Psychological Review
Vol. 62, No. 3, 1955

SYMPOSIUM ON THE PROBABILITY APPROACH IN PSYCHOLOGY *

REPRESENTATIVE DESIGN AND PROBABILISTIC THEORY IN A FUNCTIONAL PSYCHOLOGY [1]

EGON BRUNSWIK
University of California

The movement in psychology most directly concerned with the challenge of unification with the exact sciences is behaviorism. At the present time, two major factions are discerned within behaviorism. One, represented chiefly by Hull (26), tries to elaborate classical behaviorism into a tightly woven, formalized system of strict laws about intraorganismic processes in the nomothetic tradition of physics. The other, inaugurated chiefly by Tolman and his program of molar or "purposive" behaviorism (47), attempts to deal with behavior "relative to some end" and thus to restructure behaviorism in the tradition of the utilitarianism of Darwin and of early American functionalism.

It may be argued that nomothetic behaviorism overexpands physicalism beyond the necessary observational and procedural core and includes unessential borrowings from the specific thema of physics. A functionally oriented objective psychology, on the other hand, dealing as it does with organism-environment relationships at the more complex level of adjustment, may be seen as falling in line with a more searching interpretation of the historical mission of psychology. The present paper concentrates on summarizing and expanding earlier contentions (2, 7, 8, 48) to the effect that the environment to which the organism must adjust presents itself as semierratic and that therefore all functional psychology is inherently probabilistic, demands a "representative" research design of its own, and leads to a special type of high-complexity, descriptive theory.[2] This program provides not only the necessary thematic diversification from the classical

* The series of papers by Egon Brunswik and by Leo Postman, and of the following discussion papers by Ernest R. Hilgard, David Krech, Herbert Feigl, and Egon Brunswik, are adapted from the first part of a symposium held, under the same title and under the chairmanship of Edward C. Tolman, at the *Berkeley Conference for the Unity of Science,* University of California, July 1953.

A contribution by Kenneth R. Hammond to a second, more practically oriented part of the original symposium will appear in the next issue of this journal (20).

The Institute for the Unity of Science in Boston, which sponsored the conference, has contributed approximately one-half the publication cost of these papers.

[1] The present paper has been considerably expanded beyond the original exposition read at the symposium of which it was part. However, care was taken not to alter the substance of the argument on which the subsequent paper by Postman and the ensuing discussion are based.

[2] The expansions beyond the earlier publications listed concern mainly the use of a behavioral example at the beginning of the paper; the brief consideration of such semi-representative policies as "canvassing"; certain comparisons with factorial design and the analysis of variance, as well as with non-functionalistic uses of probability in psychology; and a discussion of actual and potential applications to the clinical-social area and to related domains.

natural sciences but also leads to the long overdue internal unification of psychology.

We will develop our arguments first with the use of an example involving behavior as a "constant function" of a characteristic end state, and then in reference to the functionalism of the perceptual constancies where progress along methodological lines is somewhat further advanced.

SYSTEMATIC DESIGNS AND THE STUDY OF DISTAL ACHIEVEMENT

Behavior as constant function. One of the earlier functional behaviorists, Holt, suggested that the movements of an individual be defined in terms of "that object, situation, process . . . of which his behavior is a constant function. . . . So in behavior, the flock of birds is not with any accuracy, flying over the green field; it is, more essentially, flying southward" (24, pp. 161–166). This statement, rather paradigmatic of functionalist modes of thought, involves selective description. The preferred hypothesis contains reference to a remote end (south); or, in the words of Heider (21), it sets "distal determination" over "proximal determination," that is, in our case, over description in terms of momentary position (green field).

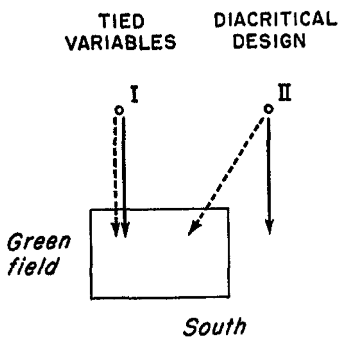

FIG. 1. Systematic designs in the study of behavior constancy (applied to an example from Holt, 24).

In defending his seemingly teleological attitude Holt points to the emphasis some positivist physicists—we may think primarily of Mach—place on the relatively descriptive study of "functional relationships"; these may connect events over space or time regardless of the traditional tracing of causal chains in near-action.

Tied variables. Functionally irrelevant generalizations. Holt's problem of constant function involves generalization. One of the most time-honored traditions in experimentally testing generalizability demands that one, or perhaps a few, conditions be varied in a planful manner decided upon by the experimenter while all others are held constant. The purpose is to assure isolation of the so-called independent variables. For their arbitrary orderliness and confinement such designs may be called "systematic."

For example, we may move the birds backward along the line of their flight, say, to position I in Fig. 1. The birds may persist in their original direction and in this sense show generality of behavior. But it is easily seen that this experiment is irrelevant to Holt's chosen alternative. The two directions, "southward" (solid arrow) and "over the green field" (broken arrow) coincide; allowing for all possible types of response, the two variables involved—south vs. non-south, and green-field vs. non-green-field—are perfectly correlated and thus inseparable so far as the available evidence is concerned. This constitutes artificially induced perfect confounding, and may be labeled "tied-variables" design or, in short, tied design.

Responder replication, that is, repetition with new individuals or with the original individuals at other occasions, is likewise irrelevant to Holt's alternative, regardless of the interindividual or intraindividual consistencies that may be observed. This must be pointed out in view of a rather deeply ingrained trend in psychology of throwing the entire problem of generalization and of statistical significance onto the responder rather than onto the situation (7, pp. 36 ff.; further drastic evidence to this effect, unearthed by Hammond [18, 19], will be referred to in the section on clinical application).

Diacritical confrontation. Splitting of tied clusters vs. isolation of variables. The real testing of Holt's preferred south hypothesis does not even begin until adherence of behavior to the southward direction is made situationally incompatible with adherence to the green field. This could be achieved, say, by moving the birds sideways to position II in Fig. 1. The two arrows issuing from the new starting point are now divergent rather than parallel. The responder is placed at the crossroads and forced to take sides. The previously tied situational factors are now "confronted" (2); we may speak of this variant of systematic design as "diacritical" design.

We soon discover that southwardness is still tied to such factors as the general area of start, temperature and other climatic conditions, topographical landmarks, magnetic cues, and so forth; and so is the greenness of the field to its squareness or size. What we have accomplished in diacritical design is to separate or "split" one original encompassing cluster into two subclusters of tied variables; but we have not really "isolated" our variable as it may have seemed at first glance, and therefore are not yet entitled to speak of its attainment as a constant function.

Perception as constant function. A concrete perceptual problem comparable to Holt's problem of the constancy of southward flight is that of size constancy. The alternative here is between invariance of the response relative to the measured (or computed) sizes of the stimulus impact at the retina (or at a parallel photographic screen) as the proximal variable, which we will call *P*, and invariance relative to the measured sizes of physical bodies underlying this impact as the distal variable, *B*. Size constancy involves the predominant focusing of the response on *B*.

Classical psychophysics as pseudo-univariate design. With distances variant as they are in daily life, there is a certain degree of statistical independence between *B* and *P* despite the existing causal nexus between them. During the classical phase of psychophysics, however—still strongly in evidence today—the implicit design policy was artificially to tie the distal and proximal variables. For size this is achieved by holding the distance from the observer constant. A good example is the Galton bar. The task is molded closely after ordinary physical length measurement except that the lines are laid up lengthwise rather than being superimposed. This creates a tied-variables design which is comparable to case I in Fig. 1. Note that the tying of the two variables is the direct result of a celebrated device of systematic design, the holding constant of a third variable (in our case, distance).

This design may also be cast into the form of a table of presence, or scattergram. Figure 2 presents the major systematic designs in their minimal form, assuming only two levels of strength for each of the situational variables. In the case of the classical tied-variable design the two variables are perfectly correlated. All entries lie along a diagonal. Photographic size (assumed to be plotted vertically as in Fig. 3) is always large when bodily size (plotted horizontally) is large, and vice versa. And judgments correct (or incorrect) concerning one of these variables are

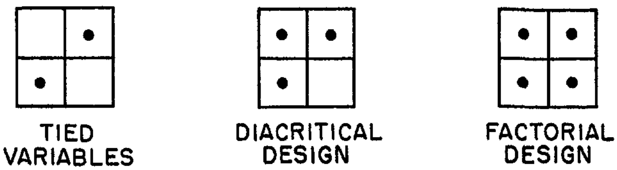

FIG. 2. Minimal scattergrams of systematic designs. Repeatable in k dimensions.

automatically correct (or incorrect) concerning the other, just as Holt's birds, by flying south from position I, automatically must fly over the green field. The classical design therefore precludes decision as to whether the response focuses on proximal (photographic) or distal (bodily) size; nor does it allow the conclusion that the response focuses on either, for that matter.

In the classical phase, the tying of variables was done inadvertently and in considerable naiveté as to the interpretational consequences involved. Since only the bar was directly manipulated and only one scale was read in doing so, the impression prevailed that there was only one independent variable. The notion of univariate design was somewhat obliquely reinforced by the tendency to confine the concept of stimulus to the proximal stimulus variable that had been arranged to vary in unison with the bar. The possibility of varying degrees of dependent variability within the design itself was thus ignored and no provision was made to state the relationships explicitly, so much so that the term "dependent variable" could become synonymous with the organism's eventual responses. Since classical design purports to be univariate yet fails to isolate the distal from the proximal variable, it may be called "pseudo-univariate" design, and in our particular example may be specified as proximo-distally neutral design.

