become more extreme as a constant physical ratio is moved up the scale. This variance of "difference" judgments of constant physical ratios would also violate Fechner's law, assuming a subtractive model of comparison.

To give a complete account of psychophysics, a theory should not only accommodate single stimulus judgments; it should also explain the results of experiments in which the operations of comparison and combination can be tested. Theories of comparison permit a decomposition of the judgment function from the subjective scales, indicating that principles of the judgment function that apply to single stimuli also apply to judgments of stimulus comparisons. For example, Rose and Birnbaum (1975) found that "ratios" and "differences" of numbers could be fit by assuming that subjects compare stimuli by subtraction, using scale values that agree with the context-free scale derived from range-frequency theory applied to category ratings of single stimuli presented in different frequency distributions. Such experiments, which allow a separation of psychophysical and judgmental processes, impose greater constraints on the theoretical possibilities but do not as yet appear to require new scales of subjective value.

About assumptions and exponents

Robert M. Boynton

Department of Psychology, University of California at San Diego, La Jolla, CA 92093

Krueger seems to have convinced himself that despite more than a century of controversy about the matter, he has at last discovered the true relationship between physical stimulus and subjective magnitude. I am reminded at once of the sentiment of philosopher Karl Popper (1959) that

> a subjective experience can never justify a scientific statement. . . . Thus I can be utterly convinced of the truth of a statement; certain of the evidence of my perceptions; overwhelmed by the experience: every doubt may seem to me absurd. But does this afford the slightest reason for science to accept my statement? Can any statement be justified by the fact that K.R.P. is utterly convinced of its truth? The answer is, "No"; and any other answer would be incompatible with the idea of scientific objectivity. (p. 46)

There are, inevitably, unproved and probably unprovable assumptions in Krueger's argument. First, there is the crucial question of whether subjective measurement is even possible. In a recent chapter on psychophysics (Boynton 1984), I discussed this problem in a section on "Viewpoints concerning subjective measurement" (pp. 338–43). I offer several examples of the differences between "measurement" as physicists understand it and the "measurement" of sensation, and refer to the work of a famous committee appointed in 1925 by the British Association for the Advancement of Science "to consider and report upon the possibility of quantitative estimates of sensory events" (which ended as a hung jury: Ferguson et al. 1940). I also stress the fundamental problem of subjective measurement of sensory intensity, which is the lack of an unequivocal operation of addition corresponding, for example, to the laying of meter sticks end to end to determine a distance. In my view, one can conclude only that there exists a spectrum of opinion concerning the possibility of sensory measurement, ranging from liberal to conservative, not unlike the wide range of strong opinions related to political matters. In both cases, because what one feels depends on one's personal needs, subjective experiences, and biased assumptions, arguments cease to be scientific ones.

For his part, Krueger *assumes* "that different subjective scaling methods tap, with varying accuracy or fidelity, the same underlying scale"; and although he cites Marks (1974a) for a contrary view, he does not discuss it. Krueger *assumes* that Stevens's "direct methods of psychophysical measurement" provide the "primary evidence" concerning the empirical relation between stimulus and sensation. In dealing with the problem of using number scales to rate sensory impressions, he *assumes* that the number dimension is used only once in making a response and therefore that "it should have the same effect regardless of whether the subject is judging a single stimulus or the difference between two stimuli." In each instance, Krueger states explicitly that the positions he takes are based on *assumptions*. And so, despite some convincing arguments and a scholarly discussion of a very large number of references, Krueger has not succeeded in laying the matter of subjective scaling to rest any more than the others who have labored unsuccessfully in this attempt over so many years.

I also wish to comment on a technical point. The equation (sect. 1, para. 3)

$$S = (S_{max} I^b)/(I^b + \sigma^b)$$

is not the Michaelis–Menten (a.k.a. Naka–Rushton) equation unless the exponent b is unity. Also, unless $b = 1$, the function does not vary from linear to logarithmic as I increases, but instead is nonlinear over its entire extent. This is because, at very small values of I, where σ^b dominates the denominator, the function approaches a power function $S = S_{max} I^b/\sigma^b$, and a power function is linear only if $b = 1$. I am perhaps a bit sensitive about this because it seems to be a little-known fact that David Whitten and I were the ones who originally suggested the need for an exponent of less than one for I in the Naka–Rushton equation (Boynton & Whitten 1970).

Unifying psychophysics: And what if things are not so simple?

