graph through suitable amplifiers, or photographically with a string galvanometer, the exact moment of the exposure of the plate, and from the plate itself one could calculate with sufficient accuracy the exact distance of the aeroplane from the true starting or finishing post. This, however, considerably increases the cost of the required apparatus.

Mr. Capon has also patented (see Patent No. 279,959) an alternative method which is capable of very great accuracy. In this method a camera is placed at each observation post with the lens

pointed normally to the course. A film is moved at constant speed in each camera across a narrow slot. In this way a continuous photograph is obtained of a narrow strip defining the starting (or finishing) plane. Any object crossing the plane is therefore recorded, more or less distorted, however, depending on its speed and that of the film. The films can be synchronised and marked with regular time intervals by means of sparks in spark-gaps near the slots, or by other suitable means.

Experimental Method in Psychology.¹

By F. C. BARTLETT.

THE position which psychology occupies to-day in relation to the other biological sciences, both theoretical and applied, is due directly to its adoption of experimental method. Yet if the psychologist is asked to point out any single unshakable discovery of first-rate psychological importance, based directly and wholly upon experiment, his attempts to answer the question are always regarded as unsatisfactory.

As is known, the earliest experimentalists in psychology were physicists and physiologists, most of them with a strong bent towards philosophy in their outlook. They set up a standard which in various ways has cramped and confined experimental psychology ever since. When a physicist approaches a problem in which he has to state how a stimulus affects any kind of response, he is bound to lay the burden of explanation upon the stimulus. When the physiologist approaches the same type of problem, his emphasis is equally bound to be mainly upon the response side of the relationship. This he perfectly legitimately isolates so far as possible, trying to state its characteristics in terms of the functions of the mechanism immediately concerned.

These are precisely the type of problem that the psychologist has most often attempted to carry over into his own field. Let us consider one point which grew out of the early experimental work on psychophysical methods. Fechner believed that all the content of experience, from the relatively simple sensation to the most complex reasoning content, was definitely measurable. Mainly as a result of his own observations, but aided also by the earlier work of Weber, he thought he could demonstrate this experimentally by showing that stimulus intensities and sensation intensities are related by a definite principle, the value of the unit sensation varying with the modality of stimulation. In order to establish the zero point for a sensation of given mode, and also the series of just noticeable differences piled up from this point by increase of the stimulus, he carried out a wonderful and patient series of experiments. He employed and somewhat improved Weber's method of 'limits'; he developed the method of 'mean error', but he relied mainly upon the method, already proposed by

Vierordt, of 'right and wrong' cases. This raised the problem of the doubtful judgments.

A controversy grew up which has proceeded voluminously ever since, though at the moment it seems nearly to have worn itself out. Broadly, four plans for dealing with these 'doubtful' judgments have been proposed and followed:

(a) to divide them equally between 'right' and 'wrong' cases;

(b) to ignore them;

(c) definitely to instruct the observer to 'guess' when he is uncertain and then, if doubt still persists, to lump all cases together with 'guesses of greater' and 'guesses of less' as constituting an area within which stimulus variations have no corresponding sensation differences;

(d) to prohibit the observer from being doubtful.

All these devices ignore the very points which are psychologically the most interesting and important. First they tend to treat each judgment in the series as equally and independently significant, the function of the immediate stimulus. This is certainly wrong. Secondly, no judgment of this type is the expression of a simple stimulus-response situation, but of a stimulus-attitude-response situation. To demand guesses and to prohibit doubt both alike determine an *attitude* of observation which spreads over the whole experimental situation, affecting judgments which are assigned certainty just as much as the others.

When an observer enters into an experimental situation, he brings with him propensities, tendencies, preformed organised systematic modes of response, the preformed cumulative organised effect of a mass of past discriminations. The stimulus, the situation that is presented, hits off some of these. They appear in him as an 'attitude', and it is under the active control of this that he makes his responses. Only when we know more about how this is set up and about its precise effect upon the responses made, can we safely give to the latter the necessary weighting which makes their statistical treatment genuinely significant. I think that the psychophysical methods, studied from this point of view, will yet yield some enormously important results.

If the physicist in his approach to the stimulus-reaction type of problem tends to treat the stimulus regarded objectively as the main point of interest,

¹ From the presidential address to Section J (Psychology) of the British Association, delivered at Cape Town on July 26.

the physiological method of approach is equally bound to concentrate its attack upon the immediate functional mechanism. From the earliest days up to the present a great part, perhaps the greatest part, of experimental psychology has been concerned with special sense reactions. Yet the psychologist has never staked out clearly any mode of investigation or characteristic problems which are specially his own in this field. In this he has again been over-influenced by the great founders of his science.

