

PART I
EXPERIMENTAL STUDIES

CHAPTER I
EXPERIMENT IN PSYCHOLOGY

1. ITS BEGINNINGS

NO doubt there is a sense in which it is true to say that experiment in psychology is “at least as old as Aristotle”;¹ but certainly it can claim no great age as a method of *systematic* exploration of human reactions. This is a matter of some significance; for it means that, before experiment was systematically applied in psychology, the experimental method had already a long history of development in other realms, upon which the early experimenters in psychology built both their aims and their methods. All the pioneers in experimental psychology were trained either in physics or in physiology. Their influence, both for good and for ill, still remains stamped upon the accepted methods of the psychological laboratory. Further, as was natural at the time, they were often men of a strong philosophic bent. Gustav Theodor Fechner, who is usually regarded as the founder of experimental psychology, was, in fact, concerned mainly to establish a panpsychic view of the Universe. The tendency to use psychological experiment chiefly as a buttress to some all-embracing philosophical theory is one that has clung to experimental psychology ever since its earliest days, and has provided the critics of this branch of science with many of their most potent modes of attack.

Fechner took his degree in medicine, but turned soon to physics and mathematics. In developing his methods he built upon the work of E. H. Weber, the physiologist, and had the close co-operation of A. W. Volkmann, who was Professor of Comparative Anatomy at Leipzig. Helmholtz, equally and even more deservedly renowned as one of the great pioneers of experimental psychology, was also trained in medicine, but was before everything else a physicist. Wundt, the first man definitely to set before himself the ideal of a science of experimental psychology, was by training a physiologist. Hering,

¹ Myers, *Text-Book of Experimental Psychology*, Cambridge 1911, p. 1.

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Excerpt

[More information](#)

2

EXPERIMENTAL STUDIES

who often gets less credit than he deserves for his influence in certain directions upon the development of psychological research, was a physiologist. Side by side with these men were others whose interests were more theoretical and speculative. Stumpf, also trained in medicine, was mainly interested in music and philosophy, and G. E. Müller, who had a very great and important influence upon the development of psychophysics, and in many other directions also, had a philosophical background.

It is no wonder, then, that experimental psychology began, either with direct studies of special sensorial reactions, or with attempts to determine a measure of relation between physical stimuli and various apparently simple forms of resulting human reaction or human experience. And it is equally no matter for surprise that views of wide import and theoretical basis were speedily developed. Moreover, it is easy to see why attempts have constantly been made to control variations of response and experience by known variations of stimuli, and to explain the former in terms of the latter; and why it has been thought that reactions must be reduced to their 'simplest' form and studied in isolation from the mass of responses to which, in everyday life, they are related. Yet, even if we keep within the limits of special sense investigation, the tendency to overemphasise the importance of stimulus-determination and the ideal of simplification by isolation of reaction raise profound psychological difficulties. These become still more apparent as the experimental method pushes on to deal with more highly complex human responses.

2. ITS DEVELOPMENT

It was inevitable that, once the experimental method was introduced, it should sooner or later extend its application into all fields of psychological study. As every psychologist knows, the first laboratory of experimental psychology was founded by Wundt in 1879. At that very time, Ebbinghaus was trying to find a way to apply Fechner's exact methods to a study of the "higher mental processes", and particularly to memory. He succeeded to his own satisfaction and in 1885 published the essay *Über das Gedächtniss*, which even now is often characterised as one of the greatest advances ever made by experimental psychology. His ideals were the simplification of stimuli and the isolation of response. He secured the first by using nonsense syllables as his memory material, and the second, he curiously thought, followed immediately.

EXPERIMENT IN PSYCHOLOGY

3

It is worth considering in some detail what Ebbinghaus actually achieved. He realised that if we use continuous passages of prose or of verse as our material to be remembered, we cannot be certain that any two subjects will begin on a level. Such material sets up endless streams of cross-association which may differ significantly from person to person. It is an experiment with handicaps in which the weighting is unknown. Provided the burden of explanation has to be borne by the stimulus, this is obviously a real difficulty; for the stimuli have every appearance of varying from one person to another in ways incalculable and uncontrollable. There appears an easy way of overcoming this obstacle. Arrange material so that its significance is the same for everybody, and all that follows can be explained within the limits of the experiment itself. Since the experimental conditions are both known and readily analysable, the explanations can be expressed definitely and with the greatest possible certainty. Now, thought Ebbinghaus, with great ingenuity, if all the material initially signifies nothing, all the material must signify the same for everybody. Moreover, any variable significance that becomes apparent in the course of the experiment must be explained by the course of the experiment.

