doldrums. But all this suddenly changed in 1932 with the discovery of the clinical usefulness of sulfanilamide. The importance of a close collaboration between laboratory and clinic was reemphasized. Sulfanilamide and intriguing discoveries in the field of endocrinology have unveiled new horizons. The sky seems to be the limit. Scientists are tackling problems of disease, such as the cause of cancer and the prolongation of life, with not only zest and enthusiasm but confident expectation that the achievements of the past are but a minor prelude to discoveries that will transform the whole panorama of life itself.

The enactment of the New Drug Section of the Food, Drug and Cosmetic Act has provided a powerful impetus to drug research. For the first time in our national history a thorough investigation of the safety of drugs before they are marketed has been made compulsory. Thousands of drug investigations are now being conducted where previously there was only a handful. Prominent pharmacologists tell me that this law has provided an incalculable stimulus to a science that was once regarded by some as a sterile cross between physiology and chemistry. Time will show, I believe, that the new drug section of the act is one of the most significant things that has ever happened to drug research in this country and the legitimate drug industry will be among the principal benefactors of its wholesome influence.

There will always be a drug industry and individual drug manufacturers. But some manufacturers will develop and prosper; others will decline. Some will see more clearly than others that the future of their companies rests more than anything else upon the research that is stimulated by them. Others will recognize, as some already have, that we can't make much progress if we have only one, two or three bright young chemists shut up in a laboratory, puttering along on problems that are as vital as health and life, and at the same time somewhere else in another laboratory, one or two other chemists going over exactly the same ground, as out of touch with the first group as if they were working on another planet. The sooner we realize that the day of the brilliant individual investigator working alone in a hermetically sealed compartment is gone forever, the sooner will we solve problems that are far more important than the tensile strength of steel, nylon stockings, synthetic rubber or a horsepower per pound of airplane engine.

The problems of drug research are more complex than they used to be. Progress in the future will come increasingly from the collaborative efforts of groups of individuals, working under the leadership of those who have imagination and minds fertile with ideas. The brilliant investigator is indispensable, but he must have the tools to work with and the help of assistants who will act as test pilots for his ideas. There must also be a harmonious integration of the work of chemists, physicists, physiologists, pharmacologists and clinicians to produce results. I think it is about time that medicine and the drug industry gave up its small-time amateurish attempts at drug research. I think we should go to the du Pont Company, the United States Steel Corporation, the General Electric Company and the Firestone Rubber Company and see how they tackle their research problems. We must enlist the brains, the imagination and the ingenuity of thousands of chemists, physicists, pharmacologists and clinicians to solve these important problems of life and health. They are the problems that count, for without a long life and health, it really doesn't matter much whether we have nylon stockings or synthetic rubber or stratosphere planes or anything else.

ASPECTS OF MODERN PSYCHOLOGY. II

By Dr. CHARLES S. MYERS

ENGLAND

LET us now return to the fate of the psychology founded by Wundt which directly concerned itself in observing mental experience and in reducing it to its elementary terms of sensation and feeling. His former pupil, Külpe, met with Wundt's violent opposition when at Wurzburg he began to study experimentally and introspectively the processes of thought, paying particular attention to the acutely living *acts* of judging, valuing, denying, etc., and not only to the relatively lifeless *stuff*—"bundles" of sensation, percepts, images and thoughts. In Paris, Binet had already detected the occurrence of thinking without images, verbal or concrete. Külpe's school also insisted on introspective grounds that meaning was possible in the absence of images (and hence of sensations). Wundt protested that such inquiries were beyond the scope of introspection, and Titchener, endowed with vivid imagery, maintained that anyhow introspection in Külpe's school must be defective, as he himself could always detect kinaesthetic imagery in all meaning. Meaning, he said, is invariably "context"; it involves a bodily attitude of the individual facing the situation; and psychologically meaning *is* the characteristic kinaesthetic experience aroused by that bodily attitude. Few psychologists will now insist that meaning must have a sensational (or imaginal) basis, or that thought must always have imagery as its vehicle.

