RENEWAL PROCESS MODELS FOR PSYCHOPHYSICS[1]

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1. Introduction

The most venerable, although some feel unromantic, area of psychology is psychophysics: the attempt to describe and understand the abilities and limitations of people (and other organisms) to sense and identify simple changes in one or a few aspects of the stimulation impinging on them. Here, more than elsewhere in psychology, mathematics has played a significant role in the development of theory. The interplay began over a century ago at the founding of psychophysics, which can be credited jointly to Fechner's seminal[2] Elemente der Psychophysik (1860) and Helmholtz' remarkable work on audition (1863). Today, one cannot seriously enter the field without at least knowing well elementary mathematics. As an illustration of the interplay between data and mathematical theory, I shall present a self-contained application of mathematics--renewal processes--to several psychological problems. My tack will be to devote a sizable portion of the paper to the empirical background so that the issues treated by the theory will be meaningful. The remainder will outline the theory and give several applications of it.

By a simple change in stimulation I mean something like a 100 msec flash of light with some known spectral distribution, or the monaural presentation of a pure tone (acoustic sinusoid) of some intensity, frequency, and duration, and so on. It is a change from one steady state to a different one and then (usually) back to the original one, where the difference between the two steady states can be characterized in some fairly simple physical fashion--although rather more care must be taken to describe the effect of transducers (e.g., ear phones) than one might at first imagine. Such a change is normally called a signal, and I shall employ that term.

Typical questions asked by psychophysicists are:

1. To what extent can a person detect a signal of known characteristics?
2. To what extent can he identify the signal presented as one of several preassigned possibilities?

3. To what extent can he <u>discriminate</u> as different two signals presented in close spatial and temporal proximity?

4. To what extent can he <u>scale</u> the magnitude of the change involved when the signal is presented?

In each case the question is at least doubly ambiguous. Just what do we mean by "detect," "identify," "discriminate," and "scale"; and what are suitable measures of "extent"? Of course, it is impossible in a short paper to discuss systematically these issues, but some brief remarks will be useful before we consider any models of processes involved.

2.   Some Psychophysical Measures and Functions

When one asks a subject if a change has occurred--was the signal present or not, which of several possibilities has occurred, or whether one presentation is the same as or different from another, in all these cases there is usually a physically correct answer. And so one measure of "extent" is the accuracy of his answers. For example, suppose we run a series of trials in which the same change--say in the amplitude of a pure tone--occurs on a random 50% of the trials and no change occurs on the remaining 50%. Symbolizing no change as 0 and the change as 1, let N_{ij}, $i, j = 0, 1$ denote the total number of trials on which response j was made to presentation i. An obvious measure of accuracy is the relative frequency of correct responses on the trials when the signal was presented:

$$\hat{p}_{11} = N_{11}/(N_{10} + N_{11})$$

On the assumption of independent trials (an assumption which is approximately correct for well-practiced observers and which we employ throughout), $\hat{p}_{11}$ estimates the conditional probability p_{11} of responding that a signal was presented when in fact it was.

There are at least two difficulties with this measure of accuracy. First, it totally ignores what happens on the trials when no signal is presented. Were it true--which it is not--that N_{00} and N_{01} are independent of various experimental parameters, such as the proportion of catch trials or the instructions to the subject, then perhaps it would suffice to focus on $\hat{p}_{11}$ as an estimate of accuracy. In fact, if we provide information feedback on

each trial about which signal was actually presented, then as we change either

the presentation probability P or the payoffs for correct and incorrect

responses we find that $\hat{p}_{11}$ and $\hat{p}_{01} = N_{01}/(N_{00} + N_{01})$ covary. Typical data

are shown in Figure 1. The smooth curve (perhaps a straight line in the non-

linear coordinates used) thought to underlie these data points is called an
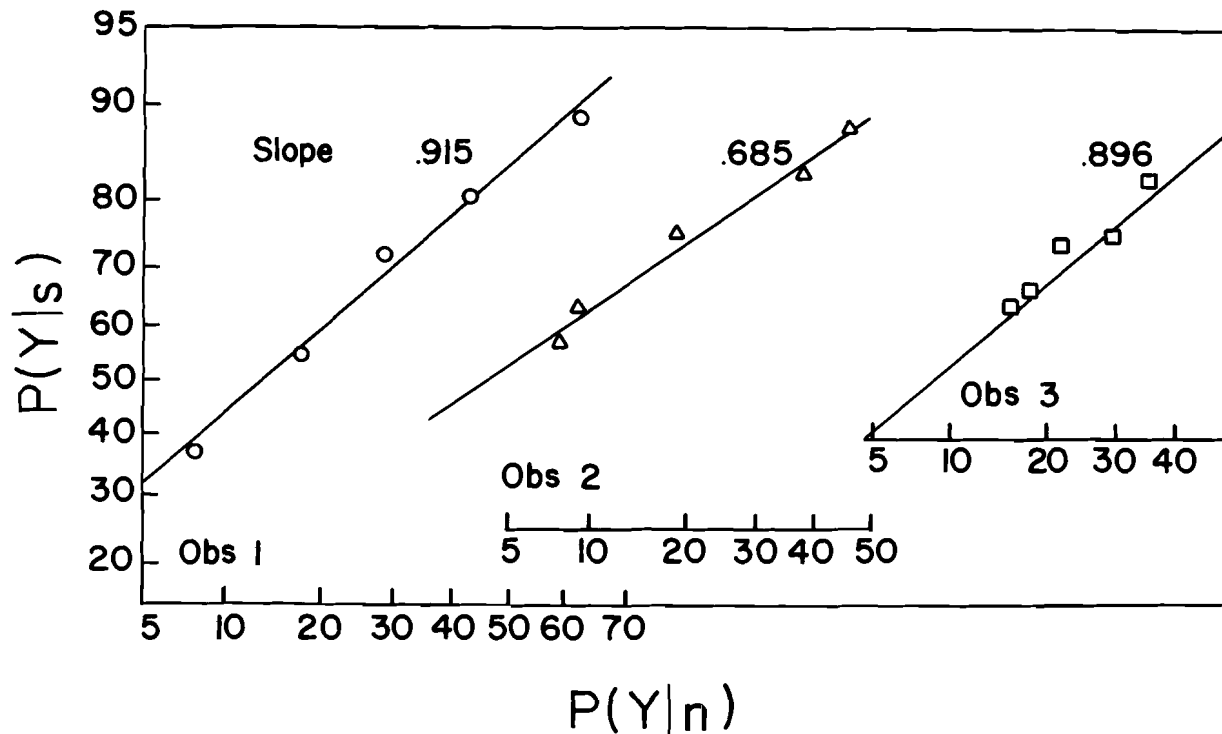

Figure 1. ROC curves for three observers from a Yes-No detection experiment involving a signal in moise (s) versus noise alone (n), i.e., Y=s=1, N=n=0. The coordinates are normal deviates. (This is Fig. 4 of Green and Luce, 1973a.)

ROC (receiver operating characteristic) curve. It describes, in essence, the

tradeoff exhibited by the subject between the two types of errors--false

positives (10) and false alarms (01). The form of the curve simply reflects

the familiar experience that under fixed stimulating conditions one can de-

crease one of these errors only at the expense of increasing the other. So

a better measure of what we are after is some parameter or parameters de-

scribing the ROC curve. As anticipated in Figure 1, we will later arrive at

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a theory in which a transformation to normal probability deviates results in an ROC curve which is close to a straight line, and so two parameters obviously describe it completely.

The story of measuring "extent" is still incomplete, even in this most simple of cases, because of the familiar tradeoff between speed and accuracy. Up to a point, at least, a subject can detect more accurately if the duration of the signal is longer; this is part of the reason most of us drive more slowly under foggy conditions. Because the magnitude of the phenomenon is startling, but not commonly known, some typical data are provided in Figure 2.
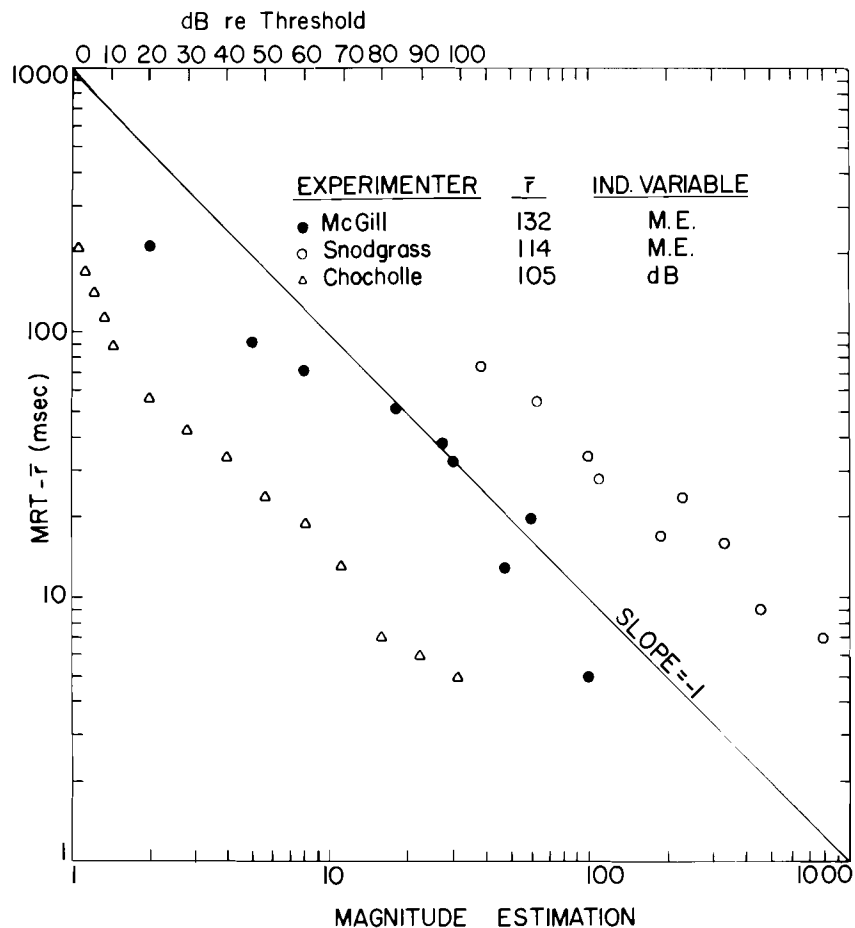

Figure 2. Plots of MRT - $\bar{r}$ versus signal intensity in dB or log ME, which is approximately proportioned to intensity in dB. The value $\bar{r}$ = MRT$_{min}$ - 5 in msec (for the Snodgrass data, the loudest signal, ME = 1500, is not shown). The sources for the data are Chocholle (1940), McGill (1963), and unpublished data of J. G. Snodgrass (personal communication, 1962). (This is Fig. 7 of Luce and Green, 1972.)