Thing constancy research as a form of multidimensional psychophysics. Factorial design. The diacritical confrontation of distal and proximal size is achieved by removing one side of the bar to a different distance from the observer, say, a smaller one. This has an effect on design analogous to the moving of the birds from position I to position II in Fig. 1. It is now incompatible for the two lengths to be equal bodily and photographically at the same time. The new combination of a relatively small bodily size with a relatively large photographic size injected into the design is plotted in the upper left corner of the center chart in Fig. 2. The forking or parting of the ways which is characteristic of diacritical confrontation may be visualized by assuming the near object to be the Standard and the far one the Comparison, the latter being left to vary along the diagonal.

By projecting the Standard two ways, along a horizontal and along a vertical line, to their respective intersections with the diagonal representing the Comparison series, we obtain two points of objective equality (or POE, formed in analogy to Woodworth's PSE for point of subjective equality [see **7**, **53**]). One represents the variable P and the other B in the experiment. By contrast, in a classical experiment the Standard coincides with a point on the diagonal and thus the two POE's merge in one point, further supporting the erroneous impression of univariate design of which we have spoken above.

The relative allegiance of the response to B or P, or the degree of perceptual "compromise," may be ascertained by inserting added values between (or beyond) the two ideal "poles of intention" (2) along the diagonal (7, p. 17, Fig. 4). The constancy ratio (called Brunswik-ratio by Woodworth) is a simple device to project the obtained PSE's onto the span between the two pole POE's.

Note that the Comparison in psychophysical experiments has often been called *the* "Variable," so that not only classical psychophysics but even our present diacritical experiment may give the superficial impression of unidimensionality, in spite of the fact that the presence of two POE's clearly marks it as a case of multidimensional psychophysics. In this sense we may say that while classical design is pseudo-unidimensional, diacritical design is crypto-multidimensional.

By adding a fourth point, diacritical design becomes the well-known factorial design (14), also shown in Fig. 2. In essence, this is no more than adding a mirror image to diacritical design, with added advantages accruing by virtue of the increased symmetry. Some of the Vienna constancy experiments have employed such a double diacritical design (2, pp. 167 ff.) but they will not be discussed here.

Variate packages and the indefinite regress of systematic design. Diacritical confrontation or factorial design may be carried to several dimensions. Their number is, or at least should be, known to the experimenter; it may be designated by k.

In analogy to what we have said about Holt's example of bird migration, all results remain contingent upon the ties existing within the more or less incidental situational instance from which systematic variation has taken its start. If we use the term "variate" for the specific values along the various variable dimensions, each concrete situation may be regarded as a "variate package." Originally, each of the particulars or variates in the package has equal claim for being singled out in the description, and one or a few factorial separations remove but little of this indeterminacy. Complete "systematic" isolation of one variable as the crucial factor would involve diacritical confrontation with a very large, and in fact indefinite, number of originally tied situational variables.

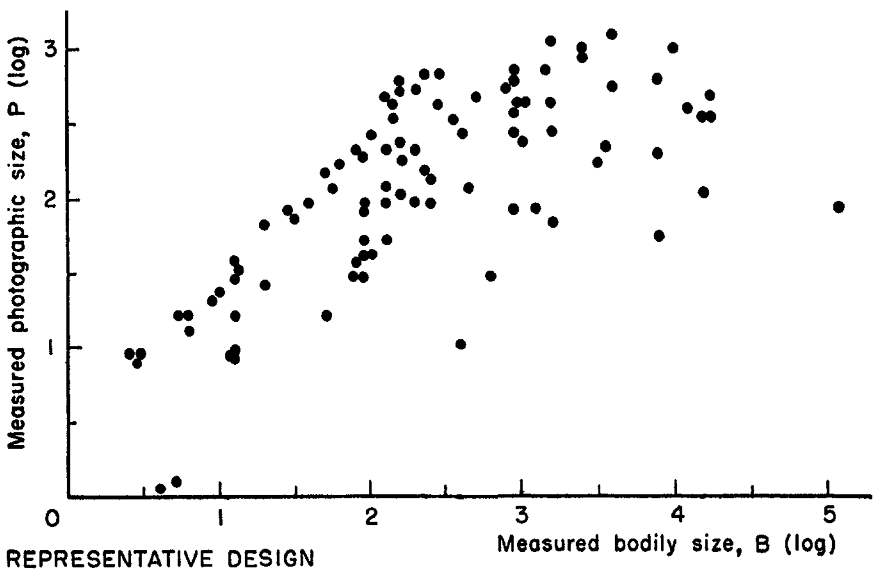

FIG. 3. Scattergram of an example of representative design used in the study of size constancy (adapted from Brunswik, 6, 7). Analogously in x dimensions.

REPRESENTATIVE DESIGN IN THE STUDY OF DISTAL ACHIEVEMENT

From a purely formal point of view the systematic confrontations would have to include the indirect cues for size and distance on which the mechanism of constancy must depend. But it is evident that setting the distal variable against its own instrumentalities would cut out the ground from under the very function whose constancy is to be put to test. Tolman (47) has stressed that all behavior requires the presence of means or "behavior supports." He has also pointed out that the only admissible operational criterion for the testing of "purpose" is the observation of the actual reaching of the end, at least in part of the behavior instances in the class under consideration. But reaching the south or any other distal goal, be it behavioral or perceptual, can obviously become a more or less stabilized function only if the flight of the birds is allowed to take adequate advantage of the natural resources of orientation and locomotion, much as such man-made stabilizers as gun sights must be tested under conditions of practical use.

Ecology and situational instance. Constant psychological function thus is intrinsically limited, or probabilistic, rather than "universal." Flying southward, being right about object sizes, or any other gross or "molar" behavioral or perceptual function can never attain the status of an ironclad and universally applicable so-called "strict" law in the sense in which these laws were idolized in the classical phase of the natural sciences. The basic aim of our initial quotation from Holt requires delimitation of a more specified universe within which the animal is set to operate. This is in line with the "syntactic requiredness" (22) to define all probability in terms of a corresponding reference class or universe. In line with biological usage we will call this universe "ecology." An ecology is defined as the natural-cultural habitat of an individual or group, but is otherwise free of contamination by the system of specific responses. Rather, the ecology is the objective, external potential offered to the organism for survival and its subordinate needs. Nourishment value of foods, as it exists prior to and regardless of its recognition or consumption by the responder, is an example of an ecological variable or set of variables; object size and its system of cues enters via its relevance for manipulation or orientation.

Since the responder merely acts like a catalyst in the definition of the ecology, the ecological environment is not to be confused with Lewin's psychological environment or "life space" which is defined as the reflection of a situation within the response system (see 5).

Situational instances in an ecology are analogous to individuals in a population of responders. Both may be considered as sets of more or less incidental variate packages. The difference is that instances can be taken apart and created at the spur of the moment while individuals usually cannot. But, as we have come to see, a program of functional research demands that they too be left as they come. We must resist the temptation of the systematic experimentalist to interfere, and must introduce a laissez-faire policy for the ecology.

Representative sampling of situations. As we cannot possibly hope to encompass the entire population of individuals in research, but must sample representatively, we must sample instances in the study of functional achievement. Taking the cue from differential psychology, we may transfer the entire formal statistical instrumentarium developed in the study of personality to functional problems as a new content. This will assure, to any desired degree *of approximation*, a balanced view of psychological function as it comes about by a synopsis of performance under comfortable conditions, manageable vicissitudes, and a due proportion of risks or well-nigh insurmountable odds.

In terms of experimental design there results a combination of constraint and license in which the experimenter is in no more than supervisory control over the adequacy of sampling. There will be a limited range and a characteristic distribution of conditions and condition combinations. If in this manner psychological experiments take on the character of statistical surveys, we may speak of "representative design" (7).

A representative design in perceptual size constancy. Since in representative design the accent is on sampling from an ecology and on the generalizability of functional constancies to this ecology, rather than on sampling and generalizability in reference to a responder population, it was deemed advantageous for demonstration purposes to confine a pilot survey of perceptual size constancy to a single subject. Using n for the number of responders and N for the size of the situation sample we thus had $n = 1$ subject, a graduate student in psychology; there were $N = 93$ object situations (6; 7, pp. 41 ff.). The objects were sampled, in a reasonably random manner, from the sizes that became "figure" to the subject in her daily routine, in package with their natural setting and accompaniment of depth cues.

Ecological validity and dependent variability within the design. Textural ecology. As in all representative design, the design in itself has the character of a result, even though this result concerns the ecology only and is no more than the precondition of psychological investigation proper. The design obtained for our size constancy survey is shown in Fig. 3. The two major distal and proximal stimulus variables are plotted logarithmically, B in terms of millimeters, and P in terms of millimeters at an assumed projection distance of 1 meter.