In reality, the experiments are much less easy than was assumed by Ebbinghaus. Any psychologist who has used them in the laboratory knows perfectly well that lists of nonsense syllables set up a mass of associations which may be very much more odd, and may vary more from person to person, than those aroused by common language with its conventional meaning. It is urged that this is no serious drawback, since it may be countered by a routine uniform exposure of the syllables and the inculcation of a perfectly automatic attitude of repetition in the learner; so that, with time and patience, each subject learns to take the nonsense syllables solely for what they are in themselves.

Once more the remedy is at least as bad as the disease. It means that the results of nonsense syllable experiments begin to be significant only when very special habits of reception and repetition have been set up. They may then, indeed, throw some light upon the mode of establishment and the control of such habits, but it is at least doubtful whether they can help us to see how, in general, memory reactions are determined.

The psychologist, of all people, must not stand in awe of the stimulus. Uniformity and simplicity of structure of stimuli are no

guarantee whatever of uniformity and simplicity of structure in organic response, particularly at the human level. We may consider the old and familiar illustration of the landscape artist, the naturalist and the geologist who walk in the country together. The one is said to notice and recall beauty of scenery, the other details of flora and fauna, and the third the formations of soils and rock. In this case, no doubt, the stimuli, being selected in each instance from what is present, are different for each observer, and obviously the records made in recall are different also. Nevertheless, the different reactions have a uniformity of determination, and in each case spring from established interests. If we were to put rock sections before all three people, the differences would still persist and might very likely be greatly exaggerated. Uniformity of the external stimulating conditions is perfectly consistent with variability of determining conditions, and stability of determinants may be found together with variability of stimuli.

So far as the stimulus side of his method goes, Ebbinghaus's work is open to the following criticisms:

(a) It is impossible to rid stimuli of meaning so long as they remain capable of arousing any human response.

(b) The effort to do this creates an atmosphere of artificiality for all memory experiments, making them rather a study of the establishment and maintenance of repetition habits.

(c) To make the explanation of the variety of recall responses depend mainly upon variations of stimuli and of their order, frequency and mode of presentation, is to ignore dangerously those equally important conditions of response which belong to the subjective attitude and to predetermined reaction tendencies.

Ebbinghaus's "great advance" involves serious difficulties if we consider only the stimulus side of his experimental situation; but when we examine also the theory of isolation of response underlying his method, still greater trouble arises. It is assumed that simplification of the stimulus secures simplification of the response. Then it appears to be assumed that this simplification of response is equivalent to isolation of response. Finally, it seems often to be assumed—though this is not a necessary implication of the method—that when we know how the isolated response is conditioned we can legitimately conclude that it is determined in this same manner when it is built with others into more complex forms of reaction.

It is always extremely difficult to say what, from the psychological

EXPERIMENT IN PSYCHOLOGY

5

point of view, constitutes simplicity of response. Sometimes we take the 'simple' response to be one that comes early in an order of development, as when we say that perceiving is 'simpler' than thinking, or that touch reactions are 'simpler' than visual. Obviously, nonsense syllables do not give us simplicity of response in this sense, for nobody dreams of laboriously learning to connect long strings of meaningless material until he enters the psychological laboratory, and by this time he must have arrived at some maturity, though perhaps he has achieved no great discretion.

Sometimes we mean that the 'simple' response is one that the agent can say practically nothing about, except that it has occurred. This is a slippery criterion, for some subjects find practically nothing to say about any response, while others seem to be able to make every reaction the subject of long analytical discussion. Certainly, nonsense material reactions stand in no favoured position in this respect amongst the mass of memory responses of daily life.

Sometimes we call a response 'simple' when it is isolated, cut off from the simultaneous functioning of other responses with which it is normally integrated. This seems to be the kind of 'simplicity' that the nonsense material type of experiment has in mind. For instance, Myers, commenting favourably upon the use of these methods, says that by employing meaningless syllables "we have been able to eliminate associations by meaning, and to arrive at the conditions affecting the sheer retentivity and reproducibility of a presentation, and to determine the number and course of the associations which are formed among the members of a series of such objects. It is true that the conditions laid down may depart somewhat widely from those which obtain in daily life. But only from such simple beginnings can psychological knowledge advance beyond that stage which had already been reached before the application of experiment".¹ That is to say, in our experiments we want to deal with *pure* memory, or with recall uncontaminated by any of the related functions with which it is accompanied in daily life.

There is, however, only one way of securing isolation of response, and that is by the extirpation or paralysis of accompanying functions. This is one of the perfectly legitimate methods of the physiologist. It can be argued that the psychologist, who is always claiming to deal with the intact or integrated organism, is either precluded from using this method, or at least must employ it with the very greatest

¹ Myers, *op. cit.* p. 144.