Observe that over the range of from just detectable auditory signals to very loud ones, the response time decreases by a factor of at least 3. So we must adjoin to the parameters describing the ROC curve some measure of time taken before the response.

Since, in the absence of a theory, it is expensive to collect and difficult to organize all of the data that can be generated by varing this many variables, it is customary to fix some while varying the others. Usually the signal duration is fixed when studying ROC curves, and the frequency of false alarms is usually required to fall in a narrow range when we study how response time varies with signal strength.

Identification and discrimination studies with two signals are handled in much the same way. When we go to more than two signals, there are difficulties in knowing how to summarize the data, which obviously are considerably more abundant. Theoretical models usually suggest some simplifying measures, but since the half-life of specific models in psychology is brief, this is not very satisfactory for the experimenter. I will not enter into any of these problems here; see, for example, Luce and Green (1973).

As one moves into these areas, and even more so in the scaling area, one becomes aware of a basic change in experimental procedure. There need be no physically correct responses, and so feedback no longer makes obvious sense. Consider, for example, these two similar experiments. In the first, the experimenter presents pairs of tones of the same frequency and different amplitudes, and he asks the subject to judge which is louder. If we assume, as is suggested by all sorts of data, that loudness of a pure tone grows with amplitude, then there is an unambiguously correct answer. Now modify the experiment so that both the amplitude and frequency differ (recall that both affect loudness, else there would be no reason for a loudness control as well as a volume control on an amplifier); then there is no longer a correct response. More generally, if we try to understand the growth of any subjective concept such as loudness, brightness, painfulness, etc., we are asking questions for which information feedback does not seem to make any sense because only the subject can know the answer. Nonetheless, we seem to be able to elicit systematic and meaningful responses from subjects about such concepts. A number of methods are in use, and there is much controversy over which is the best. One of the currently more popular is magnitude estimation, introduced

and developed extensively by the late S. S. Stevens (1957, 1961, 1966, 1971) in which the subject is simply asked to report numerically the subjective ratio of the current signal to some signal presented earlier. Improbable as it may sound, when the subject is appropriately instructed, the data are exceedingly regular. To a good first approximation, mean magnitude estimates of signals that vary in intensity grow as power functions of the usual physical (extensive) measure of intensity. Some data, averaged over subjects, are shown in Figure 3 (see also Figure 7 in Section 5). The average exponents differ
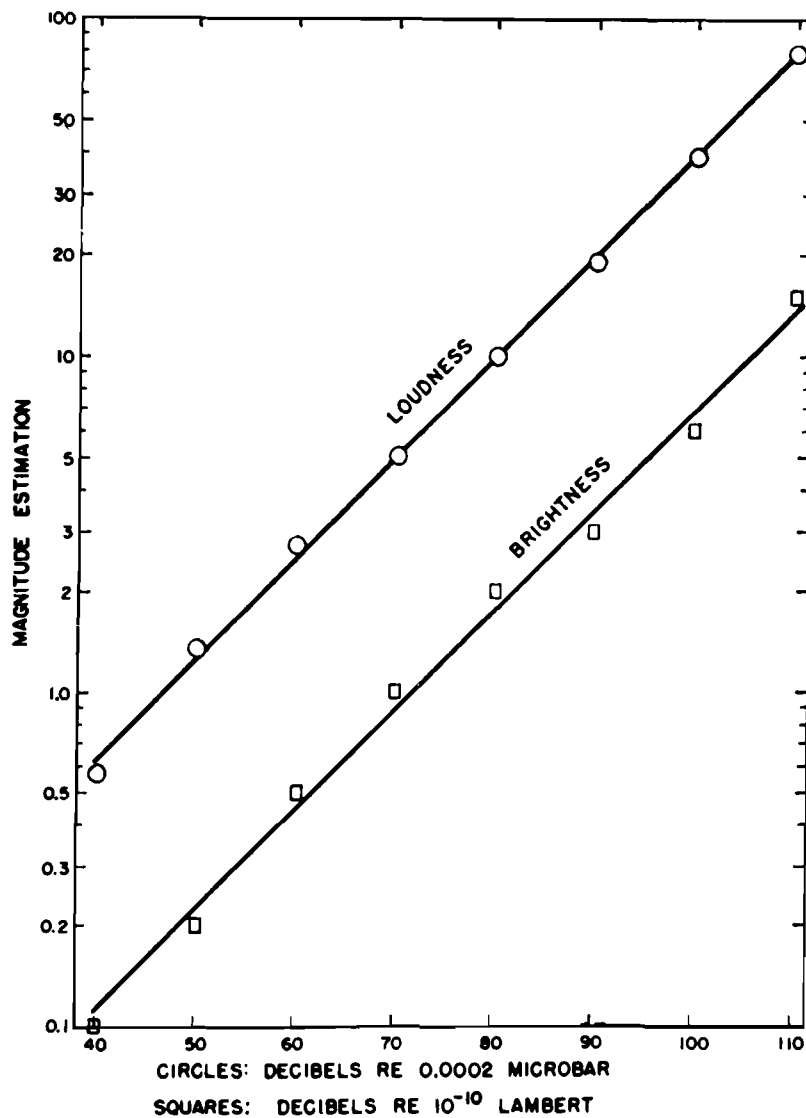

Figure 3. Magnitude estimates for loudness and brightness. Each of 32 and 28 observers, respectively, made two estimates of each stimulus, presented in an irregular order. The resulting numbers were multiplicatively normalized for each observer to have the same value at 80 and 70 dB, respectively, and the results shown are the median estimates. (This is Fig. 6 of Stevens, 1957.)

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for different modalities. For example, it is about 0.30 for loudness, whereas it is about 3.5 for shock (60 Hz, high resistance in series with the subject) as a function of voltage. Indeed, as Teghtsoonian (1971) has shown, a very simple relation holds between the exponents and the range of physical stimulation from just detectable intensities to the maximum accepted by the organism. This is shown in Figure 4, where the smooth curve is that predicted on the assumption that the same range of numbers is used to match each physical range
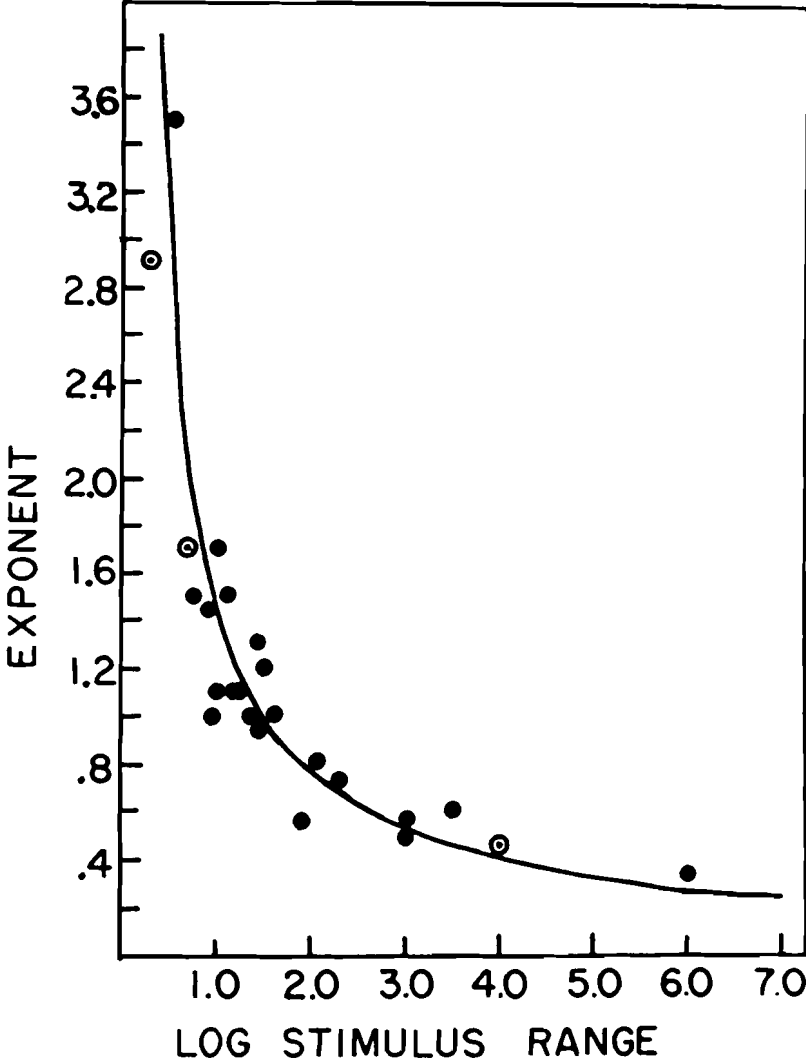

Figure 4, Average magnitude estimation exponents versus the logarithm of the maximum stimulus range, R. The continuous function is 1.53/log R, where 1.53 is a fitted constant. (This is Fig. 2 of Teghtsoonian, 1971.)

