RATIOS OF MAGNITUDE ESTIMATES[1]

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Abstract. Suppose magnitude estimates on successive trials preserve the ratio of random internal representations of the corresponding signals. Then the mean response generally exhibits drift over trials; sequential dependencies usually exist in which case the mean response ratio is not a function of intensity ratio; however, a modified function is if the mean representation is a power function of intensity. Relevant loudness data are cited. The special timing representation is wrong if the sample size is just a function of the signal, but is viable if it is one of two sizes depending on the size of the intensity ratio to the preceding signal. This attention hypothesis is an analogue for intensity of the critical-band notion for frequency.

When using magnitude estimation data in an attempt to infer the underlying psychophysical function, what estimated function should we plot? As we show, the answer is rather more subtle than it first seems.

Until recently the consensus was to follow Stevens' original approach, namely, to plot some central tendency of the responses – either mean, median, geometric mean, or some mixture of these – against some physical measure (usually additive) of the signal variable. Indeed, if one collects but two observations at each signal level from each observer, little else is possible. If, however, one decides to approach magnitude estimation much like the rest of psychophysics – using well practiced observers, collecting many observations at each of many signal levels, and, perhaps, studying more of the response distribution than just its central tendency – then other options become available. In particular, response ratios are a serious possibility.

Three motives underlie the collection of large samples of response ratios. First, magnitude estimation instructions to observers have come, over the years, to be standardized in a way that (*i*) does not assign a standard response to any particular signal and (*ii*) urges them to reflect in their responses the subjective ratio of signals. If we take our own instructions seriously, then surely it is response ratios, not the responses themselves, that matter, and so perhaps we should study them. This point was made by Krantz (1972) in the context of algebraic models for cross-modality matching, by Marley (1972) in his internal state probabilistic model, and by Ward (1973) in analyzing sequential dependencies. The reason Stevens never plotted response ratios was, presumably, because in his view the central tendency of the responses is a

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power function of signal level. The ratio of central tendencies is then the same power function of the corresponding signal ratio. Therefore, it really does not matter which function one plots, and the former is both simpler and requires fewer data. As we will see below, this argument glides over some complications in at least one quite general and moderately plausible model for the response behavior. If, indeed, we elect to study response ratios, then since N signals yield $N(N-1)/2$ unordered pairs of successive signals, we will need many more responses to each signal than we usually collect in order to maintain the same quality of the estimates. An increase in sample size of $(N-1)/2$ is needed just to estimate the mean response ratios, and unless we find a way to collapse the data over signal pairs the problem is much more severe when we wish to study the entire distribution of response ratios.

A second motive for attending to response ratios arises from one account of the existence, in both absolute judgment and magnitude estimation data, of strong sequential effects due to the preceding signal and much weaker effects due to earlier ones (Cross, 1973; Garner, 1953; Holland & Lockhead, 1968; Ward, 1972, 1973; and Ward & Lockhead, 1970, 1971). One suggestion is that they arise from the indirect effect of the observer using the preceding signal and his response to it as the standard against which to compare the present signal. We explore below one version of this hypothesis.

A third motive is an interest in the form of the distribution of response ratios. This arises from the possibility that these distributions contain useful information about the underlying representations of the signals and the decision mechanism that generates the responses. Of course, Stevens (1957, 1959, 1961a, b, 1971) held the view that variability in magnitude estimates is uninformative noise, or at best a second-order phenomenon correlated with the central tendency. As such, it is no more capable of informing us about the underlying perceptual process than the noisiness of a voltmeter is informative about either the voltage being measured or the working of the meter itself. When the variability is small, this position is probably sound; but in magnitude estimation it is not small, either between individuals or for repeated observations on the same individual. An alternative view holds that this variability reflects something basic about the perceptual process itself. If so, then we need accurate estimates of these distributions.

The Response Ratio Hypothesis

Magnitude estimation instructions seem to make sense only if the stimulus attribute causes an internal change that can be represented by a number, and these responses can be chosen so as to maintain (at least some) ratios of these representations. Further, since these responses, like those in the rest of psychophysics, are variable, it seems plausible for the representation actually to be a random variable, with some unknown probability distribution that is a function of the signal presented. To be more specific, let $\mathbf{S}_n$ be a random variable representing the signal presented on trial n – either a numbering of the signals, e.g., 1, 2, ..., or a physical measure of the attribute being varied, e.g., 20, 30, 40, ... dB, – and let $\mathbf{R}_n$ be the number (a random variable) emitted by the observer. Both of these RVs are observables, with the distribution of the former under the experimenter's control. In addition, we postulate the existence of an internal representation $\mathbf{X}(s)$ when signal s is presented; note that we assume $\mathbf{X}$ does not depend directly on the trial number, n.

