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BOOK REVIEWS

WENDELL R. GARNER. *Uncertainty and Structure as Psychological Concepts.* New York: John Wiley and Sons, Inc., 1962. Pp. 369.

It was a misfortune of psychology that it lacked a tradition of dealing with rigorous mathematical theories when psychologists were first attracted by information theory. Applications were made with simple-minded identification of psychological concepts with communication terms, without really paying attention to the meaning of the terms in the respective areas. Quite a few experiments were reported measuring human channel capacity under various experimental conditions without asking the basic question: Does the human being comply with the definition of channel in communication engineering? It is true that, in spite of this carelessness, the bulk of experiments reported demonstrated some systematic results as summarized by G. A. Miller in his concept of *The Magical Number Seven.* However, these experiments also led to various riddles and confusions as illustrated by Garner in Chapter 2 of this book. And this is undoubtedly the reason that many frustrated psychologists finally gave up information theory as useless to psychology. Still, after the waxing and waning of information theory in psychology, an important recognition remained: Information processing is one of the most significant functions of man. The recognition must eventually revive the application of information theory to psychology as a sheer necessity. Probably "application" is not a proper word. A kind of information theory must be developed which is suitable to describe as complicated an information processing mechanism as man. A first step toward such a theory was taken by McGill in his paper published in *Psychometrika* in 1954. What I call a misfortune of psychology is this: Instead of taking McGill's mathematical system (called *symmetric uncertainty analysis* by Garner and abbreviated here as SUA) as a conceptual tool in analyzing psychological problems, the tradition of psychology almost forced us to see it as another statistical testing technique analogous to the analysis of variance. As such, SUA was not so handy as the analysis of variance because of the lack of known distributions, and thus SUA failed to acquire popularity. What we needed then, and need now, is a conceptual means which logically bridges information theory to psychology. So the author could not do better in entirely leaving out of the book the significance testing aspect of SUA.

It must be pointed out that SUA is not a model of human behavior. It is a system of mathematics (or, I would rather say, of logics) so that it is infallible as far as it goes. This aspect of SUA must be clearly remembered. Information theory, developed in communication engineering, is a normative theory. It is concerned primarily with the optimal information transmission system, the system that transmits messages as rapidly and correctly as possible under given constraining conditions. On the other hand, theories in psychology are primarily descriptive, aiming at a precise description of human behavior regardless of its degree of optimality. An application of information theory to psychology cannot be made by circumventing this distinction between the required natures of theories in the two fields. One way of turning a normative theory into a descriptive theory is to regard the behavior of a system as optimal, *optimal under some unknown constraining conditions*, and then to derive the constraining conditions from the observed, assumed optimal, behavior.

This line of approach would prove useful if conscientiously followed. However, as mentioned previously, for this purpose we need a more elaborately developed information theory than that presently available. Another line of application is the one that was adopted by Garner, which would be put, according to the reviewer's liberal interpretation, as below.

Any observed response of a subject is not born in a vacuum. There must be a stimulus that causes the response. In general, however, the stimulus immediately preceding a response is not the unique determiner of the response. The response is made within the context of the totality of possible alternative stimuli, and furthermore it is also dependent upon the *task* given to *S*, where the task may be recognized as a set of rules according to which *S* is to respond to the given stimulus. Taking this for granted, it becomes a simple truism that the observed response can meaningfully be analyzed only if the real nature of the task given to *S* is clearly understood by *E* himself. In spite of this plain fact, we must admit that the necessity of *task analysis* has been neglected in psychology. There are quite a few published experimental reports in which the conclusion can be drawn almost directly from the nature of the task given to *S*. Task analysis is also very important in comparing experimental results obtained under different experimental conditions. Comparison and mutual checking of separate experimental results are the prerequisites to the accumulation of scientific knowledge, but if they are done without a legitimate basis, more confusion is added to what we already have in psychology in plenty.

Now, one of the major achievements in this book is the demonstration of the usefulness of information theory or, more specifically, SUA as a means of task analysis. ("Task analysis" is the term coined by the reviewer.) A substantial part of the book, in particular Chapter 5 and later chapters, is devoted to the use of SUA as a task analysis, although this nature of the book is not made very explicit. The work is done well. In spite of the fact that the issues brought up are controversial, and no clear-cut solutions to the problems are given nor intended to be given, the power of SUA task analysis is well demonstrated. The author lucidly illustrates how misunderstandings of the real nature of experimental results are prevalent, casts doubts on traditional interpretations, suggests new interpretations and new ways of investigation, and through all this, effectively demonstrates to the reader that SUA task analysis helps one to think clearly and to raise psychologically meaningful questions not confounded by the logical structure or the specific task given. Evidently, recognizing the true nature of problems is the first step toward solving them eventually. The author's goal in writing this book was apparently to take this first step. The goal seems to have been achieved.

However, it seems obvious to me that the process of achieving this goal was not an easy one, and the hardship is clearly reflected in the presentation of the book. This is a pioneering work, and like any other pioneer, Garner must have made his way through this "no man's land" without the aid of paved straight roads, any known landmarks to keep the right direction, and so on. The outcome is this book; the reader might almost feel the agonies of the author. I often found considerable difficulty in figuring out what he was getting at, and sometimes later discovered that he was taking a long roundabout route. Occasionally, there are also confusions and misevaluations of his own findings. For these, I can hardly blame him.

This is a pioneering work, and no pioneering work can be entirely free from occasional confusions and misevaluations. As a reviewer, and also as one who really recognizes the importance of this work, I thought that it was a part of my duty to straighten out some of the disputable points as much as I could, so that this really significant work might be appreciated by a broader audience. The result is this excessively long review. I hope it deserves its length.

Before entering into a detailed review, let me begin with a brief summary presentation of SUA. Since SUA is the core of the book, no detailed comments can be made without assuming some notion of it on the part of the reader.