Marc Brysbaert and Géry d'Ydewalle

Department of Psychology, University of Leuven, B-3000 Leuven, Belgium
Electronic mail: *gery@blekul11*

Krueger holds that "a unified psychophysical law is proposed in which each jnd has the same subjective magnitude for a given modality or condition, subjective magnitude increases as approximately a power function of physical magnitude with the exponent ranging from near 0 to 1 . . . and subjective magnitude depends primarily on peripheral sensory processes." In this commentary, we will confine ourselves to visual perception and investigate what happens if just one factor of the stimulus constellation – background luminance – is altered, and what this can tell us about the unified psychophysical law proposed by Krueger. Background luminance is important for Krueger's theorizing because all brightness studies on which he relies presented small visual stimuli against a uniform background.

First, it is surprising that Weber's law still needs to be refuted in a 1989 review article, and that this has to be done on the basis of deviations at the very low end of the brightness continuum (Holway & Pratt 1936; Woodworth 1938). (Note that the situation may be different for audition; Jesteadt et al. 1977.)

As early as 1916, Cobb noted that the Weber fraction is smallest when stimulus and surround have about the same intensity and that it increases as the difference between stimulus and surround grows (though more so when the stimulus is darker than the background). These results were confirmed later by Heinemann (1961) and Brysbaert and d'Ydewalle (in press; see Figure 1). The data in Figure 1 are difference limens obtained with the up-and-down transformed response rule (Wetherill & Levitt 1965) using two stimuli with a diameter of 8.2 degrees against a background of 41 × 20.5 degrees. Stimuli were separated by 8.2 degrees. If the data are plotted as Weber fractions against the difference between the stimulus and the background, a positive relationship is obtained with a steeper

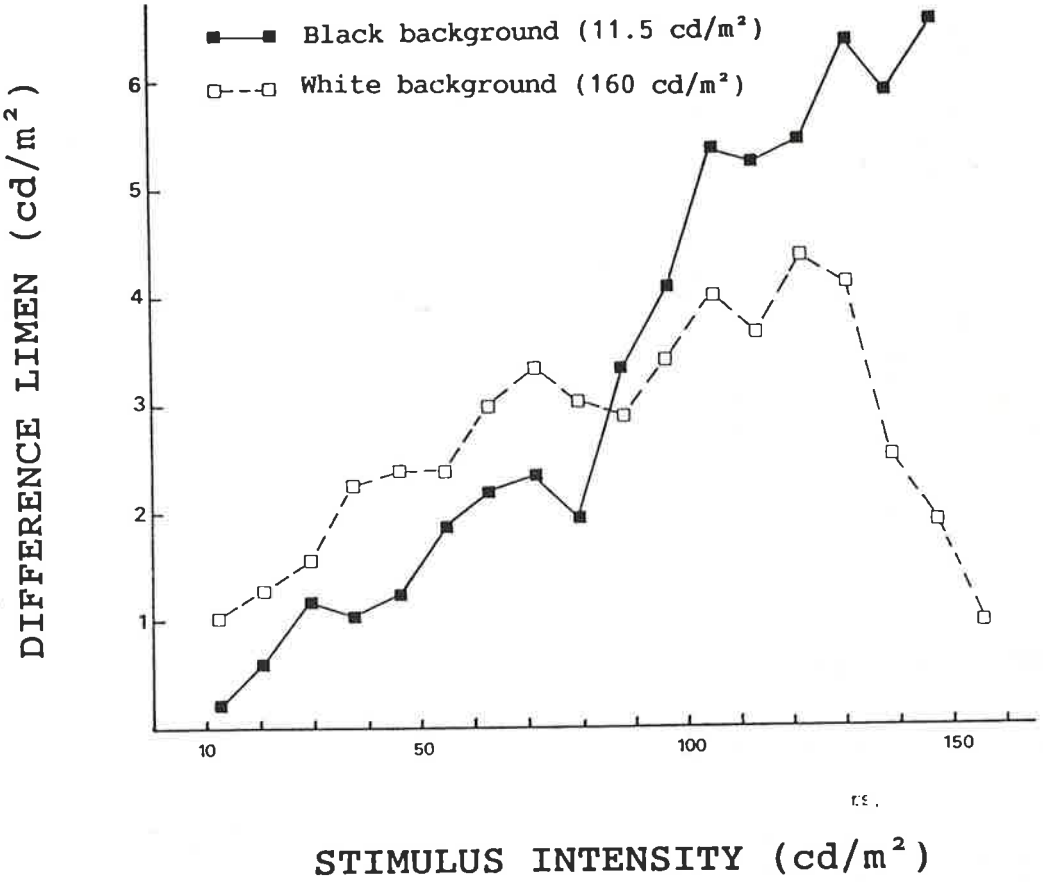

Figure 1 (Brysbaert and d'Ydewalle). Difference limens as a function of stimulus intensity and background luminance.

slope for the white background than for the dark (Brysbaert & d'Ydewalle, in press; see also Heinemann 1961). Hence there are more compelling reasons to distrust Weber's law in brightness perception than the drop of the Weber fraction at the very low end of the scale.