Equally important was the experimental evidence adduced by Külpe's school in favor of the wide influence of the "determining tendency" and "attitude" which, once set up, influence the nature and direction of future mental processes. We try in vain to recall a forgotten name: this effort evokes a "determining tendency" that continues to act unconsciously until, it may be, more favorable conditions being established, the forgotten name arises unbidden to consciousness. Or again, when the figures, say, 3 and 9, are presented to us, we may by "chance" association at once think of 12 or 27 or 6 or 3; but if previous instruction has given our minds a "set" in the direction of addition or multiplication or subtraction or division, an "attitude" has been formed which will result not in *any one* of these numbers, but in the *appropriate one* of them, at once appearing in consciousness.

A determining tendency is something very different from an associative tendency. It is a purposive perseveration, a goal-seeking drive, capable of using alternative mechanisms for procuring its end, whereas an association is a mere coupling or mechanical connection, of variable strength, rigidly uniting the terms *a* and *b* associated. Obviously, as has been urged already, mere association strength is not sufficient to explain the results of mental activity in daily life. Repetition of *a* and *b* merely increases the strength of such connection, whereas with purposive activity and, in particular, the determining tendency (to use an example of Lewin's, where *a* is the desire to post a letter, and *b* is the sight of a mail box) the connection virtually or wholly ceases when once the goal has been achieved.

But it would be absurd to assert that association plays little or no part in the operations of the mind. Yet this of late has been the tendency of certain schools of psychologists who have rightly recognized its limitations. Laboratory experiments with senseless syllables show irrefutably that under these abstract, artificial conditions the process of association takes place in learning them and indeed that the strength of association and perseveration are the principal factors determining their recall. Under more natural conditions, however, when sensible material has to be learnt, the mechanical process of association is overshadowed; but even here it becomes more prominent, the simpler and the poorer in meaning be the material that has to be learnt, the simpler be the mind of the learner, or the less liable be what has been learnt to the distortions of interest and to the perspective influences of what F. C. Bartlett has termed "schemata."

It is similarly absurd to condemn sensations to nonexistence in a diatribe against the dangers of the crude mosaic atomism to which I have already directed attention. We know perfectly well from experience what a fairly pure sensation is and how it differs from a perceived object. We may be ready to admit that percepts are not built up mosaically out of sensations, that, on the contrary, vaguely meaning percepts came first in the developing experience of the infant and that pure sensations came later to be differentiated from them by abstraction. But it would be ridiculous to deny that mental life is served by integration as well as by differentiation. I recall the experience of an artist friend of mine who was painting a street in the bazaars of Cairo. Behind him stood a crowd of *fellahin,* gaping in astonishment at his work. At length he heard one of them, presumably of higher intelligence than the rest, slowly say, "That's a door, that's a window, there's a roof," and suddenly, "Why it's a house!"

In rebellion against the extravagances of mosaic atomism and of associationism, nevertheless, and welcoming introspection, the *Gestalt school* was founded by Wertheimer, with the energetic support of Köhler, Koffka and Lewin, all four of whom have found a happy refuge in the United States of America from recent German intolerance. The importance of form, shape and pattern has also been incorporated into Krüger's school of *Entwicklungspsychologie,* succeeding Wundt's, at Leipzig. The *Gestalt* psychologists have studied the laws determining the forms, and the stability and changeability of the forms, of optical figures, and the relation between the figure and the ground from which it stands in relief or in which it may be almost unrecognizably imbedded; and they have applied these laws also to conditions affecting certain intellectual and conative processes. They have studied the occurrences and the dynamics of what they interpret as *Gestalten* in the physical field; they have claimed to find analogies between these *Gestalten* and those in the psychological field; and indeed some have tried to trace physiologically corresponding, "isomorphic" patterns (with their stresses, tensions and pictorial vectors) within the sensory and nervous systems (in terms of electric potential or osmotic pressure)—a psycho-physical parallelism in *Gestalten* which it is hard to accept in any strict sense when meanings, values, traits, drives, purposes and personalities are considered. Stern, suspicious of a repetition of a mosaic of entities, here of *Gestalten,* once criticized this school in the words "Kein Gestalt ohne Gestalter." But of late Lewin, one of its leaders, has been busy trying to "patternize" personality itself, and has been likewise treating the environment in a "topological" way, introducing an applied "mathematical topology" and endeavoring apparently by a development of *Gestalt* and dynamic psychology to found a school of *topological psychology.*