Consider a multivariate contingency table. Let X, Y, $\cdots$, Z denote the set of variables concerned. Each variable is assumed to take only a finite number of discrete values. (Values may be nonnumerical.) Let x_1, x_2, $\cdots$, x_n be the alternative values of X, and let the values of the other variables be denoted analogously. The entry in each cell of the multivariate table is assumed to be a probability, not a relative frequency. For example, the entry in the cell corresponding to the values $(x_i, y_j, \cdots, z_k)$ is the probability of the joint event $(x_i, y_j, \cdots, z_k)$. From the probability distribution over the whole table, one may obtain marginal probability distributions for various combinations of variables. For example, the marginal distribution for the variable X is given by

$$p(x_i) = \sum_{j, \cdots, k} p(x_i, y_j, \cdots, z_k).$$

For any subset of variables its uncertainty is defined as the entropy, or the information measure, of the probability distribution characteristic to the subset. For example, $U(XY)$, the uncertainty of the subset of variables X and Y, is given as

$$(1) \qquad U(XY) = -\sum_{i,j} p(x_i, y_j) \log p(x_i, y_j).$$

(The arguments X and Y generically represent any two of the random variables constituting the multivariate system. This is true for all the equalities introduced below.) The reason why this measure is called uncertainty is well known. Uncertainty is nonnegative, and it satisfies the following relation *if and only if X and Y are independent.*

$$(2) \qquad U(XY) = U(X) + U(Y).$$

The nominal uncertainty of a variable X, denoted $U_0(X)$, is the maximum possible value of $U(X)$ under the limitation that the number of values of X, n, is fixed. This maximum value is realized when the marginal distribution of X is homogeneous. Then, as is well known, $U_0(X) = \log n$.

Redundancy (absolute) of a subset of variables, for example X and Y, is defined as

$$(3) \qquad R(XY) = U_0(X) + U_0(Y) - U(XY).$$

Redundancy (relative) is given by dividing $R(XY)$ by $U_0(X) + U_0(Y)$. Redundancy is nonnegative.

Now we will introduce a few functions which will play the central roles in SUA, functions which are all derived from the uncertainty function. Conditional uncertainty $U_Y(X)$ is defined as

$$(4) \qquad U_Y(X) = U(XY) - U(Y).$$

Simple contingent uncertainty $U(Y{:}X)$ is defined as

$$(5) \qquad U(Y{:}X) = U(Y) + U(X) - U(XY).$$

It is obvious that $U(Y : X) = U(X : Y)$; i.e., simple contingent uncertainty is symmetric. Multiple contingent uncertainty $U(Y : X_1 \cdots X_N)$ is obtained by replacing the single variable X in (5) by a subset of variables $(X_1, \cdots, X_N)$:

$$(6) \qquad U(Y{:}X_1 \cdots X_N) = U(Y) + U(X_1 \cdots X_N) - U(YX_1 \cdots X_N).$$

Likewise, a function $U(X_1 : X_2 : \cdots : X_N)$ is defined as

$$(7) \qquad U(X_1 : X_2 : \cdots : X_N) = \sum_i U(X_i) - U(X_1X_2 \cdots X_N).$$

This function is called constraint by Garner, but since constraint means another function, too, we will call it multiplex contingent uncertainty here just for the convenience of reference. Multiplex contingent uncertainty is also symmetric.

At this point some interpretations of SUA functions so far introduced would be appropriate. Conditional uncertainty is the notion called equivocation in information theory. $U_Y(X)$ is the average residual uncertainty of X

when the value of Y is known. Therefore, $U_Y(X) = 0$ means that the value of X is completely specified if the value of Y is known. This relation is important in the task analysis to be discussed soon, where X commonly signifies the stimulus variable and Y the response variable designated as correct by the given task. Then $U_Y(X) = 0$ characterizes an important class of tasks in which no response is correctly associated with more than one stimulus. For convenience, let me call such a task a normal task.

Simple contingent uncertainty is formally equivalent to the notion known as the amount of transmitted information in information theory—X being input variable, Y being output variable, and $U(Y : X)$ representing the amount of information transmitted from input to output. Let input be the stimulus, output the response actually made by S to the given stimulus, and $U(Y : X)$ the amount of information transmitted from stimulus to response. This measure, and others too, and therefore the whole system of SUA may then be used as a descriptive analytical tool when SUA is applied to a data contingency table. Chapters 2 through 4 of the book are devoted to the descriptive use of SUA for psychological problems. Although Garner's view given in these chapters is interesting, its importance is certainly less than that of the issues brought up in the later chapters, and we will not touch here upon the topics discussed in these early chapters.

A fact which is of a central importance in SUA is that the "transmission" interpretation of $U(Y : X)$ given above is not its only interpretation. A comparison of (3) and (5) will immediately reveal the resemblance between the forms of simple contingent uncertainty and redundancy. As a matter of fact, simple contingent uncertainty *is* redundancy if the probability distributions of X and Y are both homogeneous. No transmission-type interpretation is possible of multiplex contingent uncertainty. It is a measure similar to redundancy, and like redundancy it represents, in a certain unambiguous way, the extent to which the system concerned is structured. In general, the redundancy of a system (a set of variables) is expressed as the sum of a contingent uncertainty and a distributional constraint defined as

$$DC(X_1X_2 \cdots X_N) = \sum_i (U_0(X_i) - U(X_i)). \tag{8}$$

This distributional constraint component of redundancy often vanishes in ordinary experimental designs (when X_i represents stimulus dimensions). The condition for obtaining zero DC is that the marginal probability distribution over each stimulus dimension be homogeneous; in other words, alternative stimulus values must be used equally often for each stimulus dimension. For convenience, let us tentatively call a task homogeneous if it uses a set of stimuli satisfying the above condition. (This condition is made clear in Chapter 6 but not in Chapter 5 where the author often refers to $U(X_1 : \cdots : X_N)$ as redundancy.)

Our last important SUA notion to be introduced here is interaction uncertainty:

$$U(Y: X_1X_2) = U(Y: X_1) + U(Y: X_2) + U(\overline{YX_1X_2}), \tag{9}$$

where $U(\overline{YX_1X_2})$ is the interaction among the three variables Y, X_1, and X_2. Higher-order interaction uncertainties are defined by expanding a multiple contingent uncertainty like the one on the left-hand side of (9) but involving more than two X variables. The way of expansion is very similar to the way that the total sum of squares is expanded into main effects and interactions in the analysis of variance. The resemblance is not superficial. The total system of SUA is mainly concerned with the logical relationships among several functions derived from a certain function U. The equalities of SUA (at least those given in this book) are not at all dependent upon the definition of the original U function. Inequalities stating that uncertainties, conditional uncertainties, and contingent uncertainties are all nonnegative are all that depend upon the assumption that U is entropy. This independence of the main body of SUA from the original meaning of U is not a weakness of SUA but is rather its potential power. It is easy to show that, by changing the definition of U, we have the analysis of variance (as an analytical tool, not as a testing technique). Although underdeveloped and incomplete at present, SUA might possibly be developed into a branch of algebra, such as theory of groups, graph theory, and the like, an algebra which particularly fits the purposes of psychology.