Yet it is not the psychologist's view of the problem, but his handling of it, that has been faulty. Suppose we are investigating some specific sensory threshold. The response of our observer will be determined by a number of groups of factors. There are the physical characters of the stimulus: its intensity defined physically, its duration, often its medium of conduction to the sense organ concerned, relevant facts of its physical and chemical structure. There is the absolute sensitivity of the local responding physiological system, though this can never be absolutely measured short of cutting it out of its organic setting, which is just what the psychologist must not do, though the physiologist certainly may. There is the order of presentation of the stimulus in its series, which raises problems already touched upon in connexion with the psychophysical methods; and the correlated physiological questions of the state of adaptation at the moment of the sensory system.

Operating over and above and through all of these are the tendencies, attitudes, moods, intellectual and emotional habits of the observer, the states variously characterised as states of confidence, hesitation, doubt, timidity, assertiveness, certainty. An image, flashing out suddenly at a given point, may change the whole character of a response and of succeeding responses. So may the verbalisation or formulation of a judgment.

These so-called higher mental processes are precisely the psychologist's main concern. They can be experimentally set up and controlled to a large extent, while the other factors are kept relatively constant. It is our business to show how they are set up, and how they then powerfully determine reactions within the special sense field, even in the simplest cases of reaction. It is of no use simply to say that there *are* these determinants, and then to take refuge in a fruitless hypothetical physiology of the central nervous system. Perhaps the time may come when the bulk of sense psychology will be swallowed up in physiology, but that time has not come yet, and if we act as if it has we gain little but deserved suspicion from other scientific workers.

Already there are a few experimental studies of special sense problems from this point of view. They are less frequent and less thorough than they should be, and this is because the psychologist has generally been content to follow his more physically and physiologically minded predecessors, instead of envisaging the problems of sensory reaction for himself. When the psychologist studies a special sense response, it is his business to try to show how

that response, carried out in its normal organic setting, is being determined directly by facts other than those of the immediate sensory mechanisms. Cues other than those of the special sensory stimulus are operating through response systems of a higher, or more complex, order than those of the directly excited sense. For these response systems at present psychological names have to be used, and we have to show how they come into play and what they do.

It should therefore be particularly interesting to turn to the experimental attack upon the higher mental processes. I propose to take as typical the study of the responses called 'remembering' and 'recognising'.

The experimental investigation of memory is dominated by the work of one man who is commonly supposed to have been a great benefactor of experimental psychology, but who, in spite of his impressive work, seems to me to be the errant leader of a very sheep-like flock. In 1885 Hermann Ebbinghaus published his new programme for experiments on memory processes. Already, since 1879, he had been studying his own modes of recall by methods which were at that time novel. The publication of his results settled the direction of flow of the main stream of experiments on memory from that day to this.

As is now well known, Ebbinghaus's great innovation was the use of nonsense syllables for memorising. He claimed four great advantages for these over any other type of material: they are simple; they are homogeneous; they can be indefinitely combined, but in all combinations the material remains essentially on the same level; they "admit of quantitative variation which is adequate and certain". It is fairly easy to show that not one of these four claims is in fact sound.

There is thus the old difficulty in a new form, the exaggerated respect for the stimulus or the situation. The psychologist is studying the complex responses of a highly developed organism and how they are determined. They have been called forth to meet the claims of a very unstable and varying objective environment. They are no doubt in many ways more stable than that environment. But if the environment is violently simplified it is mere superstition to trust that they also get simplified in a corresponding manner. They become different, but are just as likely to become yet more highly complex. Stability of determination, not simplicity of structure in objective determining factors, is what we need to make our experiments convincing. Stability of determination is compatible with complexity, and even with considerable variation of objective determinants. Somehow an experimental method has to be developed which recognises this fact.

I turn for a moment to experimental work on 'recognition' merely to bring out one further point of method. Since very early days an enormous amount of work has been done on this topic. It has issued in five or six different theories no one of which can claim finality. The diversity of this result is due to different causes, but one is perhaps

particularly important. There is a strong tendency, when any complex response like recognition is being studied, to attempt to draw a ring round it and to seek its explanation within these imposed limits. Thus the explanation of recognition is sought in something that happens at the moment of recognition. This is surely wrong. An object or event may be recognised or not largely on a basis of how it was reacted to in the prior perception. A sound, for example, may be heard: it will not be recognised unless it is so listened to that it possesses qualities, characteristics, a setting, and a significance. To the persistent study of a complex mental response as if its psychological explanation must be found inside an imaginary circle that encloses it, much of the disrepute into which experimental psychology tends to fall may be traced. It is perhaps the last and subtlest form of the outworn 'faculty' psychology.