The manner of covariation, defining what we have called dependent variability within the design, may in first approximation be expressed by a correlation coefficient. In our case the Pearson r between the logarithms of B and P is .70. This coefficient estimates the cue potential of the proximal variable P relative to the distal variable B in the given ecology, in analogy to the way the validity of a test relative to a personality trait may be ascertained for a population of responders. A correlation between ecological variables, one of which is capable of standing in this manner as a probability cue for the other, may thus be labeled "ecological validity." The study of ecological validities, being bivariate correlational, defines what we may call a structural or textural ecology, in contradistinction to the emphasis on unidimensional distribution (of temperature, precipitation, size of population centers, etc.) which is more typical of biological and a part of cultural ecology.

Our particular coefficient indicates that large retinal impacts are somewhat more likely to be caused by relatively large objects, regardless of distance. Considerable as this relationship may seem, it is, as we shall see in a moment, trifling in comparison with the final achievement of the constancy mechanism. This gain becomes possible only by an additional utilization of distance cues. For some of the less valid of the commonly listed perceptual depth criteria, such as vertical position, subdivision of space, and brightness, Seidner has found moderate ecological validities ranging to about .4 (report on preliminary data in 7, pp. 47 ff.). It is easily seen that not even the so-called primary depth cues, such as binocular disparity, are foolproof in our ecology. For example, binocular disparity is present in the stereoscope, yet depth is absent in the underlying reality; in viewing reality through a camera, on the other hand, binocular disparity is absent while depth is present in the chain of causal ancestry.

In our present example the analysis was not carried to an explicit treatment of depth criteria and other context factors. With the use of photographs, such as those Seidner had available for his analysis of depth cues, a great variety of them could be analyzed in a single enterprise. In fact, Fig. 3 must be seen as combining but two out of a practically unknown number, x, variables. Since covariation must be allowed to take its natural course, the different juxtapositions would of course not look exactly alike, as they do in factorial design, but would merely be analogous, forming in the end an x-dimensional space in which all factors could be considered simultaneously.

Quite aside from the avoidance of the pitfalls of systematic design—in which all factors held constant are lost for the investigation, and the resultant tied-variable clusters only lead to confusion —representative design, while cumbersome and laborious, is thus potentially a very economical technique.

Projections of the frequencies in Fig. 3 upon the main axes are shown in Fig. 4. Our sample, restricted to sizes not tilted into the third dimension and thus by-passing the problem of shape constancy, is represented by the solid curves. It is reassuring to find the distribution for B fairly normal. (The third graph shows the distribution for distance as related to a dependent distance axis that may be imagined to run across the upper right-hand part of Fig. 3 under 45°, forming a triangle with the main axes; the crowding of both small and intermediate objects along the

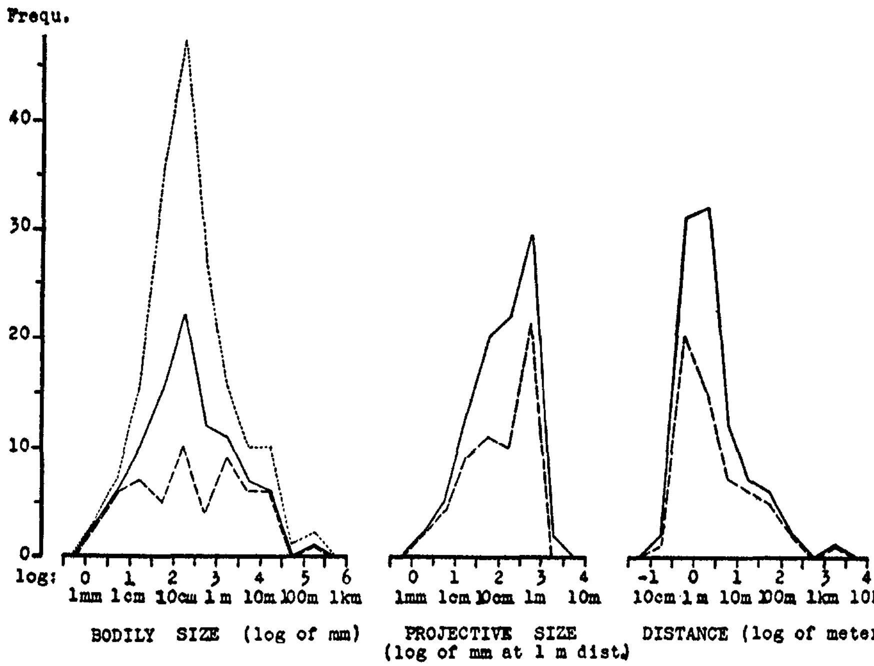

FIG. 4. Frequency distributions of three ecological variables for the representative design shown in Fig. 3 (solid curves; the dotted curve includes tilted objects and the broken curves refer to a subsample of vertical objects, both not discussed here). (From Brunswik, 6.)

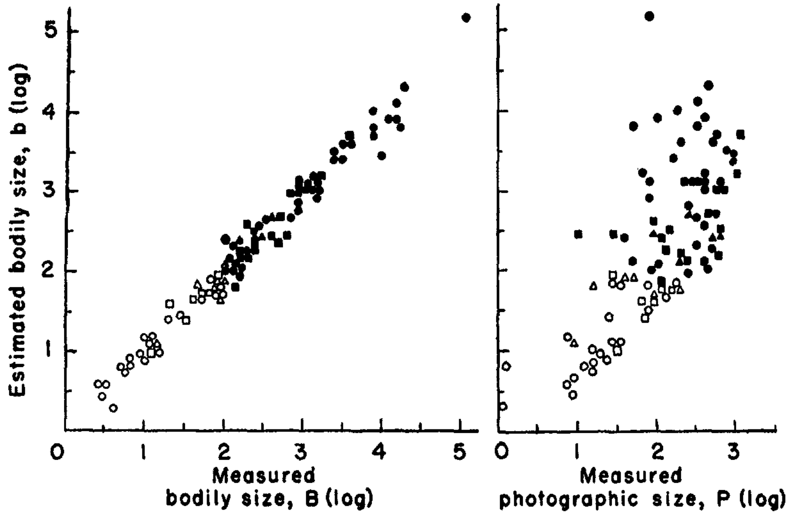

FIG. 5. Scattergrams of perceptual achievement (functional validity) for a subject responding to the representative design shown in Fig. 3. (Open symbols are used here to designate objects smaller than 10 cm., see below; the differences in shape are irrelevant in the present context.) (Adapted from Brunswik, 6, 7.)

10-inch limit of near vision leads to skewed distribution and is the chief source of the curvilinearity that may be observed in Fig. 3.)

Functional validity (*achievement*). We now turn to covariation between the distal stimulus and the response variable which will define perception in its approximation to "constant function" as understood by Holt. Figure 5, left, shows the stimulus-response scattergram for the logarithms of the perceptual estimates, *b* (so labeled because they were given in the natural, naive-realistic attitude toward bodily size, *B*) as plotted against the logarithms of *B*. The entries cling fairly close to the diagonal, much more closely than the major cue variable, *P*, was found to cling to *B* in Fig. 3. Appraising the maximum errors committed in the particular estimates, however, we find that some entries deviate as much as one-half of a power of 10 from the diagonal, corresponding to about three- to four-fold over- or underestimation of the length in question.

A generic summary description of the degree of perfection of size constancy may again be sought in the correlation coefficient. This represents what may be called the "functional validity" (7) or "achievement" (*Leistung*, 2; for definition see 5, p. 255) of the class of responses, *b*, relative to the distal stimulus variable, *B*. For the total sample of 93 situations this correlation is close to .99 (more accurately, .987 when computed from ungrouped logarithms with three significant places [see 7, p. 44]), in spite of the occasional sizable errors referred to above.

By contrast, the functional validity of *b* relative to photographic size, *P* (Fig. 5, right), is only .73; in addition, this is quite close to the purely ecological association of *B* and *P*, .70, so that little independent focusing of *b* on *P* is indicated. Perceptual restructuring toward photographic size (painter's attitude, *p*) raises the correlation of the response with *P* no more than to .85. Other attitudes, deliberately inviting intellectually controlled judgment ("betting") and thus the "stimulus error," will not be discussed here, nor shall we go into the question as to whether or not the estimates *b* and *p* are purely perceptual.

Ecological replication and ecological significance of differences. The analogy to the statistics of individual differences may be carried still further. We may consider each new chance selection of items like those in Fig. 3 as an "ecological replication" of the original sample. In representative design the traditional quest for repetition under "identical" external conditions ceases to be legitimate. Instead, we must seek comparable conditions as drawn from a common universe, just as in differential psychology there is no repetition in the strict sense when other or even the same individuals are used.

Problems of the ecological significance of differences in the statistical sense may be raised, and handled, accordingly. The size of the sample to be used in computation is now given by the number of situations, N, rather than by the number of subjects, n. In this sense the ecological generalizability of distal rather than proximal focusing, in the area of size constancy with distance variant, has been established for our one subject; and even the more crucial in a wide array of relatively moderate ecological validity coefficients of depth cues, referred to above from the material of Seidner, have been found significantly above zero.

In the strict, technical sense, our representative survey of size constancy is completely void of interindividual generalizability. An approach to responder generality was made by using the recorder as a second subject; his distal functional validity was .993, quite close to the .987 for our subject. Furthermore, Dukes (12) has independently obtained a correlation of .991 for a six-year-old boy, using a different sample ($N = 67$) and a somewhat different technique.

