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THE TWO DISCIPLINES OF SCIENTIFIC PSYCHOLOGY[1]

LEE J. CRONBACH

University of Illinois

NO man can be acquainted with all of psychology today, as our convention program proves. The scene resembles that of a circus, but a circus grander and more bustling than any Barnum ever envisioned—a veritable week-long diet of excitement and pink lemonade. Three days of smartly paced performance are required just to display the new tricks the animal trainers have taught their charges. We admire the agile paper-readers swinging high above us in the theoretical blue, saved from disaster by only a few gossamer threads of fact, and we gasp as one symposiast thrusts his head bravely between another's sharp toothed jaws. This 18-ring display of energies and talents gives plentiful evidence that psychology is going places. But whither?

In the simpler days of psychology, the presidential address provided a summing-up and a statement of destination. The President called the roll of the branches of psychology—praising the growth of some youngsters, tut-tutting patriarchally over the delinquent tendencies of others—and showed each to his proper place at the family table. My own title is reminiscent of those grand surveys, but the last speaker who could securely bring the whole of psychology within one perspective was Dashiell, with his 1938 address on "Rapprochements in Contemporary Psychology" (15). My scope must be far more restricted.

I shall discuss the past and future place within psychology of two historic streams of method, thought, and affiliation which run through the last century of our science. One stream is *experimental psychology;* the other, *correlational psychology.* Dashiell optimistically forecast a confluence of these two streams, but that confluence is still in the making. Psychology continues to this day to be limited by the dedication of its investigators to one or the other method of inquiry rather than to scientific psychology as a whole.

[1] Address of the President at the Sixty-Fifth Annual Convention of the American Psychological Association, New York, New York, September 2, 1957.

A stream of thought is identified by many features: philosophical underpinnings, methods of inquiry, topical interests, and loci of application. The experimental and correlational streams have all these aspects, but I am concerned with them as disciplines within scientific psychology. The job of science is to ask questions of Nature. A discipline is a method of asking questions and of testing answers to determine whether they are sound. Scientific psychology is still young, and there is rapid turnover in our interests, our experimental apparatus and our tests, and our theoretical concepts. But our methods of inquiry have become increasingly stable, and it is these methods which qualify us as scientists rather than philosophers or artists.

THE SEPARATION OF THE DISCIPLINES

The experimental method—where the scientist changes conditions in order to observe their consequences—is much the more coherent of our two disciplines. Everyone knows what experimental psychology is and who the experimental psychologists are. Correlational psychology, though fully as old as experimentation, was slower to mature. It qualifies equally as a discipline, however, because it asks a distinctive type of question and has technical methods of examining whether the question has been properly put and the data properly interpreted.

In contrast to the Tight Little Island of the experimental discipline, correlational psychology is a sort of Holy Roman Empire whose citizens identify mainly with their own principalities. The discipline, the common service in which the principalities are united, is the study of correlations presented by Nature. While the experimenter is interested only in the variation he himself creates, the correlator finds his interest in the already existing variation between individuals, social groups, and species. By "correlational psychology" I do not refer to studies which rely on one statistical procedure. Factor analysis is correlational, to be sure, but so is the study of Ford and Beach (23)

relating sexual behavior to differences along the phylogenetic scale and across the cultural spectrum.

The well-known virtue of the experimental method is that it brings situational variables under tight control. It thus permits rigorous tests of hypotheses and confident statements about causation. The correlational method, for its part, can study what man has not learned to control or can never hope to control. Nature has been experimenting since the beginning of time, with a boldness and complexity far beyond the resources of science. The correlator's mission is to observe and organize the data from Nature's experiments. As a minimum outcome, such correlations improve immediate decisions and guide experimentation. At the best, a Newton, a Lyell, or a Darwin can align the correlations into a substantial theory.

During our century of scientific psychology, the correlators have marched under many flags. In perhaps the first modern discussion of scientific method in psychology (1874), Wundt (54) showed how "experimental psychology" and "ethnic psychology" (i.e., cross-cultural correlations) supplement each other. In one of the most recent (1953), Bindra and Scheier (4) speak of the interplay of "experimental" and "psychometric" method. At the turn of the century, the brand names were "experimental" and "genetic" psychology, although experimenters were also beginning to contrast their "general psychology" with the "individual psychology" of Stern and Binet.

In 1913, Yerkes made the fundamental point that all the correlational psychologies are one. His name for this branch was "comparative psychology."

> Although comparative psychology in its completeness necessarily deals with the materials of the psychology of infant, child, adult, whether the being be human or infra-human; of animal or plant [!]—of normal and abnormal individuals; of social groups and of civilizations, there is no reason why specialists in the use of the comparative method should not be so distinguished, and, if it seems necessary, labelled (55).

Even in advocating research on animals (56), Yerkes is emphatic in defining the goal as correlation across species. In France, *la psychologie comparée* continues to include all of differential psychology; but in America, as Beach (2) has lamented, comparative psychology degenerated into the experimental psychology of the white rat and thereby lost the power of the correlational discipline.

Except for the defection of animal psychologists, the correlational psychologists have remained loosely federated. Developmental psychologists, personality psychologists, and differential psychologists have been well acquainted both personally and intellectually. They study the same courses, they draw on the same literature, they join the same divisions of APA.

Experimental and correlational psychologists, however, grew far apart in their training and interests. It is now commonplace for a student to get his PhD in experimental psychology without graduate training in test theory or developmental psychology, and the student of correlational branches can avoid experimental psychology only a little less completely. The journals of one discipline have small influence on the journals of the other (14). Boring even dares to say (5, p. 578) that there is a personality difference between the fields: the distinction being that correlational psychologists like people!