The first hypothesis that comes to mind, the one suggested by Ward (1973), is that the observer uses his representation and response on the preceding trial as a standard against which to generate the current response from the current representation, i.e., for all trials n,

$$\mathbf{R}_n = C\mathbf{R}_{n-1}\mathbf{X}(\mathbf{S}_n)/\mathbf{X}(\mathbf{S}_{n-1}). \tag{1}$$

This model is, at best, a first approximation since by induction we see that

$$\mathbf{R}_n = C^{n-1}\mathbf{R}_1\mathbf{X}(\mathbf{S}_n)/\mathbf{X}(\mathbf{S}_1). \tag{2}$$

So, in effect, given the first representation and response, R_n is uniquely determined by the present representation, and no sequential dependencies are predicted. The data showing their existence are overwhelmingly clear.

The failure of this simplest model leads one to consider slightly more complicated alternatives. One idea is that an internal representation of a signal is destroyed when it is used to generate a response. Put another way, a representation of a signal can be stored, but once it is removed from memory to generate a response it is lost. If so and if representations from successive trials are used to generate the responses, then it is necessary to assume that, when signal s is presented, two independent representations, $\mathbf{X}(s)$ and $\mathbf{X}^*(s)$, arise. We do not assume that the distributions of $\mathbf{X}$ and $\mathbf{X}^*$ are necessarily the same. Presumably the quality of these representations is poorer in some sense than when only one is needed as, say, in detection and discrimination experiments. The fact that peripheral neural representations of visual and auditory signals are encoded on a large number of parallel fibers means that it would not be difficult for the nervous system to use one set for $\mathbf{X}$ and another for $\mathbf{X}^*$. Given these two independent representations, the *response ratio hypothesis* takes the form

$$\mathbf{R}_n = C\mathbf{R}_{n-1}\mathbf{X}(\mathbf{S}_n)/\mathbf{X}^*(\mathbf{S}_{n-1}). \tag{3}$$

This hypothesis has been studied by Marley (1973) (with $C=1$ and under strong restrictions on the distributions of $\mathbf{X}$ and $\mathbf{X}^*$) and by Luce and Green (1974). We summarize here the main conclusions of the latter paper. First, we make the following weak assumptions about the distribution functions of $\mathbf{X}$ and $\mathbf{X}^*$:

(i) $F(s)=E[\mathbf{X}^*(s)]$ exists for all signals s.

(ii) $F^*(s)=E[1/\mathbf{X}^*(s)]$ exists for all signals s.

(iii) The distribution function of $\mathbf{S}_n$ is independent of n and $B=E[F(\mathbf{S}_n)\,F^*(\mathbf{S}_n)]$ exists.

Equation 3 together with these three assumptions lead to the following four equations involving expected responses and response ratios:

$$E(\mathbf{R}_n \mid \mathbf{S}_n = s) = (CB)^{n-1}\mathbf{R}_1 F(s), \tag{4}$$

$$\frac{E(\mathbf{R}_n \mid \mathbf{S}_n = s \;\&\; \mathbf{S}_{n-1} = s')}{E(R_n \mid S_n = s)} = \frac{F(s')\,F^*(s')}{B}, \tag{5}$$

$$E\left(\frac{\mathbf{R}_n}{\mathbf{R}_{n-1}} \,\middle|\, \mathbf{S}_n = s \;\&\; \mathbf{S}_{n-1} = s'\right) = CF(s)\,F^*(s'), \tag{6}$$

$$\frac{E\left(\frac{\mathbf{R}_n}{\mathbf{R}_{n-1}} \,\middle|\, \mathbf{S}_n = s \;\&\; \mathbf{S}_{n-1} = s'\right) E(\mathbf{R}_n \mid \mathbf{S}_n = s)}{E(\mathbf{R}_n \mid \mathbf{S}_n = s \;\&\; \mathbf{S}_{n-1} = s')} = CB\,\frac{F(s)}{F(s')}. \tag{7}$$

