In these models the notions of space-patterning, and of integration within space-patterning, from all that I understand of these attempts, are in good agreement with the general thinking of neurophysiologists.

I would end this summary here, although much more could have been said in general as well as on each particular point. I only hope that I did not introduce too many distortions into my statements on the different contributions as I have recalled them.

CHAIRMAN: Thank you, Dr. Fessard; the audience has answered your question! Although Dr. Fessard is primarily a neurophysiologist he has always been in close contact with students of behavior and started his own career as an engineer; so he also is plunk in the centre of the triangle.

Dr. Duncan Luce, although identified officially as Professor of Psychology at the University of Pennsylvania, is similarly a multi-threat man, his training being primarily in mathematics and related matters. Dr. Luce . . .

R. DUNCAN LUCE

Introduction

Thank you Dr. Gerard. Ladies and Gentlemen.

One surprise that this Symposium held for me was in the discipline of "casual mathematics"; I have been belatedly introduced to the marvelous plasticity of the triangle. And since that plastic triangle has been one of our themes, I would be remiss not to add still another touch of flexibility. Recall that the basic triangle places behavior at the top corner—just how that was decided I do not know—and neurophysiology and theory at the two bottom ones. This, I must insist, is a little too simple: the theory "corner" is, itself, a triangle. To be sure, the other two corners are no doubt structured —as triangles, perhaps?—but let me attend only to the three facets of theory that we have seen. First, there are the models, often analogue machines actually realized in the metal, that attempt to reproduce some of the known neurophysiological facts and phenomena. Second are those, mostly formulated as programs for digital computers, that exhibit some, loosely coupled, relations to either gross behavior, neural behavior, or both. These programs do not purport to model anything specific about organisms as such, but rather they are designed to carry out certain gross functions similar to those exhibited by some organisms, e.g., the ability to recognize patterns, to play chess, etc. Third, and finally, there are theories, cast primarily in mathematical terms, that explicitly aim to summarize in compact form some of our knowledge about gross behavior; Dr. Estes has given us a good example.

In my attempt to summarize some aspects of this Symposium, I shall not undertake to deal with the neurophysiological edges of this compound triangle. I am entirely too ignorant of these matters to devise even a moderately coherent summary, let alone a balanced one. Rather, I shall confine my remarks to the edge that connects behavior to models of behavior. I shall try to classify sensibly the several behavioral studies that have been described, to remind you of some types of behavioral results that have been neglected, and to indicate features of these studies that strike me as of possible importance to neurophysiologists. It should not be forgotten that all of this work stems from experimental psychology, and so we have omitted a considerable body of possibly relevant results and observations from the clinical and social areas.

This behavioral emphasis—both by me and by Dr. Gerard in his organization of the Symposium—presupposes that the research of psychologists is or should be of concern to neurophysiologists. Why? The only reason I can see is that knowledge of behavioral data imposes some, quite likely weak, constraints upon the neurophysiological theorist.

I do not intend by this to contradict the much repeated observation that, because of the vast complexity of the nervous system, neither gross behavior, on the one hand, nor neural behavior and organization, on the other hand, can, in practice, be deduced from a knowledge of the other. This symmetric pair of propositions is undoubtedly correct, but neither is inconsistent with the belief that models or data at one level place some limitation upon the acceptability of models at the other level. That at least must be our faith, else why should we participate in a Symposium such as this?

Complex Perceptual Phenomena

The behavioral studies that have been described here can be conveniently grouped into three broad classes. The first may be dubbed *complex perceptual phenomena*. Perhaps the best, and surely the most extreme, example of what I mean are the vivid illusions demonstrated by Dr. MacKay; these are of the same ilk as the illusions first brought into prominence by the gestalt psychologists. Although no other studies were quite as dramatic as MacKay's slides and movie, I am nonetheless inclined to say that some of them also involve phenomena that are complex, at least at the stimulus and peripheral neural levels, and perceptual at the behavioral level. For example, Dr. Fitts cited work in which subjects live for a considerable time in a prism-distorted environment, and the experimenter is interested in how well they manage to adapt to their distorted perceptions. Dr. Broadbent described some of his research in which different inputs are applied to the two ears; how much difficulty a subject has in processing and recalling the information appears to depend upon a number of factors, including the instructions used. Even Dr. Schouten's rigorous work on pitch perception involves what I would view as a fairly complicated set of perceptual phenomena.