I said at the beginning that the early experimentalists in psychology were physicists and physiologists with a strong bent towards philosophy. If in the history of the subject experimental psychologists have shown themselves too submissive to physical and physiological methods, it is even more true that they have often pursued philosophical ideals. This pursuit is in full cry still. A very brief consideration of current movements which originate in the laboratory will illustrate this point.

There is probably no contemporary movement in psychology which has more profoundly influenced psychological thought in English-speaking countries than the so-called *Gestalt Psychologie*. I have, as every experimental psychologist must have, a very great admiration for the brilliant work of Wertheimer, Köhler, and Koffka. It has shed much new light on old problems, as well as a good deal of old light on new problems. It starts specifically from experiments upon visual perception and its primary method is that of phenomenological description. When we are presented with a perceptual situation, what is it that we experience? The answer is inevitable and is one which, from this point of view, has, I think, always been given. We cannot describe our experience in this sort of situation as a mosaic of tiny bits each corresponding with its isolable part of the stimulus or situation. The blue sky which is seen is not, as Köhler says, made up of an infinite number of blue sensation units, but is seen as a continuous blue expanse. The moving dots and lines, in Wertheimer's experiments, are seen, not as stationary points in temporal relations, but as a unitary and indivisible movement.

Sometimes the experiments are more behaviouristic, but still it is the attitude of phenomenalistic description that determines their interpretation. The animal that has been trained to react positively to *a* and negatively to *b* at once reacts positively to *b* if *a* is removed and *c* is introduced bearing that relation to *b* which *b* had to *a*. This must be because the initial reaction was not to *a*, or to *b*, and not to *a* and *b* and a relation between *a* and *b*, but to a total indivisible situation only to be described as *a-b-in-relation*. Again, an animal is

set a complex problem and typically achieves the solution suddenly; for what we call the 'correct' solution is a reaction to the total situation as built, or figured, or formed.

Thus a fundamental psychological question tends to be: "What is the nature and what are the characteristics of these indefeasible forms or patterns which stand over against all our reactions, compelling them to be as they are?" In our answer we can easily slip into the persistent error of over-emphasis of the objective side of the situation-response problem. I do not say they do this, but in his doctrines of physical *Gestalten*, Köhler comes very near it. Or again, looking to the response side, we may try to build up inside the responding mechanism a complex system, somehow corresponding to the integrated phenomenal situation. Then, as in Köhler's theories of ionic concentration within the central nervous system, we are almost sure to slip into sheer speculative physiology. Finally, the phenomenological attitude seems bound to issue in a comprehensive theory about the nature of the world as we know it, rather than in a scientific study of the determination of human response. It is the latter alone which is truly amenable to experimental treatment.

There is another contemporary movement, also originating in Germany, much less widely influential in other countries at the moment, but likely to attract more and more attention. This springs from the work of Prof. E. R. Jaensch. It also has a definite experimental basis. Jaensch experimentally discovered a type of imagery which seemed to lie somewhere between the after-sensation on one hand and the genuine memory image on the other. I cannot attempt to describe his extremely keen investigation of this *eidetic* imagery, as he called it. At the moment the main point is that he considered it demonstrable that a proneness to eidetic imagery is correlated with a number of other reaction tendencies. He elaborated a theory of the twofold division of all human subjects into integrate and disintegrate types. The integrate is the artistic, synthetic type, taking everything as a whole and having an inevitable accompaniment of persistent marked temperamental qualities and tendencies. The disintegrate is the scientific, analytic type, tending to split up presented situations and to deal with them piecemeal, and he also has his invariable, persistent, accompanying temperamental character.

Now this view does certainly seem to be biological in bent, and it is being explored throughout by experimental study. Moreover it strives, and, I think, successfully, to avoid that artificiality which, as we have seen, hangs over conventional laboratory methods for the investigation of the higher modes of human response. It rightly treats our problems as problems of reaction tendencies, of their determination and their grouping. But it does seem to be rather in a hurry with its sweeping generalisations.