Among the notions of SUA, probably the most important are the contingent uncertainties which are used as measures of "structure" by the author. Consider the equality (5) (or (6) or (7)). It is seen from (2) that the right-hand side of (5) vanishes when the variables in the set considered are mutually independent. As the dependency, or correlation, between the variables increases, contingent uncertainty increases. It is obvious that the author uses the term "structure" in the sense of probabilistic dependency. Taking this for granted, one thing which is not obvious is, then, why the measure is called contingent *uncertainty*. In the last chapter on page 339, in the paragraph which begins with the statement, "*Uncertainty is prerequisite to structure,*" the author says: ". . . to have structure is to have uncertainty. Furthermore, to increase structure is also to increase uncertainty, and it is this aspect of the problem which is conceptually so important." I agree that uncertainty is prerequisite to structure—structure in the sense used by the author. But in my opinion, it is more important to recognize that structure is not uncertainty. More specifically, structure (as a quantity) is neither uncertainty nor certainty, but is the amount of uncertainty reduced. The amount of structure defined as contingent uncertainty is a concept very close to the amount of information. In fact, the amount of information is quite generally defined as simple contingent uncertainty, and therefore it is just one special measure of the amount of structure. There is a question which often bothers the student of information theory: Why can information and uncertainty, two almost diametrically opposite concepts, be represented by the same measure? This is an artifact due to the simple illustrative examples used in a textbook on information theory. In a case when there is no equivocation, the received message causes the complete reduction of the original uncertainty. Therefore, in this case, and in this case alone, the original uncertainty and the obtained information are equal. In general, however, uncertainty and information are distinctively different concepts even though they are closely related. The same is true for uncertainty and structure. Consider redundancy defined in (3). Redundancy, as well as contingent uncertainty, is a measure of structure. They both have a form which permits the interpretation that structure is the amount of uncertainty reduced. The sum of U_0's on the right-hand side of (3) may be looked upon as the uncertainty of a system of which only the skeletal structure, i.e., the number of alternative states of the system, is known. Now, the second term of the expression of redundancy is the real uncertainty of the system. Redundancy is the difference between two uncertainties, hypothetical and real, thus permitting the interpretation that it is the amount of uncertainty reduced by knowing the reality. Contingent uncertainty also has a very similar form. The only difference is that U_0 in redundancy is replaced by U (marginal uncertainty) in contingent uncertainty. In general, therefore, the amount of structure is the extent to which the real system is more restricted than hypothetically assumed. If this interpretation is appropriate, then it also implies that the hypothetical uncertainty may be chosen fairly arbitrarily; that is, it may be considered, formally speaking, just as a conventional origin of the scale of structure. This freedom in choosing the origin gives us a real advantage in applying SUA to psychological problems. The reason is as follows: The amount of structure is the difference between hypothetical uncertainty and real uncertainty about the given system. In an application of SUA to psychological problems, the system is the task given or any subtask under consideration. Then by taking as the hypothetical uncertainty the S's prior uncertainty about the task or subtask when he is put in the experiment, we may interpret the amount of structure as the maximum amount of uncertainty reduction the S may obtain by learning the real uncertainty involved in the task or subtask. As will be shown later, this interpretation helps us clear some of the ambiguities left in the book where the role of redundancy is discussed.

Going back to the earlier point, I think that contingent "uncertainty" and interaction "uncertainty" are misnomers. No interpretation whatsoever is possible which makes their definitions compatible with the notion of uncertainty as such.

In Chapter 5, the author completes his exposition of SUA. This chapter certainly provides the key to all the important issues discussed in this book, but at the same time it is the most confusing chapter of all. One of the sources of confusion is that the author there definitely shifts from the descriptive use of SUA to its use as a tool for task analysis. It should be clear to the reader that some shift is made there. The concept of structure is introduced, and contingent uncertainties are now referred to as constraints. It may not be so clear to the reader, however, that by *response* Garner now means the correct response specified by the task, not the response actually made by S. The confusion may further be increased by the fact that the author continues to talk about S's actual performance too. Most of the confusions would be removed, however, by understanding the author's intention this way: Task analysis is important just because the nature of the task, or the structure of the task, is the principal determiner of the S's performance under the task. Taking an extreme case, the nature of the task is the only determiner of S's performance when the task is so easy as to allow S a perfect performance. In general, it is true that S's performance can hardly be evaluated properly unless the structure of the task is clearly understood.

The basic equality used by the author for task analysis is

$$U(Y: X_1 : X_2 : \cdots : X_N) = U(Y: X_1X_2 \cdots X_N) + U(X_1 : X_2 : \cdots : X_N). \tag{10}$$

This equality can be derived directly from (6) and (7). Now suppose that Y represents the correct response variable used in a given task and X_1, X_2, $\cdots$, X_N the stimulus dimensions used. Then the first term in the right-hand side of (10) (multiple contingent uncertainty) represents the structure holding between the correct response variable and the stimulus variable. Under this interpretation of variables, this multiple contingent uncertainty is called *external constraint*, and it may be interpreted as the amount of information transmitted by an ideal subject who executes the task perfectly. This measure would then serve as a reference value to describe the actual S's performance, and it gives the upper limit of possible transmission if the task is normal, i.e., if

$$U_Y(X_1X_2 \cdots X_N) = 0. \tag{11}$$

Most of the tasks dealt with in this book are normal. The second term in the right-hand side of (10), the multiplex contingent uncertainty now being called *internal constraint*, certainly represents the structure within the dimensions of stimuli used in the task. The left-hand side of the equality, which is also a multiplex contingent uncertainty, is now called *total constraint*. With this new terminology the equality (10) is now verbally stated as follows: *The total constraint is the sum of the internal constraint and the external constraint.*

It is important to remember equality (10) and its verbal statement in order to understand correctly the task analysis done by the author. One thing, however, bothers me. In Chapter 5, the author keeps emphasizing the importance of the above statement—to quote one example (page 174) "... if total constraint is held constant, then internal and external constraint are interchangeable, such that one increases by exactly the same amount as the other decreases. This fact prevents independent manipulation of internal and external constraint." This statement is right, but it is trivial. If one increases a part of a whole while keeping the total constant, the rest must decrease exactly by the same amount. Since (10) is an equality, not an equation, nothing more than this trivial truth can be drawn from it. There are occasions, however, when a logically trivial statement acquires some real, normative implications—that is, when the definitions from which the statement is derived are forgotten. Consider, for example, a homogeneous task. With a homogeneous task, stimulus redundancy is identical to internal constraint. Now, redundancy is a familiar concept. However, many people would have forgotten its exact definition. By substituting "redundancy" in place of internal constraint in the author's statement cited above, the statement comes to serve as a warning: *If total constraint is held constant,* external constraint decreases by exactly the same amount as redundancy increases. This rephrased statement really has an interesting implication as will be seen soon.