On the one hand, these studies, especially the most complex ones such as MacKay's, seem to evoke compelling introspective effects about which there is considerable inter-subject agreement. On the other hand, the more complex and persuasive these effects, the less certain we seem to be about what to do with them in the laboratory. In many cases we know little more than that a phenomenon exists. I rather doubt that neurophysiologists will find it useful for some time to come to accept, as a measure of the adequacy of their theories, the challenge of explaining complex perceptual phenomena. Ultimately, of course, any good behavioral theory and, probably later, any good neurophysiological theory will have to account for these effects, but it seems unwise to hold any serious hope of devising adequate explanations now or in the near future.

Transient Behavior

Let me turn, then, to a second major category of behavioral studies, namely, *transient behavior*. Most importantly, this class includes learning, but it may also include other transients of behavior. The only learning studies described during the Symposium, those reported by Drs. Estes and Konorski, are special in at least one important respect: they involve repeated trials. In such designs, physical time is subdivided into discrete units during each of which the subject is confronted with what, from the experimenter's point of view, is a well-specified set of alternative responses. This makes it easy to record the responses, which are then studied as a function of trials, of different schedules and amounts of reward and punishment, etc. In practice, the observed responses are usually averaged in some fashion and one examines overall properties—statistics—of the process. In the simplest case, one may look to see how the average total number of errors varies as a function of different experimental treatments. Any adequate theory of behavior is expected to account, to within statistical errors of observation, for any statistic one chooses to calculate, including complicated sequential ones.

Several features of these studies deserve comment. First, the experiments with a trial structure, and others as well, exhibit a curious lack of reality which, although familiar, is really a bit worrisome. Life is not usually divided into trials, at least not in this sense. Why then do we so often cast our experiments into a trial mold? One reason —ease of data recording—was given earlier; another is that we do not know very well how to construct theories for processes in which discrete responses can occur at the discretion of the subject. Most of our mathematical and, in practice, verbal theories apply only to repeated trials, in spite of the fact that in their natural environments organisms rarely confront such clearly defined repetitions of the same or similar situations. One reason that we find it difficult to formulate adequate models for discrete events in continuous time is, I believe, our inability to characterize the processes whereby an organism creates sets of alternatives for himself; that is, we do not understand the mechanisms whereby every now and then it comes to view the world as partitioned into choices. Because we do not understand this phenomenon—our concepts are so feeble that it is hardly permissible to mention the problem in public—we substitute experimenter imposed choices at regular time intervals. We do this for our convenience, not because it necessarily abstracts an important feature of the subject's environment. We do it with the uneasy faith that he will see a choice as existing when we say that it does and that his definition of the alternatives will prove to be the same as ours.

There are exceptions to the trial paradigm; perhaps the best known is the operant conditioning work associated with the name of B. F. Skinner (see Ferster and Skinner, 1957; and Skinner, 1938). In much of that work trials are not defined, the subject is free to respond at any time, and his response rate is the basic behavioral measure. He is rewarded according to some temporal pattern, measured either in physical time or in terms of sequences of responses. For example, in one type of schedule a response is rewarded if and only if a fixed period of time has elapsed since the last response; in another type, every *n*th response is rewarded. In at least one respect, operant conditioning data should be more congenial to neurophysiologists than those obtained with a trial structure: rates of bar pressing are not formally much different from rates of neuron firing. Unfortunately, we have not heard much about such work here, one reason being, I suspect, that the psychological theoretician has had little to say about these data for, as I pointed out, we do not have adequate theories for experiments without trials.

A second feature of many learning studies is the fact that stimuli, in the sense of simple response-evokers, are not a primary factor. It is true that discriminative stimuli are sometimes used, as for example in Dr. Konorski's work, but in many learning experiments the stimulus situation is the same from trial to trial. What is believed to be mainly controlling the behavior, and certainly what is studied, is the pattern or schedule of outcomes. This Symposium has devoted little of its attention to outcomes and how they affect information processing in the nervous system. So far as I can see, neurophysiologists attend more closely to psychological results describing the dependence of responses upon sensory stimuli and less to the equally powerful, if temporally more remote, effects of outcomes on later responses. One senses a wistful desire for simple, reflexive organisms; there is little doubt that ordinary laboratory mammals do not qualify. I shall return to this point later in a slightly different context.