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Excerpt

[More information](#)

caution. However this may be, it is certain that such isolation is not to be secured by simplifying situations or stimuli and leaving as complex an organism as ever to make the response. What we do then is simply to force this organism to mobilise all its resources and make up, or discover, a new complex reaction on the spot. The experimental psychologist may continue the responses until he has forced them into the mould of habit. When he has done that they have lost just that special character which initially made them the objects of his study.¹

The third assumption, that when we have studied isolated reactions we can at once conclude that they operate in just this manner and with just these conditions as partial constituents of more complex responses, is less important. Undoubtedly Ebbinghaus, and to a greater extent many of his imitators, made this assumption. And not a few of the assertions made about recall on the basis of this assumption are no doubt correct, just because, as we have seen, the method nowhere approaches its vaunted 'simplicity'. But the study of isolated reactions has a value in and for itself, and though its conclusions must be generalised with great caution, they often yield hints with regard to integrated response that could not easily be obtained in any other way.

I have dealt at this length with the nonsense syllable experiments, partly because they are generally regarded as occupying a supremely important place in the development of exact method in psychology, and partly because the bulk of this book is concerned with problems of remembering studied throughout by methods which do not appear to approach those of the Ebbinghaus school in rigidity of control. But most of what has been said could be applied, with the necessary change of terminology and reference, to the bulk of experimental psychological work on perceiving, on imaging, on feeling, choosing, willing, judging and thinking. In it all is the tendency to overstress the determining character of the stimulus or of the situation, the effort to secure isolation of response by ensuring simplicity of external control. The methods of the great physical and physiological pioneers, often brilliantly successful in the study of special sense reactions, and in the elucidation of certain psychophysical problems, have overspread the whole of psychological science. Yet all the while new problems, most of them concerned with conditions of response that

¹ For a brilliant illustration of this point see Sir Henry Head, *Aphasia and Kindred Disorders of Speech*, Cambridge 1926.

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Excerpt

[More information](#)

EXPERIMENT IN PSYCHOLOGY

7

have to be considered as resident within the organism—or the subject—itsself have been forcing themselves to the front, and it is more than time that their implications were explicitly stated.

3. STATISTICAL DEVELOPMENTS

The “father of experimental psychology” in England was Francis Galton. A brilliant and original investigator, a man of independent mind and position, he possessed a thoroughly good general biological training, together with a wide range of interests. He was as convinced as anybody could be that, whenever possible, science must deal with quantities, and at the same time his thoroughly humanistic outlook impressed upon him that in all psychological experiments there must be a mass of conditions imperfectly controlled, and incapable of being varied one at a time. He thought he saw a way out of the difficulty by adopting a statistical treatment of observational and experimental results. Such treatment gives, not, indeed, the mode of determination of the individual reaction, but a picture of trends of response and of their interrelations. Since the measures which express these trends and their relations sum up in short-hand fashion the results of a very large number of cases, it may be assumed that they are free from the accidental limitations of the individual instance, and that they state, within limits that may also be statistically indicated, conclusions that hold good over the whole field of investigation.

Largely by direct influence, but probably in part also because Galton’s outlook contains something that is peculiarly attractive to the English temperament, the methods initiated by him have become very widely used in English psychology, and have been greatly developed by his successors.¹ In fact statistical method has profoundly influenced psychological investigation everywhere throughout the world. The earlier belief, that experimental psychology might easily be made to yield ideal experimental situations, has almost wholly disappeared.

This, needless to say, has its own drawbacks. Statistical methods are, in a way, scientific makeshifts. They are devices for handling instances in which numerous conditions are simultaneously operating. They do not show how all these conditions are related, and by

¹ Especially, as all the psychological world knows, in the extremely important work of Prof. C. E. Spearman.

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Excerpt

[More information](#)

themselves they throw no light upon the nature of the conditions. To get any indication of these matters we still have to rely upon whatever experimental pre-arrangement of conditions is possible, and upon what can be learned from careful observational study of the instances upon which we are at work.

Thus, as Yule says: "The very fact that the experimenter is compelled to use statistical methods is a reflection on his experimental work. It shows that he has failed to attain the very object of experiment and exclude disturbing causes. He should ask himself at every stage: 'Are these disturbing causes really inevitable? Can I in no way eliminate them, or reduce their influence?'... In any case it should always be the aim of the experimenter to reduce to a minimum the weight of statistical methods in his investigations".¹ On looking over any fair sample of the immense bulk of work upon applied statistics in contemporary experimental psychology, a questioning mind will find it impossible to avoid the conviction that a very large number of investigators care little about the arrangement or observation of the conditions of those reactions which they are studying, providing only that they can obtain large numbers of reactions and treat them statistically.