Certainly the scientific values of psychologists are sharply divided. Thorndike (9, 44) recently asked American psychologists to rate various historic personages by indicating, on a forced-choice questionnaire, which have made the greatest contributions to psychology. A factor analysis of the ratings shows two distinct factors (Figure 1). One bipolar factor (irrelevant to our present discussion)

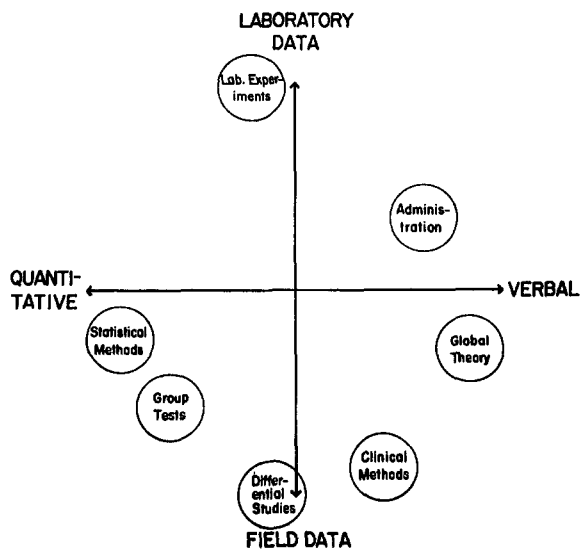

FIG. 1. Factors accounting for esteem of leaders in psychology by American psychologists (based on correlations presented by Thorndike, 44, corrected for attenuation and refactored).

ranges from verbal to quantitative psychologists. The other factor has at one pole the laboratory experimenters like Stevens, Dodge, and Ebbinghaus, and at the opposite pole those like Binet, May, and Goodenough who collect and correlate field data. A psychologist's esteem for the experimenters is correlated −.80 (−1.00, corrected for attenuation) with his esteem for scientists who use correlational methods.

There was no such schism in 1913 when Yerkes stated the program of correlational psychology. Genetic psychology and experimental psychology were hard at work on the same problems. Terman demonstrated in his 1923 presidential address (43) that the mental test was within the tradition of experimental, fundamental research in psychology, and had quotations to show that the contemporary experimentalists agreed with him. Wells and Goddard, in 1913, had been asked to lecture on mental tests within the Holy Temple itself, the Society of Experimental Psychologists. And, in 1910, the High Priest Titchener had said:

> Individual psychology is one of the chief witnesses to the value of experiment. It furnishes the key to many, otherwise inexplicable differences of result, and it promises to allay many of the outstanding controversies. . . . There can be no doubt that it will play a part of steadily increasing importance (46).

But when Terman spoke in 1923, the common front had already been fatally breached. Watson had announced that experimental treatment could make and unmake individual differences at will, thus stripping them of scientific importance. Thurstone had taken the first firm stride in the opposite direction:

> I suggest that we dethrone the stimulus. He is only nominally the ruler of psychology. The real ruler of the domain which psychology studies is the individual and his motives, desires, wants, ambitions, cravings, aspirations. The stimulus is merely the more or less accidental fact . . . (45, p. 364).

The personality, social, and child psychologists went one way; the perception and learning psychologists went the other; and the country between turned into desert.

During the estrangement of correlational and experimental psychology, antagonism has been notably absent. Disparagement has been pretty well confined to playful remarks like Cattell's accusation that the experimental psychologist's "regard for the body of nature becomes that of the anatomist rather than that of the lover" (7, p. 152), or the experimentalist Bartlett's (1, p. 210) satire on the testers emerging from World War I, "chanting in unaccustomed harmony the words of the old jingle

> 'God has a plan for every man
> And He has one for you.' "

Most correlationists have done a little experimenting in the narrow sense, and experimenters have contributed proudly to testing work under wartime necessity. But these are temporary sojourns in a foreign land. (For clear expressions of this attitude, see 5, pp. 570–578 and 52, p. 24.)

A true federation of the disciplines is required. Kept independent, they can give only wrong answers or no answers at all regarding certain important problems. It is shortsighted to argue for one science to discover the general laws of mind or behavior and for a separate enterprise concerned with individual minds, or for a one-way dependence of personality theory upon learning theory. Consider the physical sciences as a parallel. Physics for centuries was the study of general laws applying to all solids or all gases, whereas alchemy and chemistry studied the properties and reactions of individual substances. Chemistry was once only a descriptive catalogue of substances and analytic techniques. It became a systematic science when organized quantitative studies yielded principles to explain differences between substances and to predict the outcomes of reactions. In consequence, Mendeleev the chemist paved the way for Bohr the physicist, and Fermi's physics contributes to Lawrence's chemistry; the boundary between chemistry and physics has become almost invisible.

The tide of separation in psychology has already turned. The perceiver has reappeared in perceptual psychology. Tested intelligence and anxiety appear as independent variables in many of the current learning experiments. Factor analytic studies have gained a fresh vitality from crossbreeding with classical learning experiments (e.g., 18, 22). Harlow, Hebb, Hess, and others are creating a truly experimental psychology of development. And students of personality have been designing subtle combinations of experimental and correlational method (see, for example, 29) which may ultimately prove to be our parallel to the emergence of physical chemistry.

CHARACTERIZATION OF THE DISCIPLINES

In the beginning, experimental psychology was a substitute for purely naturalistic observation of man-in-habitat. The experimenter placed man in an artificial, simplified environment and made quantitative observations of his performance. The initial problem was one of describing accurately what man felt, thought, or did in a defined situation. Standardization of tasks and conditions was required to get reproducible descriptions. All experimental procedures were tests, all tests were experiments. Kraepelin's continuous-work procedure served equally the general study of fatigue and the diagnosis of individuals. Reaction time was important equally to Wundt and to Cattell.