A third feature that bears comment is that in those learning studies with a trial structure, we usually report only average data for groups of subjects. One rarely sees the data for individuals. One reason—perhaps the most important—is that the process of learning is viewed as probabilistic as well as transient, and it is not easy to estimate response probabilities for single subjects. The estimate of a probability based upon an individual's responses over a block of 50 or so trials has little meaning unless the probability is constant, or nearly so, throughout the block, in which case no visible learning is occurring. That being so, there is hardly any alternative to performing some sort of averaging over groups of subjects. This, however, is a risky business unless we have some sort of independent evidence to tell us when two subjects have approximately the same response probabilities, and I do not know of any such criteria except in uninteresting cases.

It seems foolhardy to attempt to explain such average behavior in neurophysiological terms, because it is likely that the average subject does not correspond at all well to any of the individuals composing the average. If a process is highly non-linear, and most models of the learning process suggest that it is, and if different subjects have appreciably different parameters, then the average behavior will in general have a totally different mathematical structure from that of any of the individuals, even when they all satisfy the same mathematical equations.

Asymptotic Behavior

My third general category of psychological studies is, or at least I think that it should be, of greatest interest to neurophysiologists, and so I shall dwell upon it somewhat more. It consists of the studies of static or, more precisely, *asymptotic behavior*—the experiments in which we attempt to get the subject beyond the transient learning phase and only record data when his behavior has settled down and is stable. Most of these studies use a trial design, and so everything said above about trials in learning experiments applies here without change.

If we are really convinced that we are observing asymptotic behavior—unfortunately, it is never easy to be sure—then it is no longer necessary to deal with group averages because many averages calculated over an individual subject's sequence of responses are meaningful at asymptote. Nevertheless, in much of the existing literature only group averages are reported, but my point is that this is not necessary and it is increasingly more common to find data for individual subjects. There are difficulties: journal editors do not take kindly to vast numbers of graphs, but with a little ingenuity one can often be quite efficient in presenting a lot of data in a little space. The data from individual subjects seem to support the earlier suggestion that differences in numerical parameters exist even when a single mathematical theory apparently describes all subjects.

Oddly enough, even when data for individuals are reported they are often average data. I do not refer to the averaging used to estimate probabilities—this we cannot avoid as long as we postulate probabilistic processes—but rather to further, unnecessary collapsing of the data. This is very common in the studies influenced by information theory (for surveys see Attneave, 1959; and Luce, 1960a), where the data reported are, for example, the information measure (sometimes called the uncertainty or entropy) of the subject's distribution of response probabilities. The information measure of a distribution is an average—a peculiar one, to be sure, but nonetheless an average. This, or any other average over response classes, may be a convenient summary of the data, but unless one knows a great deal about the process underlying the response behavior, it is likely that any averaging over responses conceals a good deal of information (in the informal sense) about what is going on. To my mind, we have been somewhat misguided during the past 10 or 15 years into too much averaging of our data.

Consider, for example, the approximately linear relation that has been found between the mean response latency in a choice situation and Shannon's information measure of the distribution of stimulus presentations or, perhaps better, of the distribution of response probabilities. This result is probably true as far as it goes—quite a few studies support it. But when one looks at it carefully, the relation does not seem to be exactly linear and, what is worse, the relation between mean latency for a particular response and its probability of occurring (or that of the presentation probability of the stimulus for which it is the correct response) is apparently not the logarithmic relation that one might have expected. If it were, then the average result would be easy to explain, but it does not seem to be. Thus, except in a crude sense, we still do not really know what relation exists between latency and either response or stimulus probabilities, and I cannot believe that it will be discovered as long as distinct experimental events are averaged together.

Psychophysical Relations

In probing a little more deeply some of the research on asymptotic behavior, it will be useful to distinguish—again, our magic number!—three types of data plots, only two of which have been mentioned during the Symposium. The missing one is at least as important as the other two. The first are the *psychophysical relations*, which I shall refer to as ψ–φ relations, in which a psychological (response) measure, ψ, is plotted as a function of an underlying physical variable, φ, with everything else held fixed. Stevens' (1961) power law, which states that on the average the numbers emitted by a subject to various intensities of stimulation are a power function of some usual physical measure of intensity (e.g., energy), is an example of a ψ–φ relation. So is the familiar psychometric function in which the probability of detecting a stimulus difference is plotted as a function of a physical measure of that difference. The ψ–φ relations are among the most familiar results from experimental psychology, and they appear to have been of greatest interest to neurophysiologists, mainly, I suspect, because they relate responses to the same sort of sensory stimuli often used in their work.