In applying (10) to psychological tasks, the author confines himself within the class of normal tasks characterized by (11). (Actually, the author uses a more restricted class of tasks characterized by $U_Y(X_1 \cdots X_N) = U_{X_1 \cdots X_N}(Y) = 0$. That is, he talks about tasks such that one-to-one correspondence holds between stimuli and associated correct responses. However, (11) is all that is needed for the author's arguments in Chapter 5.) Now, by applying (11) to (10) we have

$$(12) \qquad \sum_i U(X_i) = U(X_1 X_2 \cdots X_N) + U(X_1 : X_2 : \cdots : X_N),$$

since $U(Y) = U(YX_1X_2 \cdots X_N)$ if (11) holds. The reader will recognize that (12) is identical to (7), the equality by which multiplex contingent uncertainty is defined. However, (12) has something more than what is involved in (7); the three terms in (12) now have specific meanings—total constraint, external constraint, and internal constraint from left to right, respectively. It must be correctly understood that such meanings can be given to the three terms of (12) just by virtue of the assumption (11).

This point seems to need a further emphasis. As has been mentioned repeatedly, SUA is not a theory nor a model, but it is a system of logics. Inasmuch as it is a logical system, it helps us think clearly and carry out logical deductions correctly. One should not expect that anything very novel would turn up from the main body of SUA itself. But if any material assumption, like the equation (11), is added to the system of SUA, various implications of this particular assumption will be effectively uncovered by the power of the prefabricated logical system of SUA. Whenever this happens in an application of SUA, it is very important to keep in mind that all the novel aspects of the deductions made come from the assumption itself and not from SUA. I point this out here because, although Garner occasionally reminds us of the existence of the assumption (11), a casual reader would very likely overlook the important role played by the assumption. For example, there is the statement on page 152 in Chapter 5: "Redundancy has no effect on total constraint as long as each variable retains its maximum uncertainty." Here, "redundancy" means internal constraint. Maximum or not, as long as (marginal) uncertainties of the variables are held constant, the left-hand side of (12) remains constant. Since this term is total constraint under (11), any introduction of redundancy which keeps the sum of marginal uncertainties intact does not affect total constraint. It is clear that the above statement does not stand alone but calls for (11) or a similar assumption for its justification.

Now we will see how (12) is used for the analysis of normal tasks, and what the task analysis is really like. The external constraint, $U(X_1X_2 \cdots X_N)$, which is often called *stimulus variability* by Garner, represents the maximum possible transmission from stimulus to response, or it may be viewed as the actual transmission to be made by an errorless ideal subject. So where "transmission" is legitimately regarded as a major dependent variable, as in discrimination experiments, the value of stimulus variability may play a central role. Consider, for example, two normal discrimination tasks, one with high stimulus variability and the other with low stimulus variability. If a high real transmission is obtained with the former task and a low one with the latter, this result might just be reflecting the difference in the maximum possible transmission in the two tasks and thus mean nothing empirically valuable. One should not be disappointed by this sort of weak conclusion drawn from task analysis. One great merit of having SUA task analysis is that it allows one to build an empirical theory or a model upon the logical basis of SUA—for example, a model that will predict actual transmission on the basis of the maximal transmission. Many information theoretical concepts, like channel capacity, noise, coding, etc., will be useful in constructing such a model. (One such has been published recently by Garner and Lee in *Perceptual and Motor Skills*, 1962, **15**, 367-388.) In this book, however, the author does not make clear his view on the relationship between ideal performance and actual performance. This is not very surprising since he does not even make explicit that in a considerable part of the book he is working on the ideal subject's ideal performance, and not on the S's actual performance.

Our next problem is the question about the effect of internal constraint upon discrimination transmission. For the sake of simplicity, we will assume that the given discrimination task is not only normal but also homogeneous, i. e., $U(X) = U_0(X)$ for every stimulus dimension. Then internal constraint is identical to redundancy. The answer to the above question, however, depends upon how we interpret this question. In (12), there are three terms, and two of them can be fixed independently. If we choose stimulus variability and redundancy as independent variables, obviously redundancy has no effect upon ideal transmission, since ideal transmission is simply identical to stimulus variability. But if we choose stimulus variability as the dependent variable in constructing a discrimination task, then (i) ideal transmission increases with total marginal uncertainty for a fixed redundancy, (ii) ideal transmission decreases with redundancy for fixed marginal uncertainties. (These correspond to the two conclusions given in the book on p.165.) One may notice that the second statement above is a version of the previously given warning, which will now be rephrased: As long as total constraint is held constant, the effect of redundancy upon ideal transmission is detrimental. (This is also true for tasks which are not normal.) In this form, the statement is certainly nontrivial, since it appears to contradict our common belief that redundancy should help discrimination. The truth is simple, however. Redundancy helps discrimination only under certain circumstances. But in order to introduce redundancy in a way that will help discrimination, one cannot, in general, keep total constraint constant. For the purpose of illustration, consider a discrimination task in which a random noise is present. Suppose that the stimulus is unidimensional and takes two values with equal frequency. The ideal transmission per trial is one bit if noiseless. Assuming that even the ideal subject suffers from noise, the presence of noise decreases the ideal transmission. This deleterious effect of noise can be substantially overcome, however, if the task is modified by making the stimulus multidimensional but keeping the stimulus values on different dimensions completely correlated. This modification does not change the ideal transmission in the noiseless case. However, it would increase the probability that the one-bit discrimination task is correctly executed, if noise operates independently upon different dimensions. This is a well-known effect; it is just one example of the use of redundancy to combat noise. But, obviously, total constraint, or the sum of marginal uncertainties, must increase to use redundancy this way. (In the Garner-Lee paper cited above, a more elaborate discussion is given on this point.)

So far, we have discussed the primarily detrimental effect of redundancy upon discrimination performance. Garner points out in the same context that the form of redundancy as well as its amount is important when we deal with the effect of redundancy. Because of the significance of this issue I would like to go a little farther in its formalization than he does. With an appropriate modification of (9), we can expand total internal constraint into a series of partial internal constraints (simple contingencies and interactions):

$$(13)\qquad U(X_1 : X_2 : \cdots : X_N) = \sum_{i<j} U(X_i : X_j) + \sum_{i<j<k} U(\overline{X_i X_j X_k}) + \cdots + U(\overline{X_1 X_2 \cdots X_N}).$$

For the sake of simplicity, let us assume that the task is homogeneous. Then the left-hand side of (13) is total redundancy within the stimulus system. The right-hand side of (13) then specifies how the total redundancy is allocated into partial redundancies. How the total amount of redundancy is allocated over component partial redundancies then defines the "form" of redundancy of the system.