The results shown in Figure 1 cast doubt on Weber's law, but need not contradict Krueger's statement that the subjective jnd is constant. In that case, however, Krueger can no longer reconcile the data of Figure 1 with one of his other major statements, that the psychophysical function is a simple "compressive function." To illustrate our position, Figure 2 gives an estimate of how the psychophysical function should look to account for the data of Figure 1, if the statement about a constant subjective jnd is true.

Only when the background is darker than the stimuli does a simple compressive function account for the data. Otherwise, a more complex function must be invoked. Note that the Brentano–Ekman–Teghtsoonian model produces about the same picture as Figure 2 (actually, that model was originally used to interpret the data of Figure 1; (Brysbaert & d'Ydewalle in press; see also Brysbaert & d'Ydewalle 1988).

The psychophysical function for brightness discrimination is likely to have at least two (probably opposite) components if Krueger's assumption of constant subjective jnds is right. One component is the intensity of the stimulus as indicated by Krueger, and the other is the difference between the intensity of the stimulus and the background. Disregarding the latter component, as Krueger did, can only provide an approximation of the true psychophysical function when the background is darker than the stimulus.

We admit that Krueger's target article was based mainly on studies using a dark background and that it provides a better understanding of that particular instance, but we cannot accept his (and others') assumption that a dark background is no background. Such an assumption would only be valid if there were a linear relationship between the intensity of stimulus and background. This is very unlikely, as can easily be demonstrated by the fact that the bisection value between two standard stimuli changes as the background luminance alters (Brysbaert & d'Ydewalle 1984; in press; Delboeuf 1873, p. 50; Helson 1964, p. 178). Therefore, the difference between stimulus and background needs to be included in every psychophysical law, when the background is assumed to have a subjective magnitude of zero just as when it is not.

Putting things together, it is clear that Krueger needs to drop one of his major statements: either that the subjective magnitude increases solely as a power function of the physical magnitude of the stimulus with an exponent ranging from near 0 to 1, or that each jnd has the same subjective magnitude. We think that the former should more logically be dropped than the latter because it does not substantially affect the situation described in the target article (i.e., a background with subjective magnitude equal to zero).

Krueger was very successful in bringing together evidence that different measurement scales point to the same underlying psychophysical process, but we doubt that he has formulated the true unified psychophysical law. His law must account for more phenomena than it does now. Elsewhere (Brysbaert & d'Ydewalle 1988) we have stated that a general theory of brightness perception should, among other things, give an answer to the following questions: Why does Stevens's exponent differ for partition scales and ratio scales (Stevens 1975)? Why is Stevens's exponent influenced by the luminance of the background or a glare source (Stevens 1975)? Why does the subjective average between two brightnesses increase as background luminance increases (Brysbaert & d'Ydewalle 1984; Helson 1964, p. 178)? Why does the difference limen and the Weber fraction increase when the difference between stimulus and surround increases