We discuss each separately. Equation 4 is the expression for the ordinary magnitude function. It exhibits two important features. First, except when $C = 1/B$, this expectation drifts over trials. The existence of drift was shown clearly by Ward (1973) and by Green & Luce (1974) in their data, and it has been noted informally by others; its existence is one of the reasons that many experimenters are hesitant about collecting repeated responses from individual observers. One might anticipate that a well-practiced observer would learn to choose C so as to eliminate the drift, but the process is not completed even after thousands of trials. Obviously, the existence of drift in this model, and in some data, make it difficult to study the distribution of responses to a given signal since the estimated distribution is a mix of the actual distribution and the drift. Second, aside from the drift, the magnitude function is proportional to the mean, $F(s)$, of the $\mathbf{X}(s)$ representation.

Equation 5 is a convenient form in which to study sequential effects. We see that they exist if and only if $F(s)\,F^*(s)$ depends on s; moreover they are assimilative – the response on trial n tends toward the signal on trial n – as in the data (see Figure 1 below) if and only if $F(s)\,F^*(s)$ is an increasing function of s.

Equation 6 is the natural response ratio to study. A desirable feature of this function is that it exhibits no drift; an undesirable one is that the right side depends on two different functions of s and s'. The only case in which they would be the same is if $F^*(s') = D/F(s')$; however, this is equivalent to there being no sequential dependencies, which appears to be empirically false (see the data in the next section, but also the interpretation in the following one).

We would prefer to study a function in which $F(s)/F(s')$ appeared on the right, which is the reason for the peculiar function in Equation 7. Note that to the extent that F is a power function of signal intensity, Equation 7 depends only on the ratio of signal intensities, not on their individual values.

In the context of ratio estimation, Equation 6 has received some attention. Writing $\mathbf{R}(s, s')$ for the ratio judgment when s and s' are presented, the analogue to Equation 1 seems appropriate,

$$\mathbf{R}(s, s') = C\mathbf{X}(s)/\mathbf{X}(s'),$$

also

$$E[R(s, s')] = CF(s)/f^*(s'),$$

where

$$f^*(s') = 1/F^*(s').$$

Comrey (1950) (see Torgerson, 1958, pp. 104–112) suggest the special case $C=1$ and $f^*=F$. Sjöberg (1962), using data from three papers, compared this with $C \neq 1$ and $f^*=F$ and with the general result. He found the first two cases inadequate, as we would expect if there are sequential dependencies. These ideas, along with statistical procedures and further data, are reported in Sjöberg (1971). Svenson and Åkesson (1966a, b; 1967) studied empirically the function

$$E[R(s,s')]\,E[R(s',s)] = C^2 \frac{F(s)\,f^*(s)}{F(s')\,f^*(s')}.$$

Some Data

According to Equation 5, a good way to estimate $F(s')\,F^*(s')$, and so to study sequential effects, is to average $E(\mathbf{R}_n \mid \mathbf{S}_n = s \;\&\; \mathbf{S}_{n-1} = s')/E(\mathbf{R}_n \mid \mathbf{S}_n = s)$ over s. These functions for the data reported by Cross (1973) and Ward (1973), both averaged over observers, and for the six observers studied by Green & Luce (1974) are shown in Figure 1. In each case, the sequential effect is assimilative.

The six observers of Green & Luce and the group data of Cross are all remarkably similar, with relatively slight sequential effects. Ward's group data exhibit a much more pronounced effect.

The model Cross fitted to his data is, in essence, ours with

$$F(s) \sim I(s)^{\beta - b}$$
$$F^*(s) \sim I(s)^{2b-\beta}$$

and so

$$F(s)\,F^*(s) \sim I(s)^b,$$

which means the plotted functions should be straight lines with slope b. In his case $\hat{b} = 0.027$; the other estimated slopes are given in the figure caption. Whether the data are best described by power functions is obscure; they do not give a bad fit, but many other functions would be at least equally good.

Since in our data FF^* is nearly a constant, it should not matter greatly whether we plot Equation 6 or 7. They, together with the ordinary magnitude function, all normalized to agree at the left-most point, are shown in Figure 2, and as we anticipated there is little difference between the ratio functions. Note, however, that for observers 2, 4, and 5 the ratio functions differ by as much as 10 dB from the magnitude functions and are considerably closer to power functions. We do not know if this is due to drift or something else.