The example brought up by the author to demonstrate the importance of the form of redundancy is as follows: Consider a discrimination task (normal and homogeneous) in which the ideal subject cannot attend to all of the dimensions of the displayed stimulus because of noise. Assume, for example, that he can attend to only two out of the total N dimensions and that the selection of the two is made at random from trial to trial. Then the average amount of transmission made by the ideal subject per trial, Av $U(Y : X_i X_j)$, is given by

$$\text{Av } U(Y: X_i X_j) = k \sum_{i<j} U(Y: X_i X_j),$$

where $k = 1/\binom{N}{2}$. Since the task is normal,

$$U(Y: X_i X_j) = U(X_i X_j) = U(X_i) + U(X_j) - U(X_i : X_j),$$

and therefore

$$\text{(14)} \qquad \text{Av } U(Y: X_i X_j) = (N - 1)k \sum_{i} U(X_i) - k \sum_{i<j} U(X_i : X_j).$$

It is obvious from (14) that the effect of introducing redundancy (corresponding to the second term on the right) holding total constraint (corresponding to the first term) constant is detrimental to transmission. However, it is also obvious from (14) that no partial redundancies other than simple contingencies have any effect at all upon transmission when S attends to only two of the total stimulus dimensions. In other words, there is even the possibility that the ideal transmission is increased by introducing redundancy holding total constraint constant if the increase in redundancy means an increase in interactions which more than makes up for a decrease in simple contingencies. This is possible only if it is known that the number of dimensions to which the ideal subject can attend is always two. The crucial point here, however, is not that increased redundancy may still help discrimination, but that redundancy can be used effectively only if the structure of noise is known. Of course, the effective use of redundancy necessitates some kind of matching between the structure of redundancy and the structure of noise.

Besides discrimination tasks, paired-associate tasks and free-recall tasks are touched upon in Chapter 5. It is true, as the author argues, that there is not much formal difference in SUA forms between discrimination and paired-associate tasks. But they are certainly different in their psychological implications. Accordingly, an SUA treatment different from that used in the analysis of discrimination tasks (in which the primary problem is noise) is called for to deal with paired-associate tasks (where the primary concern is how *S* learns the given external constraint). Since the author does not go very far in this problem, I will also refrain from going into any serious discussion on this topic except to make one suggestion. With a discrimination task, *S* normally knows the external structure, but he cannot produce that much transmission because of the presence of noise. (The term "noise" is used here in a very loose way.) On the other hand, with a paired-associate task, *S* cannot produce transmission identical to the amount of external constraint simply because he does not know the form of external constraint. So the primary concern in the paired-associate task is how *S* learns external structure, and, in this vein, I would like to have the reader recall one of my earlier comments: Simple contingency (external constraint) is a measure of structure which has a form of hypothetical uncertainty minus the exact uncertainty of the system. So if one takes *S*'s real uncertainty at each moment as the hypothetical uncertainty, he will obtain a convenient measure of how much of the structure is left for *S* to learn. In doing this, however, one must expand the present system of SUA, and I here will be satisfied by pointing out that SUA has a potentiality of handling learning-type psychological problems.

The author gives the free-recall task a position in contrast to the other two kinds of tasks so far discussed. There are two assertions explicitly made: (i) In free-recall, what *S* is to learn is internal constraint; (ii) The effect of redundancy upon free-recall performance is detrimental. I have objections to both of these statements. First, there is a logical difficulty in defining the internal constraint of a free-recall task in which *S* is to learn a unique list—where formally no probabilities are involved. This is a difficulty which, in spirit, is very close to the difficulty in defining the redundancy of a unique visual pattern pointed out by the author in Chapter 6. My objection to the second statement comes from the nonlinearity of the relation between the task-difficulty of a free-recall problem and the redundancy (as the author defines it) within the list. The nonlinearity is demonstrated easily. To take an obvious example, there is substantially no task-difficulty in free-recall if the list is 100 per cent redundant, meaning that there is a single word in the list. On the other hand, as Garner points out, there is little task-difficulty in learning a list of no redundancy, since *S* can generate the whole list by combining every component with every other—if *S* knows the components.

One may wonder why Garner's second statement given above means to put the free-recall task in a position in contrast to the discrimination task. As a matter of fact, this comes from the same source which brought most of the confusions in Chapter 5. It seems to me that when he wrote Chapter 5 the author was under a strong conviction that redundancy must aid discrimination. This conviction is right; redundancy aids discrimination if it is so used. Garner, however, took an unfortunate strategy in proving it; he attempted the proof under the assumption of constant total constraint, where, as we have already seen, the effect of increased redundancy is primarily detrimental. Even though he arrived at this conclusion, it seems to me that it was with mixed feelings and he occasionally makes a statement in favor of his original conviction. At any rate, my greatest regret concerning Chapter 5 is that the author did not rearrange it. In Chapter 6 where he no longer sticks to the unreasonable assumption of constant total constraint, most of the confusions in Chapter 5 are substantially straightened out!

In the latter half of the book, beginning with Chapter 6, various problems in experimental psychology are analyzed and discussed in terms of the concepts and the system of SUA developed in the first half of the book. Although many interesting developments are made and valuable suggestions are given there, I shall not go very far into reviewing all the subjects discussed. This does not mean, however, that these chapters are relatively less important than Chapter 5. On the contrary, most substantial contributions of this book to contemporary psychology are found in these later chapters. But I can easily justify spending most of the space of this book review in discussing Chapter 5. First, Chapter 5 is the hardest part of the book to understand, and the later chapters are relatively easy to read once the reader survives it. Furthermore, as I see it, the greatest merit of this book lies not in the actual accomplishments presented, but rather in the future potentiality of the method demonstrated in it. So the reader should not be misled to the impression that the following brief comments exhaust the interesting topics discussed. What I shall cover will be about one-hundredth of the important topics touched upon by the author—which are so diverse, so inspiring and so disputable that the reader might possibly be motivated to write another book if he is really involved in the author's arguments.

In Chapter 6, the author discusses the problem of pattern perception, the problem which usually shapes itself in the discrimination task with a multi-dimensional stimulus structure. This is also the area in which most of the experimental studies on the effect of redundancy have been made. As the author points out, the experimental results so far obtained do not lend themselves to an easy, straightforward interpretation about the effect of redundancy. It is demonstrated, however, that many ambiguities and contradictions existing in the literature can be straightened out by the application of SUA task analysis, even though many problems are yet left open.