The distinctive characteristic of modern experimentation, the statistical comparison of treatments, appeared only around 1900 in such studies as that of Thorndike and Woodworth on transfer. The experimenter, following the path of Ebbinghaus, shifted from measurement of the average mind to measuring the effect of environmental change upon success in a task (51). Inference replaced estimation: the mean and its probable error gave way to the critical ratio. The standardized conditions and the standardized instruments remained, but the focus shifted to the single manipulated variable, and later, following Fisher, to multivariate manipulation. The experiment thus came to be concerned with between-treatments variance. I use the word "treatment" in a general sense; educational and therapeutic treatments are but one type. Treatment differences are equally involved in comparing rats given different schedules of reinforcement, chicks who have worn different distorting lenses, or social groups arranged with different communication networks.

The second great development in American experimental psychology has been its concern with formal theory. At the turn of the century, theory ranged far ahead of experiment and made no demand that propositions be testable. Experiment, for its part, was willing to observe any phenomenon, whether or not the data bore on theoretical issues. Today, the majority of experimenters derive their hypotheses explicitly from theoretical premises and try to nail their results into a theoretical structure. This deductive style has its undeniable defects, but one can not question the net gains from the accompanying theoretical sophistication. Discussions of the logic of operationism, intervening variables, and mathematical models have sharpened both the formulation of hypotheses and the interpretation of results.

Individual differences have been an annoyance rather than a challenge to the experimenter. His goal is to control behavior, and variation within treatments is proof that he has not succeeded. Individual variation is cast into that outer darkness known as "error variance." For reasons both statistical and philosophical, error variance is to be reduced by any possible device. You turn to animals of a cheap and short-lived species, so that you can use subjects with controlled heredity and controlled experience. You select human subjects from a narrow subculture. You decorticate your subject by cutting neurons or by giving him an environment so meaningless that his unique responses disappear (cf. 25). You increase the number of cases to obtain stable averages, or you reduce N to 1, as Skinner does. But whatever your device, your goal in the experimental tradition is to get those embarrassing differential variables out of sight.

The correlational psychologist is in love with just those variables the experimenter left home to forget. He regards individual and group variations as important effects of biological and social causes. All organisms adapt to their environments, but not equally well. His question is: what present characteristics of the organism determine its mode and degree of adaptation?

Just as individual variation is a source of embarrassment to the experimenter, so treatment variation attenuates the results of the correlator. His goal is to predict variation within a treatment. His experimental designs demand uniform treatment for every case contributing to a correlation, and treatment variance means only error variance to him.

Differential psychology, like experimental, began with a purely descriptive phase. Cattell at Hopkins, Galton at South Kensington, were simply asking how much people varied. They were, we might say, estimating the standard deviation while the general psychologists were estimating the central tendency.

The correlation coefficient, invented for the study of hereditary resemblance, transformed descriptive differential research into the study of mental organization. What began as a mere summary statistic quickly became the center of a whole theory of data analysis. Murphy's words, written in 1928,

recall the 'excitement that attended this development:

> The relation between two variables has actually been found to be statable in other terms than those of experiment . . . [Moreover,] Yule's method of "partial correlation" has made possible the mathematical "isolation" of variables which cannot be isolated experimentally. . . . [Despite the limitations of correlational methods,] what they have already yielded to psychology . . . is nevertheless of such major importance as to lead the writer to the opinion that the only twentieth-century discovery comparable in importance to the conditioned-response method is the method of partial correlations (35, p. 410).

Today's students who meet partial correlation only as a momentary digression from their main work in statistics may find this excitement hard to comprehend. But partial correlation is the starting place for all of factor analysis.

Factor analysis is rapidly being perfected into a rigorous method of clarifying multivariate relationships. Fisher made the experimentalist an expert puppeteer, able to keep untangled the strands to half-a-dozen independent variables. The correlational psychologist is a mere observer of a play where Nature pulls a thousand strings; but his multivariate methods make him equally an expert, an expert in figuring out where to look for the hidden strings.

His sophistication in data analysis has not been matched by sophistication in theory. The correlational psychologist was led into temptation by his own success, losing himself first in practical prediction, then in a narcissistic program of studying his tests as an end in themselves. A naive operationism enthroned theory of test performance in the place of theory of mental processes. And premature enthusiasm [2] exalted a few measurements chosen almost by accident from the tester's stock as the ruling forces of the mental universe.

In former days, it was the experimentalist who wrote essay after anxious essay defining his discipline and differentiating it from competing ways of studying mind. No doubts plagued correlationists like Hall, Galton, and Cattell. They came in on the wave of evolutionary thought and were buoyed up by every successive crest of social progress or crisis. The demand for universal education, the development of a technical society, the appeals from the distraught twentieth-century parent, and finally the clinical movement assured the correlational psychologist of his great destiny. Contemporary experimentalists, however, voice with ever-increasing assurance their program and social function; and the fact that tonight you have a correlational psychologist discussing disciplinary identities implies that anxiety is now perched on *his* windowledge.

Indeed, I do speak out of concern for correlational psychology. Aptitude tests deserve their fine reputation; but, if practical, validated procedures are to be our point of pride, we must be dissatisfied with our progress since 1920. As the Executive Committee of Division 5 itself declared this year, none of our latter-day refinements or innovations has improved practical predictions by a noticeable amount. Correlational psychologists who found their self-esteem upon contributions to theory can point to monumental investigations such as the *Studies of Character* and *The Authoritarian Personality*. Such work does throw strong light upon the human scene and brings important facts clearly into view. But theories to organize these facts are rarely offered and even more rarely solidified (30; 31, p. 55).

POTENTIAL CONTRIBUTIONS OF THE DISCIPLINES TO ONE ANOTHER

Perhaps it is inevitable that a powerful new method will become totally absorbing and crowd other thoughts from the minds of its followers. It took a generation of concentrated effort to move from Spearman's tetrad equation and Army Alpha to our present view of the ability domain. It took the full energies of other psychologists to move from S-R bonds to modern behavior theory. No doubt the tendency of correlationists to ignore experimental developments is explained by their absorption in the wonders and complexities of the phenomena their own work was revealing. And if experimentalists were to be accused of narrow-minded concentration on one particular style and topic of research, the same comment would apply.