In contemporary English psychology there is only one complete and world-explaining system of this sort, and that is the system which has been

developed by the keen and penetrating work of Prof. C. Spearman. Here again we are invited to begin with experiment, not merely upon perceptual processes, not merely upon imagery, not merely upon more complex intellectual responses still, but upon any psychological problem whatever. Wherever we begin we shall speedily find illustrated the working of the few immutable laws upon which all mental structure, and it may be the very universe itself, are built. They stand indeed at the portals of our science. Know them and everything is plain; ignore them and all is confused. They are conceived, not as tendencies serving the ends of biological adaptation, but after the fashion of physical principles of universal scope describing the inevitable structure and frame of mental life.

That Prof. Spearman has done more for the development of several fields of psychological research than any other living Englishman cannot be disputed. His contributions to the development of psychological statistics, and his work in the field of the investigation of intelligence must give him a permanent place in the history of the subject. Yet the notion of experiment mainly as a tool for laying bare and illustrating the operation of a few basic and fixed laws must seem to the biologically minded investigator very unsatisfying.

No survey, however scrappy, of contemporary movements in experimental psychology can be satisfactory without some reference to behaviourism. Of all the movements this is the one which is most thoroughly experimental, alike in its methods and in its formation of problems. It has laid firm hold on the point of view that experimental psychology is an investigation of the conditions determining high level biological reactions in animal and man. It is so round in its denunciation of philosophy that its excessive readiness to systematise its own principles of explanation is amusing. We can see this readiness in the haste with which it has exalted the principle of 'conditioned reflex' into an all-embracing explanation, though many of the problems of human response concern the emergence of new effector functions and conditioned reflex has nothing to do with this, and though conditioning at the human level is excessively speedier and often far more stable than anything that has ever been experimentally observed.

We can see it, also, in the behaviourist's dogmatic assertion that the development of consciousness within any type of biological response never makes any difference in subsequent response. Such dogmatism is only another instance of the experimental psychologist's fatal proneness to run beyond his data. It is explicable in the light of a study of the origins of behaviourism, for it was by the adoption of behaviouristic methods alone that the investigation of animal response below the human passed from the anecdotal and analogical stage and became genuinely a part of biological science. But to push the principles involved into the whole of human psychology is just as bad as to carry out some departmental investigation into perceiving, or imaging, or thinking, or some sensorial function, and then to use the results forthwith

as a master-key to all the problems of human determination.

Some of the reasons why experimental psychology has often attracted unfavourable criticism and failed to hold its students should now be clear. In work on the special senses it has frequently attempted to deal with problems that the physicist or the physiologist with their specialised training could solve more satisfactorily. In dealing with the higher mental processes it has been over-impressed with the necessity of standardising objective situations and has constantly proceeded as if the simplification of a stimulus were equivalent to the isolation of a response. Persistently it has shown unnecessary readiness to build upon specialised investigations wide systems which pretend a finality and universality that they do not possess.

I believe the time has now come for pushing these criticisms vigorously and for attempting to meet them in practice. It could scarcely have come much earlier. After all, whatever the limitations of his outlook, it may fairly be claimed that the experimental psychologist has done more than anybody else to keep alive the interest in special sense problems. As for the work upon relatively more complex processes, a new and struggling science was almost bound to imitate methods already fully established in other fields and to exploit them so far as they would go.

Holding all this firmly in mind, what conclusions can we draw? First, it follows that the experimental psychologist must claim that for the present, and perhaps for always, he is as much clinician as experimenter. He has not merely to arrange conditions and record results. There seems to be a notion abroad that there is so much uncharted ground in psychology that an investigator can do anything he pleases, and so long as he observes everything possible, his results are bound to be significant. This is utterly false. His observation is definitely that of a man with a problem, and generally also with a personality, in view; and it is by consequence almost glaringly selective. He is not alone among experimenters in this respect.

From a reading of the theory of the matter one might be tempted to suppose that the best experimenter would, once the experiment is arranged, be merely a rather complicated and delicate recording and calculating device. Those who have a reputation for brilliant experimental work in any field singularly fail to impress this character upon the intelligent and sympathetic onlooker. Anybody who by experiment is going to discover anything important about the determination of human reactions must first have developed a certain character of human reaction for himself. If this is to be used against him when he claims validity for his discoveries, it is a sort of stone he can return with some effect, whoever his opponents may be.

Yet it is extraordinarily important that the experimental psychologist should not be exclusively concentrated upon the particular reaction which he is specifically studying. Just because it is the intact subject, the intact organism, that we are concerned with, the conditions of any reaction are

apt to branch widely. The problem for us, for example, is not to find out how the eye sees or the ears hears, but how the animal and man do. No doubt we can answer this problem only in an imperfect way, but it takes us no nearer perfection to cut the ear or the eye out of the man. This is true with increasing force as we go higher in the level of response. Indirect cues are neither to be ignored, nor to be cut out, but definitely to be studied.

