in our universities neither by chairs nor by laboratories. We have laboratories for physiology, but to show how little interest physiologists take in general biology I may mention the fact that the editor of a physiological annual review excludes papers on the development and fertilization from his report, as in his opinion, this belongs to anatomy. On the other hand, anatomists and zoologists must give their full energy to their morphological investigations and have, as a rule, neither the time for experimental work nor very often the training necessary for that kind of work. Only the botanists have kept up their interest in general biology, but they of course pay no attention to animal biology. In working out this short review of the development of biology during the last century I have been impressed with the necessity of our making better provisions for that side of biology where, in my opinion, the chances for the great discoveries seem to lie, namely, *general* or *experimental* biology. JACQUES LOEB.

THE PROBLEMS OF EXPERIMENTAL PSYCHOLOGY.*

THE first difficulty that confronts one, as one attempts to envisage the problems of experimental psychology, is the difficulty of definition. What is a psychological experiment? What is the scope of experimental psychology? Is experiment simply a method of work, applicable to all or to some special parts of the psychological system; or is experimental psychology a distinct branch of psychology, sharply marked off from other and coordinate branches?

The program of this congress would seem to have decided the issue in the latter sense; for we find sections of general psychology, of comparative and genetic psychology, of abnormal psychology and of social psychology, arranged alongside of our own section of experimental psychology. If, then, I wished to take shelter behind the plan of the program, I might, with some show of justification, confine myself to the discussion of those problems in normal, human, adult psychology which still form the staple material of experimental investigation in the laboratories, and might omit all reference to the extensions of the experimental method to outlying fields. Such a course would, nevertheless, be unsatisfactory. The extensions of the method are coming to play a larger and larger part in psychological discussions and in our psychological literature; and it behooves us to take up a stand with regard to them, positive or negative, appreciative or critical. I shall try not to shirk this duty. Let me say, however, at the outset —and I shall have more to say upon the matter presently—that, whatever else experimental psychology may be, there can be no doubt that the subjects to which the program apparently limits us are experimental psychology. The examination, under strictly controlled and properly varied conditions, of the normal, adult, human mind—this is psychological experiment in its pure, primary and typical form. And it is this typical experimental psychology the problems of which we have, in the first place, to consider.

In approaching this question of the problems of experimental psychology, it seemed to me that the surest key to the future lay in the accomplishment of the past. The best way to find out what experimental psychology has to do is, I thought, to make certain of what it has already done. With this idea in mind, I naturally had recourse to our bibliographies—the American bibliography of the *Psychological Review*, and the German of the *Zeitschrift f. Psychologie*. The result was not encouraging. We

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all knew, of course, that the plan of arrangement of these two yearly lists is by no means the same. What I, for one, had not realized was the fact that the plan of arrangement of both is eminently unsystematic. We use a bibliography, and find it useful; we do not need to enquire further regarding it. But I do not believe that any psychologist, of whatever school, could write a systematic psychology on the lines laid down in these bibliographies. This fact—if fact it is—seems worthy of a passing remark; for it indicates, in a concrete and definite way, that in spite of the enormous increase of our psychological knowledge, within the last few decades, we are still very far from any complete or rounded science of psychology. I am not so much disposed to blame the bibliographers—I take their lack of system to be unavoidable—as I am to draw a long breath at the amount of work which still remains for us to do.

Finding that I could not avail myself of the bibliographies, I took the bull by the horns, and went to the psychological journals. I listed and analyzed the experimental papers in the *Philosophische Studien,* the *Zeitschrift f. Psychologie,* the *Année psychologique,* the *American Journal of Psychology* and the *Psychological Review;* not with any view of substituting a classification of my own for the classifications now employed, but simply with the intention of finding out what was there. If you object that these five journals are not coextensive with experimental psychology, I must reply that they are at any rate representative, and that the duration of human life is limited. Even so, I am not sure that the game was worth the candle. I earned, perhaps, by hard work, the right to stand upon this platform; but I found out very little that I did not know before.