The spell these particular theories and methods cast upon us appears to have passed. We are free at last to look up from our own bedazzling treasure, to cast properly covetous glances upon the scientific wealth of our neighbor discipline. Trading has already been resumed, with benefit to both parties.

The introduction of construct validation into test theory (12) is a prime example. The history of this development, you may recall, was that the

[2] This judgment is not mine alone; it is the clear consensus of the factor analysts themselves (see 28, pp. 321–325).

APA's Committee on Psychological Tests discovered that available test theory recognized no way of determining whether a proposed psychological interpretation of a test was sound. The only existing theory dealt with criterion validation and could not evaluate claims that a test measured certain psychological traits or states. Meehl, capitalizing on the methodological and philosophical progress of the experimenters, met the testers' need by suggesting the idea of construct validity. A proposed test interpretation, he showed, is a claim that a test measures a construct, i.e., a claim that the test score can be linked to a theoretical network. This network, together with the claim, generates predictions about observations. The test interpretation is justified only if the observations come out as predicted. To decide how well a purported test of anxiety measures anxiety, construct validation is necessary; i.e., we must find out whether scores on the test behave in accordance with the theory that defines anxiety. This theory predicts differences in anxiety between certain groups, and traditional correlational methods can test those predictions. But the theory also predicts variation in anxiety, hence in the test score, as a function of experience or situations, and only an experimental approach can test those predictions.

This new theory of validity has several very broad consequences. It gives the tester a start toward the philosophical sophistication the experimenter has found so illuminating. It establishes the experimental method as a proper and necessary means of validating tests. And it re-establishes research on tests as a valuable and even indispensable way of extending psychological theory.

We may expect the test literature of the future to be far less saturated with correlations of tests with psychologically enigmatic criteria, and far richer in studies which define test variables by their responsiveness to practice at different ages, to drugs, to altered instructions, and to other experimentally manipulated variables. A pioneering venture in this direction is Fleishman's revealing work (21, 22) on changes in the factorial content of motor skills as a function of practice. These studies go far beyond a mere exploration of certain tests; as Ferguson has shown (19, 20), they force upon us a theory which treats abilities as a product of learning, and a theory of learning in which previously acquired abilities play a major role.

Perhaps the most valuable trading goods the correlator can offer in return is his multivariate conception of the world.

No experimenter would deny that situations and responses are multifaceted, but rarely are his procedures designed for a systematic multivariate analysis. The typical experimental design and the typical experimental law employ a single dependent variable. Even when more than one outcome is measured, the outcomes are analyzed and interpreted separately. No response measure, however, is an adequate measure of a psychological construct. Every score mixes general construct-relevant variance with variance specific to the particular measuring operation. It is all right for the agriculturist to consider size of crop as the fundamental variable being observed: that is the payoff for him. Our task, however, is to study changes in fundamental aspects of behavior, and these are evidenced only indirectly in any one measure of outcome.

The correlational psychologist discovered long ago that no observed criterion is truly valid and that simultaneous consideration of many criteria is needed for a satisfactory evaluation of performance. This same principle applies in experimentation. As Neal Miller says in a recent paper on experiments with drugs:

> Where there are relatively few facts it seems easy to account for them by a few simple generalizations. . . . As we begin to study the effects of a variety of drugs on a number of different behavioral measures, exceptions and complexities emerge. We are forced to reexamine and perhaps abandon common-sense categories of generalization according to convenient words existing in the English language. As new and more comprehensive patterns of results become available, however, new and more precise generalizations may emerge. We may be able to "carve nature better to the joint" and achieve the simplicity of a much more exact and powerful science (32, pp. 326–327).

Theoretical progress is obstructed when one restricts himself to a single measure of response (34). Where there is only one dependent variable, it is pointless to introduce intervening variables or constructs. When there are many response variables, however, it is mandatory to subsume them under constructs, since otherwise we must have a separate set of laws for every measure of outcome. Dealing with multiple response variables is, as Miller says (33), precisely the problem with which the factor analysts have been concerned. Factor analysis, by substituting formal for intuitive methods, has been of great help in locating constructs with which to summarize observations about abilities. It is rea-

sonable to expect that multivariate treatment of response measures would have comparable value in experimental psychology.

Experimenters very probably have even more to gain from treating *in*dependent variables as a continuous multivariate system. The manifold treatment categories in a Fisherian design are established a priori. In agriculture, the treatment dimensions the farmer can manipulate are obvious: fertilizer, water, species of seed, and so on. In a more basic science, we require genotypic constructs to describe situations, constructs like the physical scientist's temperature and pressure. The conditions the psychologist most easily manipulates—stimulus form, injunction to the subject, strength of electric shock —are not chosen because we intend to apply these specific conditions when we get around to "controlling behavior." They are used because these conditions, we hope, embody scientifically useful constructs.

The experimenter has no systematic way to classify and integrate results from different tasks or different reinforcers. As Ferguson remarks (**20**, p. 130; see also **19**, p. 100): "No satisfactory methodology has emerged for describing particular learning tasks, or indicating how one task differs from another, other than by a process of simple inspection." We depend wholly on the creative flair of the theorist to collate the experiments and to invent constructs which might describe particular situations, reinforcements, or injunctions in terms of more fundamental variables. The multivariate techniques of psychometrics are suited for precisely this task of grouping complex events into homogeneous classes or organizing them along major dimensions. These methods are frankly heuristic, but they are systematically heuristic. They select variables with minimal redundancy, and they permit us to obtain maximum information from a minimum of experimental investment.