It may well be that in many contexts individuals in a population are more homogeneous or stereotyped than are situations in an ecology, and that the ascertainment of ecological generality may be a more challenging task than that of responder-populational generality. Harvey needed only one person to demonstrate the circulation of the blood, and Ebbinghaus needed only himself as a subject to lay the foundation for much of modern learning theory, ecologically narrow as this theory may be.

To complete our analogies between responder-populational and ecological generalization problems, we may set physical size measurement and intuitive or critical estimates of size in analogy to a battery of tests given, not to n persons, but to N situations of the environment. Each type of observation or attitude represents one of these tests. The problem of the degree of "objectivity" of various classes of observation, and thus of certain scientific approaches, can then be handled statistically in terms of their inter- or intraindividual "observational reliability" as tests, in which a sample of situations has taken the place of a sample of persons (see 7, p. 33 and 8, pp. 11 f.).

Representative separation and mathematical isolation of variables. It will be remembered that under systematic design a true isolation of variables can be achieved only by a virtually infinite series of diacritical confrontations or factorial designs piled upon one another. Representative design, while not laying claim to full isolation, separates variables to the extent to which they are separated in the particular ecology but no further, and does not tolerate any artificial perfect tyings (or untyings) between variables. Variables may thus be said to be "representatively separated." From a systematic point of view, a good deal of spuriousness remains built into the textures studied under this policy, samples as these textures are of an ecology that likewise is complexly textured. Representative design is not afraid of this spuriousness; in fact, it welcomes it for the sake of the behavior supports it allows in the execution of the functional approach.

The challenge of further isolation must be met by after-the-fact, mathematical means, as in the study of individual differences. For example, we may use partial correlation as a mathematical means of holding constant a certain variable. Partialing out P from our above correlation between b and B (and thus in effect reducing the ecological validity of P from .70 to 0) still yields a functional validity as high as .98; whereas factually eliminating the sizes under 10 cm. in Fig. 3—to the right of $\log B = 2$—and thus reducing the ecological validity of P from .70 to .14 in a quasi-systematic move, reduces the functional validity to .95 (see also 7, Fig. 9 and the accompanying text).

It must also be noted that, in contradistinction to systematic design, the process of analysis may be stopped at any point, falling back on the nonreductive aim of functional research, together with the assurance that the unresolved part of the associations is safely within the fold of the ecology to which the investigation has been geared from the beginning.

SYSTEMATIC EXPERIMENTS WITH REPRESENTATIVE FEATURES

We now turn to certain experimental policies, some of them common, which may be considered transitory between systematic and representative design. Representative features may be injected in otherwise systematic designs in a variety of ways.

Quasi-representative choice of variables and of their variation or covariation. Some measure of representativeness may be achieved by the choice of variables with particular life relevance, such as "value," as a factor in constant function. Since ecology embraces cultural norms held valid by the law enforcement policies of a society, along with those connected with physical law or geographical contingency, monetary value becomes a challenge to perceptual attainment on a par with other object properties. Cognitive considerations of this kind, rather than the emotive or motivational aspects of value, per se, prompted the study of what could be called perceptual value constancy. In experiments concerning the apparent numerosity of stamps and of coins, Zuk-Kardos and Fazil (reported in 2, pp. 140–150) as well as Ansbacher (referred to in 7) found number constancy with value variant and value constancy with number variant fairly high, although tainted with compromise between the two variables.

Another quasi-representative step is to gear the manner of variation or of covariation between variables to the general scheme of natural conditions in a planfully controlled way, as when the association between a certain cue or means and the object or reward is made probabilistic rather than absolute (4, 10).

Experiments centered about an exemplary instance. Successive omission vs. successive accumulation of cues. A certain effort toward representativeness is discernible whenever a "lifelike" situation is taken as the starting point of the experiment. In the field of size perception such experiments are likely to abandon, at least in some of their phases, the chin rests, darkrooms, screens with small openings, alleys of edges without thickness, or other laboratory paraphernalia in vogue during the late nineteenth century. This liberalization owes much to David Katz (29) and his work in another area of perceptual constancy—color constancy with illumination variant. For size constancy, a study by Holaday (23; see also 7, p. 23), and the studies by Holway and Boring (25), by Gibson (16), and to some extent one by Joynson (28) have proceeded by essentially the same functional scheme.

In each case there is what we may call a core or "exemplar" situation—somewhat arbitrarily chosen, to be sure

—which contains a fairly natural array of cues. Superimposed upon this core is a greater or lesser number of systematic variations so that the design is still fundamentally systematic in its ramifications. The systematic part usually effects what may be called "successive omission" from the originally unknown, or at least not fully scrutinized, array of cues, while the more nearly classical approach tends to eliminate all natural cues, then building up from nothing in a technique of "successive accumulation" (7, pp. 22 ff.).

By and large the functional studies of size constancy have borne out Hering's old assumption of "approximate size-constancy" according to which more distant objects tend to be slightly underestimated (although not nearly as much as would correspond to the shrinkage of their photographic size). But it is significant, from the methodological point of view, that in some of the studies the most favorable cue conditions yielded on the average slight over- rather than undercompensation for distance ("overconstancy"), and that the over-all level of "compromise" varies considerably from study to study. In addition, laboratory experiments inadvertently employing certain atypical contexts or backgrounds will yield drastically different results, not to speak of essentially traditional experiments that programmatically employ grossly distortive configurations, like the situations recently created by Ames (27).

Under the aspect of representative design, all these systematic experiments must be viewed as ecological single cases or "instances" with artificial elaboration that leaves a large portion of the core elements untouched. Each of these experiments is indisputable in its results, but at the same time is of unscrutinized ecological generalizability. The variate packages constituting these experiments or experimental settings may be projected somewhere in the x-dimensional manifold of representative design which we have exemplified for two dimensions in Fig. 3. Each imaginary point, or small, orderly group of points, in such a space represents a potential systematic experiment. Mostly there is little technical basis for telling whether a given experiment is an ecological normal, located in the midst of a crowd of natural instances, or whether it is more like a bearded lady at the fringes of reality, or perhaps like a mere homunculus of the laboratory out in the blank.

As a matter of principle, individual sample situations, no matter how lifelike, cannot answer the funtional problem as to the degree of perceptual constancy, even though by the use of responder replication or by systematic variation their results may become generalizable in certain directions and standardized for testing purposes. Only representative design can answer this problem. By a set of analyses not reflected in the figures of the present paper, the ecological generalizability of the principle of perceptual compromise, or of "approximate" size constancy as originally suggested by Hering, has been established for our subject along with the broader principle of distal rather than proximal focusing.

Canvassing as accidental quota sampling of the ecology. In still other cases an entire array of individual systematic experiments may appear to be laid out after quasi-representative principles, so as to cover the ecology or the mediational pathways by a vaguely conceived "one of a kind" rule. This tacit sampling procedure forms a counterpart to what polling statisticians might describe as a most rudimentary form of stratified, "quota" or proportionate sampling and is usually of the highly erratic type sometimes labeled "accidental" in statistics. We will speak of this primitive type of coverage of the ecology as "canvassing."

In the field of the perceptual constancies, the attempted extension of basic principles, such as that of distal focusing or of compromise, from size to shape to color constancy, is an example of canvassing. A cross-departmental extension to "loudness constancy" with distance of sound source variant was undertaken by Mohrmann (37), yielding generally similar results. Kinesthetic experiments with falling bodies, with and without the aid of visual cues, have established the functioning of a perceptual "weight constancy," with speed (and thus with kinetic energy) variant (2, pp. 161 f.). The above-cited experiments on value constancy further augment the picture of canvassing of the perceptual constancies.

Truncated factorial design and an impasse in the analysis of variance. The basic intent of representative design is toward design proper; that is, it concerns the employment of a statistical selection device for the stimulus sample; only secondarily is it concerned with statistics as a procedure of evaluation. Its relevance to analysis of variance is thus indirect, and derives solely from the fact that this evaluation technique is an adjunct to factorial design, conceived in its shadow and thus wrought with its inherent inadequacies.

At first glance factorial design may appear ideal, since all plots are filled, while in representative design normally only part of the plots is filled; representative designs would thus seem readily extractable from factorial designs if anyone should wish to extract them, but not vice versa. The first catch is, however, that some of the intercombinations of variates may be incompatible in nature or otherwise grossly unrealistic.

An example is furnished by a study of the perceptual impression values of schematized faces by Brunswik and Reiter (2, 11; for a summary and dis-

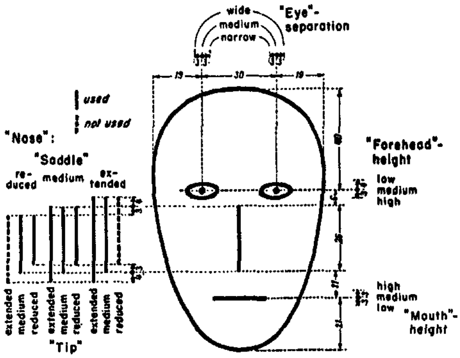

FIG. 6. Five-factorial schematized face (adapted from Brunswik and Reiter, 11).

cussion see 7, pp. 40 f.). In effect, this was an early factorial design, with a $3 \times 3 \times 3 \times 3 \times 3$ layout (Fig. 6). Fortunately for our argument, we were naive enough to depart from the complete array of intercombinations so far as two of the five "facial" variables chosen are concerned. The inadvertently unorthodox part is the truncated treatment of the "nose," as plotted in Fig. 7. Two opposite corners are left empty so that there are only seven noses instead of nine, making for a total of $3 \times 3 \times 3 \times 7 = 189$ facial schemata. The result is an oblique relationship in the artificial ecology of the design which is not unlike the representative ecological correlation shown in Fig. 3 for size constancy, even though with a strange orderly tint that bespeaks the systematic origin of the stimulus distribution.