As we noted earlier, to the extent that these functions appear to be power functions, the quantity of Equation 7 should depend only on the signal ratio, not the individual values. Let $H(s;d)$ denote the left side of Equation 7, where $d = I(s)/I(s')$, and let

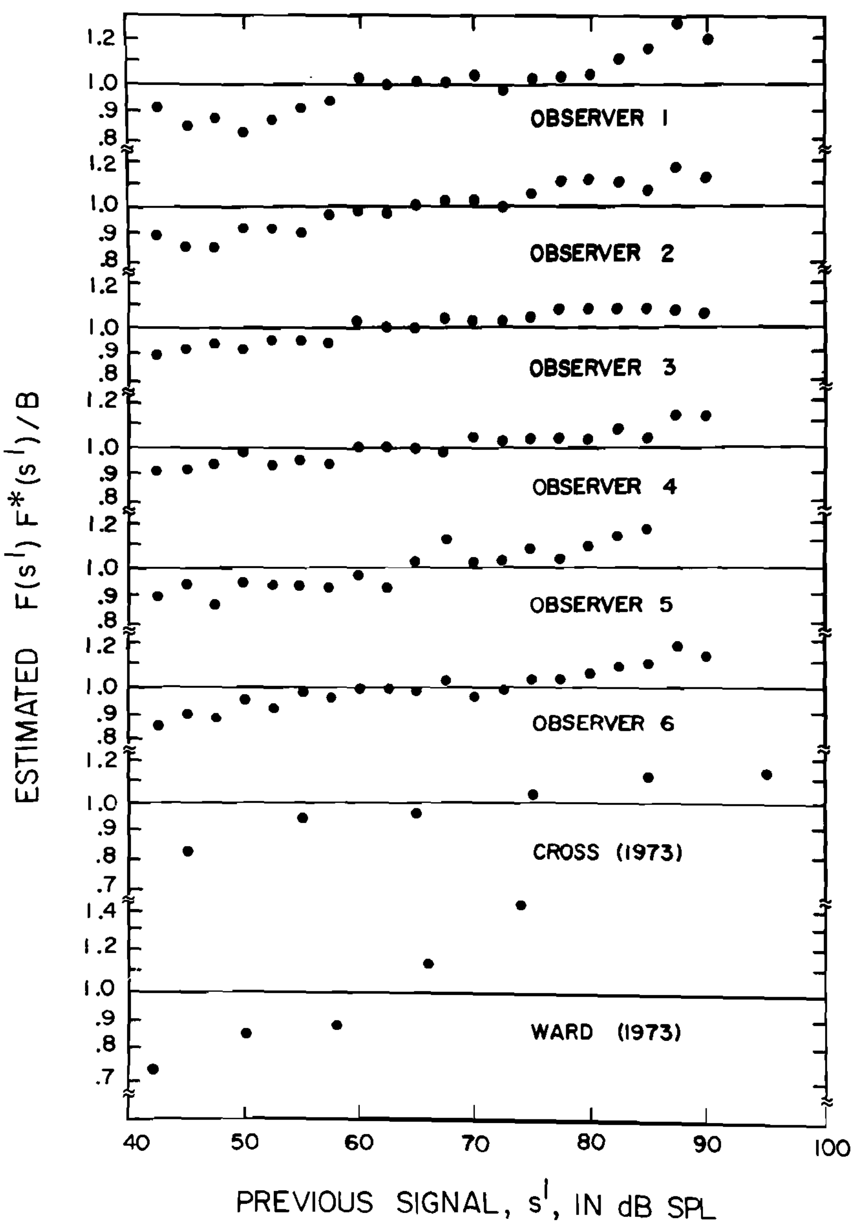

Fig. 1. Estimates of $F(s')\,F^*(s')/B$ using Equation 6 from group data reported by Cross (1973) and Ward (1973) and from individual data reported by Green & Luce (1974). Reading from top to bottom, the exponents of power functions to fit these data are: 0.030, 0.028, 0.019, 0.019, 0.027, 0.022, 0.028, and 0.088. (This is Figure 1 of Luce & Green, 1974.)