Psychocontingency Relations

A second type of relation is that between a behavioral measure and some quantity that has to do with, for example, the probability distribution of stimulus presentations or the outcomes administered to the subject. Following Dr. Stevens' welcome suggestion, I shall call these *psychocontingency relations*, abbreviated as ψ–χ relations. In classical psychophysics, explicit outcomes were not used and so such functions were rarely, if ever, plotted, although psychologists were aware of the effects of the presentation distribution. In some of modern psychophysics, outcomes are employed in much the same way they are in the study of learning, and so it is sensible to ask how a response measure, ψ, depends upon some function, χ, of the outcomes and presentation probabilities. Just which functions χ should be examined is not yet clear, but various possibilities, some of which have been suggested by particular psychophysical theories, are currently under investigation.

An example of a ψ–χ relation has already been mentioned, namely, the plot of mean latency against the Shannon information measure of the presentation distribution. This can be plotted whether or not outcomes are employed. A second example, which illustrates the difficulty we now have in knowing how to characterize the relevant contingency variable, also involves latencies. Consider a simple reaction time experiment in which the subject is to make a single response to a perfectly detectable stimulus. Classically, he was asked to respond as rapidly as possible without making too many anticipatory responses. To bring these instructions under better experimental control, suppose that we fine him a cents if his response occurs earlier than τ ms. after the signal, fine him c cents if it occurs later than τ' ms. after the signal, and reward him b cents if it falls between τ and τ' ms. Such a "band payoff" is illustrated in Fig. 1. It is found that as we shift the location of the band, the subject's reaction time distribution tracks it, but with certain changes in shape, as can be seen for the subject shown in Fig. 2. One is confident that the values of a, b, c, τ, and τ' all affect the distribution

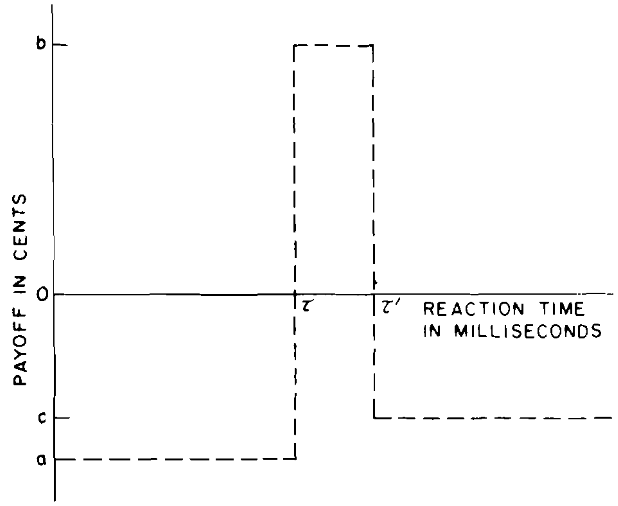

Fig. 1. Band payoff scheme: for responses less than τ milliseconds after the stimulus, the subject receives (loses) a cents; for responses between τ and τ' milliseconds, he receives b cents; and for responses slower than τ' milliseconds, he receives (loses) c cents.