Like others who have become habituated to any one outlook, the *Gestalt* school tended at first to belittle every other outlook and to bring within its ambit too much that would more readily and with greater likelihood be explicable in other terms. To study patterns already formed need not involve neglect of, or contempt for, the study of the parts apart from their combination. It may well be, as the *Gestalt* school urges, that the true significance of any part of a whole can not be realized until the properties of the whole are understood. And it is unquestionable that errors of interpretation ensue from the study of abstract "vivisected" parts of the whole mental (as of the nervous) system. But whatever the size of the whole that is investigated, it must in practice be only a part abstracted from a still larger whole.

Lewin does not share the view that psychology need use the same dynamic concepts as physics uses, nor that all psychological explanations need rest ultimately on physical facts; he does not see the necessity for accepting "the philosophical Utopia of a single universal science." The "topological" concepts which he introduces in the form of spaces, boundaries, distances and directions are mathematical, not physical in nature. They represent, he tells us, an attempt to treat psychology by means of applied mathematics, using a concept of space (developed by mathematics from the part-whole relationship) which is by no means identical with physical space—thus providing a visual and dynamic representation of behavior which is for him a function both of the person and of his environment. "Topological" space implies that we are dealing with mathematical relationships which can be characterized without measurement.

Whether we can by Lewin's methods replace anthropomorphic explanations by purely mathematical concepts and relations of this attractive and promising kind must be left to the future. The problem is conceived in a clearly different spirit from that which in physics causes controversy between experimental and mathematical physics, and from that which faces the measurement of psychical processes (*e.g.,* of sensation by Fechner and of abilities by deviations from the average scores at mental tests) and the determination of unit mental abilities or qualities by the mathematical methods of factorial analysis. There is a general agreement that sensations can not be measured, at all events in the same sense as physical objects; and that Fechner's law can only be reached by the aid of mathematical operations which treat the symbols in utter disregard of what they psychologically stand for. The psychological standpoint is deliberately discarded during the application of such mathematical methods, and thus the resulting formula, though not without its uses, is not without its difficulties.

The labors of the *schools of factorial analysis,* especially of Spearman, Thomson, Stephenson and Burt in Great Britain and of Thorndike, Thurstone, Kelley, Hotelling and others in America, will undoubtedly bear useful fruit. But the concept of mental factors, like that of faculties long ago, needs to be carefully watched. Can the mind be in complete truth conceived as having a structure of ultimately independent unit factors, some "general" to a very large number of mental processes, others perhaps common to a comparatively small "group," others "specific" to particular mental processes? Is character, for instance, to be conceived as an aggregate of separate traits and other unit factors? Are the units independent of the whole? Has the whole no influence over the complexion of its parts?

When in factorial analysis we determine the correlation between any two abilities, we are correlating the scores made at pairs of tests, that is to say measurements of *behavior,* of outward expression or movement. How can we legitimately pass from this to the correlation between *psychological* processes? Any one mental test (like any complex muscular action) can be performed by any one person now in one way, now in another, using different mental abilities (as, in the case of complex movement, using different muscles) to achieve the same end. And when the factors have been mathematically analyzed, what is their *psychological* nature? This can only be guessed at, and the guesses confirmed by their proven utility. Then, again, what psychological warrant have we for the various mathematical steps we take in factorial analysis? Have they at each stage strictly psychological meaning, or are we neglecting this for purely statistical operations which end with results that relate to the average, but have no precise reference to any individual member, of the group? And yet in some general way, for broad common traits and for the total group, factorial analysis, like the enumeration of a few common instincts, proves useful and deserving of encouragement, although it must fail for the unique characters of the personality of the individual. In this direction there is greater promise in a variant of factorial analysis which has been specially urged by W. Stephenson and C. Burt. Here pairs of tests applied to a group of persons are replaced by a group of tests applied to pairs of persons.