(so one parameter is estimated from the data). Further evidence that this scaling procedure is meaningful are the results from matching experiments in which the subject is asked, for example, to match the brightness of a light to the loudness of a tone. This curve is again a power function with an exponent which can be predicted from the two magnitude functions on the assumption that the numbers are matched within a constant factor. Data for various modalities matched against handgrip are shown in Figure 5; the straight lines are those predicted from magnitude estimation data.
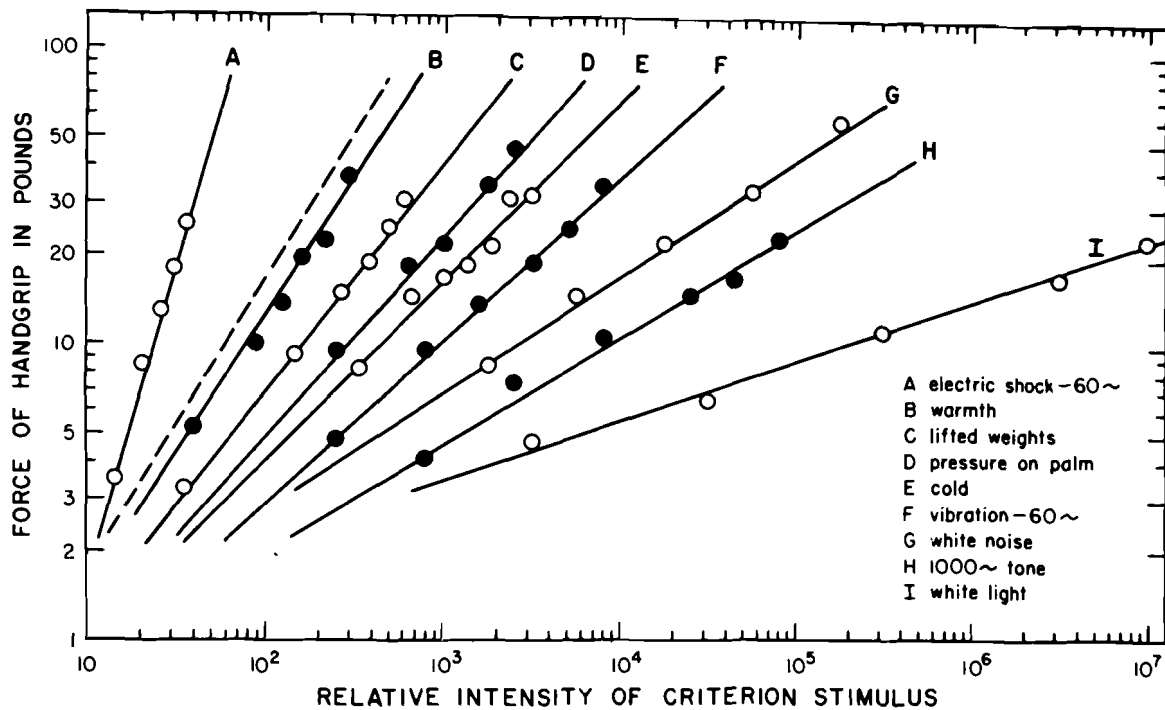

Figure 5. Cross modal matches between force of handgrip and nine other continua. (This is Fig. 1 of Stevens, 1966.)

I hope that these brief remarks are sufficient to convince you, first, that a number of regular, readily reproduced functions exist in psychophysics and, second, that a parsimonious theory to account for all of them does not immediately come to mind.

3.   Classical and Modern Psychophysical Theorizing

We may conveniently date the classical tradition of theorizing to Fechner and the modern one to World War II. The classical view holds that a relatively straightforward relation exists between the signal input and the response output, and the task is to describe it compactly. Put another way, the whole organism is seen as a sensory transducer, which one tries to describe in such a way that a small set of estimated parameters is sufficient to permit predictions in a wide variety of experiments involving the same set of signals. According to this definition, Stevens and most of those who have studied magnitude estimation are in the classical tradition, even though the method is relatively recent. For this reason, some tension has existed between this group and those in the modern tradition.

Beginning with the war period, the physical theory of signal detectability (developed in connection with electronic detection of aircraft and sonar detection of ships) was adapted and incorporated into psychophysics, with the main impact, during the 1950's and early 60's, being due to Tanner and Swets (1954) and Green and Swets (1966). It explicitly focused on the tradeoff between errors, leading to an elaborate experimental probing of these ideas.[3] The theory, even in its simplest form, gives a good account of detection and discrimination data, suggesting that subjects, rather than acting as simple transducers, are more like statisticians deciding between hypotheses on the basis of imperfect information. Considerably more than the transduction of signals is involved because response criteria are subject to experimental manipulation independent of the signal. The interpretation is that, in some manner, motivational factors entering through the instructions, presentation probabilities, and information feedback determine the values selected for the criteria. Thus, the major change from the classical view is that responses depend not only on the signals, but on the subject's goals as well. It is true that classical psychophysicists were to a degree aware of these

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motivational factors--severe reprimands by stern professors had maintained low false alarm rates among undergraduate subjects--but they did not choose to study or theorize about the error tradeoff which they were manipulating. In particular, they were highly insensitive to the fact that the slope of the ROC curve is close to infinite at a low false alarm probability (p_{01}). To this day, much work in vision continues to ignore the ROC curve.

In spite of its continued widespread use and success, the theory of signal detectability has three telling weaknesses. First, and in practice the most damning, is the grave difficulty theorists have had in generalizing it to designs involving more than two signals. The basic problem is that the simple numerical partition of the two-signal case generalizes to unspecified regions in (n-1)-dimensional space in the n-signal case. Second, at a conceptual level it is quite unclear how people are supposed to accumulate the information they are assumed to know about the distributions of likelihood ratio under the two hypotheses. Interpreted literally, a person should not be able to detect signals without extended periods of practice in a psychophysical laboratory, which is absurd. And third, the theory lacks any natural role for time in making psycholophysical judgments. The only proposal is that sampling sensory information requires a quantum of time, and repeated samples are taken until a Wald-type decision criterion is met (Audley and Pike, 1965; Laming, 1968; Stone, 1960). Although some data support this general view of a variable decision time, there is no strong empirical support for the idea of a natural quantum of time. Part of the difficulty is that some data require the quantum be no more than a millisecond or two, whereas others suggest that something of the order of 100 msec would be natural.

A fundamental feature of this modern approach is the division of the problem into two parts--the sensory transduction into a hypothetical internal representation of the signal (likelihood ratio) followed by a decision procedure which accepts this representation as one input and some of the motivational features of the design as another input which are then combined through a decision rule to select a response.

A development from another quarter--neurophysiology--meshes neatly into this point of view. In the early 1940's (Galambos and Davis, 1943) and much more intensively recently (Kiang, 1965, 1968; Rose, Brugge, Anderson, and Hind,

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1967) recordings have been made on single peripheral auditory nerve fibers of cats and monkeys under a variety of stimulating conditions. In brief, the message is that the transduction effected by the ear is wildly nonlinear and, although moderately simple in its way, there is little likelihood that we could have inferred it from overall psychophysical data--at least no one did. These physiological data suggest a somewhat different strategy for psychophysical theorizing, namely accept the neurophysiologists' description of the recording of the signal into the language of pulses in the nervous system, and then consider what the central nervous system (CNS) does functionally as a decision device operating on this information. Given the recoding, or more accurately a model of it suggested by the data, our problem is to guess at simple decision rules that are adequate to account for the range of psychophysical data. This is the strategy pursued during the past 10 years by several people; perhaps the most significant papers are Green and Luce, 1971, 1973a; Luce and Green, 1972; McGill, 1963, 1967; Siebert, 1965, 1968, 1970.

What surprises did these physiological data hold? There were several, some having to do with intensity, some with frequency, and some, not fully in, with combinations of tones. Here I shall focus only on intensity. One way of describing these data is to say that the neural pulse trains are point events from a stochastic process in time, whose parameters are controlled by the signal. Even if these parameters change instantaneously with changes in the signal, it takes time to accumulate enough pulses for the CNS to realize this. Moreover, the parameters are such that the firing rate on a single fiber remains at its (non-zero) resting level up to some intensity (this value depends on the frequency of the signal) at which point it increases by a factor of from 2 to 10 over an intensity change of two to three orders of magnitude (20-30 dB), after which it maintains a nearly constant rate. Although there do not yet exist physiological data to support it, this suggests that the full dynamic range of 12-14 orders of magnitude is shared by a number of fibers which, as a bundle, constitute a single channel.[4] We shall assume that such a channel exists functionally and that it, in effect, has a dynamic range of between two and three orders of magnitude. This postulate is distinctly hypothetical at present. Note that the transduction from intensity into neural pulses includes a simple nonlinearity, namely, that the rate is a nonlinear function of intensity (approximately, a power function, see Sections 5 and 6) and it includes a much messier one, namely, that the observable is not

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the instantaneous rate, but a discrete sequence of pulses.

In addition to the partial representation of intensity on individual nerve fibers, intensity is also represented across fibers since the total number of active fibers increases with intensity (see 4). Thus, two extreme postulates suggest themselves. One, known as a place theory, assumes that the rate information provided by individual fibers is ignored except to decide whether or not a fiber is active and it is simply the total number (or, perhaps, set) of active fibers that carries intensity information. The other extreme, sometimes known as a frequency, periodicity, or better, temporal theory, assumes that the pulse rate on single fibers carries the intensity information and the only role of the many active fibers is to build up channels to cover the full dynamic range and to increase the total sample size that can be obtained in a brief time. Obviously, mixes of these two theories are also possible.

I do not know of any decisive argument to select between them, although to my mind certain weak arguments tend to favor the temporal view. In particular, there is no very natural way in the place theory to account for the large changes in response time with intensity, whereas there is (see the next section) in the temporal one. In any event, I shall pursue the temporal theory here.

4.   Counting and Timing Models

According to the temporal view, then, whenever we ask the subject to make judgments about the intensity of signals, this is translated by the CNS into questions about estimates of rates from a number of parallel channels. So fundamentally the problem is reduced to one of estimating rates from a sample of a stochastic process. Our discussion of how this may be done follows Luce and Green (1972) and Green and Luce (1973b).

The problem faced by the CNS is analogous to that of a committee standing on the overpass of a highway with J lanes, charged with detecting, rapidly and accurately, changes in the rate of traffic flow.[5] Suppose that the rate in each lane can vary from about one per second (which would occur with traffic flowing at 60 mph and spaced at 88 ft.) to one every 15 minutes. This range of two orders of magnitude is comparable to our estimates of the range handled

by a neural channel, but the absolute rates are about 1000 times faster--from 1000 per sec to 10 per sec, or even less. How should the committee proceed?

The most obvious way is to generalize the usual method of estimating a heart rate: allocate to each person as many lanes as he can monitor and have him count the number of cars that pass in each during a fixed period of time and submit these numbers to the chairman, who then estimates the rate by calculating the average count-to-time ratio per lane. When the ratio in two successive time intervals differs enough, he concludes that there has been a change in rate. We call this a counting procedure because the random variable observed is the count obtained in a fixed period of time. The difficulty with this approach is that the problem in question is not really comparable to heart rates, which differ by at most a factor of 3, not 100. When the range is so large, no single choice of a time seems appropriate. If one chooses a short time, the estimates for slow rates degenerate; if one chooses a long time, the estimates for fast rates are overly good and terribly slow. A fixed time and a large range automatically produces no variation in response time, but a large variation in sample size and so in the quality of the estimate.