The reason for the departure from strict orderliness of the design was

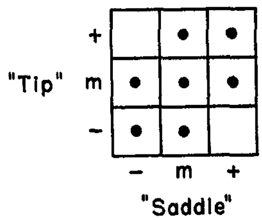

FIG. 7. Truncated treatment of two out of the five variables shown in Fig. 6.

given by the desire—still somewhat vague at the time—to achieve better representativeness. One of the desiderata was to make the schemata look as "facelike" as feasible; the omission of two of the nine possible noses was prompted by their unusually bizarre and ridiculous appearance which threatened to spoil the seriousness of an already precarious attitude on the part of the subjects. In many other psychological contexts certain intercombinations may be unrealistic or disruptive in similar or in some other ways.

Like the design, our original evaluation was makeshift. Years later the main effects and some of the first-order interactions were found significant in terms of analysis of variance as applied to the 189 original impressions of intelligence (composite from 10 adult subjects), although this of course implies little concerning the ecological generalizability to live faces. For our schemata it appeared that impressions of intelligence are aided primarily by high or medium forehead, medium mouth, and medium nose.

In the analysis of the impression value of the "nose" the omission of the two cells necessitated—as my statistician colleagues, Drs. Rheem F. Jarrett and Robert Rollin, informed me—

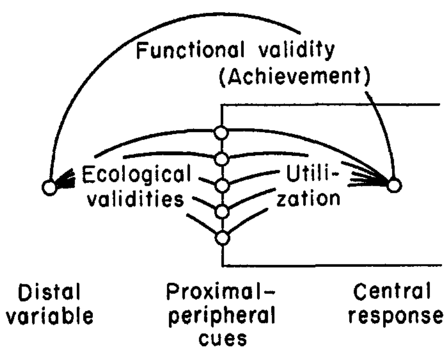

FIG. 8. The lens model as applied to perceptual constancy (adapted from Brunswik, 8).

the employment of such *ad hoc* procedures as the "missing plot" or the "unequal numbers in cells" technique (30, pp. 220 ff.). These techniques are not only artificial and rather uncommon; what is more, they involve the somewhat hypocritical pretense that part of the data was lost, or at least that the material is not quite as it ought to be. Under representative design, involving as it does variate packages in many more dimensions than factorial design could handle practically, oblique distributions are not only legitimate but will in most cases be dictated by nature or by culture, and thus be mandatory. Evaluation techniques will have to be fitted to the materials that may be obtained under such design, rather than vice versa.

MAJOR VARIETIES OF PSYCHOLOGICAL THEORIZING

We now turn to the implications of our above considerations for psychological theory. Three types of psychological theorizing will be discussed: (*a*) theory as the ratiomorphic explication of probabilistic functional—notably distal—achievement and of its strategy; (*b*) theory as the customary nomothetic and more or less molecular reduction of function; and (*c*) theory as comparative methodology, both within psychology and between psychology and the other sciences.

Theory as a ratiomorphic model of functional achievement and its strategy. Focusing and vicarious mediation. The representative study of distal constant function, as reported above, is schematically described in Fig. 8. The wide-arching functional validity coefficient constitutes a generalized statement of the organism's perfection in the attainment of a given distal variable; it falls under the concept of "descriptive theory" by virtue of its generality alone. In addition, an inventory or mapping of the full array of outpost variables attained by an individual or species, as well as of the degree of such focusing ("psychology in terms of objects," 2, 3), promises to lead to further generalizations concerning preferential focusing on distal vs. proximal variables at large, concerning the relationship between life relevance and distality, and concerning related biological-functional problems.

As is further indicated in Fig. 8, the over-all functional arc may be broken down into an extrasystemic and an intrasystemic constituent, called respectively, ecological validity and utilization. The general pattern of the mediational strategy of the organism is predicated upon the limited ecological validity or trustworthiness of cues which we have observed earlier in this paper. This forces a probabilistic strategy upon the organism. To improve its bet, it must accumulate and combine cues. Thus we arrive at a better understanding of the principle of mutual substitutability, or "vicarious functioning," of means (or cues) for each other which Hunter, Tolman (47, ch. i), and most other behaviorists looking for a structural criterion have incorporated in their basic definitions of behavior or purpose (see 8, ch. ii). Hence the lens-like model in Fig. 8, which may be taken to represent the basic unit of psychological functioning. No matter how much the attainment is improved, however, distal function remains inherently probabilistic.

In the light of this model all "constant," or rather, quasi-constant function, be it "intuitive" or explicit, can be explicated as a statistical reasoning process remindful of Helmholtz's "unconscious inference," albeit without its introspectionistic and perfectionistic overtones. Forming a Latin-Greek hybrid, we may speak of this as a "ratiomorphic" theory of achievement (9).

One of the most important aspects of such a theory concerns the relationship between ecological validity and utilization. Ideally, cues should be utilized in accordance with their validity. But here we must inject, among other things to be brought up later in this paper, the element of "cost" to the organism, just as we must ask for the cost of an automobile along with its efficiency in budgeting our expenditures. Functional theory here takes on certain features of economic theory.

Theory as nomothetic reduction. We have left the discussion of the systematic approach, in favor of that of representative design, at the point where the former was about to embark on a diacritical confrontation of constant function with its own cues or other mediating instrumentalities. This type of confrontation has been practiced extensively in psychology, notably in its more classical phases. In the founding period of behaviorism, Watson and Lashley (50) set out to transport homing birds in closed cages over distances sometimes far outside their natural range. Others have since used anesthetics, faradic cages, or rotation on a phonograph turntable in the dark throughout the journey (45). The artificially distortive laboratory experiments on the perception of size and distance to which we have referred above as preceding, and in part paralleling, the quasi-representative and representative studies of the last quarter century fall in the same category.

The objective of this type of behavior or perception study is to trace performance step by step to identifiable processes, cues, or tracks of mediation; in the end they are to be "reduced" to the "laws" of one of the more microscopic, more "fundamental" disciplines, notably physiology. Traditionally, many psychologists have seen in such a reductive explanation of behavior the major task of psychological theory. The nomothetic behaviorism of Hull and of his sympathizers, mentioned at the beginning of this paper, reveals its reductionist aim most clearly in the use of a "physiologizing" terminology.

To the study of distal function and of its grand strategy the reductive approach adds the study of tactics; to the study of achievement and of its macromediation, both of which fall within the province of functional-representative design, it adds the study of micromediation, which falls in the province of nomothetic-systematic design. The functional approach has its place mainly in the appraisal of the interplay and relative contribution or weight of factors in the adjustment to a given ecology, while the reductive approach reveals the technological details of the machinery that brings about such adjustment.

Theory as comparative methodology: observational unity and thematic diversity of science. The injection of physiology into the discussion brings us to a branch of psychological theory which overlaps with the philosophy of science and is best labeled comparative methodology of science. One of the major concerns of such a science of science, or metatheory, is with the basic unity of the sciences. In present-day psychological discussion this problem is subsumed mainly under the watchword "operationism." Here it is often forgotten that the basic requirement for scientific exactitude is a relatively modest one, and in a certain sense a more commonplace one than anticipated. It involves no more than the inter- and intrasubjective univocality of observation and communication which is sometimes called "methodological physicalism" (8), but should better be specified as observational or procedural physicalism; but it does not include the univocality of prediction which is the major *raison d'être* of the nomothetic approach (5).

We may therefore take the position that in the end the unity of science is better served by allowing the reaffirmation or elaboration of this unity to be superseded by a working out of the thematic diversity of the sciences within the minimum common platform. This diversity of themata involves both the aim of the different disciplines and the designs capable of serving these aims.

Thematic physicalism and the nomothetic-reductionist-systematic syndrome. The different explicit and implicit trends toward the unification of science which have dominated the last half century have been under the spell of a somewhat stereotyped image of physics. The thematic element in this cliché may be explicated as the emphasis on the "general," and notably on the strict, univocal regularities or laws which possess universal applicability; this is also known as the nomothetic approach. Universality of law presupposes homogeneity of the universe; hence it matters little where and when and over how large an area a phenomenon is studied. Experimental design may thus safely be left to the convenience and liking of the experimenter and thus become systematic. And, as Mises (36) has pointed out in discussing probabilism in physics, macrolaws have their origin in differential equations, that is, in principles conceived for minute space-time splinters. The triad of nomothetic aim, microreductionist procedure, and systematic design which we have come to recognize as a syndrome in traditional psychology thus is revealed as an emulation of a pattern indigenous to the specific thema of physics.

As has been pointed out in greater detail elsewhere (8, sec. 9), the basically elementistic character of physical law is not obviated by, and has little to do with, the empirical fact of a probabilistic microstructure in thermodynamics or within the atom. Nor must such structures be seen as related to, or in any way supporting, our arguments for the probability approach in psychology; functional psychology is macroprobabilistic in that the identity of the individual case is maintained (as in a scattergram) while it is lost in the physical macrolaw which for ordinary practical purposes is absolute.