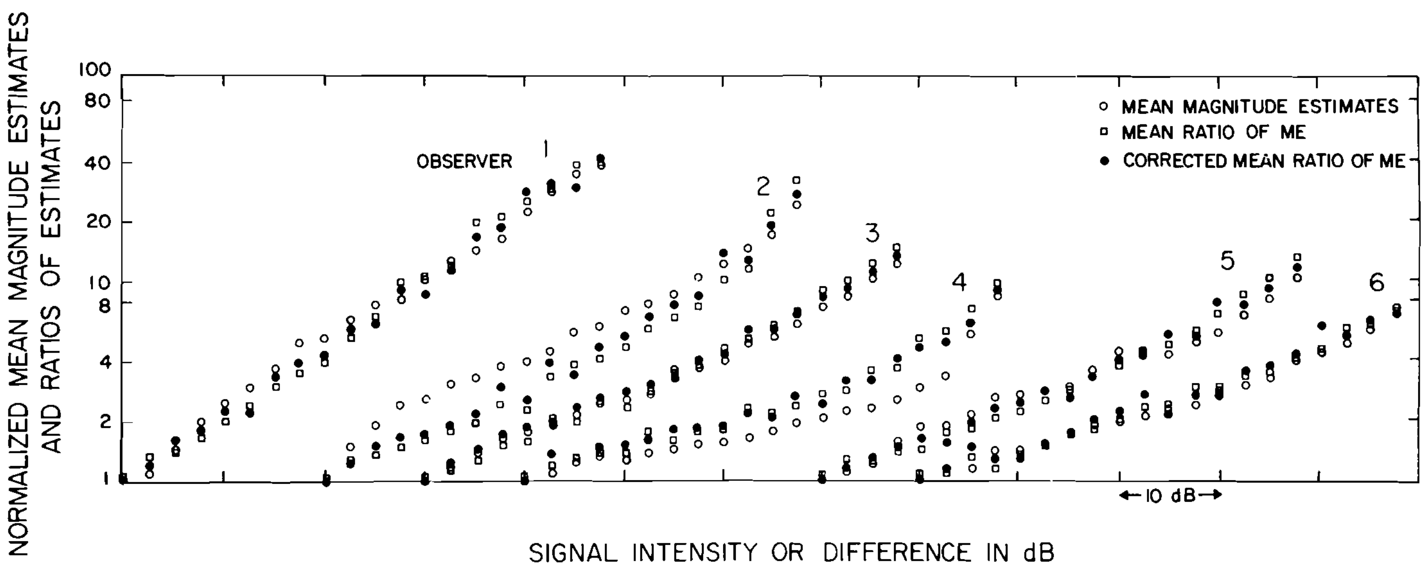

Fig. 2. Plots of mean magnitude estimates, mean ratio of successive magnitude estimates, corrected (Equation 7) mean ratio of successive magnitude estimates versus signal intensity in dB. The first is normalized to be 1 at the lowest signal, 42.6 dB SPL, and the latter two to be 1 at the smallest signal difference, 0 dB. (This is Figure 3 of Luce & Green, 1974.)