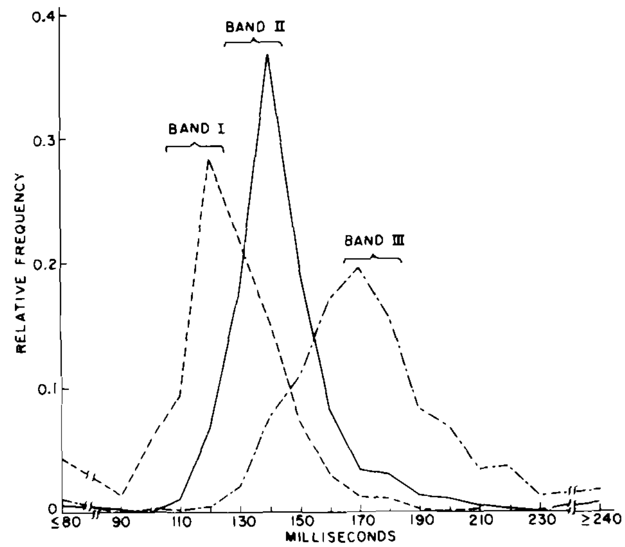

Fig. 2. Reaction time data for one subject (AB) collected by Miss Gay Snodgrass under the direction of Dr. Eugene Galanter. A warning signal was followed in approximately 2 seconds by the stimulus: both were 1000 cps tones of 143 ms duration at 30 db above .001 volts. The payoffs were divided into three parts, the center of which is shown for each distribution. Responses outside the center band were fined 1¢, those within it rewarded by 2¢ (in the notation of the text and Fig. 1, $a = c = -1$, $b = 2$; in Band I, $\tau = 105$ ms; in Band II, $\tau = 125$ ms; in Band III, $\tau = 165$ ms; and in all three, $\tau' = \tau + 20$). Information was fed back after each response. The order of sessions was II, III, I. Each distribution is based upon approximately 500 responses.

to some degree. Without a theory, it is difficult to know what function of these five variables might be relevant or what aspects of the latency distribution should be plotted against it. Nonetheless, there can be little doubt that something systematic exists between latency and the payoff function.

It should be noted in passing that the latency distributions of Fig. 2 are different from those classically reported. The means of the fastest ones are very fast by classical standards and at least one of the distributions is much more peaked than the usual gamma distributions that have been fitted to much reaction time data.

Although we do not yet know much about the relevant contingency variables, the existing data leave little room to doubt that these variables have a profound and relatively prolonged effect upon the behavior of subjects in even the simplest sensory experiments. Work in signal detectability theory and allied topics (see Green, 1960; Licklider, 1959; or Luce 1963a) suggests that we do yet have a very accurate understanding of many ψ–φ relations because we have failed to examine explicitly the effects of χ variables on ψ ones. (This point will become clearer when we consider our third kind of relation.) That being so, I cannot help but feel that any neurophysiological theory that fails to provide room for contingency inputs to the subject is incomplete and, quite possibly, misleading. It has bothered me how little has been said at this Symposium about how the nervous system deals with outcomes and presentation probabilities.

Psychopsychological Relations

A third type of behavioral plot, which to my recollection has not been discussed at all, is what we may call *psychopsychological relations,* or, for short, ψ–ψ relations. Such plots establish the trading or exchange relations that exist between two psychological (response) variables. Let me illustrate the sort of thing I mean by two examples.

Suppose that on each trial of an experiment one of two possible stimuli occurs, one of which we may call the signal, s, and the other no signal, n, or, if you will, signal and noise. The task for the subject is to identify which has occurred, and so, in effect, he responds either "yes, it is there" or "no, there was no signal". Several different models have been proposed to account for subjects' behavior in such experiments; in broad outline, the models are quite similar although in detail they make different predictions (see Luce, 1963a). It is assumed that as a result of stimulation the subject enters into one of several possible internal states that constitute his possible representations of the sensory inputs. The probability distributions over these states differ depending upon whether s or n was presented. In the simplest threshold model there are only two states, which correspond to whether or not the threshold is exceeded (see Luce, 1963b). In the mathematically more complex signal detectability model, which was sketched by Dr. Broadbent, the internal states form a continuum (see Green, 1960; Licklider, 1959; or Luce, 1963a). Given this internal representation of the stimulus, it is next assumed that the subject operates on this information in a way that depends in part upon the contingency variables of the experiment. This is known as the decision phase of the overall response process. It is typical of almost all current "psychophysical" models to have a pure sensory process followed by a pure decision process.

Let $p(Y|s)$ denote the conditional probability that the subject says "Yes" when s is presented and $p(Y|n)$ the same thing when n is presented. The models yield equations of the form:

$$p(Y|s) = f(\eta,d)$$
$$p(Y|n) = g(\eta,d),$$

where f and g are functions determined by the theory, η is a parameter (or collection of parameters) that represents the stimulus situation, and d is a parameter (or collection