General vs. particular. Physical law, geographic fact, ecological correlation. The first to warn against the overestimation of the general over the particular in science was Windelband (52). In a somewhat stilted application to personality and to the humanities, he was led to suggest the well-known distinction between nomothetic and idiographic disciplines. Unfortunately, the latter term encompasses both the low-brow, strictly enumerative approach to historic-geographic fact seen in isolation, and the extremely high-brow emphasis on the "unique" lawfulness of the individual or culture; more properly, the latter case should therefore be labelled the "idionomothetic" approach.

Within the natural sciences, an example of the purely enumerative approach is given by those branches of geography that deal with topographical mapping. The fact that no application of the general physical laws is possible without the constants, parameters and boundary conditions furnished by geographic types of information is frequently neglected in theoretical discussion. Except at the level of control ordinarily accessible only to the physicist observer, these constants are not available (at least not fully so) to the responding organism. Hence the chains from select distal to proximal to central variables in perception are chains of (probable) partial, rather than of total, causation. The universal lawfulness of the world is of limited comfort to the perceiver or behaver not in a position to apply these laws, and he therefore must rely largely on whatever snitches of particular or semigeneralized information he may be able to assemble. This is what we meant earlier in this paper by the assertion that ordinarily organisms must behave as if in a semierratic ecology.

With data from our representative size-constancy survey described above, the relation between physical law and ecological correlation is illustrated in Table 1. The first row of ecological validities has been introduced above in discussing Fig. 3, and the remaining two pairs are added here from our material. Partial correlations derived from either of the two columns of three co-

TABLE 1

TEXTURAL ECOLOGY AND PHYSICS

(Adapted from Brunswik **7**, Fig. 10)

Variables Correlated	Ecological Validities		Nomothetic Approach		
			Partial Correlation		Law of Physical Optics
	Full Sample ($N = 93$)	Sizes over 10 cm. ($N = 59$)	Variable "Held Constant"	Coefficient Obtained	
$B \times P$	.70	.14	D	1.00	$B = PD$
$B \times D$	.77	.88	P	1.00	
$D \times P$	.08	−.34	B	−1.00	

efficients yield (in as close approximation as may be expected) the perfect positive or negative correlations listed in the third column of figures. These in turn reflect the well-known proportionality law, given still further to the right. Since derivation is possible from all alternate ecologies (as exemplified by our sample of 93 vs. the subsample of 59 situations mentioned earlier), it is evident that textural ecology adds valuable probabilistic information to the vastly distilled relational information incorporated in the universal laws of physics. Essentially this is information abstracted and summarized from the geographies that make up the ecology, and is information of the type finite organisms may best be able to absorb in learning, notably in probability discrimination with partial reinforcement.

It is for the same reasons that the laws of triangulation, which underlie the binocular depth mechanism and which only recently have once more been the starting point for nomothetic treatment of this mechanism (32; see also 17), are of somewhat academic interest so far as the actual perceiver is concerned. And for similar reasons we stressed, earlier in this paper, the limited validity of this mechanism within our cultural ecology, in which the perceiver is routinely exposed to optical instruments as well as to flat pictures as substitute means of access to three-dimensional reality.

Nonfunctionalistic (intraorganismic) uses of probability in psychology. It will be remembered that in our molar-functional view of organism-environment interplay, uncertainty is a feature of the relationships between the organism and the distal environment, to wit, of proximal-distal relationships in the case of ecological validity and of central-distal relationships in the case of functional validity; uncertainty is not seen as a necessary feature of intraorganismic processes. In a certain sense this is perhaps in reversal of a tradition which has seen nothing but law in the environment, but was indeterministic or vitalistic so far as the reacting organism is concerned.

Some recent movements have viewed processes within the organism from the probability point of view. While probabilistic functionalism is inherently intersystemic, their approach is intrasystemic. We have no quarrel with them except that we must make it clear that they are confined to problem conceptions which are oversimplified from the standpoint of molar functional psychology, encapsulated as they are within the boundaries of the organism.

Intraregional statistical approaches in psychology may be classified under three major headings: central, peripheral, and peripheral-central. An example of a central statistical theory is the study of "random nervous nets" as developed in the framework of mathematical biophysics by McCulloch and Pitts (33), later in collaboration with Landahl (31). Concern is not with "this" neurone synapsing on "that" one, but rather with "gross" distribution of tendencies and probabilities associated with points or regions in the net. This statistical "rather than deterministic" biophysics was used by Rashevsky (39) as an underpinning for his fundamental equations of mathematical biophysics much in the manner in which the kinetic theory of gases serves as a reductive support to thermodynamics. Rapoport and Shimbel (38, 42) have extended the theory to the dynamics of social interaction.

An application of probability to a peripheral problem is given by the "statistical behavioristics" of Miller and Frick (35). The exposition is encapsulated within the single region of overt responses. The study of "stochastic" (i.e., to a certain extent predictable) word sequences in common English which has come in vogue recently is more or less bodily transferred to an analysis of human "courses of action" —which are related to the "strategies" in the playing of games (49)—and of the "dependent probabilities" resulting from the fact that the preceding occurrence of a response does not always return the system to the original state. Here the only cue considered is given by the preceding time series of events of the same kind. This procedure is not indigenous nor particularly congenial to the thematic content of psychology. Rather, it is somewhat mechanically transplanted from those segments of cybernetics in which unidimensional sequential distribution and correlation are predominant (51)—another instance of falling for ready-made gadgets, even though at a more elevated level.

Skinner's (43) concept of "probability of response" may likewise be classified under the heading of peripheral encapsulation, the term being no more than a fanciful expression for (relative) frequency of response as one of the traditional scores in learning experiments involving the motor output.

The "statistical learning theory" of Estes (13) is an example of a peripheral-central theory, or perhaps of a peripheral-peripheral theory crossing through the entire organism from input to output. Based as it is on evidence from systematic experiments, there is reason to doubt that the "behavior samples" and "statistical samples of environmental events" which are linked by the theory are even tacitly envisaged as representative samples.

Rudimentary emergence of the concept of cue in cybernetics and the theory of telecommunication. Outside of psychology, and within movements which in some of their more obvious aspects have become templates for most of the intraorganismic probabilism just cited, a first step in the direction of the bivariate type of correlation analysis which is covered by the concept of ecological validity is made by Wiener (51) in his analysis of double time series. Such series are, as Wiener points out, most conspicuous in economico-sociological and meteorologico-geophysical applications, since in both instances the relative lead of one time series with respect to another may well give much more information concerning the past of the second than of its own. For example, on account of the general eastward movement of the weather, Chicago weather may be more important in the forecasting of Boston weather than Boston weather itself. It will be noted that even here the comparison stays within the same kind or physical denomination ("weather"); in such cases comparisons or correlations are not yet genuinely bivariate as are those between proximal cues and objects, or between means and goal attainments.

A more direct exposition of those mathematical principles of communication which are of particular relevance to the understanding of focusing by vicarious functioning as it occurs in psychological mechanisms has been given by Shannon and Weaver (41). In terms of the vocabulary of the special brand of telecommunication engineering involving semicontrolled media, to which the theory has been geared, perceptual cues and behavioral means are like "signals" in "coded messages." The mediating channels are contaminated with interferences or constraints of their own. The result is equivocation. It is then "not in general possible to reconstruct the message with certainty by any operation on the signal." Shannon's diagram showing the fanning out of "reasonable causes" (messages, inputs) for a given "high probability received signal" or effect, and of "reasonable effects" (signals, outputs) from a given "high probability message" or cause in a channel, bears formal resemblance to the equivocal types of coupling between intra- and extraorganismic regions to which this writer called attention twenty years ago (2, Fig. 2) and which can also be read into our diagram of the lens model in Fig. 8.

Whenever the "capacity" of a channel is less than the richness of variability of the source from which it accepts messages, the channel is "overloaded." Then it is

> impossible to devise codes which reduce the error frequency as low as one may please. . . . However clever one is with the coding process, it will always be true that after the signal is received there remains some undesirable (noise) uncertainty about what the message was (41, p. 111).

We may add that, in quite the same manner, the crux of organismic adjustment obviously lies in the fact that distal perceptual and behavioral mediation must, by the nature of things, in the general case rely on overloaded channels, with the ensuing limited dependability of all achievement mechanisms. And we may note that at least part of the trouble lies with the overloading and noise in the external rather than the internal medium.

Shannon and Weaver point to one means by which the chances of error can be decreased, however. This is "redundancy," as exemplified by, but by no means restricted to, verbal repetitiveness. When there is noise there is some real advantage in not using a coding process that eliminates all the redundancy, for the remaining redundancy helps combat the uncertainty of transmission. The reader will recognize that the vicarious functioning of cues and of means—functional patterns we came to acknowledge as the backbone of stabilized achievement—may be viewed as special cases of receiving or sending messages through redundant, even though not literally repetitive, channels. The probability of error, given by the variety of possible causes or effects that could result in, or be produced by, the type of event in question, can thus be minimized; this is the case in the gain of the over-all functional validity (.99) over the ecological validity of the major retinal cue (.70) in our representative survey of size constancy referred to above, in which the organism acts as an intuitive statistician.[8]

A suggestion of extending the theory of communication to multivariate patterns of mediation has recently been made by McGill (34). It is hoped that this will open up the full scope of vicarious functioning to formal treatment.