In suggesting that examining treatment conditions as a statistical universe is a possible way to advance experimental thinking, I am of course echoing the recommendations of Egon Brunswik (**6**, esp. pp. 39–58). Brunswik criticized the Fisherian experimenter for his ad hoc selection of treatments and recommended that he apply the sampling principles of differential psychology in choosing stimuli and conditions. A sampling procedure such as Brunswik suggests will often be a forward step, but the important matter is not to establish laws which apply loosely to a random, unorganized collection of situations. The important matter is to discover the organization among the situations, so that we can describe situational differences as systematically as we do individual differences.

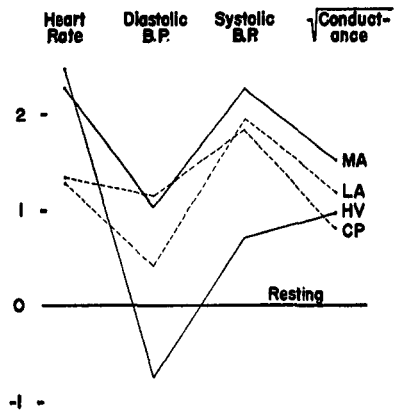

FIG. 2. Mean response to four stressors expressed in terms of resting standard scores (data from **50**).

Research on stress presents a typical problem of organization. Multivariate psychophysiological data indicate that different taxing situations have different effects. At present, stressors can be described and classified only superficially, by inspection. A correlational or distance analysis of the data groups treatments which have similar effects and ultimately permits us to locate each treatment within a continuous multidimensional structure having constructs as reference axes. Data from a recent study by Wenger, Clemens, and Engel (**50**) may be used as an illustration. Figure 2 shows the means of standardized physiological scores under four different stress conditions: mental arithmetic,

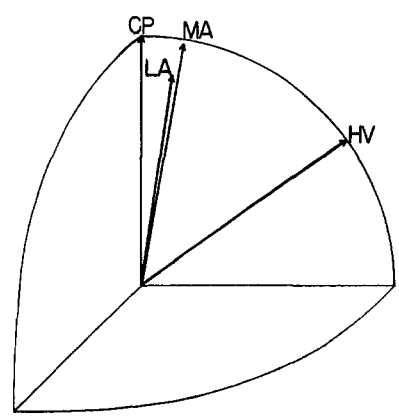

FIG. 3. Multivariate diagram showing similarity between four stressors.

a letter association test, hyperventilation, and a cold pressor. The "profiles" for the four conditions are very significantly different. I have made a distance analysis to examine the similarity between conditions, with the results diagrammed in Figure 3. There is a general factor among all the treatments, which distinguishes them from the resting state, and a notable group factor among three of them. According to these data, a mental test seems to induce the same physiological state as plunging one's foot into ice water!

Much larger bodies of data are of course needed to map the treatment space properly. But the aptness of an attempt in this direction will be apparent to all who heard Selye's address to the APA last year. His argument (40) that all stressful situations lead to a similar syndrome of physiological changes is strongly reminiscent of Spearman's argument regarding a general factor linking intellectual responses. The disagreement between Selye and other students of stress clearly reduces to a quantitative question of the relative size of specific and nonspecific or general factors in the effects of typical stressors.

APPLIED PSYCHOLOGY DIVIDED AGAINST ITSELF

Let us leave for the moment questions of academic psychology and consider the schism as it appears in applied psychology. In applied psychology, the two disciplines are in active conflict; and unless they bring their efforts into harmony, they can hold each other to a standstill. The conflict is especially obvious at this moment in the challenge the young engineering psychology offers to traditional personnel psychology.

The program of applied experimental psychology is to modify treatments so as to obtain the highest average performance when all persons are treated alike—a search, that is, for "the one best way." The program of applied correlational psychology is to raise average performance by treating persons differently—different job assignments, different therapies, different disciplinary methods. The correlationist is utterly antagonistic to a doctrine of "the one best way," whether it be the heartless robot-making of Frederick Taylor or a doctrinaire permissiveness which tries to give identical encouragement to every individual. The ideal of the engineering psychologist, I am told, is to simplify jobs so that every individual in the working population will be able to perform them satisfactorily, i.e., so that differentiation of treatment will be unnecessary. This goal guides activities ranging from the sober to the bizarre: from E. L. Thorndike and Skinner, hunting the one best sequence of problems for teaching arithmetic, to Rudolf Flesch and his admirers, reducing *Paradise Lost* to a comic book. If the engineering psychologist succeeds: information rates will be so reduced that the most laggard of us can keep up, visual displays will be so enlarged that the most myopic can see them, automatic feedback will prevent the most accident-prone from spoiling the work or his fingers.

Obviously, with every inch of success the engineer has, the tester must retreat a mile. A slight reduction in information rate, accomplished once, reduces forever the validity and utility of a test of ability to process data. If, once the job is modified, the myopic worker can perform as well as the man with 20/20 vision, Snellen charts and orthoraters are out of business. Nor is the threat confined to the industrial scene. If tranquilizers make everybody happy, why bother to diagnose patients to determine which treatments they should have? And if televised lessons can simplify things so that every freshman will enjoy and understand quantum mechanics, we will need neither college aptitude tests nor final examinations.

It is not my intention to warn testers about looming unemployment. If test technology is not greatly improved, long before the applied experimentalists near their goals, testing deserves to disappear. My message is my belief that the conflicting principles of the tester and the experimenter can be fused into a new and integrated applied psychology.

To understand the present conflict in purposes, we must look again at historical antecedents. Pastore (36) argues with much justice that the testers and classifiers have been political conservatives, while those who try to find the best common treatment for all—particularly in education—have been the liberals. This essential conservatism of personnel psychology traces back to the days of Darwin and Spencer.