The experiments reported in this area are usually of the following design: Two pattern discrimination tasks are compared, one with high redundancy within the given set of patterns, and the other with little or no redundancy. In both tasks, the sizes of the sets of patterns are equal, and the tasks are normal. This then means that the stimulus variability, or ideal transmission $U(Y : X_1 \cdots X_N)$, is the same for both tasks. In this type of experimental design, what is held constant is external constraint, not total constraint, and accordingly the "redundant" task should have more total constraint than the "nonredundant" task. Now, what effect would this increased total constraint have upon actual transmission (not on ideal transmission, since ideal transmission is the same for both tasks)? It is in general hard to find any definite psychological meaning for total constraint, but if tasks are normal, as they are in this case, total constraint is just the sum of marginal uncertainties. And the sum of marginal uncertainties, as I have repeatedly suggested, somehow corresponds to *S*'s initial uncertainty about the given set of stimuli. Taking this interpretation for granted, we may say that there is more to learn in the structure of the "redundant" task than in that of the "nonredundant" task and therefore it is expected that the *S*'s performance will be poorer with the redundant task, at least at the beginning. On the other hand, if there is any noise—a factor that may hinder *S* from attending to the whole stimulus—*S* would be more protected from making an error with the "redundant" task, if he has already learned the redundancy. In general, the greater the redundancy the more there is to learn, since *S*'s original uncertainty should be greater with greater redundancy. But once he has learned the structure of redundancy completely, then greater redundancy means greater protection. This one thing is already enough to complicate the issue of the effect of redundancy. It is, however, further complicated by the fact that partial redundancies have differential effects upon discrimination performance when noise is present—and noise is always present if a discrimination experiment is to make any sense at all. For example, as may be seen in (14), interaction-type partial redundancies can have no effect upon discrimination if the noise prohibits *S* from attending to more than two stimulus dimensions. From this, one may derive a conclusion: If redundancy is to be introduced to combat noise, an effective form of redundancy is to have large simple contingencies, although the most effective form cannot be determined unless the structure of noise is given. This conclusion may seem at variance with Garner's statement that large simple contingencies are bad for discrimination. This statement is also true, but it is true only if the compared tasks share the same total constraint.

Now the question is: What if the two tasks share both total constraint and external constraint? The only possible answer is that it all depends on the nature of noise. By now it should be apparent that the effect of redundancy upon discrimination is a considerably complicated issue, and it is not at all surprising that no straightfoward effect of redundancy has been reported in the literature. The demonstration of the logically complicated structure of pattern discrimination is a good example of the power of SUA, but SUA can certainly do more once the specific structure of the task is given and the nature of the noise operating is specified—for which a special analysis of the types of errors *S* makes must be made. Then SUA can tell fairly precisely what type of performance one can expect as a result.

Under certain circumstances, the above considerations are still insufficient. Very often the investigators of pattern perception talk about the redundancy of a unique pattern. Garner points out that redundancy of a unique pattern can be defined only with a certain assumption about *S*'s subjective population of which the particular unique pattern is regarded as a sample. This is the problem concerning *S*'s original uncertainty I have been talking about, and generally speaking this issue of *S*'s original uncertainty is relevant not only to the case of the unique pattern but also to any given set of patterns. The author gives an interesting example, although in a different context: Consider a 3×3 matrix pattern in which each cell can be either black or white. Suppose that only those matrices with only two cells black are selected as the set of stimuli to be discriminated. This means that only 36 matrices are used out of 2^9 possible matrices, so that the selected set of stimuli is quite redundant. This value of redundancy is given, of course, under the presumption that the color of each cell is taken as a variable, or dimension, of the given stimulus. But what if *S* knows that there are always only two black cells and takes the positions of these two black cells as the relevant variables to describe the system? The answer is clear: There is no redundancy in the given set of stimuli. It would very probably be true that *S*'s original uncertainty about the given stimulus is about 9 bits unless *E* explicitly tells *S* that there are only two black cells all the time. But in this particular case, even if not told, *S* would quickly learn the constraint—the nature of redundancy—and then would switch from his original 9-variable conception of the system to the new 2-variable conception of the system. This is a process of recoding. I am not contending that the "learning of redundancy" always means a recoding process, but I think it is true that very often learning of redundancy occurs by means of recoding. The most important fact of all is that one cannot talk about redundancy unless one specifies the variable-system used by *S*, and that the crucial psychological problems involved here are what variable-systems are natural to *S* and how *S* modifies them through experience.

In Chapter 6 and Chapter 7 the redundancy of language and how the redundancy is utilized in human verbal behavior are discussed. Many aspects of this issue are discussed, but the principal one is the analysis of the form of language redundancy in SUA terms and the relevance of the form to the use of language redundancy. These chapters are rich in ingenious suggestions which will certainly arouse interest and controversy among psychologists in this field. I will not, however, go into any detail for these chapters.

In Chapter 8 perceptual-motor tasks are discussed. The author's major arguments are upon the advantage of response lag. The necessary materials for the argument are mostly provided by the considerations made in the preceding two chapters on language, where the advantage of bilateral prediction over unilateral prediction in dealing with a time series is discussed. Theoretically, the longer the response lag the more the amount of information available for the prediction. But in perceptual-motor tasks the utility of quick response is usually involved. Besides that, there is a problem of finite memory span, which is certainly one of the major sources of noise in human performance. There should be an optimal response lag in each perceptual-motor task, but the optimality depends upon many factors. The author also argues that the optimal amount of (relative) redundancy in the stimulus series is about 50 per cent. This would, by and large, be true, although the argument would have been made more persuasive if the author added to it some considerations on how the optimal lag is related not only to the amount but also to the form of redundancy of the given time series.

In Chapter 10 the concept-formation task is discussed. It is obvious that concept tasks are in many ways similar to paired-associate tasks. But there is one essential difference between these two: Paired-associate tasks are generally normal, but concept tasks cannot be normal by definition. The "concept" in concept-formation experiments is the common label attached by the task to more than one stimulus, but not all stimuli, in the stimulus set. Furthermore, it is customary in concept tasks for the stimulus dimensions to be well defined and to involve irrelevant dimensions, the values of which have nothing to do with the concepts, or correct responses. Strictly speaking, virtually any experiment whatsoever involves one or another irrelevant stimulus dimension, like accidental sounds in the experimental room, etc. Such an unintentional stimulus is so obviously irrelevant that *S* is always ready to disregard it. In concept learning, however, irrelevant dimensions are intentionally added to serve as a "conceptual noise" so to speak. Therefore, the learning of external structure in a concept task necessarily involves learning to disregard irrelevant dimensions.

In Chapter 10, as in the other chapters, the author's major concern is to investigate the possible effects of redundancy upon performance. On the basis of a few relevant experiments he makes some very interesting suggestions. First, the search for structure is inherent in behavior. To this point, few psychologists would object, except some die-hard *S-R*ians. Secondly, structures in the form of simple contingencies or lower-order interactions are easier to notice and learn than higher-order interactions. This also sounds reasonable. From these plausible assumptions, the author makes predictions: If redundancy exists within relevant dimensions and not in irrelevant dimensions, this structure within relevant dimensions will help *S*'s quick disregard of irrelevant dimensions. If the reverse is true, *S*'s attention will tend to be attracted to irrelevant dimensions, and the process of concept formation will be slowed down. Such effects of redundancy will be more pronounced if the redundancy is more heavily loaded on simple contingencies. Although experiments more elaborately designed than those presently available are needed to give full credit to this hypothesis, the hypothesis itself is already giving a good credit to the author's original claim that SUA helps to raise psychologically meaningful questions.

