intensity discrimination at 1000-hz, 40-db sensation level. Entries are mean values expressed in decibels derived from linear functions for individual observers fitted by the method of least squares.

Response (%)	Corso	Jerger	Difference (db)
0	1.03	0.54	0.49
50	1.50	1.21	0.29
100	1.96	1.88	0.08

Confirmation of data. Stevens (*1*) has commented on my data on loudness discrimination (*3*), since only two rectilinear poikilitic functions were obtained in a set of 35, neither of which yielded the predicted intercept ratio. The validity of these data is best demonstrated by a comparison with the findings of Jerger (*8*) shown in Table 1. It is seen that the differences between means for the two studies range from 0.08 to 0.49 db. This is indeed remarkable agreement. Furthermore, the intercept ratios for both studies fall within the distribution reported by Neisser (*7*).

NQ problems. Apart from statistical considerations, NQ theorists face three major difficulties.

1) Up to this time, they have failed to establish the specific conditions under which rectilinear psychometric functions may be obtained with any degree of regularity. It is held that two factors mitigate against such functions: (i) shifts in the observer's quantal criterion of judgment and (ii) overall fluctuations in the observer's sensitivity (*1*). Practice sessions under quantal conditions with well-motivated observers and appropriate instructions should be sufficient to resolve the first factor, but this has not occurred (*3*). The second factor creates even greater difficulty, since it involves the logical development of NQ theory. The prediction of a rectilinear psychometric function is dependent upon the equiprobability of surplus values; this probability distribution is generated on the assumption of large overall fluctuations in the observer's sensitivity relative to the quantum size. The NQ theorists, therefore, must resolve the paradox: Organismic fluctuations are said to obscure quantal functions, but without the assumption of fluctuations, NQ theory in its present form cannot be derived.

2) The second major difficulty is that NQ theory as a general theory of sensory discrimination should successfully encompass both absolute and differential thresholds. Stevens (*1*) does not expect supportive data from studies on absolute thresholds. If NQ theory is valid, evidence for or against the theory should be obtainable from either approach (*9*), but studies on absolute thresholds have generally failed to support NQ theory (*10*).

3) The third major difficulty relates to the basic notion of neural quantum. Various investigators (*11*) have indicated that the all-or-none principle of nerve activity forces sensory theories to consider the effects of fixed units of influence. In NQ theory these units are viewed as functionally distinct units in the neural processes underlying discrimination which impose a limit on the resolving power of the sensory system (*2*). I have shown, however, that the "steps" in Békésy's (*12*) study on the audibility function at low frequencies were not indicants of quantal units, but experimental artifacts (*9*).

The all-or-none principle, however, does not necessarily dictate a "neural quantum" in the sense of NQ theory and it may be advantageous to consider the output of a sensory system in terms of the well-established intensity-frequency principle of nerve activity. While it does not appear that a single function can adequately describe this relationship for all sensory systems (*13*), an S-shaped function has been obtained in single auditory nerve fibers in the cat (*14*) which may suggest support of the phi-gamma hypothesis. Nevertheless, on the assumption that the output of a sensory system can be described in terms of discrete "pulses" yielding a continuous stochastic process of interarrival times as a monotonic function of signal intensity, an elegant theory has recently been advanced to account for reaction time and a variety of psychophysical findings (*15*).

Conclusion. No one seriously questions that *some* poikilitic functions have been experimentally produced and that in a *few* instances the predicted intercept ratio has been obtained. The question is whether these findings are to be considered as methodological artifacts or genuine expressions of the basic quantal functioning of sensory processes in discrimination. Since the results of most studies do not support NQ theory to the exclusion of the phi-gamma hypothesis, the question remains unanswered.

Furthermore, the advent of signal detection theory and neural timing theories, which typically do not assume the existence of a sensory threshold, have provided new explanations of sensory discrimination that obviate the need for an answer. The continuity-discreteness question is now mainly of historical interest, with the design of experiments in current psychophysics and other areas of psychology being highly influenced by the signal detection paradigm. There is some evidence that NQ theory, as modified by a response bias model or a multistate model, can describe the changes in psychometric and iso-sensitivity functions which occur when quantal conditions are shifted to yes-no or temporal forced choice experiments (*16*). A significant recent development suggests that the same decision mechanism, operating on normal distributions of stimulus effects, may underlie both signal detectability measures and those of traditional psychophysical procedures (*17*). Consequently, both the quantum and the differential threshold which describe the observer's performance by a single measure that depends on the criterion as well as sensitivity are replaced by two indexes that estimate d' and β of signal detection theory. Thus, a rapprochement may be reached between classical and contemporary psychophysics.

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18. I regret that while Professor S. S. Stevens kindly provided me with the opportunity to comment on the prepublication manuscript of his article (*1*), circumstances prevent my extending to him the same personal and professional consideration.

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Because of the death of S. S. Stevens, I have been asked to comment on the disagreement between Corso (*1*) and Stevens (*2*) on the nature of elemental sensory units. The issue is not an easy one to resolve, but I do not find the comments by Corso helpful in settling the issue.

The basic problem is this: if we increase the intensity of a sensory signal by a small amount, is the resulting change in sensation a discrete or a continuous one? One would think this an easy issue to decide, if not by psychological experimentation, then perhaps by physiological investigation. But in fact, as with so many other matters that appear to be straightforward scientific questions, once the underlying issues are examined with some care, they are seen to be very complex.