Factorial analysis has already indicated the likely truth that special abilities are less highly differentiated from one another in early childhood than at adolescence, thus providing analogy with the view, supported by experimental evidence, that in the developing organism the central nervous system is originally undifferentiated in function as well as being relatively structureless. As Coghill has stated, "dominant organic unity," "undifferentiated reactions of the whole organism," are present from the beginning: the form of the pattern of behavior is not "simply a combination or coordination of reflexes" originally isolated from each other.

One outstanding difficulty is that the number of factorial units analyzed from any one set of data depends on the nature of the mathematical operations employed. Herein the various schools of factorial analysis at present differ, reaching interpretations of any specific investigation as different from one another as the schools of Freud, Jung and Adler in their analysis of the role of the unconscious would reach in any particular case. For Spearman factorial analysis yields each time but two factors—a "general" factor known in some circumstances as general intelligence and a "specific" factor peculiar to each ability. Associated with this finding has developed his *neo-genetic school,* which refuses any countenance to *Gestalt* principles and spreads its net far more widely than can be suitably covered by its two most important principles—the "eduction of relations" and the "eduction of correlates." Their value is particularly evident in clarifying the definition of general intelligence.

Wherever we turn, whatever methods we consider, it would seem that both the total wholes and the component parts require appropriate study in order to arrive at a true and complete psychology. Once again, we are forced to the conclusion that psychology needs to be studied not only from the mathematical and the mechanistic but also from the humanistic and teleological standpoints, and alike from the introspective, behavioristic and *Gestalt* standpoints, according to the purpose which the study is intended to serve and the conditions under which the study is undertaken.

At a time when physicists are complaining that they do not know now what mechanism means nor what matter means, when many of them realize that what "scientific" or mathematical investigation has to say about the universe represents by no means all that is significant about it, when biologists recognize self-conservation—the struggle of organisms for their existence—and are no longer confident about the blindness of evolution, it would be rash to condemn any standpoint or any school of psychology among those we have examined. We may justly complain that any single current concept, *e.g.*, that of the reflex, of association, of *Gestalt,* or of factors, is inadequate, and that broader or multiple concepts are desirable. At the present time we observe a growing tendency of these schools to welcome each other's features that can usefully blend together. Orderliness and teleology are not inconsistent with one another: generalization and individuation are of equal importance. We have good reason, in view of the many-sidedness of psychology, to welcome, not to deride or to suspect, the active energy displayed by its various schools—so long as intolerance, injustice and the other evils common to dictatorship and totalitarianism are avoided. We have only to look back a half-century to realize the enormous strides that psychology has made, in refinement and expertness of introspection, observation and interpretation, in delicacy of discriminating terminology, in the conception of the unconscious, in the application of mathematical methods, diversity of aims, concepts and methods of approach and in the rich harvest that has resulted from the uses of psychology as an applied science.

OBITUARY

EUGENE DAVENPORT

EUGENE DAVENPORT was born in Woodland, Michigan, on June 20, 1856, and died in his old home on March 31, 1941. His parents were pioneers and the boy was brought up under pioneer conditions. While helping his father on the farm as a lad he made up his mind to get a college education. In due time he entered the Michigan State College of Agriculture, receiving the B.S. degree in 1878. This was followed by the M.S. in 1884, the M.Agr. in 1895 and the LL.D. in 1907. In 1920 Iowa State College conferred on Dean Davenport the LL.D. degree, as did the University of Kentucky in 1913 and the University of Illinois in 1931.

The ten years immediately following his graduation from college were spent in operating the home farm. In 1881 he married Emma J. Coats. They had two children, one of whom died in infancy. In 1888 he was appointed assistant botanist at the Michigan Agricultural College and Experiment Station and the following year was made professor of practical agriculture and superintendent of the college farm, a position which he held for two years. The year 1891–92 was spent in São Paulo, Brazil, as president of the Collegio Agronomica. Owing to the failure of that institution to receive government support, Dr. Davenport returned to the United States and in 1895 and 1896 was appointed dean of the College of Agriculture and then director of the Experiment Station of the University of Illinois. Here his great career really began. His task was to build a college and put agricultural education on a college level. In order to accomplish that purpose it was necessary to change public sentiment in the state.

When Dean Davenport went to Illinois there was a college of agriculture only in name. It was not that there was no work in agricultural education being done, but it was on a low level and wholly unsys-