The theory of evolution inspired two antagonistic movements in social thought (10, 42). Darwin and Herbert Spencer were real determinists. The survival of the fittest, as a law of Nature, guaranteed man's superiority and the ultimate triumph of the natural aristocrats among men. As Dewey put it, Spencer saw "a rapid transit system of evolution

. . . carrying us automatically to the goal of perfect man in perfect society" (17, p. 66). Men vary in their power of adaptation, and institutions, by demanding adaptation, serve as instruments of natural selection among men. The essence of freedom is seen as the freedom to compete for survival. To Spencer, to Galton, and to their successors down to the present day, the successful are those who have the greatest adjustive capacity. The psychologist's job, in this tradition, is to facilitate or anticipate natural selection. He seeks only to reduce its cruelty and wastage by predicting who will survive in schools and other institutions as they are. He takes the system for granted and tries to identify who will fit into it. His devices have a conservative influence because they identify persons who will succeed in the existing institution. By reducing failures, they remove a challenge which might otherwise force the institution to change (49).

The experimental scientist inherits an interpretation of evolution associated with the names of Ward, James, and Dewey. For them, man's progress rests on his intelligence; the great struggle for survival is a struggle against environment, not against competitors. Intelligent man must reshape his environment, not merely conform to it. This spirit, the very antithesis of Spencerian laissez-faire, bred today's experimental social science which accepts no institution and no tradition as sacred. The individual is seen as inherently self-directing and creative. One can not hope to predict how he will meet his problems, and applied differential psychology is therefore pointless (39, p. 37).

Thus we come to have one psychology which accepts the institution, its treatment, and its criterion and finds men to fit the institution's needs. The other psychology takes man—generalized man—as given and challenges any institution which does not conform to the measure of this standard man.

A clearer view of evolution removes the paradox:

> The entire significance of the evolutionary method in biology and social history is that every distinct organ, structure, or formation, every grouping of cells or elements, has to be treated as an instrument of adjustment or adaptation to a particular environing situation. Its meaning, its character, its value, is known when, and only when, it is considered as an arrangement for meeting the conditions involved in some specific situation (16, p. 15).

We are not on the right track when we conceive of adjustment or adjustive capacity in the abstract. It is always a capacity to respond to a particular treatment. The organism which adapts well under one condition would not survive under another. If for each environment there is a best organism, for every organism there is a best environment. The job of applied psychology is to improve decisions about people. The greatest social benefit will come from applied psychology if we can find for each individual the treatment to which he can most easily adapt. This calls for the joint application of experimental and correlational methods.

INTERACTION OF TREATMENT AND INDIVIDUAL IN PRACTICAL DECISIONS

Goldine Gleser and the writer have recently published a theoretical analysis (11) which shows that neither the traditional predictive model of the correlator nor the traditional experimental comparison of mean differences is an adequate formulation of the decisions confronting the applied psychologist. Let me attempt to give a telescoped version of the central argument.

The decision maker has to determine what treatment shall be used for each individual or each group of individuals. Psychological data help a college, for example, select students to be trained as scientists. The aim of any decision maker is to maximize expected payoff. There is a payoff function relating outcome (e.g., achievement in science) to aptitude dimensions for any particular treatment. Figure 4 shows such a function for a single aptitude. Average payoff—if everyone receives the treatment—is indicated by the arrow. The experimentalist assumes a fixed population and hunts for the treatment with the highest average and the least variability. The correlationist assumes a fixed treatment

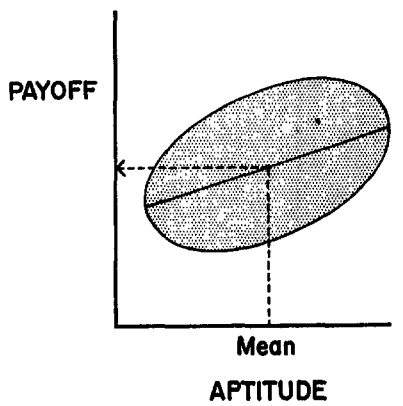

FIG. 4. Scatter diagram and payoff function showing outcome as a function of individual differences.

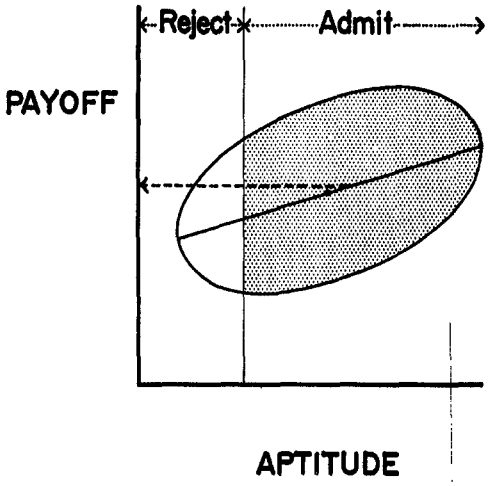

FIG. 5. Increase in payoff as a result of selection.

and hunts for aptitudes which maximize the slope of the payoff function. In academic selection, he advises admission of students with high scores on a relevant aptitude and thus raises payoff for the institution (Figure 5).

Pure selection, however, almost never occurs. The college aptitude test may seem to be intended for a selection decision; and, insofar as the individual college is concerned only with those it accepts, the conventional validity coefficient does indicate the best test. But from a societal point of view, the rejects will also go on into other social institutions, and their profit from this treatment must be weighed in the balance along with the profit or social contribution from the ones who enter college. Every decision is really a choice between treatments. Predicting outcome has no social value unless the psychologist or the subject himself can use the information to make better choices of treatment. The

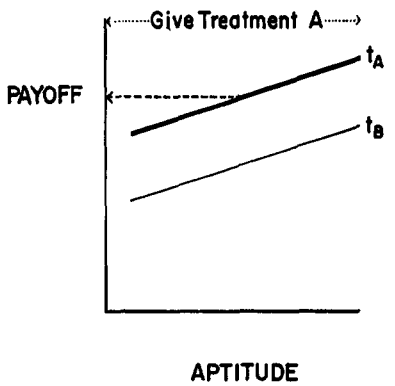

FIG. 6. Payoff functions for two treatments.