This suggests holding the sample size more nearly fixed by fixing the size of the count to be obtained from each lane and simply recording the time it takes to get that count, and this time is reported to the chairman. We call this a timing procedure because the observed random variable is the time required to achieve a fixed count. Such a rule has the clear virtue of exhibiting one qualitative feature of the data not captured by the counting rule, namely, that responses to slow rates are slow and to fast ones, fast.

Although these data clearly favor a timing procedure over a counting one, one wonders if there may not be circumstances when the counting rule is used. One argument for suspecting that this may be possible is the fact that to make a timing estimate the CNS needs, functionally, to be able to count, to time, and to divide; however, with all three of these abilities available, it should be able to institute a counting rule when that is appropriate. The only problem, it seems, is to motivate the subject sufficiently to relinquish the uniform quality of estimates for all intensities. One way to do this is to make it expensive for taking a long time in reaching a decision, which he will tend to do for weak signals (slow neural pulse rates) when using a timing rule. Of course, it is possible that the brain is so inflexibly wired that only the

timing rule is available; if not, however, penalties for slow responses should effect a switch.

The next question is whether we have any chance of observing a behavioral difference. Here, finally, we must invoke some mathematics. Consider the following experimental design. There are two tones, 0 and 1, which differ only in intensity, 1 being more intense than 0. On each trial, exactly one is presented, the schedule being random, but equally probable. A signal is continued until the subject responds by identifying which he thinks it is, after which there is feedback as to the accuracy of his response and the payoff he is to receive. The accuracy payoff o_{ij} is a sum of money for response j to presentation i; it is positive when $i = j$ and negative when $i \neq j$; varying the values o_{ij} is one way to generate an empirical ROC curve. The deadline payoff is simply a fine (with no payment for accuracy) whenever a response is slower than the deadline.

At a theoretical level, let us suppose that when a signal is presented, there are identical renewal processes on each of the J channels. By a renewal process is meant a point process in time such that the time between successive points--interarrival times (IAT)--are independently and identically distributed. The best known example is the Poisson process in which the distribution of IATs is exponential; it is the model of pure temporal uncertainty, somewhat analogous to a uniform distribution in the finite case. Obviously, the model is already highly idealized since all of the channels are assumed to be statistically identical, which is not true of the fibers. Denote by M_i and V_i the mean and variance of the distribution characterizing the renewal process for signal i, and suppose that $M_0 > M_1$ (the more intense signal has the higher rate) and that

$$V_i \quad \text{and} \quad M_i^3/V_i \quad \text{are both strictly increasing functions of } M_i \quad (1)$$

(This is obviously true in the Poisson case since $V_i = M_i^2$.) If a counting rule is used, we assume a fixed time δ (which, however, is some function of the deadline imposed and so can be manipulated experimentally) during which a count is observed. If a timing rule is used, we assume a fixed count $\mathcal{K} + 1$ per channel during which the time for $\mathcal{K}$ IATs is observed. The decision must rest either on the random variable $\mathcal{N}$, which is the total count over the J channels observed in time δ, or the random variable $\mathcal{T}$, which is the total time for $\mathcal{K}$ IATs summed over J channels. Since the mean time between pulses

is smaller for the more intense signal, it corresponds to a larger count and a smaller overall time. So plausible decision rules (which have been shown in the theory of signal detectability to fulfill various conditions of optimality) are to establish criteria, dependent upon the payoff structure, and to respond that the more intense signal was presented whenever either the count exceeds its criterion or the total time is less than its criterion, depending on which rule is in use.

Assuming that is so, let us derive the form of the ROC curve in each case. First, the counting rule. We invoke the following well-known central limit theorem (Feller, 1966, p. 359): in a renewal process with $E(IAT) = M$ and $V(IAT) = V$, the number of counts $N(\tau)$ observed in time τ is asymptotically normally distributed with mean τ/M and variance $\tau V/M^3$, i.e.,

$$\lim_{\tau \to \infty} P\left[\frac{N(\tau) - \tau/M}{(\tau V/M^3)^{1/2}} \leq z\right] = \int_{-\infty}^{z} \eta(o,1) \tag{2}$$

where

$$\eta(\mu,\sigma^2) = \frac{1}{(2\pi)^{1/2}\sigma} e^{-(x-\mu)^2/2\sigma^2}. \tag{3}$$

Assuming that $J\delta = \tau$ is sufficiently large for this approximation to be good, we see that with a criterion c, we may write

$$P_{i1} \cong \int_{-\infty}^{z_i} \eta(0,1), \tag{4}$$

where

$$z_i = \frac{c + J\delta/M_i}{(J\delta V_i/M_i^3)^{1/2}} \tag{5}$$

is the normal deviate corresponding to the probability P_{i1}. Frequently it is convenient to represent a probability in terms of its normal deviate, especially when, as in this case, two deviates are linear functions of one another since, by eliminating c, we obtain

$$z_1 = \left[\frac{V_0}{V_1}\left(\frac{M_1}{M_0}\right)^3\right]^{1/2} z_0 + (J\delta)^{1/2}\left(1 - \frac{M_1}{M_0}\right)\left(\frac{M_1}{V_1}\right)^{1/2} \tag{6}$$

as our expression for the ROC curve. By assumption 1, the slope of this curve is less than 1; in the Poisson case it is $(M_1/M_0)^{1/2}$.

For the timing model, the argument is similar, but is based on <u>the</u> central limit theorem (Feller, 1966, p. 253), which for a renewal process with $E(IAT) = M$ and $V(IAT) = V$ asserts that the total time $\mathcal{T}$ required to accumulate ℓ IATs is asymptotically normally distributed with mean ℓM and variance ℓV, i.e.,

$$\underset{\ell \to \infty}{\text{limit}} \; P\left[\frac{\mathcal{T}(\ell) - \ell M}{(\ell V)^{1/2}} \leq z\right] = \int_{-\infty}^{z} \eta(0,1). \tag{7}$$

Taking $\ell = J\mathcal{K}$, we find immediately that the ROC curve is given by

$$z_1 = \left(\frac{V_0}{V_1}\right)^{1/2} z_0 + (J\mathcal{K})^{1/2} \frac{(M_0 - M_1)}{V_1^{1/2}}, \tag{8}$$

which again is a straight line. By assumption 1, it has a slope greater than 1, equal to M_0/M_1 in the Poisson case. Thus, the slope of the ROC curve is a clear criterion as to which procedure is in use.

A second criterion can be found by looking at the response times. These times are the sum of two parts: that taken up accumulating information about the signal, called the <u>decision time</u>, and all other times, including those for computations, transmissions, muscle movements, etc., called the <u>residual time</u>. Let the mean of the residual time be denoted by $\bar{r}$; then for the counting model the mean response time for signal i and response j is

$$MRT_{ij} = \bar{r} + \delta. \tag{9}$$

The prediction is that it is independent of i and j; indeed, if the decision and residual times are independent, it is not just the mean, but the whole distribution that is predicted to be independent of i and j. In the timing model, the decision time is more complicated because it is determined by the slowest of the J channels to observe $\mathcal{K}$ IATs. Denote by $h(J,\mathcal{K},\sigma)$ the mean of this time when the renewal process has mean 1 and variance σ^2. Then, for a process with mean M_i and variance V_i,

$$MRT_{ij} = \bar{r} + h(J, \mathcal{K}, V_i^{1/2}/M_i)M_i \tag{10}$$

In the Poisson case, $V_i^{1/2}/M_i = 1$, and so for it and any other case in which this ratio is nearly constant we may eliminate h and write

$$\text{MRT}_0 = \left(\frac{M_0}{M_1}\right)\text{MRT}_1 - r\left(\frac{M_0}{M_1} - 1\right), \tag{11}$$

where we have dropped the response subscript j since, by Equation 10, it does not matter. So, as we vary the deadline, we should find a linear relation between the mean response times; moreover, the slope should be identical to that of the corresponding ROC curve.

Green and Luce (1973a) ran three observers in such an experiment using a faint 1000 Hz tone in noise for 1 and noise alone (0 intensity signal) for 0. The design was as described, with deadlines varying from 250 msec to 2000 msec. When the deadline applied to all trials, the mean response time was the same in all four cells except for the two longest deadlines, where there was some tendency for the signal trials to be slower than the noise ones; we return to this discrepancy below. The ROC data (Figure 1) were well fit by straight lines with estimated slopes of 0.92, 0.90, and 0.69, all supporting the counting model. When three other observers were run in exactly the same experiment except that the deadline applied only to signal trials, both the MRT and ROC data were well approximated by straight lines and the pairs of estimated slopes were

1.34, 1.30; 1.48, 1.47; and 1.38, 1.37.

The timing model was clearly supported.

A striking way to show up the differences between the models and between these two sets of data is as a trading relation between speed and accuracy. A very common measure of accuracy, suggested by the theory of signal detectability, is to compute the value of z_1, called d', corresponding to $z_0 = 0$. For the counting model, Equation 6 yields

$$d' = A\delta^{1/2}, \tag{12}$$

and for the timing model Equation 8 yields

$$d' = \frac{M_0}{M_1^{1/2}} A\lambda^{1/2}, \tag{13}$$

where

$$A = J^{1/2}\left(1 - \frac{M_1}{M_0}\right)\left(\frac{M_1}{V_1}\right)^{1/2}. \tag{14}$$

Eliminating δ between Equations 9 and 12 yields

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$$(d')^2 = \begin{cases} A(MRT - \bar{\tau}), & MRT \geq \bar{\tau} \\ \\ 0 & , \quad MRT < \bar{\tau} \end{cases} \qquad (15)$$

for the speed-accuracy trade in the counting model.