If I am to indicate, briefly, the results of this enquiry, I must premise that we are agreed upon the distinction, within experimental psychology, between the properly 'psychological' and the psychophysical attitudes. The object of the 'psychological' experiment, as I am now using the phrase, is introspective acquaintance with the processes and formations of a given consciousness. The object of the psychophysical experiment, as we have recently been reminded by G. E. Müller—I suppose that we are all fresh from a reading of his 'Psychophysische Methodik'—is a numerical determination. Thus, the object of the simple reaction, regarded as a psychological experiment, is the introspective analysis of the action consciousness, given under certain fixed conditions; the object of the same experiment, regarded psychophysically, is the ascertainment of a representative time-value and of the manner and limits of its variation. Both points of view are covered by the general term 'experimental psychology'; both types of experiment are valuable; but the two must not be confused. If, now, we look at the contents of the *Philosophische Studien,* the oldest established of our five journals, we find that three departments of experimental investigation are preferred high above the rest: sensation, perception and action. There is, moreover, a very definite trend towards psychophysics, so that, *e. g.,* at least two fifths of the articles that deal with sensation must be classed outright as psychophysical. The remaining experimental papers may be subsumed under the headings: association of ideas, attention, feeling, memory and recognition, the organic accompaniments of the mental life, the range of consciousness, the processes involved in the activities of reading and writing, and the time consciousness. What we find in the other four journals is a continuance of interest in these same problems, but a continuance of interest which is combined with a shift of emphasis from psycho-

physics to psychology, and a widening of the area of experimental work. Thus in the *Studien* there are about twice as many articles on sensation, psychological and psychophysical, as there are on perception; in the *American Journal,* the articles on perception are more numerous than those on sensation; in the *Psychological Review* there are, roughly, three articles on perception for every two on sensation, while the strictly psychophysical papers may almost be counted upon the fingers of one hand; and the *Année psychologique,* if I have counted aright, has practically as many articles on memory as it has on perception, and more of either than it has on sensation, while the spirit of the work has, from the first, been adverse to psychophysics. Or again, the contents of the *American Journal* may, with some manipulation, be brought under the same headings that served for the *Studien,* save that one additional caption must be made for studies of voluntary movement (other than reactions) and of the experiences of effort and fatigue; while those of the *Zeitschrift* and the *Psychological Review* require at any rate three or four new rubrics, to cover work done upon mental inhibitions, the process of learning, motor automatisms and motor dispositions, habit, etc. I do not wish to labor this point, even if I must leave it with some sense of injustice to the periodicals under review. You know, without my telling you, and I knew, without going to the magazines, that the course of experimental psychology in recent years has been away from simple psychophysical determinations, and towards introspective analysis; and that the experimental method has been continually extended from the simpler processes to the more complex—whether to complexes hitherto untouched by experiment, or to unfamiliar phases of familiar mental formations. All that a study of the journals can do is to quantify and define these facts. I should like to add, however, that their study has brought home to me, in a very vivid way, the immense complexity and far-reaching interconnection of the mental life. The contents of experimental papers are oftentimes so varied that only a classification *a posteriori* is possible; and, oftentimes again, results that are but incidental to the given topic of investigation prove later on to be fundamental for problems from which this topic had seemed disconnected and remote.

So much, then, by way of preparation. Let us now, in the light of it, attempt to formulate the present problems of experimental psychology. You will remember that I am speaking of experimental psychology *sensu stricto*—of the experimental investigation of the normal, adult, human consciousness. I wish that I could proceed systematically. But, in the existing condition of the science, it is better to be topical. We may, however, begin in a quasi-systematic way, by considering the three fundamental problems of sensation, affection and attention.