Secondly, no experimental psychologist must profess, with unvarying belief, the dogma of constancy of objective conditions. If, biologically speaking, human reactions had been built up to meet a series of unchanging environments, emphatic insistence upon rigidity of conditions would be justifiable. Obviously, they are not so built. So far as the psychologist is concerned, many of the most important characters that dominantly set the course of our reactions belong directly to the organism with which he is dealing, to its immediate and remote past history and to its present specific and general state of adaptation.

In the third place, the position which I have stated carries with it that the experimental psychologist, at the end of his studies, has to be satisfied with indicating trends, directions, proclivities rather than dogmatic laws. His phenomena are essentially biological, in process of development, displaying no hard-and-fast boundaries anywhere. He may formulate dogmatic laws, and

use experiments as imperfect illustrations; but this is the wrong order of things, though it has been by far the commonest and is still the easiest.

Finally, there is the question of the relation of the results of specific experimentation to the claims of general systematic theorising. I am not for one moment for the haphazard experiment that has no idea, no broadly formulated problem, behind it. Also I would condemn that scatter of descriptive results, unco-ordinated, unsystematised, which is common in many directions nowadays. We must explain our results and not merely collect and exhibit them. Yet I would urge that when we have, for example, satisfactorily stated the conditions of some particular perceptual reaction, we have no more right to pronounce magisterially upon a complex problem of reasoning than a physiologist who has studied respiratory functions has to pretend at once to clear up the secrets of spinal reflexes. No doubt the physiologist would never for a moment attempt to do this, but unfortunately it is not so easy to answer for the pretensions of the experimental psychologist in a like case. It may even be that all our specific studies will lay bare common broad principles of the determination of response. Even so, the broad principles are not the explanation of the specific problem, and for whatever they may be worth, before we erect them into a comprehensive system, we must have the specific problems widely and patiently worked out.

Obituary.

SIR E. RAY LANKESTER, K.C.B., F.R.S.

EARLY in the year 1879 I received a letter from the eminent comparative anatomist, Kitchen Parker, advising me to come over to Cambridge to take Francis Balfour's course of lectures in comparative embryology. I sailed from New York two days later, and by far the leading personal impression of my life was in meeting Francis Balfour in the great court of Trinity College, Cambridge. At this first moment he seemed to me a superman, and the impression was continually strengthened during the frequent and ever memorable contacts in lecture room, laboratory, afternoon bicycling trips, and evening dinners in Balfour's rooms. On these weekly occasions he was wont to invite two or three of his students, including William B. Scott and myself, to meet sympathetic colleagues of his from London, Cambridge, and Oxford. Among the latter I recall especially young Oscar Wilde, who was just beginning to attract attention; Henry N. Moseley, fresh from the *Challenger* voyage; and E. Ray Lankester, then professor of zoology and comparative anatomy at University College, London.

This fortunate early acquaintance with Lankester ripened into a lifelong friendship, sustained in more recent years by active correspondence and interchange of ideas. I was never able to induce Lankester to visit America, because, as he freely confessed, there was one thing in the world which he hated and dreaded, and that was a sea voyage—probably from unhappy experiences in crossing the English Channel which undoubtedly hurt his

pride as a physiologist and anatomist! But on every occasion when I returned to England I eagerly refreshed my personal acquaintance with him, spending delightful hours in his rooms in Chelsea, partly recalling our Cambridge days but more frequently discussing the newer phases of our knowledge of the ascent of man revealed by the discovery of the rostro-carinate flints along the shores of East Anglia. It was probably Lankester who suggested this highly appropriate name for these 'beak-keeled' implements found by J. Reid Moir of Ipswich.

I consider Lankester's warm support of Reid Moir's excavations and his courageous advocacy of the *human origin and manufacture* of these primordial flints, against the incredulous and indifferent attitude of the reigning archæologists of the day, one of the most striking evidences of his independence of judgment and of his powers of observation. He loved to handle the few specimens of these flints of East Anglia and of Piltown, Sussex, which he kept in his home, and he forcibly and often vehemently championed their authenticity. On one very salient point we always differed; namely, on the geological age of the rostro-carinates. He steadily insisted that they were Pleistocene, while I maintained that they were Pliocene, a distinction with a very important difference.

As the last survivor of the great group of Victorian zoologists and a veteran of many scientific and geological contests, Lankester represented all the ardent convictions and intensities of feeling