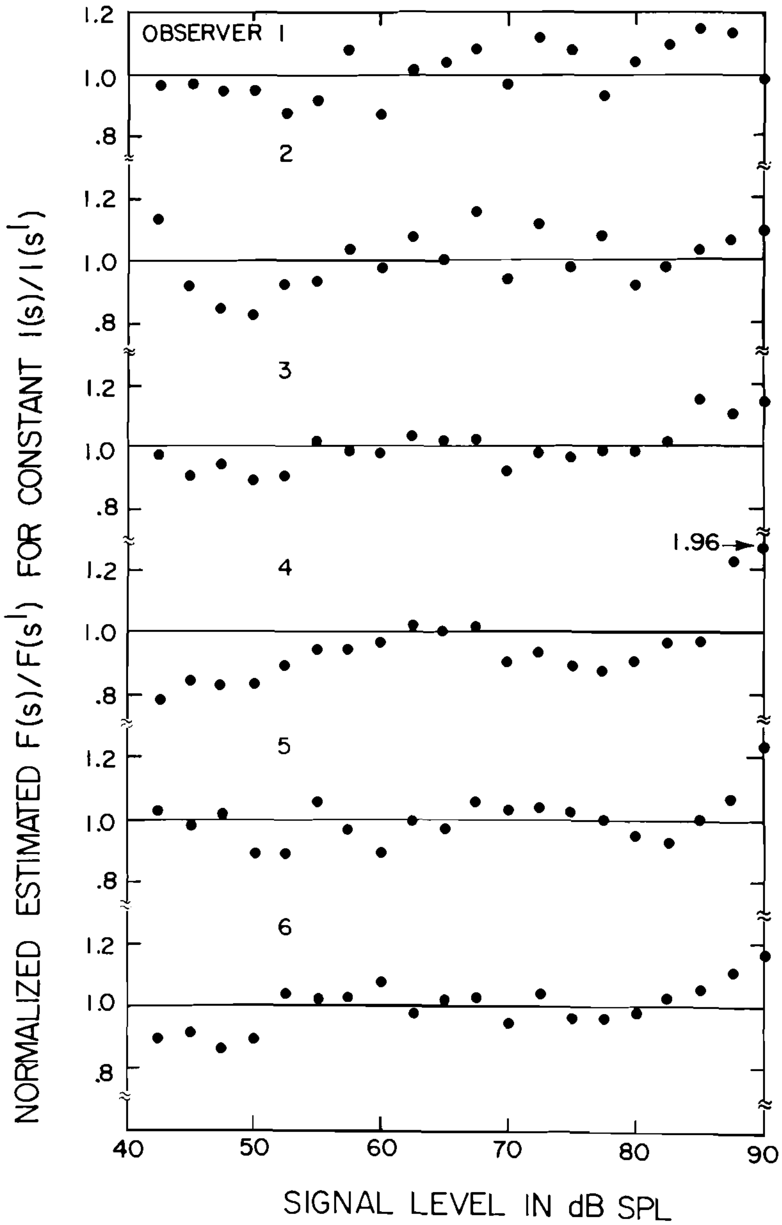

Fig. 3. Estimates of $(1/m)\Sigma_d[H(s; d)/H(\cdot; d)]$ versus $I(s)$. According to Equation 7, this function should be 1 if $F(s)$ is a power function of $I(s)$. (This is Figure 2 of Luce & Green, 1974.)

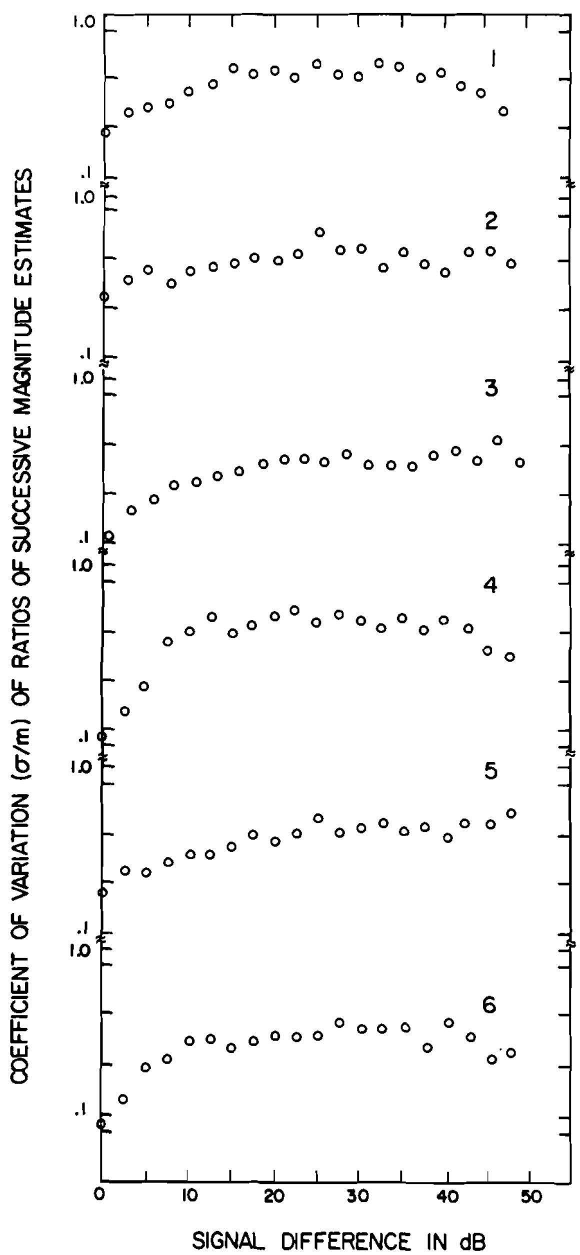

Fig. 4. Coefficient of variation (standard deviation/mean) of response ratio on successive trials as a function of signal ratio (in dB) on those trials. (Adapted from Figure 5 of Green & Luce, 1974.)