CLINICAL AND RELATED ASPECTS OF REPRESENTATIVE DESIGN

There are several ways in which representative design has become involved with social and clinical psychology. Two of them deal with sampling aspects exclusively, and another two concern both design and the functional theory of the vicarious functioning of cues.

[8] The present statistical application of redundancy is not to be confused with one recently suggested by Attneave (1). In his case the concern is with the exploitation (extrapolation) of *strict* law as it holds over limited stretches of space (or time), say, along part of the contour of an ink bottle. In an earlier context, this writer has spoken of such regularities of limited scope as "local laws" (*Lokalgesetze*, 2, pp. 209, 212). By virtue of their strictness their treatment falls under our above-mentioned heading of an idionomothetic approach; its only relation to the probabilistic approach lies in the fact that the area to which a local law is limited may be considered as a subecology, and that the organism may have to proceed by basically similar mechanisms to ascertain either of the two types of limited regularities, the strict or the probabilistic.

Representative sampling of persons in the role of objects. The case in which not only the responding subjects but also the stimulus objects are persons furnishes perhaps the most obvious demonstration of the necessity for representative design. It is for this symmetry of subject and object that such media as the social perception of personality from photographs seem to offer the best chance of convincing the designer that there must be a sizable N of social objects alongside the customary sizable n of subjects (judges). Yet a survey of the respective literature revealed that, apparently by force of a somewhat thoughtless, content-bound tradition, the object N is on the whole pitifully inadequate in comparison with the subject n, with fallacies of generalization ensuing (7, ch. vi and p. 38).

Hammond has been especially astute in exposing comparable lapses in the logic of design and of evaluation in other areas of social psychology (18) and in clinical psychology, notably in the study of the effect of the sex or personality of the examiner upon Rorschach and other projective test results (19)—even on the part of standard texts in psychological statistics. It turned out that it was a surprisingly widespread practice to apply only one (responder-populational) test of significance using n, and to tacitly consider this test to cover ecological generality in the same breath.

Representative sampling of stimulus configurations as test situations. Concerning representativeness as to the purely physical stimulus configurations used in testing, test designers have so far not pressed beyond the stage designated as canvassing, earlier in this paper. At best, tests were either picked to reflect roughly the current distribution of systematic laboratory experimentation—in particularly fortunate cases, such as in Thurstone's factorial study of perception (46), with an emphasis on more recent and more complex types of experimental problems—or there was an informal effort to assemble a battery of "close-to-life" situations. If the tests were selected from a pool of existing tests the term "sampling" was occasionally applied, but it was usually forgotten that sampling from an artifact, even when this sampling itself should be random, merely perpetuates earlier bias (7, pp. 50 f.).

Similar considerations apply to the customary so-called sampling of behavior, of acts, or of traits from a trait universe (including the construction of adjective check lists), so long as the respective universes or sampling procedures are but informally scrutinized.[4] The degree of representativeness is in each of these cases determined not by the most but by the least representative step in the chain of defining the universe and of drawing the sample. Since trait sampling concerns the responder rather than the ecology, and hence is not part of representative design proper, this is added here merely parenthetically.

From a technical point of view it is not sufficient to have one close-to-life test, or even many of them. As we have pointed out in the main part of this paper, there must be balanced coverage of life. A mere multitude of situations may be designated as M tests, reserving N for the size of truly representative ecological samples. (M is chosen as the letter preceding N, or as the first letter in *many;* the capital letter is used in slight departure from the practice suggested earlier [7, pp. 34 f.]). Only for a true N can technical tests of ecological significance as envisaged in representative design be applied.

[4] Guttman states, seemingly without being much concerned about the underlying problem, that "the processes of sampling people and of sampling items are not at all identical; random sampling, stratified or not, is used for the first, but is not applicable to item construction" (44, p. 54).

Such stimulus configurations as the Rorschach inkblots are biased variate packages, in the sense of the word as defined earlier in this paper. They invite what Sander (40) has called "realistically-meaningful" (*sinnhaft-bedeutungsvoll*) or "ontotropic" tendencies, in contradistinction to the "geometric-ornamental" or "eidotropic" tendencies (toward *Prägnanz* or "good form," such as abstract circles, squares, or other purely geometric formations) as described by the gestalt psychologists proper. Since these ontotropic tendencies are considered to represent a higher developmental stage than the formalistic eidotropic tendencies, both in ontogenesis and in the "actualgenesis" of the instantaneous perceptual experience, a more genuine sampling of stimulus patterns and the setting up of more representative evaluation categories may become diagnostically valuable.

Allowance of vicarious functioning as a representative feature in test construction. As we have seen, one of the most important principles of functional theory is that of vicarious functioning. Its earliest recognition stems not from academic psychology but from one of the theoretical antecedents of clinical psychology, psychoanalysis. Under the direct influence of psychoanalysis, Frenkel-Brunswik (15) has developed the study of "alternative manifestations" of the latent dynamic structure of needs on a statistical basis, using multiple correlation with overt behavior elements; the way to the rational reconstruction of clinical intuition which is asked for in another line of Vienna tradition, logical empiricism, was thus opened. The relationships that may or may not exist with Lazarsfeld's subsequent theory of "latent structure" (44) will not be discussed here.

In my own work the attention to vicarious functioning has concentrated on perception rather than on behavioral expression (and, within perception, on physical objects or on the more static traits of social objects), and the implications of vicariousness upon representative design have been developed through this medium (7, ch. v, vi, viii; for more general discussion see 8, ch. ii). In his contribution to the present symposium Hammond (20) has expanded this to demand allowance of multiple mediation patterns for, and an attendant multiple cue analysis of, clinical judgment by recourse to the requirements of representative design.

Ecological validity and utilization of clinical cues. A problem ensuing from the work of Frenkel-Brunswik and of Hammond, just cited, is the question about the extent to which the ecological validity of the potential cues available to clinical intuition is duplicated by the weight given these cues in their utilization on the part of the responding clinician. Hammond and his collaborators at Colorado (20), and, independently, Smedslund at Oslo (in an as yet unpublished study), have recently found cases of gross discrepancy in this respect, involving both the ignoring or even the reversal of valid cues and the overutilization of cues of low validity.

Pitfalls of systematic design in biological application. As we have seen, representative design is especially indispensable whenever the relative contribution of different variables in a functional context is the subject of investigation. But it must not be forgotten that any systematic experiment, regardless of how oddly conceived it may be, represents at least one actual or potential ecological instance, and in this sense is a bit of reality; in addition, as we have seen, it may be the only means of obtaining knowledge in the reductive context of science.

Impressive as are the achievements of reductive experimentation in the biological sciences, the dangers resulting from the tying of variables and from other characteristics of nonrepresentative designs have rarely been completely avoided. The history of science furnishes ample evidence of harmful effects of systematic design upon practice or theoretical outlook. As a layman, one may think of hygiene in medicine with its dramatic changes between the complete neglect and the excesses of aseptic or antiseptic policies; the boiling of milk and devitaminization; anemia and the eating of liver, and so forth. All of these practices appear as playballs of variables arbitrarily selected for study, which thus acquire undue prominence and throw the picture of the interplay of factors out of balance; and the short-cuts involved in the ecological overgeneralization of results may be even more serious. The biochemist or nutritionist who shuns eggs but is a chain smoker, and the cancer specialist who does the opposite, are too familiar examples to require elaboration.

Systematic design in cliché literature. We may also regard the "world" of popular novels and movies as an artificial, cultural subecology. Its outstanding feature is the presence of clichés; these include both personality stereotypes and plot formulas. Clichés are similar to experiments using systematic design. Factors that show some degree of independent variability in real life are artificially tied. Old-fashioned opera plots or soap melodramas that "drip with generosity" share with cowboy movies what David Hume would have called "inseparable" associations of noble character, overpowering strength, fairness, courage, youth, final success, and so forth, or of their opposites. The cliché is a "worn" case or incident, by no means impossible or nonexistent but made prominent out of all proportion to its frequency, and to the detriment of all other types of incident. Like the systematic design of experiments, it superimposes artificial "laws" upon an ecology which it thereby depletes.

In the case of the systematic experiment it is these artificial laws in the design which, as we have suggested, are at least in part responsible for the often striking lawfulness of the results. At this point the suspicion arises that the didactic role which systematic experimentation obviously plays in the mental economy of the scientist, by virtue of the simplicity and order it both requires in the design and furnishes in the result, may outweigh the fact-finding competence of systematically designed experiments. Certainly the more drastically simplified forms of art of which we have spoken, and which are so similar in pattern to the systematic experiment, are clearly didactic rather than informative in any realistic sense; we do not go to the movies to find out about life or to form a scientifically airtight theory about personality, in spite of the fact that movies may sometimes be helpful in temporarily smoothing the perplexities of life.

The main function both of art and of systematic experimentation, then, is to shake and mold us by exaggeration and extreme correlation or absence of correlation. But exaggeration is distortion, and this distortion must in science eventually be resolved by allowing the more palatable systematic design to mature into, and to be superseded by, the more truthful representative design.