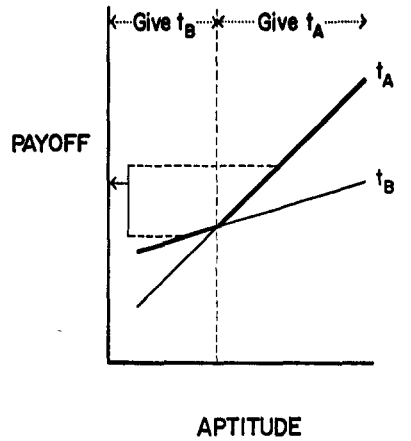

FIG. 7. Payoff functions for two treatments.

prediction must help to determine a treatment for every individual.

Even when there are just two treatments, the payoff functions have many possible relationships. In Figure 6 we have a mean difference between treatments, and a valid predictor. The predictor—though valid—is useless. We should give everyone Treatment A. In Figure 7, on the other hand, we should divide the group and give different treatments. This gives greater payoff than either treatment used uniformly will give.

Assigning everyone to the treatment with the highest average, as the experimentalist tends to recommend, is rarely the best decision. In Figure 8, Treatment C has the best average, and we might assign everyone to it. The outcome is greater, however, if we assign some persons to each treatment. The psychologist making an experimental comparison arrives at the wrong conclusion if he ignores the aptitude variable and recommends C as a standard treatment.

Applied psychologists should deal with treatments and persons simultaneously. Treatments are characterized by many dimensions; so are persons. The two sets of dimensions together determine a payoff surface. For any practical problem, there is some best group of treatments to use and some best allocation of persons to treatments. We can expect some attributes of persons to have strong interactions with treatment variables. These attributes have far greater practical importance than the attributes which have little or no interaction. In dividing pupils between college preparatory and non-college studies, for example, a general intelligence test is probably the wrong thing to use. This test,

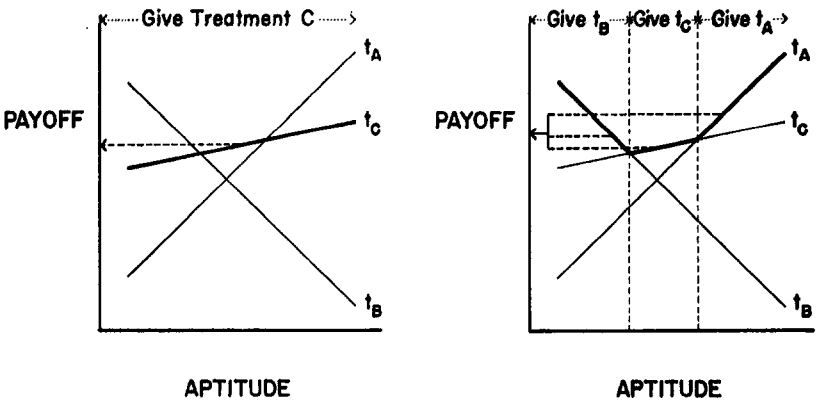

FIG. 8. Payoff functions for three treatments.

being general, predicts success in all subjects, therefore tends to have little interaction with treatment, and if so is not the best guide to differential treatment. We require a measure of aptitude which predicts who will learn better from one curriculum than from the other; but this aptitude remains to be discovered. Ultimately we should *design* treatments, not to fit the average person, but to fit groups of students with particular aptitude patterns. Conversely, we should seek out the aptitudes which correspond to (interact with) modifiable aspects of the treatment.

My argument rests on the assumption that such aptitude-treatment interactions exist. There is, scattered in the literature, a remarkable amount of evidence of significant, predictable differences in the way people learn. We have only limited success in predicting which of two *tasks* a person can perform better, when we allow enough training to compensate for differences in past attainment. But we do find that a person learns more easily from one *method* than another, that this best method differs from person to person, and that such between-treatments differences are correlated with tests of ability and personality. The studies showing interaction between personality and conditions of learning have burgeoned in the past few years, and the literature is much too voluminous to review in passing. Just one recent finding will serve in the way of specific illustration, a study done by Wolfgang Böhm at Vienna (38, pp. 58–59). He showed his experimental groups a sound film about the adventures of a small boy and his toy elephant at the zoo. At each age level, a matched control group read a verbatim text of the sound track. The differences in average comprehension between the audiovisual and the text presentations were trivial. There was, however, a marked interaction. For some reason yet unexplained, a general mental test correlated only .30 with text learning, but it predicted film learning with an average correlation of .77.[3] The difference was consistent at all ages.

Such findings as this, when replicated and explained, will carry us into an educational psychology which measures readiness for different types of teaching and which invents teaching methods to fit different types of readiness. In general, unless one treatment is clearly best for everyone, treatments should be differentiated in such a way as to maximize their interaction with aptitude variables. Conversely, persons should be allocated on the basis of those aptitudes which have the greatest interaction with treatment variables. I believe we will find these aptitudes to be quite unlike our present aptitude measures chosen to predict differences *within* highly correlated treatments.

THE SHAPE OF A UNITED DISCIPLINE

It is not enough for each discipline to borrow from the other. Correlational psychology studies only variance among organisms; experimental psychology studies only variance among treatments. A united discipline will study both of these, but it will also be concerned with the otherwise neglected interactions between organismic and treatment variables (41). Our job is to invent constructs and to form a network of laws which permits prediction. From observations we must infer a psychological description of the situation and of the present state of the organism. Our laws should permit us to predict,

[3] Personal communication.

from this description, the behavior of organism-in-situation.