$H(\cdot\,; d)$ denote the average of this over all s. We have plotted the average of $H(s; d)/H(\cdot\,; d)$ over all signal ratios d versus $I(s)$ in Figure 3. If the model is correct and F is a power function, then this function should be equal to 1. Although there is a slight tendency for it to increase with $I(s)$ over most of the range and a distinct increase at the most intense signal, which may be due to deviations of F from a power function, to a good first approximation, $H(s; d)$ depends on d and not on s.

Even for our well-practiced observers, there is evidence that response drift occurred, and so it seems advisable to study the distribution of response ratios rather than of the responses themselves. Various studies (Luce & Mo, 1963; Schneider & Lane, 1963) indicate that the standard deviation of magnitude estimates is roughly proportional to the mean, so we elected to plot that ratio for $\mathbf{R}_n/\mathbf{R}_{n-1}$ as a function of $I(s)/I(s')$. This is shown in Figure 4. It is evident that σ/m grows rapidly at first and tapers off to a constant at separations of 20 dB and more.

In some cases, σ/m may decrease again for large intensity ratios. Inspection of the distribution, together with the anomaly in Figure 3 for the 90 dB signal, suggests that the observers may have absolutely identified this extreme case. In contrast, we suspect that the very small variability in the ratio when the signals are close is an important result. In particular, as we discuss in Green & Luce (1974), it may relate to some of the perplexing range effects in absolute identification experiments.

Timing Model and Selective Attention

Luce & Green (1972) proposed a timing model of the following character. When signal s is presented, it activates on each of J channels (not necessarily neural fibers, but some abstraction from them) independent Poisson processes of common intensity $\mu(s)$. The intensity function is assumed to be an increasing function of signal intensity $I(s)$ and, indeed, various data suggest that it is approximately a power function of I. Whenever the observer is to answer a question about $I(s)$, he is assumed to take some sample of these Poisson processes from which he estimates $\mu(s)$. In the timing model, the sample size is assumed to be, $k(s)=J(s)\,\kappa(s)$, where $J(s)$ is the number of channels excited and $\kappa(s)$ is the sample size from an individual channel. The sum $\mathbf{T}$ over channels of the times from the first to the $(\kappa+1)$ st pulse on each channel has a gamma distribution of intensity $\mu(s)$ and order $k(s)$. Since the mean is $k(s)/\mu(s)$, this suggests using the representation $\mathbf{X}(s)=k(s)/\mathbf{T}(s)$. Assuming that, it is easy to show that

$$F(s) = \mu(s)\,k(s)/(k(s)-1)$$
$$F^*(s) = 1/\mu(s),$$

and so

$$F(s)\,F^*(s) = k(s)/(k(s)-1).$$

This implies that if the sample size $k(s)$ is a constant, then the timing model exhibits no sequential dependencies. (Essentially the same result is true for the counting model; see Luce & Green, 1974.)

It is also not difficult to show that the ratio of two such RVs, one with sample size k and the other with k^*, and therefore the ratio of responses on successive trials, is distributed according to the Beta distribution of the second kind with parameters k and k^*. From this it is almost immediate that

$$\left(\frac{\sigma}{m}\right)^2 = \frac{k + k^* - 1}{k^*(k-2)},$$

which means that if the sample sizes are constant, then σ/m is a constant for the timing model.

Both of these statements are clearly contradicted by the data, and so either the timing model is wrong or the sample sizes are not constant. If they are not constant, it is evident that no simple postulate involving a change just with signal intensity is adequate to account for both of these functions. Some rather more subtle idea is needed.

One that appears to account sensibly for these data is the existence of a selective attention mechanism that is the intensity analogue of critical-frequency bands. Specifically, let us suppose that in terms of pulse rates of Poisson processes, there is a band of fixed width in which most attention can be focused in the sense of collecting a large sample, and for all rates outside that band, a considerably smaller sample is collected. For simplicity, we assume $k(s) = k^*(s)$ in both cases, and that the two sample sizes are K and K_0, where $K_0 < K$. Second, we assume that the observer is free to center the band where he wishes in advance of the signal presentation; for this experiment, in particular, he is assumed to center it around the estimated rate of the preceding signal.