CONCLUSION: UNITY OF SCIENCE AND UNITY OF PSYCHOLOGY

Our considerations in comparative methodology have brought into focus the thematic diversification that is possible within the over-all unity of exact, observationally physicalistic science. They may facilitate the cheerful relinquishment of the overheated nomothetic bias under which the development of psychology has long suffered and which is making the establishment of a molar-functional psychology an uphill battle. Acceptance of the probabilistic conception, both for the propaedeutic study of ecology and for the functional analysis of perceptual or behavioral achievement, not only sets normative psychology on the right track; it also unites it methodologically with differential psychology, thus lending ideological support to the inherently statistical character they both share, and making a virtue of this often bemoaned necessity.

Psychology thus acquires the distinctive, well-circumscribed internal unity and coherence which we have long searched for, and the reality of which many of us have doubted. This unity emerges, as does perhaps all good unity of—and in—science, as one of methodology or grand design. In both of its major aspects, representative design and functional theory, it centers about probabilistic texture and the ways of its exploitation by the organism. The fact that organismic achievement and its reconstruction in psychology are characterized by certain similarities with predictive procedures in economics, meteorology, and especially in the study of telecommunication through uncontrolled, noisy external media, should help to bring into focus the family of sciences in its entirety. Such a focusing should help to set in proper perspective our traditional thematic dependence on, or need for succorance from, a select few of the older disciplines which may have appeared particularly glamorous, especially physiology and physics.

REFERENCES

1. Attneave, F. Some informational aspects of visual perception. *Psychol. Rev.*, 1954, **61**, 183–193.
2. Brunswik, E. *Wahrnehmung und Gegenstandswelt.* Vienna: Deuticke, 1934.
3. Bbunswik, E. Psychology as a science of objective relations. *Phil. Sci.*, 1937, **4**, 227–260. (Errata corrected 1938, **5**, 110.)
4. Brunswik, E. Probability as a determiner of rat behavior. *J. exp. Psychol.*, 1939, **25**, 175–197.
5. Brunswik, E. Organismic achievement and environmental probability. *Psychol. Rev.*, 1943, **50**, 255–272.
6. Brunswik, E. Distal focussing of perception: size-constancy in a representative sample of situations. *Psychol. Monogr.*, 1944, **56**, No. 1 (Whole No. 254).
7. Brunswik, E. *Systematic and representative design of psychological experiments.* Berkeley: Univer. of California Press, 1947. (Also in J. Neyman [Ed.], *Berkeley symposium on mathematical statistics and probability.* Berkeley: Univer. of California Press, 1949.)
8. Brunswik, E. *The conceptual framework of psychology.* Chicago: Univer. of Chicago Press, 1952. (*Int. Encycl. unified Sci.*, Vol. I, No. 10.)
9. Brunswik, E. "Ratiomorphic" models of perception and thinking. In N. Mailloux (Ed.), *Proc. 14th Int. Congr. Psychol., Montreal, 1954,* in press.
10. Brunswik, E., & Herma, H. Probability learning of perceptual cues in the establishment of a weight illusion. *J. exp. Psychol.*, 1951, **41**, 281–290.
11. Brunswik, E., & Reiter, L. Eindrucks-Charaktere schematisierter Gesichter. *Z. Psychol.*, 1937, **142**, 67–134.
12. Dukes, W. F. Ecological representativeness in studying perceptual size-constancy in childhood. *Amer. J. Psychol.*, 1951, **64**, 87–93.
13. Estes, W. K. Toward a statistical theory of learning. *Psychol. Rev.*, 1950, **57**, 94–107.
14. Fisher, R. A. *Design of experiments.* Edinburgh: Oliver & Boyd, 1935.
15. Frenkel-Brunswik, Else. Motivation and behavior. *Genet. Psychol. Monogr.*, 1942, **26**, 131–265.
16. Gibson, J. J. Motion picture testing and research. Washington, D. C.: U. S. Government Printing Office, 1947. (*AAF Aviat. Psychol. Program Res. Rep.* No. 7.)
17. Graham, C. H. Visual perception. In S. S. Stevens (Ed.), *Handbook of experimental psychology.* New York: Wiley, 1951. Pp. 868–920.
18. Hammond, K. R. Subject and object sampling: a note. *Psychol. Bull.*, 1948, **45**, 530–533.

19. HAMMOND, K. R. Representative vs. systematic design in clinical psychology. *Psychol. Bull.*, 1954, **51**, 150–159.
20. HAMMOND, K. R. Probabilistic functioning and the clinical method. *Psychol. Rev.*, in press.
21. HEIDER, F. Environmental determinants in psychological theories. *Psychol. Rev.*, 1939, **46**, 383–410.
22. HEMPEL, C. G. *Fundamentals of concept formation in empirical science.* Chicago: Univer. of Chicago Press, 1952. (*Int. Encycl. unified Sci.*, Vol. II, No. 7.)
23. HOLADAY, B. E. Die Grössenkonstanz der Sehdinge. *Arch. ges. Psychol.*, 1933, **88**, 419–486.
24. HOLT, E. B. *The Freudian wish.* New York: Holt, 1915.
25. HOLWAY, A. H., & BORING, E. G. Determinants of apparent visual size with distance variant. *Amer. J. Psychol.*, 1941, **54**, 21–37.
26. HULL, C. L. *Principles of behavior: an introduction to behavior theory.* New York: D. Appleton-Century, 1943.
27. ITTELSON, W. H. *The Ames demonstrations in perception.* Princeton: Princeton Univer. Press, 1952.
28. JOYNSON, R. B. The problem of size and distance. *Quart. J. exp. Psychol.*, 1949, **1**, 119–135.
29. KATZ, D. *The world of colour.* (Trans. by R. B. McLeod from the 2nd German Ed. [1930].) London: Kegan Paul, 1935. (First German edition, 1911.)
30. KENDALL, M. G. *Advanced theory of statistics.* Vol. II. (3rd Ed.) New York: Hafner, 1951.
31. LANDAHL, H. D., MCCULLOCH, W. S., & PITTS, W. A statistical consequence of the logical calculus of nervous nets. *Bull. math. Biophysics,* 1943, **5**, 135–137.
32. LUNEBURG, R. K. *Mathematical analysis of binocular vision.* Princeton: Princeton Univer. Press, 1947.
33. MCCULLOCH, W. S., & PITTS, W. A logical calculus of the ideas immanent in nervous activity. *Bull. math. Biophysics,* 1943, **5**, 115–133.
34. MCGILL, W. J. Multivariate transmission of information and its relation to analysis of variance. *Hum. Fact. Operat. Res. Lab.*, 1953, Rep. No. 32.
35. MILLER, G. A., & FRICK, F. C. Statistical behavioristics and sequences of responses. *Psychol. Rev.*, 1949, **56**, 311–324.
36. MISES, R. v. *Probability, statistics and truth.* New York: Macmillan, 1939.
37. MOHRMANN, K. Lautheitskonstanz im Entfernungswechsel. *Z. Psychol.*, 1939, **145**, 145–199.
38. RAPOPORT, A. Cycle distributions in random nets. *Bull. math. Biophysics,* 1948, **10**, 145–157.
39. RASHEVSKY, N. *Mathematical biophysics.* (Rev. Ed.) Chicago: Univer. of Chicago Press, 1948.
40. SANDER, F. Experimentelle Ergebnisse der Gestaltpsychologie. *10th Kongr. exp. Psychol., Bonn,* 1928, 23–88.
41. SHANNON, C. E., & WEAVER, W. *Mathematical theory of communication.* Urbana: Univer. of Illinois Press, 1949.
42. SHIMBEL, A., & RAPOPORT, A. Statistical approach to the theory of the nervous system. *Bull. math. Biophysics,* 1948, **10**, 41–55.
43. SKINNER, B. F. The experimental analysis of behavior. In *Proc. 13th Int. Congr. Psychol., 1951.* Stockholm: Lagerström, 1952. Pp. 62–91. (Also in *Amer. Psychologist, 1953,* **8**, 69–78.)
44. STOUFFER, S. A., *et al. Measurement and prediction.* Princeton: Princeton Univer. Press, 1950.
45. THOMSON, A. L. *Bird migration.* (Rev. Ed.) London: Witherby, 1942.
46. THURSTONE, L. L. *A factorial study of perception.* Chicago: Univer. of Chicago Press, 1944.
47. TOLMAN, E. C. *Purposive behavior in animals and men.* New York: Century, 1932.
48. TOLMAN, E. C., & BRUNSWIK, E. The organism and the causal texture of the environment. *Psychol. Rev.*, 1935, **42**, 43–77.
49. VON NEUMANN, J., & MORGENSTERN, O. *Theory of games and economic behavior.* (2nd Ed.) Princeton: Princeton Univer. Press, 1947.
50. WATSON, J. B., & LASHLEY, K. S. *Homing and related activities of birds.* Washington: Carnegie Institution, 1915.
51. WIENER, N. *Extrapolation, interpolation, and smoothing of stationary time series.* New York: Wiley, 1949.
52. WINDELBAND, W. *Geschichte und Naturwissenschaft.* Strassburg: Heitz, 1894.
53. WOODWORTH, R. S. *Experimental psychology.* New York: Holt, 1938. (2nd Ed., with H. Schlosberg, New York: Holt, 1954.)