The problem is that we are talking about detecting the absolutely minimal change of sensation possible. In the normal procedure for this type of study, the detection of brief increments of pure sinusoidal tones is examined as a function of the size of that increment. Generally, changes in the level of a steady tone of some 3 to 6 percent are detectable from somewhere between near 0 to around 100 percent of the time. These are very small changes in signal intensity, so that even the small, normally present "twitches" in middle-ear muscle tension can probably create changes in auditory sensations that are greater than those produced by the signal. An observer hears shifts in the level of the tone even in the absence of an actual increment, shifts that exceed those generated by real signals. This problem of internally generated noise plagues all investigators of sensory functions, and, important for the issue here, the internal noise causes the subject to establish a decision criterion that will minimize the reporting of spurious changes and, hopefully, maximize the detection of real signals.

Now, what of the arguments of Corso? Basically, I find his discussion offers little that is new. For example, we are told that most experimenters test the adequacy of the linear fit of the psychometric function to the data. That may or may not be so, but careful analysis of the assumptions underlying the neural quantum theory indicates that linearity is not a necessary condition (*3*). A much more fundamental prediction is that, if basic sensations are the result of elementary discrete units, then the ratio of the signal amplitude that is just always detectable to the signal amplitude that is just never detectable should be a rational number. The value of it depends upon the observer's criterion. Usually it should be 2, but different decision criteria will cause the number to be 3/2, 3/1, or even 4/3, as every investigator from Békésy on has been careful to point out. I find numerous data in the literature that meet these requirements.

Corso requests that investigators use more adequate statistical tests of the validity of their hypotheses. That is a reasonable request, but unfortunately, the ability of a statistical test to confirm a particular theoretical distribution depends heavily upon the cleverness of the scientist in formulating the correct theoretical alternatives to be considered. When I look at how well Stevens has managed to fit a one-parameter model through the data collected by Corso [figure 10 in the article by Stevens (*2*)], I am more impressed by the evident fit than I am by Corso's disclaimer because he found a chi-square test to be not significant. The chi-square is not the proper test when one is predicting observations of 0 and 100 percent. More to the point, the tests performed by Corso assumed both that the results should be linear and that the observer manages a constant, fixed-decision criterion. Both of these assumptions are suspect, and neither are very important for the underlying hypothesis.

Finally, Corso asks about the analysis of absolute thresholds and of the physiological evidence. In both these cases, I do not follow his arguments. My understanding of the quantum theory does not allow me to make testable predictions about its effect on measures of absolute threshold, so I am somewhat surprised to read that Corso has managed to bridge that theoretical gap, derive the predictions of the theory, and find the data not to be confirmatory. The physiological data are simply not convincing, one way or the other. Many discrete physiological phenomena exist, such as the number of neural responses that occur in response to a signal. Many continuous phenomena exist, such as the time between successive neural responses. At the moment, I find the physiological data to be supportive of whichever of the theories one wishes to believe.

I still find it impossible to reach any firm conclusion about the nature of the underlying sensory processes. I am not ready to agree with Stevens' view that the matter is settled, even though it was nice to see some of my old data resurrected and spoken of so highly. But I certainly find myself quite unimpressed by the counterarguments presented by Corso. Moreover, despite the years that have passed since the original investigations, and despite the growth of our understanding of psychophysics in general, there have been almost no experimental studies directed at this problem in approximately 10 years. The rise of signal detection theory has indeed given new sophistication to the analytical techniques and understanding of the contemporary psychophysicist (*4*). It has also led to an almost complete lack of attention to the analysis of basic noise-free detection phenomena. The subtle nature of the discrete mechanism, if it exists, will require direct attack with carefully designed experiments. It is unlikely to appear as a side effect in the study of a different problem. Moreover, when one uses sophisticated pay-off measures and probabilistic presentations of signals, techniques which are such an essential part of experiments done in the tradition of signal-detection theory, there is almost guaranteed less stability in the maintenance of a decision criterion. Decision strategy appears to be at the heart of the matter in distinguishing between continuous and discrete sensory states. Krantz (*5*) offers a thorough analysis of the situation and suggests several possible experimental tests. New data are needed which examine both operating characteristics and conditional response probabilities.

In summary, I find Corso's rebuttal to the article by Stevens does not help in selecting between the competing theories. The paper by Stevens was useful, for it put together in one convenient place most of the favorable arguments and data. Were I forced to choose sides, I would clearly lean in favor of a quantal hypothesis, but I would prefer not to do that. It is my belief that the issue is a fundamental one, as yet unresolved one way or the other (*6*).

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6. Were S. S. Stevens able to read my reply, I am sure that he would respond to me with the same patient voice that I have heard so many times, demonstrating that we certainly did not need more data, for did not his article indicate the large amount of supportive data that already exist? Moreover, he would probably be sure to remind me that Millikan only needed to measure the charge of the electron once: the failure of others to replicate that measurement says more about the difficulties of the experiment (and the skills of the investigators) than of the truth of the observation. And so it would have continued. My feeling is simply that it would indeed have been better had Stevens been able to reply in his own behalf.
7. I thank David Green, R. Duncan Luce, Edward Newman, and Didi Stevens for their helpful comments.

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Biological Proportions

McMahon (*1*) has given an excellent demonstration of the structural principles limiting the proportions of organisms and, consequently, the metabolic rates, by using data for terrestrial mammals and tree trunks. It is not clear from his discussion, however, whether his argument is equally applicable to aquatic organisms, which are under very different structural constraints. Tensile strength is often more important than buckling or bending limits in aquatic forms.

His correlation of metabolism with body weight raised to the ¾ power rather than total body surface seems less easy to generalize. In plants, support is not a metabolically active process. Also, plants and many aquatic animals have greatly expanded surface areas to increase absorption of energy or material (*2*); in these organisms surface area may be relatively more significant to function (*3*) and thus metabolism than in the organisms discussed by McMahon.

The application of engineering principles to biological problems shows great promise, as McMahon has demonstrated. I hope that he will extend his work to a greater variety of organisms.

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