For the timing model, Equation 10 must be developed more fully before we can eliminate $\mathcal{K}$. If we let $\psi_{\mathcal{K}}$ denote the distribution of the $\mathcal{K}$ IATs, then by definition

$$h(J,\mathcal{K},\sigma) = J \int_0^\infty x\, \psi_{\mathcal{K}}(x) \left[\int_0^x \psi_{\mathcal{K}}(y)\, dy \right]^{J-1} dx.$$

Assuming that $\psi_{\mathcal{K}}$ is approximately normal, which for $\mathcal{K} \geq 5$ is not a bad approximation in the Poisson case, it follows readily that

$$h(J,\mathcal{K},\sigma) \cong \mathcal{K} + 1 + (\mathcal{K}+1)^{1/2} H(J),$$

where $H(J)$ is the mean of the largest of J random variables distributed $\eta(0,1)$. If we substitute this into Equation 10 and introduce the variable

$$T_i = MRT_i - \bar{\tau} - M_i - V_i^{1/2} H(J)$$

and the constant

$$B_i = V_2^{1/2} H(J)/M_i,$$

then eliminating $\mathcal{K}$ between Equation 10 and Equation 12 yields the speed-accuracy trade

$$(d')^2 = \begin{cases} \dfrac{M_0^2}{M_1 M_i} A\left\{ T_i + \dfrac{M_i B_i}{2} \left((B_i + 2) - [(B_i + 2)^2 + \dfrac{4T_i}{M_i}]^{1/2} \right) \right\}, & T_i \geq 0 \\ \\ 0 & , \quad T_i < 0. \end{cases} \qquad (16)$$

Three qualitative differences can be seen by comparing Equations 15 and 16. First, the last point for which $d' = 0$ is larger in the timing model than in the counting one by the amount $M_i + V_i^{1/2} H(J)$. Second, because the times MRT_1 and MRT_2 are different in the timing model, there are two trading relations. Third, the initial slopes of Equation 16 (timing) are greater than that of Equation 15 (counting) by factors $(M_0/M_1)^{1/2}$ and M_0/M_1. Figure 6 shows the data plotted in this way, with the data for the observers combined in the first experiment and separated for clarity in the
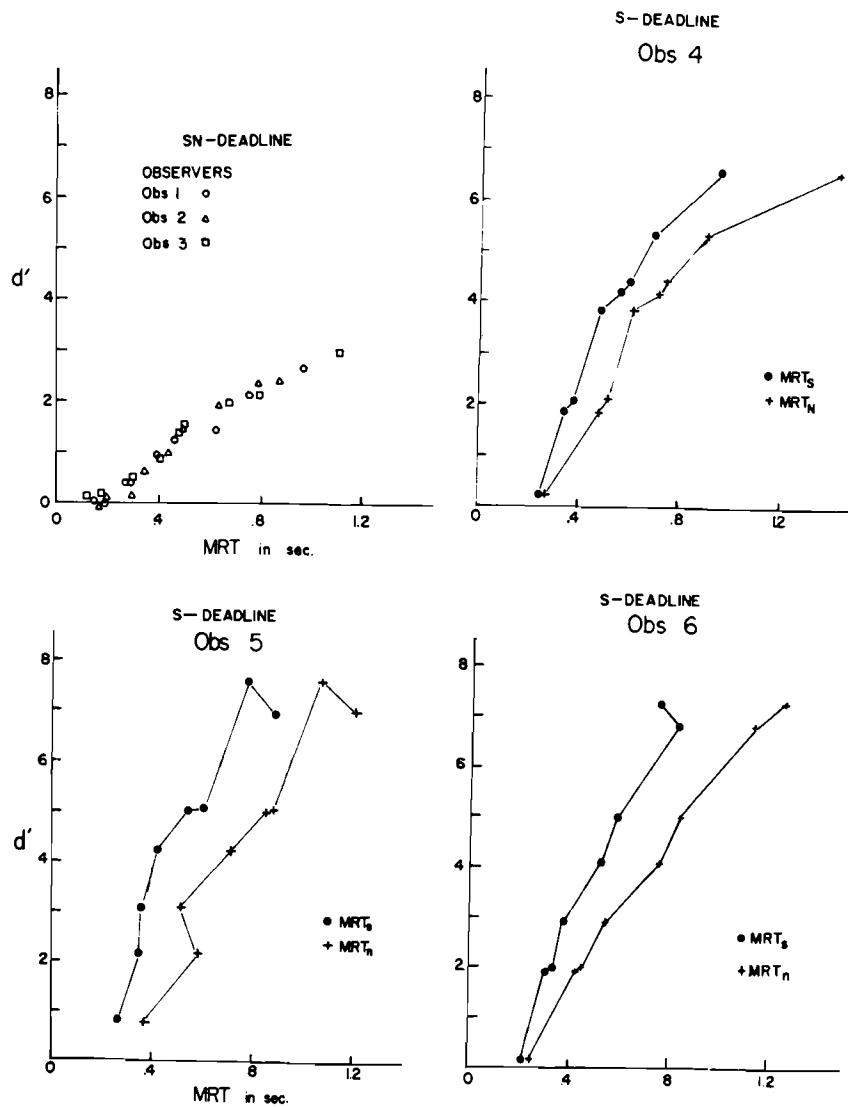
Figure 6. Plots of d' versus MRT for two experimental conditions (see text). This is Fig. 11 of Green and Luce, 1973a.)

second one. We see that the qualitative predictions are sustained. In fact, the growth of the function in the second experiment is so much more rapid than in the first that the accuracy at long deadlines is considerably greater using the timing rule than the counting one. The evidence suggests that two of the three observers realized this and switched to timing behavior for the long deadlines, thus producing the discrepancy in times mentioned earlier.

Whether or not one takes seriously the theoretical interpretation given these data, two points are worth noting. First, the theory led to running the

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experiment. Second, the data make it unambiguously clear that different con-
trols on the subject's timing of his responses make for rather different results
about the ROC curves and the speed-accuracy tradeoff. Although experimenters
have for a number of years taken care to control and report signal durations,
for their values can affect the results, the data discussed above strongly sug-
gest that such control may not be sufficient to encompass the temporal subtle-
ties involved. One must keep in mind that at least two modes of behavior are
available, and one must seek experimental designs that elicit the one desired.

5.   Absolute Identification and Magnitude Estimation

This and the next section illustrate some of the difficulties encountered
by this theory. Those described in the present section are entirely conceptual,
whereas those in the next are, at this stage, primarily technical.

Both absolute identification (AI) and magnitude estimation (ME) experi-
ments involve the presentation on each trial of just one of n signals, with
the sequence of presentations being random and equiprobable. (In much of the
ME literature, the signals are only repeated a few times and the data are aver-
aged over observers, but here we will consider only the data from individual
observers with each signal being presented repeatedly.) Assuming that the
signals vary only in one physical dimension--say intensity or frequency--the
observer may attempt to identify absolutely each presentation, usually by asso-
ciating the integers 1, 2, ..., n with the ascending sequence of signals. In
ME the observer is free to associate any positive number (usually limited to
rationals or some restricted decimal representation) to each presentation, but
subject to the request that his assignments preserve his sense of the subjec-
tive ratios among the signals.

It has been known for a long time (the earliest study was probably Pollack,
1952) that with 10 or more signals the probability of a correct identification
in the AI experiment is approximately independent of the range of the signals so
long as it exceeds some minimum value. For intensity of 1000 Hz tones, the min-
imum range is about 20 dB. Put another way, then, increasing the separation be-
tween successive signals from 2 dB to 6 dB has virtually no effect on the proba-
bility of correctly identifying them, provided there are enough signals. Of
course, when there are only two signals to be identified, such a change alters
the probability from somewhat above chance (1/2) to virtually perfect (1).

response. A prime added to any symbol simply refers to the trial preceding the one for which an unprimed symbol is used. Our response hypothesis, then, is simply

$$\frac{R}{R'} = \frac{T'}{T} \ .$$

(17)

By elementary distribution arguments, we see that

$$P(\frac{R}{R'} = x) = P(\frac{T'}{T} = x)$$

$$= \frac{1}{B(k,k')} \ \frac{x^{k'-1}(\mu'/\mu)^{k'}}{(1 + \mu'x/\mu)^{k+k'}} \ ,$$

(18)

where

$$B(k,k') = \frac{(k - 1)!(k' - 1)!}{(k + k' - 1)!} \ .$$

Equation 18 is the beta distribution of the second kind. Its mean m and variance σ^2 are readily calculated, from which

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

(19)

Observe that the right side is independent of the rate parameters μ and μ', but since $k = J\mathcal{K}$ may depend on intensity (through J) the ratio can still be a function of intensity.