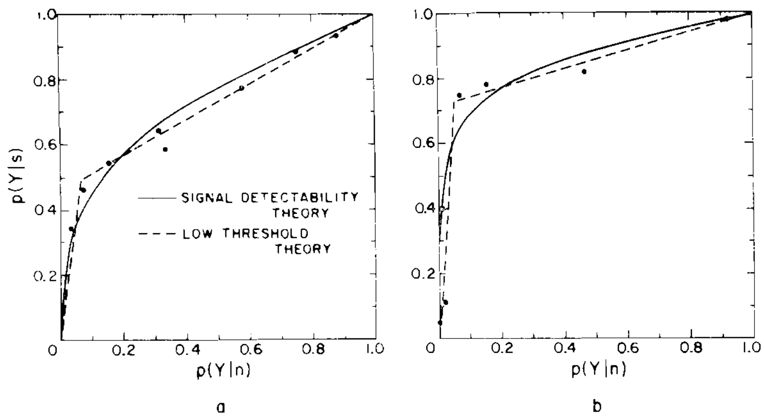

Fig. 3. Isosensitivity data reported by Norman (1962) for subject 6. A 1000 cps, 0.030 volt pure tone background was presented monaurally. The subject detected small energy increments lasting 143 ms. In Fig. 3a the increment was $\frac{\Delta V}{V} = 0.019$; in Fig. 3b it was 0.022. A presentation probability of 0.5 was used and the points were generated by different payoff matrices, which in Fig. 3a ranged from

$$\begin{pmatrix} 1 & -1 \\ -4 & 4 \end{pmatrix} \quad \text{to} \quad \begin{pmatrix} 4 & -4 \\ -1 & 1 \end{pmatrix}$$

and in Fig. 3b from

$$\begin{pmatrix} 1 & 0 \\ -100 & 0 \end{pmatrix} \quad \text{to} \quad \begin{pmatrix} 1 & -25 \\ -2 & 1 \end{pmatrix}$$

Each point in Fig. 3a is based upon 600 observations (300 per coordinate) except for the middle two which are based upon 1200 observations. In Fig. 3b, the middle three points are based upon 300 observations and the remaining four on 200. The solid theoretical curve is from signal detectability theory and the dashed one from a low threshold theory (both curves were fit by eye).

of parameters) that represents the decision situation. In those cases where d is simply a number, it is possible to eliminate it from the pair of equations to obtain an equation of the form:

$$p(Y|s) = F[p(Y|n), \eta],$$

which is independent of the value of the decision parameter d. For η fixed, which it is supposed to be if we hold the stimulating conditions constant, this ψ–ψ relation is known as an iso-sensitivity curve or a receiver operating characteristic (ROC curve). By varying the parameter d of the decision process, which we can do by using various payoff matrices or presentation probabilities, we can generate empirical plots of p(Y|s) versus p(Y|n). When we do, we obtain data of the sort shown in Fig. 3. The two theoretical curves shown arise from the threshold and signal detectability theories just mentioned.

In summary, then, the iso-sensitivity curve is a ψ–ψ relation that, in a sense, represents the sensory process with the decision process factored out. The decision process, which is as yet ill understood, determines which particular point of the iso-sensitivity curve will arise under given experimental conditions; but the shape and constants of the iso-sensitivity curve are, according to these theories, independent of the contingency variables and depend only upon the nature of the sensory process and the particular stimulating conditions.

A plot of one response variable against another is not only an interesting way to view the data—rather more revealing than one might expect—but it also suggests that some of the more classical ways of looking at these data may be inappropriate or misleading. Consider, for example, the psychometric function (which is a typical ψ–φ relation), i.e., the plot of p(Y|s) against a physical measure of s. In the classical literature this has always been assumed to be a well-determined function, just the sort of function that a good neurophysiological theory might be expected to explain. However, at the behavioral level there are many psychometric functions for the same subject under the same stimulating conditions. For example, changing the payoff matrix changes the psychometric function, as shown in Fig. 4. We can probably salvage the idea that a single