There was a time when experimental psychologists concerned themselves wholly with general, nonindividual constructs, and correlational psychologists sought laws wholly within developmental variables. More and more, nowadays, their investigations are coming to bear on the same targets. One psychologist measures ego involvement by a personality test and compares the behavior of high- and low-scoring subjects. Another psychologist heightens ego involvement experimentally in one of two equated groups and studies the consequent differences in behavior. Both investigators can test the same theoretical propositions, and to the extent that their results agree they may regard both procedures as embodiments of the same construct.

Constructs originating in differential psychology are now being tied to experimental variables. As a result, the whole theoretical picture in such an area as human abilities is changing. Piaget (37) correlates reasoning processes with age and discovers a developmental sequence of schemata whose emergence permits operational thought; Harlow (24) begins actually to create similar schemata in monkeys by means of suitable training. It now becomes possible to pursue in the controllable monkey environment the questions raised by Piaget's unique combination of behavioral testing and interviewing, and ultimately to unite the psychology of intelligence with the psychology of learning.

Methodologies for a joint discipline have already been proposed. R. B. Cattell (8) has offered the most thorough discussion of how a correlationist might organize data about treatment and organism simultaneously. His factor analytic procedures are only one of many choices, however, which modern statistics offers. The experimenters, some of them, have likewise seen the necessity for a united discipline. In the very issue of *Psychological Review* where the much-too-famous distinction between *S-R* and *R-R* laws was introduced, Bergmann and Spence (3) declared that (at the present stage of psychological knowledge) the equation $R = f(S)$ must be expanded into

$$R = f(S, T, D, I)$$

The added variables are innate differences, motivation, and past experience—differential variables all. Hull (26, 27) sought general laws just as did

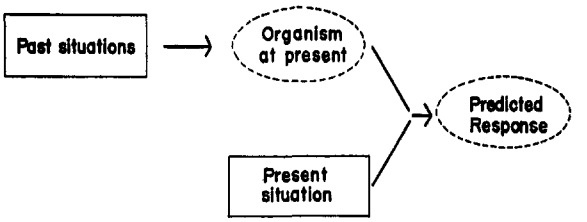

FIG. 9. Theoretical model for prediction from historic data.

Wundt, but he added that organismic factors can and must be accounted for. He proposed to do this by changing the constants of his equations with each individual. This is a bold plan, but one which has not yet been implemented in even a limited way. It is of interest that both Hull (27, p. 116) and Tolman (47, p. 26) have stated specifically that for their purposes factor analytic methods seem to have little promise. Tucker, though, has at least drawn blueprints of a method for deriving Hull's own individual parameters by factor analysis (48). Clearly, we have much to learn about the most suitable way to develop a united theory, but we have no lack of exciting possibilities.

The experimenter tends to keep his eye on *ultimate* theory. Woodworth once described psychological laws in terms of the *S-O-R* formula which specifically recognizes the individual. The revised version of his *Experimental Psychology* (53, p. 3), however, advocates an *S-A-R* formula, where *A* stands for "antecedent conditions." This formulation, which is generally congenial to experimenters, reduces the present state of the organism to an intervening variable (Figure 9). A theory of this type is in principle entirely adequate to explain,

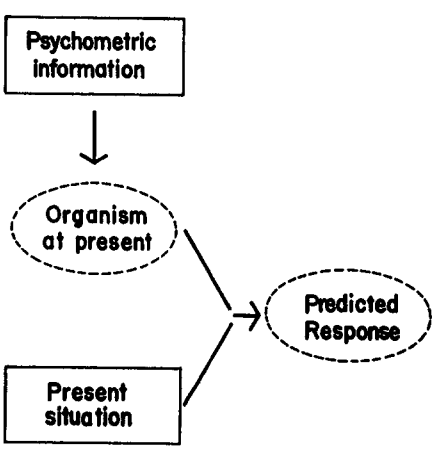

FIG. 10. Theoretical model for prediction from ahistoric data.

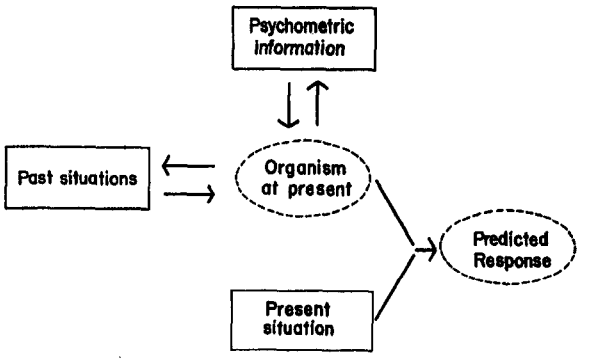

FIG. 11. Theoretical network to be developed by a united discipline.

predict, and control the behavior of organisms; but, oddly enough, it is a theory which can account only for the behavior of organisms of the next generation, who have not yet been conceived. The psychologist turns to a different type of law (Figure 10) whenever he deals with a subject whose life history he has not controlled or observed in every detail. A theory which involves only laws of this type, while suitable for prediction, has very limited explanatory value. The theory psychology really requires is a redundant network like Figure 11. This network permits us to predict from the past experience or present characteristics of the organism, or a combination of the two, depending on what is known. Filling in such a network is clearly a task for the joint efforts of experimental and correlational psychology.

In both applied work and general scientific work, psychology requires combined, not parallel, labors from our two historic disciplines. In this common labor, they will almost certainly become one, with a common theory, a common method, and common recommendations for social betterment. In the search for interactions we will invent new treatment dimensions and discover new dimensions of the organism. We will come to realize that organism and treatment are an inseparable pair and that no psychologist can dismiss one or the other as error variance.

Despite our specializations, every scientific psychologist must take the same scene into his field of vision. Clark Hull, three sentences before the end of his *Essentials of Behavior* (27, p. 116), voiced just this need. Because of delay in developing methodology, he said, individual differences have played little part in behavior theory, and "a sizeable segment of behavioral science remains practically untouched." This untouched segment contains the question we really want to put to Nature, and she will never answer until our two disciplines ask it in a single voice.

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