In data from several observers who responded several hundred times to each of 20 signals spaced equally in dB over a 50 dB range, for any given intensity ratio (equal dB difference) there is no evidence that σ/m changes greatly as the absolute level of intensity changes. This suggests that J is nearly independent of intensity and so $k \cong k'$. For each intensity ratio we therefore average all of the data over the different levels of intensity, obtaining for one observer the plots of m and σ/m versus intensity shown in Figure 7. The first thing to note is that m grows approximately as a power function of intensity, which if the sample size is independent of intensity means that μ also grows, approximately, as a power function of intensity (see Section 6 for further evidence on this point). Second, although σ/m is probably a constant for signal ratios in excess of 20 dB, it decreases by a factor of as much as 3 for smaller ratios. Assuming $k = k'$ in each case, we get estimates of sample sizes of roughly 19 and 167 respectively. The former seems very small.
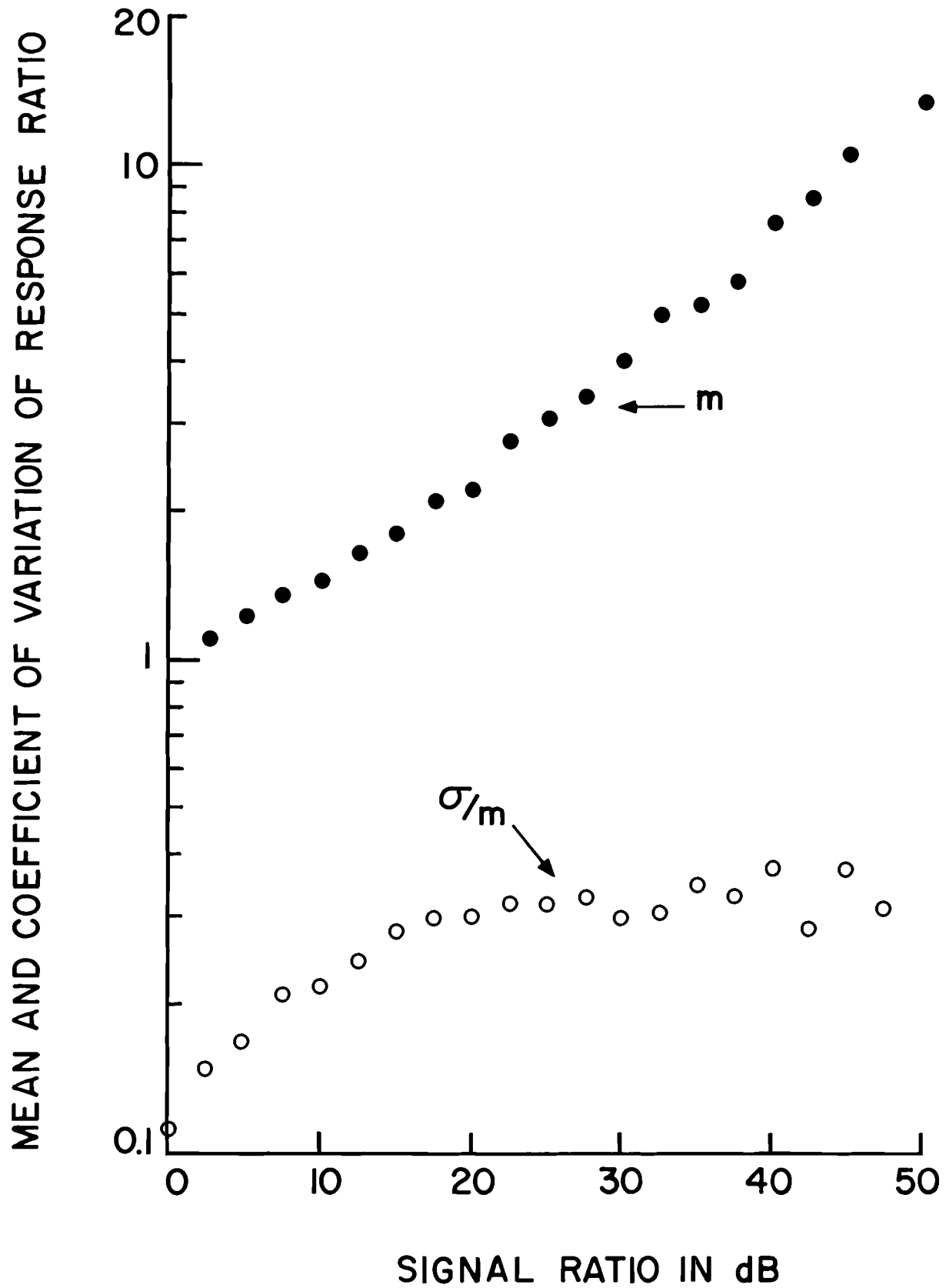
Figure 7. Mean and coefficient of variation (σ/m) of the ration of successive magnitude estimates as a function of the ration of the stimulus intensities for one observer. (Unpublished data of D. M. Green and R. D. Luce.)

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The question is whether we can make theoretical sense of these data in a way that also accounts for AI results. One idea, which although it has not yet been worked out in detail seems to have the correct qualitative features, is that the CNS is able to focus only on a limited range of intensities at any one time. An extreme version of this model supposes that it can collect a large sample, say 167, only for rates falling in a range corresponding to about 20 dB, and for rates outside that range, only a much smaller sample, say 19, is possible. This makes sense if we postulate that each nerve fiber has only dynamic range of about 20 dB, as seems to be the case, and that as intensity is changed some fibers are saturated while others are brought into play. The assumption would then be that the CNS can monitor fully, with its largest sample size, only those fibers corresponding to one 20 dB range, and that activity outside that range is monitored only with much smaller samples. This assumption makes the 20 dB limit in the physiological data account for the 20 dB edges in both the AI and ME experiments. Furthermore, it suggests experiments to test itself. If we can manipulate the range that the CNS is monitoring, then we should get predictable phenomena. For example, by introducing sequential dependencies in the signal presentation schedule, we can make the probability that two successive signals are within 20 dB of each other as low or high as we choose, which should affect the tendency of the subject to monitor near or far from the value of the previous signal. If we then compare the behavior to the exceptional signals--the near ones when near ones are improbable and the far ones when far ones are improbable--with the behavior to the common ones, we should find the former much more variable than the latter.

6.   Response Time to the Onset of a Signal

Our last application illustrates the fact that problems that are simple to formulate in the theory do not necessarily lead to solved mathematical questions.

Consider an experiment in which a signal comes on at a random time after a warning signal, and the observer is to respond to it as rapidly as possible, without, however, making too many anticipatory responses. Formally, the data from each trial consist of a pair of random variables S and R, where S

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is the time at which the signal (say, a change in intensity) comes on and $\underset{\sim}{R}$ is the time at which the subject responds. Denote by

$$f(x,t) = P(\underset{\sim}{R} = t \mid \underset{\sim}{S} = x) \qquad (20)$$

the conditional probability density that the response time is t when the signal onset time is x, and by

$$g(x) = P(\underset{\sim}{S} = x) \qquad (21)$$

the density of the signal onset times, which is under experimental control.

In terms of the model, we suppose a Poisson process with parameter ν until $\underset{\sim}{S}$ and one with parameter μ ($> \nu$) after $\underset{\sim}{S}$. Some sort of decision rule will be applied to this stochastic process, leading to a decision at time $\underset{\sim}{D}$ ($< \underset{\sim}{R}$) to intiate the response. Whatever that rule may be, let

$$\ell(x,y) = P(\underset{\sim}{D} = y \mid \underset{\sim}{S} = x) \qquad (22)$$

denote the conditional probability density that the decision time is y when the signal onset time is x. We refer to the time $\underset{\sim}{R} - \underset{\sim}{D}$ as the _residual_ time--it consists of all the times consumed by the nervous system aside from those taken up in arriving at a decision. We make the following assumptions about the residual time:

     (i) $\underset{\sim}{R} - \underset{\sim}{D}$ and $\underset{\sim}{D}$ are independent random variables.

     (ii) $\underset{\sim}{R} - \underset{\sim}{D}$ and $\underset{\sim}{S}$ are independent random variables.

     (iii) $\underset{\sim}{R} - \underset{\sim}{D}$ is a bounded random variable.

Empirically, there is some doubt whether (i) and (ii) are strictly correct. For example, the readiness to respond may be affected by the overall delay, and so by the value of $\underset{\sim}{S}$. The evidence in favor of (iii) is the boundedness of response times to intense signals. The bound appears to be of the order of 300 msec. By (ii) one can reasonably postulate a density for $\underset{\sim}{R} - \underset{\sim}{D}$, call it r, and by (i) we see that

$$f(x,t) = \int_0^t \ell(x,y)r(t - y)\,dy. \qquad (23)$$

It is convenient to divide the observable response time density into two parts corresponding to anticipatory responses and those that appear to be in response to the signal, specifically

$$f_R(t) = \frac{\int_t^\infty g(x)f(x,t)\,dx}{\int_t^\infty g(x)\,dx} \tag{24}$$

and

$$f_{R-S}(t) = \frac{\int_0^t g(x)f(x,t)\,dx}{\int_0^t g(x)\,dx} . \tag{25}$$

In the experimental analysis and data given below, the onset density was exponential,

$$g(x) = \lambda e^{-\lambda x}$$

The reason for this choice is that it makes ineffective any possible strategic considerations in responding based on how long the subject has waited.

To the best of my knowledge, the following basic question has not been answered: given a payoff function $P(\underset{\sim}{S},\underset{\sim}{R})$ (where, presumably, $P(\underset{\sim}{S},\underset{\sim}{R}) < 0$ for $\underset{\sim}{R} < \underset{\sim}{S}$), what is an optimal decision rule to detect a simple increase (or decrease) in the parameter of a Poisson process? The answer to this might provide some suggestions about the sorts of rules employed by the CNS.

A far simpler question, although not without difficulties, is to postulate the simplest rule one can think of, derive properties of ℓ, f_R, and f_{R-S}, and compare the latter two with data. The simplest rule we have thought of (see Luce and Green, 1972), which is both the most responsive and most variable way to detect a change, is to select a criterion β and compare each IAT with it, initiating a response the first time after the warning signal that IAT $< \beta$. The value selected for β will depend, of course, on the magnitude of the change to be detected and the payoffs. Assuming this rule and denoting by ℓ_0 the density ℓ conditional on a pulse at time 0, elementary probability considerations lead to the following pair of integral-difference equations for ℓ and ℓ_0:

$$\ell(x,t) = \begin{cases} \displaystyle\int_0^t \nu e^{-\nu y}\ell_o(x-y,\, t-y)\, dy, & t < x \\[2ex] \displaystyle\int_0^x \nu e^{-\nu y}\ell_o(x-y,\, t-y)\, dy + e^{-\nu x}\int_x^t \mu e^{-\mu(y-x)}\ell_o(0,\, t-y)\, dy, & t \geqq x \end{cases}$$

$$\ell_o(x,t) = \begin{cases} \nu e^{-\nu t}, & t < x \\[1ex] e^{-\nu x}\mu e^{-\mu(t-x)}, & x \leqq t \leqq \beta \quad\bigg\} \; x < \beta \\[1ex] e^{-\nu x}\, e^{-\mu(\beta-x)}\ell(0,\, t-\beta), & x < \beta \leqq t \\[1ex] \nu e^{-\nu t}, & t \leqq \beta \quad\bigg\} \\[1ex] e^{-\nu\beta}\ell(x-\beta,\, t-\beta), & \beta < t \quad\bigg\} \; x \geqq \beta \end{cases} \tag{26}$$

The technical problem is to solve these equations. Although this has not been done fully, enough is known to suggest that the model is not wholly absurd. In particular, for sufficiently large t, the boundedness of the residual times plus the fact that a slow exponential decay dominates the solution to Equation 26 permits one to show that

$$f_R(t) \cong A e^{-(\lambda+\nu')t}, \tag{27}$$

where

$$\nu' = \nu(1 - e^{-(\nu-\nu')\beta}), \tag{28}$$

and

$$f_{R-S}(t) \cong B e^{-\mu' t}, \tag{29}$$

where

$$\mu' = \frac{\mu^2\beta}{1 + \mu\beta} \tag{30}$$

and A and B are some functions of the parameters. Of course, the tails of the distributions constitute only a fraction of the data and we would really like to know the form of the entire solution, but at present we have no option but to waste much of the data.