Consider first the $F(s)\,F^*(s)$ function. For any pair of trials it is either equal to $K/(K-1)$ or $K_0/(K_0-1)$, where the former is smaller than the latter. Which value it has depends on whether or not the preceding signal, s', was in the region of greater attention. We argue that this is more probable for low intensities than for high, and hence FF^* should grow with intensity The reason is that the band is of constant width in pulse rate, which in turn is roughly a power function of signal intensity; hence, the band width in dB decreases with intensity.[3] Since the signals were equally probable and equally spaced in dB, it follows that it is somewhat more likely for a weak than for a strong signal to lie in the attention span of a randomly presented signal.

Second, consider the σ/m function. Let $P(s')$ denote the probability that the preceding signal s' lay in the region of greater attention. If the current signal, s, is close to s', then with high probability it will lie in the region of greater attention and so the estimated $(\sigma/m)^2$ is simply a probability mix of two terms:

$$P(s')\,\frac{2K-1}{K(K-2)} + [1 - P(s')]\,\frac{K + K_0 - 1}{K_0(K-2)}.$$

[3] To be more formal, suppose the upper and lower intensities of the band of greater attention are I_u and I_l, that $\mu = \alpha I^\beta$, and $\mu_u = \mu_l + \theta$ where θ is a constant. Then,

$$\log I_u/I_l = (1/\beta)\log(1 + \theta/\mu_l).$$

If, for example, $\alpha = 0.1$, $\beta = 0.3$, and $\theta = 50$, the ratio of bandwidths in dB at $I_l = 10^3$ and $I_l = 10^8$ is 3.8.

On the other hand, if the current signal lies sufficiently far from s' so that it is outside the region of greater attention, the estimated (σ/m^2) is the mix:

$$P(s')\frac{K+K_0-1}{K(K_0-2)}+[1-P(s')]\frac{2K_0-1}{K_0(K_0-2)}.$$

It is easy to see that for all $P(s')$ the latter expression is greater than the former. Were the band to be rigidly centered on the true rate of the preceding signal, then for any fixed value of s' there would be a discontinuous shift in σ/m at the boundary of the band. Since, however, the centering is assumed to be on the estimated rate of the preceding signal, which is variable, the transition is smoothed. In addition, the band width in dB varies with s', which also smooths the observed transition.

Third, consider the expected ratio of responses which, by Equation 6, is proportional to

$$F(s)\,F^*(s')=\frac{\mu(s)}{\mu(s')}\left[\frac{k(s)}{k(s)-1}\right].$$

Thus, the factor multiplying $\mu(s)/\mu(s')$ is less when s is near s' than when they are widely separated. This means that if μ actually is a power function of intensity, then the attention model implies that the observed curve in log-log coordinates should be concave up, not a straight line; this appears to be the case (see Figure 2).

Discussion

Let us make explicit where our path has led us. We began with a very simple response ratio hypothesis, embodied in Equation 3, and its predictions, formulated as Equations 4–7. Looking at the data as suggested by this model uncovered evidence of weak sequential effects and of a marked reduction in the variability of the response ratio when the separation between successive signals is small (<20 dB). Other work led us to examine a special case of the response ratio hypothesis suggested by the neural timing model. Neither the version with a constant sample size nor one with a sample size that depends just on the signal presented were consistent with these two empirical functions. For this model to hold, the sample size must depend on both the present and preceding signal. We suggested a two-state attention model in which a large sample is taken if the present signal lies within a band centered about the estimated rate of the preceding one and a smaller one is taken otherwise. This seemed adequate to explain these data. Experiments need to be designed to study this hypothesis more directly.

It is important to realize that if this line of argument is correct, then the nature of the sequential dependencies rests on the relation between the spacing of signals and that of the attention band. It is assimilative when the signals are equally spaced in dB because the band in dB narrows as intensity is increased. Were we to space the signals so that their density increases sufficiently with intensity, this model predicts an ultimate reversal in the nature of the dependencies.

To generalize the attention-timing model, one must reformulate the response ratio hypothesis. In particular, a pair of *RV*s $\mathbf{X}(s)$, $\mathbf{X}^*(s)$ is observed when s is close to s'

and another pair $\mathbf{X}_0(s)$, $\mathbf{X}_0^*(s)$ when s is far from s'. This means that $\mathbf{R}_n/\mathbf{R}_{n-1}$ is one of four possible ratios. The timing model has the special simplifying feature that $E(1/\mathbf{X}^*) = E(1/\mathbf{X}_0^*)$. This general model has not yet been studied.

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