There is one minor logical error in this chapter. On page 322 is a statement "[Holding external constraint constant] . . . the stimulus equivocation ($U_Y(X)$) is decreased by exactly the same amount that the internal constraint is increased." This statement is right, but the proof given for this statement on the next page is meaningless. The correct proof is as follows:

$$\text{Internal Constraint} = U(X_1 : \cdots : X_N) = \sum_i U(X_i) - U(X_1 \cdots X_N),$$

$$\begin{aligned}\text{External Constraint} = U(Y: X_1 \cdots X_N) &= U(Y) + U(X_1 \cdots X_N) \\ &\quad - U(YX_1 \cdots X_N) \\ &= U(X_1 \cdots X_N) - U_Y(X_1 \cdots X_N).\end{aligned}$$

Assuming that $\sum U(X_i)$ is held constant, stimulus variability $U(X_1 \cdots X_N)$ is decreased by exactly the same amount that internal constraint is increased. But, since external constraint is held constant, stimulus equivocation $U_Y(X_1 \cdots X_N)$ is decreased by exactly the same amount that stimulus variability is decreased. QED.

In the last chapter, the author recapitulates the major assertions he made. I should like to make a last comment here also, which is in agreement with one of the author's last suggestions. I think that, even though task analysis alone will do a good job in clarifying the nature of psychological problems, still better understanding will be attained by applying SUA, or its more developed version, to the structure of the person given the task and to the structure of the interaction between the person and the task. I have repeatedly pointed out the importance of explicitly specifying the structure of noise, and the structure of noise would generally be a part of either the structure of the person or the structure of person-task interaction, or both. Still greater use of SUA will be achieved by modifying it so as to be applicable to noninformation measures, a possibility mentioned earlier, since the concept of information is, though important, not sufficient to yield a complete theory of behavior.

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ELIZABETH DUFFY. *Activation and Behavior.* New York: John Wiley & Sons, 1962. Pp. 384 + xvii.

Since the early 1930's Elizabeth Duffy has been a leading proponent of the view that the level of "arousal" or "activation" plays an important role in explaining behavior. Too often her theoretical and experimental contributions in this field have been ignored, and if nothing else, this book should provide the recognition due her. The most important contribution of the book lies in the organization of over 800 references in the area of activation so that they will be more readily usable. The work of compilation and organization is a valuable contribution and should not be minimized. It could only have been done by a person with Prof. Duffy's scholarship and experience.

The central issue of the book, and an important issue for all interested in behavior, is "how much does a concept of global activation help in understanding behavior?" Duffy believes that it is necessary for understanding behavior, and this view is shared by a good many psychologists. Nonetheless, there is empirical room for those who might doubt the importance of "general activation" in psychological theory, since there is little direct objective support for the concept. Whether or not terms used to represent general arousal will be part of the armamentarium of future behavioral scientists undoubtedly depends upon the existence of reliable and valid measurements of the activation of an individual. Without satisfactory measurements the concept of arousal is doomed to oblivion. The many, many attempts at measurement of a person's degree of activation can be inferred from the extensive bibliography. For Duffy, activation is defined as "the extent of release of potential energy, stored in the tissues of the organism, as this is shown in activity or response." But she argues that activation is not the same as the amount of observable activity although there must be some relation between the two. A person may be inhibiting overt responses and yet releasing a considerable amount of energy. A cat waiting by a mouse hole would typify this circumstance. Because overt behavioral activity is not accepted as a measure of activation, it must be inferred from other criteria such as measures of peripheral and internal reactions.

For the most part psychologists have used physiological measures as indices of arousal or activation. The galvanic skin response (GSR), the electroencephalogram (EEG), and the electrical activity in muscles have been very popular measures. However, others have been used: heart rate, blood pressure, respiration, and oxygen consumption. Each presumably reflects something about the arousal status of the individual. However, the general lack of correlation found between indicators of global activation presents a conceptual problem. Typically, the correlations between physiological variables, each of which is purported to be an index of activation, are low and often not significant. Furthermore, test-retest correlations are not sizable enough to be encouraging nor is there much consistency in the responses of individuals exposed to similar situations. Given these kinds of results, advocates of activation theories must strive to find rationales to justify the use of a general activation concept.

Duffy suggests that activation might be likened to the economic strength of a community. Any one index of the economy might be only remotely related to any other, yet in its own way each would reflect the business and industrial growth of the area. Duffy points out that the analogy cannot be pushed too far. However, even in the example one might question the value of a description of the "economic condition" of a community when the indices were found to be practically unrelated. Wouldn't this indicate a rather specialized type of economic development which should be further investigated?

It may be that the pooling of many measures of activation, as suggested by Duffy, will provide the best index, but the reader will be disappointed to find that no indication is given as to how to combine the several measures. Maybe one pooled measurement is better than the individual measures taken separately. In fact, it might be possible to find beta weights in regression equations using the scores of many variables which will allow the equation to predict the results expected on the basis of activation *theory*. Perhaps someone will try it, but it has not been done. The odds are rather long against finding beta weights which would be effective across many situations and populations.

Theory has far outdistanced fact in work with arousal. Today, concepts like arousal have achieved great popularity. The enormous attention given to generalized physiological effects of stimulation of the brain stem reticular formation has been a godsend to activation theorists. However, it should be noted that the generalized effects attributed to brain stem mechanisms are becoming fewer with increased numbers of experimental studies analyzing the brain stem functions.

The book itself is well organized and should provide a boon to the specialist who deals with one or another area of arousal. Yet, the careful organization results in a certain dullness in the text because the same studies are discussed in one chapter and then in another ... and then in another. In total perspective, however, there is no doubt that Duffy has provided a true compendium of research on activation which will be of great value to the profession.

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QUINN MCNEMAR. *Psychological Statistics.* (3rd ed.) New York: John Wiley and Sons, Inc., 1962. Pp. viii + 451. $7.75

The third edition of this classic in applied statistical methods remains one of the best books of its kind in the field. A major change from the second edition is the adoption of upper case S, rather than σ, to designate the "ordinary" standard deviation, i.e., the square root of the arithmetic mean of squared sample deviations. Additions include a chapter on trend analysis; a discussion of empirical work on the effects of assumption violations on the t and F tests; a proof that s^2 is an unbiased estimator of σ^2; an algebraic determination of the expected values of variance estimates in one-way analysis of variance; and some additional nonparametric techniques.