This is just the opposite error to that of the Ebbinghaus type of experiment. There we find a naïve belief in the complete efficiency of external circumstances to produce any sort of desired reaction. Here, at its worst, we find an equally naïve belief in the value of counting, averaging and correlating responses, even though the investigator ceases to worry about the variability of their determining conditions. The first may lead to unwarrantable optimism, but the second seems to me to give up the psychological ghost altogether.

If statistical applications in the field of psychology are to have any value whatsoever, they must be both preceded by and also supplemented by observation and interpretation, and the more exact these can be made the better. We may take one of the illustrations used by Yule. The marriage rate of a country depends upon a large number of conditions, of which the state of trade and industry are suspected to form one group. That these economic considerations are relevant is in no way initially proved by statistics. It is first suggested

¹ Medical Research Council, *Industrial Health Research Board Special Report Series*, No. 28, p. 5. It would be a most excellent thing if this essay could be put into the hands of every biological research student at the beginning of his career.

EXPERIMENT IN PSYCHOLOGY

9

on the ground of common observation and knowledge. The statistician then treats the available data on both sides by his special methods, and demonstrates that the curve for the marriage rate "shows a series of oscillations or waves which rather closely reflect the general cyclical movement in trade and industry".¹ The waves, both of marriage rate and of imports and exports, are isolated, and the ordinate of the marriage rate wave is correlated with the ordinate of the trade wave of its own year and of the years immediately preceding and following. It then at once appears that the difference of phase between the two waves is small, the waves of the marriage rate lagging slightly behind those of the trade rate.

At this point interpretation comes in once more. If postponements of marriage depend solely upon the industrial conditions of the year in which they take place, it can be shown that the marriage rate wave ought to be considerably in advance of that of the trade curve. Yule was able to demonstrate that the statistics are consistent with "the simple assumption that postponements are proportional to the average conditions in the given year together with the four or five preceding years".

In this instance interpretation comes in at two points: first, to set the whole investigation going; and second, to check and direct the initial results of the investigation. At both points the interpretation rests directly upon human observation. If these considerations apply in the field of vital statistics, they are even more markedly true of the use of statistics in psychology, where very often, in spite of all the difficulties, it certainly is possible to confine and control observation to a greater or less degree. From beginning to end the psychological statistician must rely upon his psychology to tell him where to apply and how to interpret his statistics.

In this book there will be no statistics whatever. This must not be held to imply any disrespect for one of the most powerful tools which the psychologist can use. It is merely because an attempt is being made to deal with a field of research in which suspected relations must be made as definite as possible before it can become fruitful to collect and correlate masses of results. "Nothing", says C. S. Myers, "is more important than that the experimental psychologist should be well grounded in the theory and practice of statistical measurement. But at the same time nothing is more important than that he should know when and how to use this psychological knowledge and skill,

¹ Yule, *op. cit.* pp. 13-14.

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Excerpt

[More information](#)

employing them not merely mechanically and automatically, but with due regard to psychological conditions.”¹

4. EXPERIMENT IN PSYCHOLOGY

The task of the experimentalist in psychology is particularly difficult, not merely because of the multiplicity of conditions which are in all cases simultaneously operative, but also because the two great groups of these conditions often seem to work in opposing directions. “The conditions in psychological experiment are the internal conditions of the individual (or ‘subject’) on the one hand, and the conditions of his environment on the other.”² Now, as we have seen, stability of the latter is perfectly consistent with great variability of the former, and it is equally true that variability of the latter may be accompanied by stability of the former. Hence no amount of careful control of the uniformity of the external conditions alone will ensure an unchanging and known determination of response. This should be clear the moment we consider that psychological reactions are merely one class of the whole group of biological responses which have grown up to meet the needs of a constantly changing external environment. Thus the external environment may remain constant, and yet the internal conditions of the reacting agent—the attitudes, moods, all that mass of determining factors which go under the names of temperament and character—may vary significantly. These, however, are precisely the kind of determinants which are pre-eminently important for the psychologist. For example, many experiments involve the subject in repeated reactions to a uniform simple situation. Obviously those responses which come late in the series are determined in various ways by those which come earlier, and it is this type of determination with which the psychologist is often directly concerned. Again, the external conditions may vary, and the description of the responses evoked may vary, yet the mode of determination of the responses may remain substantially the same. For example, the sportsman describing a game, the politician giving an account of some current controversy of State, the musician talking about a concert, are all dealing with very diverse material, and no doubt their ways of fulfilling their task would appear very different to the observer. Yet their selection, criticism, arrangement and con-

¹ “Psychological Cautions in the use of Statistics”, *Zeit. f. angewandte Psychologie*, XXXVI, 82–6

² Myers, *op. cit.* p. 2.