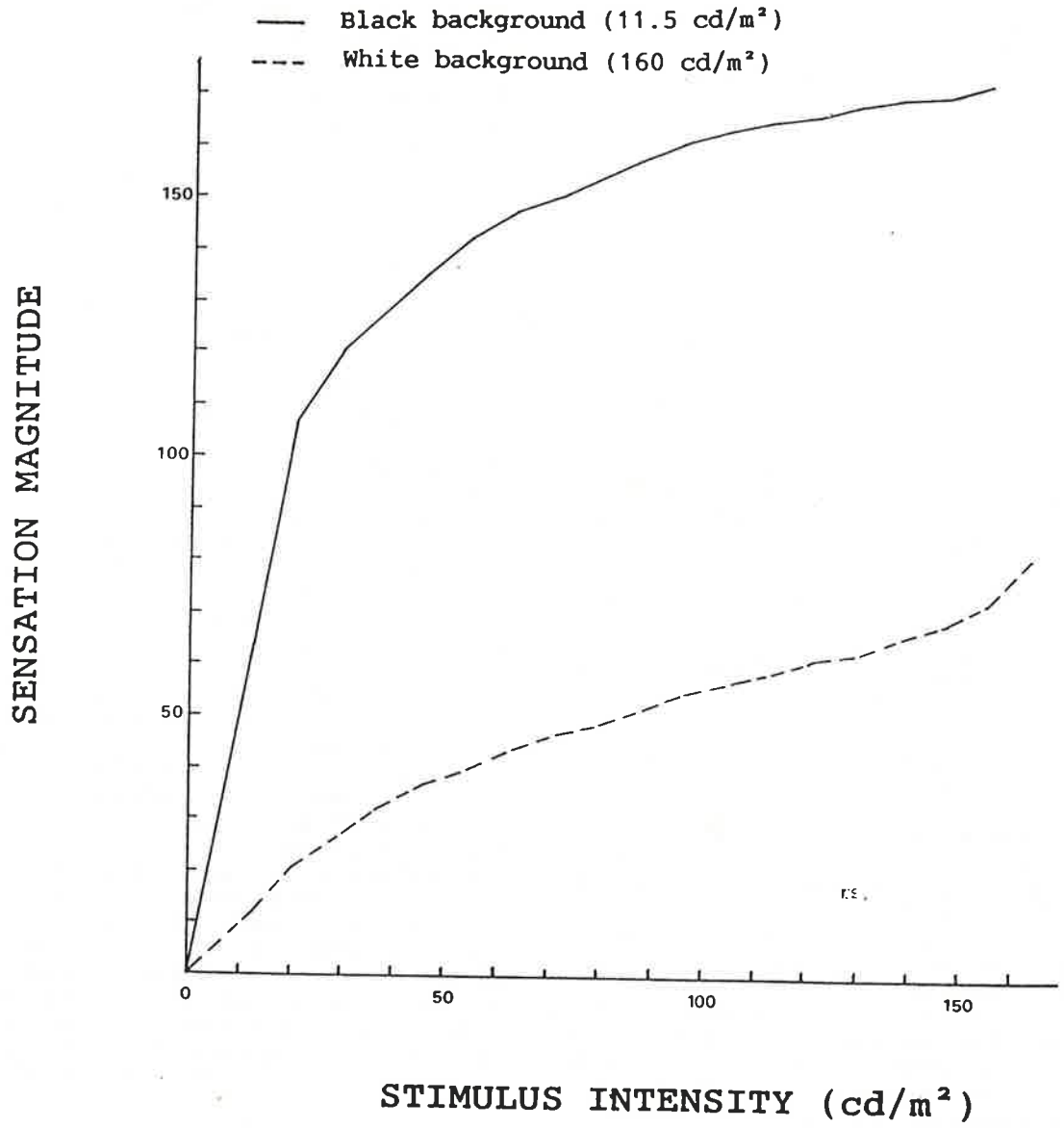

Figure 2 (Brysbaert and d'Ydewalle). Estimation of the psychophysical function against different background luminances based on the data of Figure 1 and Krueger's assumption that each jnd has the same subjective magnitude (linear interpolation between data points).

(Heinemann 1961; Brysbaert & d'Ydewalle in press; see above)? And why is brightness constancy better achieved when the surround is brighter than the stimulus (Freeman 1967)? Krueger answer only the first question, and however useful this may be, a lot of work remains to be done to establish the true psychophysical function. Whether he will then still hold that the transformations occur in early processes is very doubtful. Nevertheless, we sincerely hope he will develop the work presented here.

Jnds and ROCs

Donald D. Dorfman
Department of Psychology, University of Iowa, Iowa City, IA 52242

Many years ago, Thurstone (1927a; 1948) showed that Weber's law and Fechner's law are basically independent. He pointed out that Weber's law can be expressed as

$$P(I_i < (k + 1)I_i) = p,\ I_j = (k + 1)I_i, \qquad (1)$$

where (I_i, I_j) is a pair of stimuli, $<$ denotes the binary relation "judged less than," k is Weber's constant, and p is some constant proportion betwen 0.5 and 1 (p is often set at 0.75 when the method of constant stimuli is used). By contrast, Fechner's law is

$$S = k^{-1} \ln (I/I_o), \qquad (2)$$

where S denotes subjective magnitude, k is Weber's constant, I is stimulus magnitude, and I_o is threshold stimulus magnitude. The term S, subjective magnitude, does not appear in Weber's law.

Fechner derived his law under the assumption that each jnd has the same subjective magnitude, i.e., $\Delta S = c$. Krueger accepts that assumption within a modality in the interest of parsimony, but he provides no test of that assumption despite the fact that it is fundamental to his proposal of a unified psychophysical law. The problem is that we need a testable psychological theory of jnds; otherwise, we have no idea of what a summated jnd scale measures in a psychological sense. As Anderson put it (1974a, p. 288), "The measurement of any quantity is organically related to the empirical-theoretical net-