A first test of the model is to see whether the tails of the distributions are approximately exponential. A sample of data is shown in Figure 8,
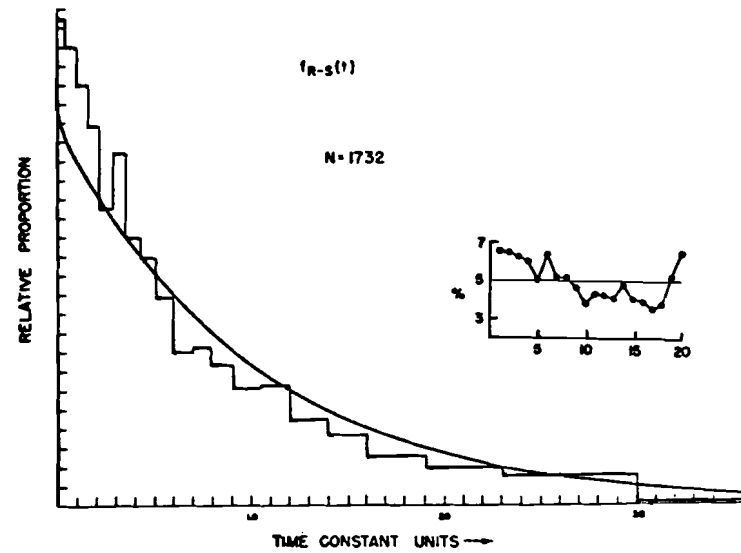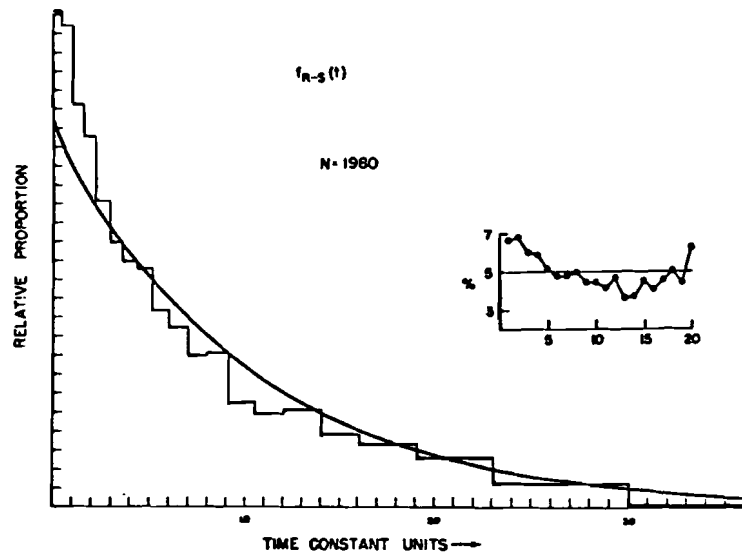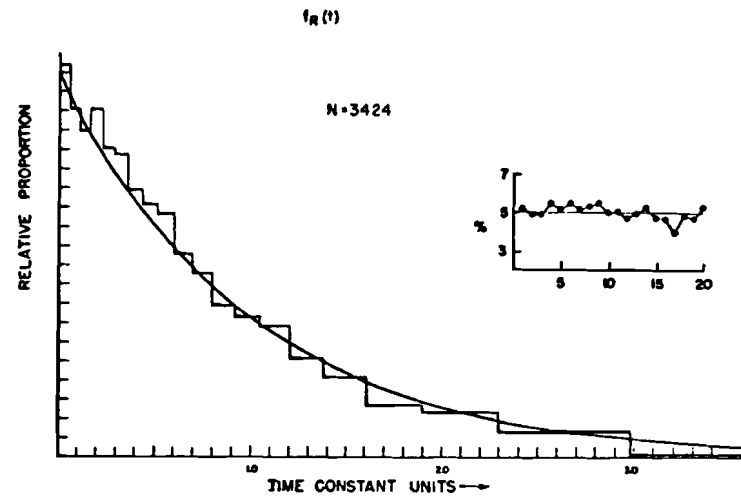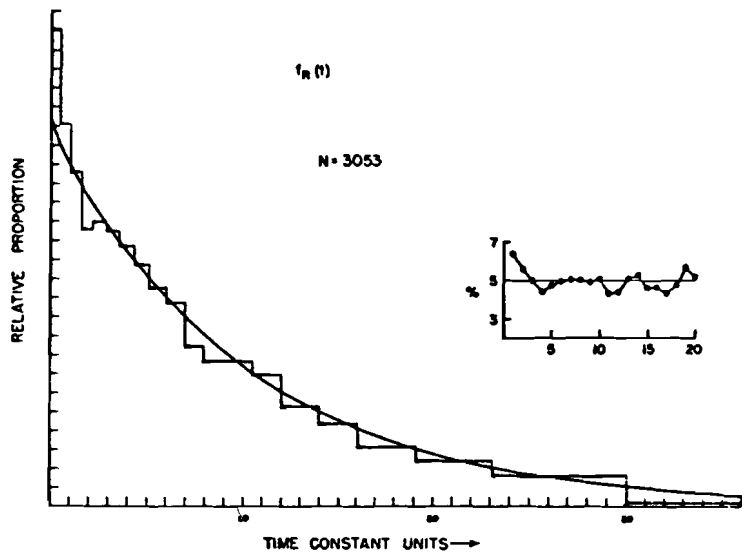
Figure 8. Tails (t ≧ 0.50 sec) of the false alarm and reaction time distributions for one observer at one intensity level. The smooth curves are the best fitting exponentials, and the inserts present the data when grouped into 20 equiprobably intervals according to those distributions. (This is part of Fig. 6 of Luce and Green, 1970.)

for times greater than 1/2 sec, and the approximation is not bad. Using

Equations 27-30 to estimate ν' and μ' for different intensities yields

Figure 9. Observe that the growth of μ'/ν' is approximately a power function

OBSERVER 1 = □
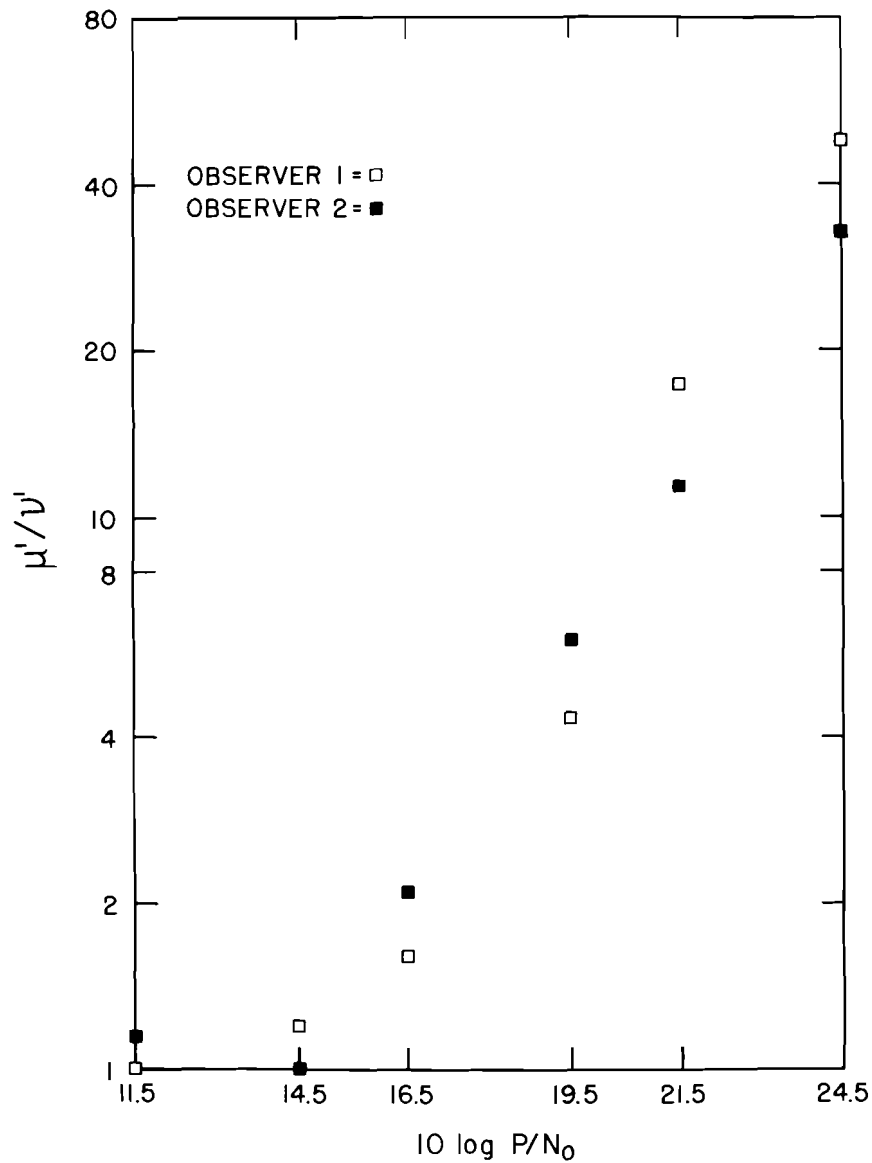OBSERVER 2 = ■

μ'/ν'

10 log P/N_0

Figure 9. Ratio of Poisson parameters estimated from the tails of
the reaction time distributions from two observers as a function
of signal-to-noise ratio in dB. (This is Fig. 9 of Luce and Green,
1970.)

of intensity, which agrees with the conclusion from ME data (Section 5). However, there is a considerable discrepancy in the estimates of the exponents: something of the order of 0.30 ± 0.15 from the ME data for a variety of subjects and about 1.5 from these reaction time data.

As Luce and Green (1972) pointed out, one source for the discrepancy is that the reaction time analysis is for only a single channel, and the following model suggests that this may be the only source. Suppose that the single channel analysis applies independently to each of J parallel channels, each with a criterion δ, and that a decision does not initiate a response but rather causes another channel to fire. This common channel, which receives inputs from each of the J channels, applies the same decision rule, but with criterion β, and it initiates a response whenever two of the J channels fire sufficiently closely.