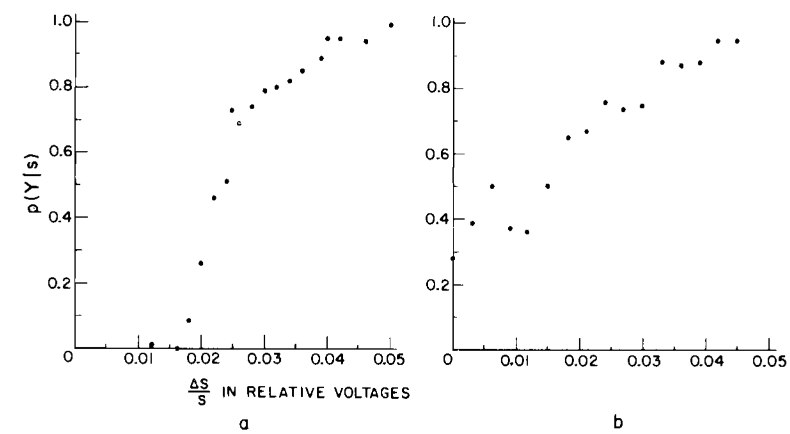

Fig. 4. Psychometric functions reported by Norman (1962) for the same subject (6) and under the same experimental conditions as described in Fig. 3. For Fig. 4a the presentation probability was 0.8; the payoff matrix was $\begin{pmatrix} 1 & -\frac{1}{4} \\ -25 & \frac{3}{4} \end{pmatrix}$ cents; each point is based upon from 80-160 observations. For Fig. 4b the presentation probability was 0.5; the payoff matrix was $\begin{pmatrix} 1 & -1 \\ -1 & 1 \end{pmatrix}$; each point is based upon 125 observations.

sensory function underlies our observations if we let the decision process account for altered response biases when we change such things as the payoffs. For more detailed discussions, see Green (1960), Luce (1963a), and Norman (1962).

As another illustration of the difficulty of simply looking at p(Y|s) and ignoring p(Y|n), consider the problem of estimating thresholds. Classically, the threshold is defined as that level of stimulation for which p(Y|s) = 1/2. Assuming that the iso-sensitivity curves vary with s in a way such as shown in Fig. 5, which they do, then it is clear that the line p(Y|s) = 1/2 does not determine a unique value of s, unless p(Y|n) is specified. Usually in threshold work one trains the subject until p(Y|n) is small, say, something less than 0.05. As can be seen in Fig. 5, this is really not an adequate specification of p(Y|n) because in this region the slopes of the iso-sensitivity curves are very steep. Thus, for a fixed value of s, a small change in p(Y|n) corresponds to a large change in p(Y|s) or, equally well, for p(Y|s) = 1/2, a small change in p(Y|n) corresponds to a considerable change in the estimate of the threshold. Consequently, the classical estimates of thresholds have to be accepted with caution because the estimation procedure is inherently unstable. Moreover, this definition of a threshold does not appear to have any natural correspondence to anything revealed by the iso-sensitivity curve.

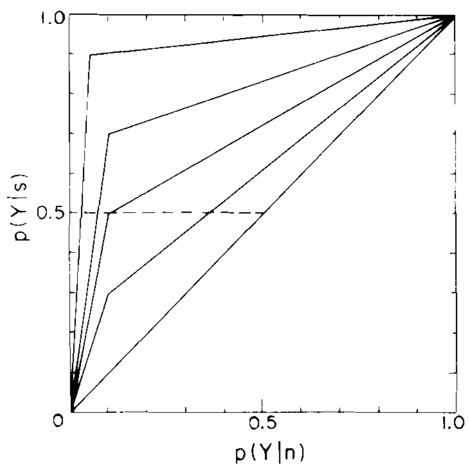

Fig. 5. A family of low threshold isosensitivity curves showing that the usual definition of a threshold, $p(Y|s) = 1/2$, does not correspond to any particular curve unless $p(Y|n)$ is specified exactly.

A numerical parameter which is a characteristic of these curves would seem to be more useful to represent a subject's sensitivity than the usual threshold measure.

A second example of a possible ψ–ψ relation is the plot of the mean latency of a stimulus-response pair against the conditional probability of the response to the stimulus. This is closely related to the ψ–χ relation of latency to the information measure of the stimulus presentation distribution; however, several theories suggest that this ψ–ψ relation is basically simple whereas the ψ–χ one is only apparently simple (LaBerge, 1962; Luce, 1960b). Whether there is a true exchange relation between latency and response probability, as the theories suggest, and what form it has is a topic currently under investigation in several laboratories.

Each of the three data plots that I have discussed has its drawbacks, stemming primarily from the fact that the response variable depends at least upon both φ and χ variables, and therefore the data cannot be conveniently represented in the plane. Both ψ–φ and ψ–χ relations are really quite complex families of functions with, respectively, the missing χ and φ variables serving as parameters of the family. The ψ–ψ relations are also families of functions, but in some cases they differ in the important respect that one of the experimenter-controlled variables is completely eliminated from the plot. For example, in the iso-sensitivity curve all sign of the contingency variables (as represented by the parameter of the decision process) has vanished.