While the use of S rather than σ may be helpful in a discussion of descriptive methods, it would have been better to discard it at the end of chapter 3 and avoid the confusion which it causes in chapters on inference. There are sounder reasons for using an unbiased estimate than the fact that $N < 30$, and one has the feeling that the author himself wishes that he had abandoned S long before Chapter 14 (Inferences About Variabilities). The $N < 30$ situation seems also to call forth the use of Student's distribution to replace the normal as an appropriate model for testing the difference between two means (Chapter 7). While it is explained later in the discussion that it is the use of s in place of σ that makes t the appropriate test statistic, the introductory statements, as well as the chapter title (Small Sample or t Technique), have already made their impression.

This book remains a mathematically sound, reasonably comprehensive how-to-do-it volume and will continue to be of value as a reference for applied work in the field. It no longer seems appropriate, however, as a basic teaching text for use by psychologists. There are signs that students in the social sciences now are not so fearful of mathematics as those of ten years ago. They should be provided with a proper basis in probability theory and a sounder theoretical foundation in statistics. Given such a background, students should be able to carry out their analyses successfully even when no published "recipe" fits their problem. Perhaps the long-term result would be an improvement in the quality of research in the behavioral sciences.

Teachers College, Columbia University RICHARD H. LINDEMAN

K. J. ARROW, SAMUEL KARLIN, AND PATRICK SUPPES. *Mathematical Methods in the Social Sciences, 1959.* Stanford, California: Stanford Univ. Press, 1960. Pp. 365 + vii. $8.50

Mathematical psychology being what it is these days, only a naive or inebriated reviewer would attempt to pass judgment on every article in a book reflecting much of the current work in the field. Those still attempting "critical experiments" to test, say "stochastic learning theory"—and one continues to hear of such experiments—need to be put face to face with the ordeal of reading in detail each of the chapters in this book. The illusion of a single monolithic theory capable of being rejected (or accepted!) would quickly dissolve. I can only try to subtly by-pass my specific assignment by confining my remarks to general statements on the trends, healthy or otherwise, suggested by these papers.

The twenty-three papers in this volume were presented at a symposium on mathematical methods in the social sciences in the summer of 1959. Nine of the papers have been grouped together under the heading Economics, four under Management Science, and the remaining ten under Psychology. The ten psychological papers, with which this review is solely concerned, include five papers on learning by Atkinson, Burke, Bush, Estes, and Suppes; four papers on measurement (or, to use a more contemporary term, choice) by T. W. Anderson, Marschak, Luce, and H. Solomon; and a paper by Galanter and Miller which attempts to show the close relationship between reflex theory and current stochastic theories in learning, psychophysics, and communication.

The degree to which mathematical theorists extend or generalize previous work is a good indication of the health of the field. It is both aesthetically and pragmatically unappealing to contemplate a continual increase in the current pool of unconnected, fragmented models. Such increases are not mitigated by the fact that each new model undoubtedly contains an element of truth for some highly artificial situation. One notes then with some pleasure Anderson's conceptualization of Guttman's recent multivariate work in terms of familiar stochastic processes, Atkinson's simultaneous treatment of both the component and pattern models of discrimination, Burke's extension of the linear operator learning model to two person interactions, Bush's elaboration of Luce's beta (learning) model, Estes' extension of Bower's random walk theory of choice, Luce's extension of his choice theory to response latencies, and Suppes' extension of the finite, discrete stimulus sampling learning theories to a response continuum.

In one sense, Marschak's analysis of binary choices (pair comparisons) is also a generalization of previous theories, in this case psychological scaling theories. Marschak's work, however, is not easily described since it has little precedent in the psychological literature, although it is not uncommon in the economic literature. Measurement or scaling theorists have generally been more concerned with producing marketable scales or tests than with verifying theories. The initial interest, for example, in Luce's choice theory stemmed from the (erroneous) belief that better scales could be produced for less, not because it was shown that the theory was true. One aspect of the problem posed by Marschak concerns the theoretical implications of various observable or testable constraints on binary choice probabilities. The constraints considered include, for example, several probabilistic versions of transitivity. The theories are for the most part extremely weak statements relating scale values (or utilities) to choice probabilities. Of particular interest are the conditions that discriminate between well-known theories (e.g., Luce, Thurstone) and those that do not. Some knowledge of these results and the method of analysis would help clarify the current confusion between the ratios and differences of scale values and between power and logarithmic functions.

The inclusion of Marschak's paper in the psychology section rather than in the economic section of this volume undoubtedly reflects some wishful thinking on the part of the editors. There appears to be no immediate danger of measurement theorists changing either their concern for reliability and validity or their veneration of scale values. For this reason one looks with hope at another trend exhibited by these papers: the gradual infiltration of learning theorists into the measurement field. It is conceivable that the learning theorists' concern for understanding behavior and their zeal for testing in detail the implications of their theories will influence the measurement field. The papers by Atkinson and Estes are two good examples of this infiltration. Included in Atkinson's theory of discrimination learning are parameters of stimulus similarity, and if the theory is correct these parameters can be used to define stimulus scale values. Questions of estimation of the parameters and goodness of fit of a variety of predictions can be answered (in principle) within the context of the theory. Estes' conceptualization of the choice process in terms of a random walk leads to, among other things, certain constraints on the choice probabilities, and for the binary case the constraints imply the existence of a Luce-type scale. In both papers, however, the existence of numbers associated with the stimuli is of less importance than understanding the behavior in question.

Fortunately the careful development of the measurement area need not depend entirely on the by-products of learning theorists. There is still Luce's constant exploration of the measurement area from within. In the present volume Luce's probing of the implications of his choice axiom leads him to a more elementary breakdown of the choice process, somewhat different from the one developed by Estes. The result, for Luce, is a logarithmic relationship between latency and choice probability. It can be expected that Luce will carry over these ideas into the learning field.

As a final observation, it is interesting to compare the points of view of two papers juxtaposed in this volume. In the first paper Estes indicates his uneasiness with theories that have the organism maximizing utility by essentially postulating (i.e., putting into the head of the organism) a maximizing faculty or propensity. His aim is to replace such complicated mechanisms by more elementary processes. In the following paper, Galanter and Miller's objection to the simple-minded view of the cortex implied in current theorizing leads them to develop a theory that puts more complexity into the head of the organism. The question of how much to put into or take out of the organism's brain is, of course, an old question and one that will continue to be settled more by the actual accomplishments of competing theories and less by the interesting argumentations of competing theorists.

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