If the rate of the underlying process is μ, then the mean IAT of the decision process is

$$\frac{1}{\mu(1 - e^{-\mu\delta})} , \tag{31}$$

as can be shown by deducing the Laplace transform of $\ell_o(0,t)$ from Equation 26, and then calculating the mean in the usual way. By a well-known theorem (Cox, 1962, p. 77-79) the superposition of J independent, identical renewal processes approaches a Poisson process as $J \to \infty$; moreover, its rate is given by

$$\mu^* \cong J\mu(1 - e^{-\mu\delta}) . \tag{32}$$

If we assume that both δ and β are sufficiently small, so that we may use linear approximations to the exponentials in Equations 28 and 32 and drop the term $\mu\beta$ in Equation 30, we have

$$\frac{\mu'}{\nu'} \cong \frac{(\mu^*)^2\beta}{1+\mu^*\beta} \cdot \frac{1 + \nu^*\beta}{(\nu^*)^2\beta} \cong \left(\frac{\mu^*}{\nu^*}\right)^2 \cong \left(\frac{\mu}{\nu}\right)^4 . \tag{33}$$

This neatly accounts for the factor of 4 discrepancy between the ME and reaction time estimates.

7. Concluding Remarks

I hope that the following points have become clear as a result of my illustrative models.

1. Psychology has empirical problems of some complexity which can be significantly illuminated by using probability models accessible to under-graduate students.

2. As in physics, radical oversimplifications (identical channels, the simplest of decision rules, Poisson processes) of the micro-structure (neural pulse trains) can, if handled with care, provide adequate qualitative and even quantitative models of the macro-structure (psychophysics).

3. Courses on stochastic processes for social science majors probably should include some material on continuous-time stochastic processes, especially Poisson processes and perhaps more general renewal ones. Psychologists are generally less familiar with such processes than with discrete time ones (especially Markov chains), and as a result they have developed little theory for situations in which responses can occur at any time (e.g., Skinnerian operant conditioning experiments) and they tend to employ experimental designs with a trial structure, which may very well seriously distort an organism's performance from what it would be under more natural conditions. Moreover, as I have tried to demonstrate, continuous time processes are probably satisfactory models for some neural activity and certainly can serve as idealized underpinnings for psychological theories.

Footnotes

Note: Figures 2, 3, and 4, copyrighted by the American Psychological Association, reprinted by permission.

1. This work has been supported in part by a grant from the National Science Foundation to the University of California, Irvine.

2. Not everyone regards Fechner's influence as salutary. The widely known American philosopher and psychologist William James (1890, p. 533,34 commented as follows:

"In 1860, Professor G. T. Fechner of Leipzig, a man of great learning and subtlety of mind, published two volumes entitled

'Psychophysik,' devoted to establishing and explaining a
law called by him the psychophysic law, which he considered
to express the deepest and most elementary relation between
the mental and the physical worlds. It is a formula for thee^e e e
connection between the amount of our sensations and the amount
of their outward causes. Its simplest expression is, that
when we pass from one sensation to a stronger one of the same
kind, the sensations increase proportionally to the logarithms
of their exciting causes. Fechner's book was the starting
point of a new department of literature, which it would be
perhaps impossible to match for the qualities of thoroughness
and subtlety, but of which, in the humble opinion of the present
writer, the proper psychological outcome is just <u>nothing</u>."

This not withstanding, as late as 1961 Stevens titled a paper
"To honor Fechner and repeal his law."

3.   The earlier work of Thurstone (1927) has some of the same formal
characteristics as the theory of signal detectability, but he and his
followers failed to emphasize the experimental manipulation of errors
and so did not have the empirical impact they might otherwise have had.

4.   How many fibers go to make up a channel, and how many channels are there?
No one really knows, but some bounds can be set. If each fiber covers
something just under two orders of magnitude, then anything less than 10
fibers per channel would be inadequate. Since there are estimated on
anatomical grounds to be something of the order of 30,000 peripheral
auditory fibers, the maximum number of channels is 3,000. If, as one
would guess, there is rather more overlap of fibers making up one
channel, something considerably less than 3,000 channels is to be
expected. Later, in Section 5, we will cite some psychological data
which under one interpretation suggest there are about 170 channels.
It is very doubtful that this means that there are something over 100
fibers per channel, but rather that the channels are frequency specific
as well. The physiological data make clear that individual fibers are
sensitive to a band of frequencies, and psychophysical data have long
been interpreted as supporting the notion of "critical" frequency bands.
Were we to assume 15 frequency bands and 12 intensity bands, then 170
fibers in each frequency-intensity band would account for the total
number.

5. This assumes that the number of channels is independent of frequency.
    Of course, as should be clear from footnote 4 and is certainly clear
    from the physiological data, the number of active fibers increases
    with intensity, but that does not imply one way or the other anything
    about the number of channels. Certainly, it would be simpler in the
    theory for J to be a constant, and as we will argue in Section 5,
    some psychophysical data suggest it may be.

6. The terms absolute judgment and category experiments are also commonly
    used.

7. Note that because the 20 signals were presented with equal probability,
    the sample size decreases with increasing signal ratio: from 366 at
    2.5 dB, through 195 at 20 dB, down to 25 at 47.5 dB. This accounts for
    the raggedness of the right end of the σ/m plot.

Bibliography

Audley, R. J. and Pike, A. R. Some alternative stochastic models of choice,
British Journal of Mathematical and Statistical Psychology, 1965, 18, 207-255.

Chocholle, R. Variations des temps de réaction auditifs en fonction de
l'intensité à diverses fréquences, L'Année Psychologie, 1940, 41, 5-124.

Cox, D. R. Renewal theory. London: Methuen, 1962 (paperback, 1967).

Durlach, N. I. and Braida, L. D. Intensity perception. I. Preliminary theory
of intensity resolution, Journal of the Acoustical Society of America, 1969,
46, 372-383.

Fechner, G. T. Elemente der Psychophysik. Leipzig: Breitkipf und Hartel,
1860. English translation of Vol. 1 by Adler, H. E. (Howes, D. H. and Boring,
E. G., Eds.). New York: Holt, Rinehart and Winston, 1966.

Feller, W. An introduction to probability theory and its application, II.
New York: Wiley, 1966.

Galambos, R. and Davis, H. The response of single auditory-nerve fibers to
acoustic stimulation, Journal of Neurophysiology, 1943, 6, 39-57.

Green, D. M. and Luce, R. D. Detection of auditory signals presented at random times: III, <u>Perception and Psychophysics</u>, 1971, <u>9</u>, 257-268.

Green, D. M. and Luce, R. D. Counting and timing mechanisms in auditory discrimination and reaction time. In D. H. Krantz, R. C. Atkinson, R. D. Luce, and P. Suppes (eds.) <u>Contemporary developments in mathematical psychology</u>, Vol. II. San Francisco: Freeman, 1973, in press. (b)

Green, D. M. and Swets, J. A. <u>Signal detection theory and psychophysics</u>. New York: J. Wiley and Sons, 1966.

Helmholtz, H. von. <u>Die Lehre von den Tonempfindungen</u>. (1st ed.) Brauschweig: Viewag, 1863. Translated into English by A. J. Ellis, 1885, and reprinted as <u>On the sensations of tone</u>. New York: Dover, 1954.

Kiang, N. Y-S. <u>Discharge patterns of single fibers in the cat's auditory nerve</u>. Cambridge, Massachusetts: MIT Press, 1965.

Kiang, N. Y-S. A survey of recent developments in the study of auditory physiology, <u>Annals of Otology, Rhinology and Laryngology</u>, 1968, <u>77</u>, 656-676.

Laming, D. R. J. <u>Information theory of choice-reaction times</u>. New York: Academic Press, 1968.

Luce, R. D. and Green, D. M. Detection of auditory signals presented at random times, II, <u>Perception and Psychophysics</u>, 1970, <u>7</u>, 14.

Luce, R. D. and Green, D. M. A neural timing theory for response times and the psychophysics of intensity, <u>Psychological Review</u>, 1972, <u>79</u>, 14-57.

Luce, R. D. and Green, D. M. Detection, discrimination, and recognition. In E. C. Carterette and M. P. Friedman (eds.) <u>Handbook of perception</u>, Vol. II, New York: Academic Press, 1973, in press.

McGill, W. J. Stochastic latency mechanisms. In R. D. Luce, R. R. Bush, and E. Galanter (eds.) <u>Handbook of mathematical psychology</u>, Vol. I, New York: Wiley, 1963. Pp. 309-360.

McGill, W. J. Neural counting mechanisms and energy detection in audition, <u>Journal of Mathematical Psychology</u>, 1967, <u>4</u>, 351-376.

Pollack, I. Information of elementary auditory displays, <u>Journal of the Acoustical Society of America</u>, 1952, <u>24</u>, 745-750.

Rose, J. E., Brugge, J. F., Anderson, D. J., and Hind, J. E. Phase-locked response to low-frequency tones in single auditory nerve fibers of the squirrel monkey, <u>Journal of Neurophysiology</u>, 1967, <u>30</u>, 769-793.

Siebert, W. M. Some implications of the stochastic behavior of primary auditory neurons, <u>Kybernetic</u>, 1965, <u>2</u>, 206-215.

Siebert, W. M. Stimulus transformations in the peripheral auditory system. In P. A. Kolers and M. Eden (eds.) <u>Recognizing patterns</u>. Cambridge, Massachusetts: MIT Press, 1968. Pp. 104-133.

Siebert, W. M. Frequency discrimination in auditory systems: Place or periodicity mechanisms? Proceedings of the IEEE, 1970, 58, 723-730.

Stevens, S. S. On the psychophysical law, Psychological Review, 1957, 64, 153-181.

Stevens, S. S. To honor Fechner and repeal his law, Science, 1961, 133, 80-86.

Stevens, S. S. Matching functions between loudness and ten other continua, Perception and Psychophysics, 1966, 1, 5-8. (a)

Stevens, S. S. Issues in psychophysical measurement, Psychological Review, 1971, 78, 426-450.

Stone, M. Models for choice-reaction time, Psychometrika, 1960, 25, 251-260.

Tanner, W. P., Jr. and Swets, J. A. A decision-making theory of visual detection, Psychological Review, 1954, 61, 401-409.

Teghtsoonian, R. On the exponents in Stevens' law and the constant in Ekman's law, Psychological Review, 1971, 78, 71-80.

Thurstone, L. L. A law of comparative judgment, Psychological Review, 34, 273-286.

Ward, L. M. Category judgments of loudness in the absence of an experimental-induced identification function: sequential effects and power functions fit, Journal of Experimental Psychology, 1972, 94, 179-187.

Ward, L. M. Repeated magnitude estimations with a variable standard: sequential effects and other properties, Perception and Psychophysics, 1973, 13, 193-200.

Ward, L. M. and Lockhead, G. R. Sequential effects and memory in category judgments, Journal of Experimental Psychology, 1970, 84, 27-34.