In my opinion, these ψ–ψ relations are likely to be of fundamental importance because they represent what are, in a sense, purely psychological constraints. These are to be contrasted with the ψ–φ and ψ–χ relations which establish connections between the subject and his environment. So far, our theories suggest that the ψ–ψ relations should be somewhat simpler than either of the other two. Specifically, the relation of the subject to himself seems to be free of some of the detailed parametric problems that have continually plagued research into his responses to his environment.

If some ψ–ψ relations turn out to be fairly simple and independent of a good many experimental parameters, then they strike me as something worthy of neurophysiological analysis. Although it is definitely premature to say just where the research on asymptotic choice behavior is headed and what basic relations ultimately will be found, neurophysiologists concerned with behavioral explanations should be aware that some subtle changes in outlook are beginning to take place in parts of psychology. Just as they have been repeatedly exasperated by psychologists who attempt to explain behavior in terms of outmoded neurophysiological concepts, so, too, psychologists would prefer to see neurophysiologists attempt to account for the best current formulations of the facts of behavior, not those of an earlier era.

REFERENCES

ATTNEAVE, F.: *Applications of information theory to psychology.* New York: Holt, 1959.

FERSTER, C. B., and SKINNER, B. F.: *Schedules of reinforcement.* New York: Appleton-Century-Crofts, 1957.

GREEN, D. M.: Psychoacoustics and detection theory. *J. acoust. Soc. Amer., 32,* 1189-1203, 1960.

LABERGE, D.: A recruitment theory of simple behavior. *Psychometrika, 27,* 375-396, 1962.

LICKLIDER, J. C. R.: Three auditory theories. In S. Koch (Ed.), *Psychology: a study of a science,* Vol. 1. New York: McGraw-Hill, 41-144, 1959.

LUCE, R. D.: The theory of selective information and some of its behavioral applications. In R. D. Luce (Ed.), *Developments in mathematical psychology.* Glencoe, Ill: The Free Press, 1-119, 1960a.

LUCE, R. D.: Response latencies and probabilities. In K. J. Arrow, S. Karlin, and P. Suppes (Eds.), *Mathematical methods in the social sciences, 1959.* Stanford: Stanford Univer. Press, 298-311, 1960b.

LUCE, R. D.: Detection and recognition. In R. D. Luce, R. R. Bush, & E. Galanter (Eds.), *Handbook of mathematical psychology,* Vol. 1. New York: Wiley, 103-189, 1963a.

LUCE, R. D.: A threshold theory for simple detection experiments. *Psychol. Rev., 70,* 61-79, 1963b.

NORMAN, D. A.: *Sensory thresholds and response biases in detection experiments: a theoretical and experimental analysis.* Unpublished PhD dissertation, Dept. of Psychol., Univer. of Penn., 1962.

SKINNER, B. F.: *The Behavior of organisms.* New York: Appleton-Century-Crofts, 1938.

STEVENS, S. S.: The psychophysics of sensory function. In W. A. Rosenblith (Ed.), *Sensory communication.* New York: Wiley, 1-33, 1961.

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MACKAY (communicated): May I add a brief comment on Dr. Luce's remarks which I was not present to hear? I think his pessimism regarding the 'laboratory-worthiness' of phenomena evoked by massive stimuli (most of which, incidentally, have no more connection with Gestalt psychology than with any other brand) reflects a difference in our criteria of simplicity. Nothing is simpler, information-wise, than a field of random noise. Physiologists may or may not find such stimuli rewarding in the study of single cells; but in studying the large-scale organization of the visual network it is hard to see how anything but massive (though still *simple*) stimuli can be expected to probe the system in the way we want.

CHAIRMAN: I am especially pleased that Dr. Luce, in giving the last formal presentation of the Symposium, has put together and emphasized, with the quantitative aspects, the influence of outcome on what precedes outcome and the probabilistic shifting depending on positive or negative reward. This is basically a problem that I laid greatest emphasis on in my opening remarks and that, above or below the verbal level of explication, really has been the guiding problem of this Symposium. Somewhere inside a brain, or inside a black box, the feedback from the environment, the success or failure of the act, leads to certain new relations at the neuronal level or the molecular level or the magnetic storage level or the programme probability level.

In all cases, the really basic problem that we face is: how the probability as between two, or more than two, outcomes altered as a result of this experience. This is true no