(1) *Sensation.*—The senses, viewed from the standpoint of psychological knowledge, fall into three principal groups. We know a great deal about sight and hearing; we know a good deal about taste, smell and the cutaneous senses; of the organic sensations, with a very few exceptions, we know practically nothing. There is work to be done—I say this emphatically—in every field; there is probably no single chapter in sense psychology that may not, with advantage, be reopened. Nevertheless, we know a great deal about sight and hearing; the literature of these senses is voluminous; advance in our knowledge lies (I am speaking in the large and quite roughly) in the hands of the few experts who have occupied themselves particularly with visual and auditory problems. And we know a good deal about taste, smell and the cuta-

neous senses; although here, doubtless, there is much steady work, rank and file work, yet to be done. We know something of the organic complex concerned in active touch, and something of the static sense. On the other hand, of the organic sensations in general we know practically nothing. Here then, as I take it, lies the immediate sense problem for experimental psychology. When we remember the importance of organic sensation in the affective life, its importance as the vehicle of sensory judgments in psychophysical work, the part it plays in the mechanism of memory and recognition or in the motives to action, its importance for the primary perception of self; when we remember the widespread character of the organic reaction set up by any sensory stimulus; when we realize that some psychological systems have recourse to it from beginning to end, while others (Wundt's recent 'Grundzüge' is an example) practically ignore it; when we remember that certain questions of prime systematic importance hinge upon it—the question of the duality of the conscious elements, of the relative range of sensation and image, of what is called affective memory, and so on: we can hardly fail to see that here is a great gap in our psychological knowledge, the filling of which calls for a persistent application of the experimental method. Of all problems in the psychology of sense that are now before us, the problem of the number, nature and laws of connection of the organic sensations appears to me to be the most pressing.

In the domain of psychophysics, I see no single problem of supreme import, but rather a need for patient, continuous work by the methods already formulated. The inherent aim of psychophysical investigation is, as I have said, the determination of the psychophysical constants. Now it is by no means difficult to vary a psychophysical method, and so to set up a claim of originality; but it requires patience and some self-sacrifice to work through a psychophysical method to the bitter end. What we now want is less ingenuity and more work—accurate, continuous work all along the line. We have methods and we have formulæ. Let us give them a thorough test. The results will be of extreme value for psychophysics, and no one need fear that they will be barren for psychology. On the contrary, no small part of our analytical knowledge of the higher processes, as they are called—processes of judgment, of comparison, of abstraction—derives straight from the method-work of psychophysics. It would, in my opinion, be time and energy well spent, if every existing laboratory were to undertake what one might term the routine work of testing out, without modification, one or other of the classical methods.

I am aware that psychophysics trenches upon large problems. I ought, indeed, to be keenly alive to these problems, seeing that for the past three years they have occupied me, with but little intermission. There is the great problem of mental measurement itself; there are the minor problems of the validity of the difference limen, the equality of just noticeable differences, the range of Weber's Law, the correlation of functional constants, and what not. If I were speaking of the history of experimental psychology, and not of its present status, I might hope to show you that more has been done towards a solution of these problems than the current statements in text-books and magazines would lead one to suppose. But, with these problems in mind, I insist that the immediate demand in psychophysics is for careful, straightforward work by the approved methods. We shall gain more from such work than from anything else.

(2) *Affection.*—When we turn to the af-

fective processes, we have no such difficulty in selecting our problems. This whole chapter in experimental psychology is one single problem. Will you believe—I had myself not realized it before—that in all the five and thirty volumes of the *Zeitschrift* there is not a solitary experimental article on the feelings? This although the same volumes contain, roughly, two hundred contributions to experimental psychology! The *Studien* has about one hundred and forty experimental papers, of which nine deal with affective psychology or experimental æsthetics: that is the best record I have found. Now look at the problems. We are not at one as regards the nature and number of the elementary affections; there are experimental psychologists who reduce all the elements of consciousness to sensations. We are not agreed whether the diversity of feelings is to be referred to a diversity of affective process proper or to a diversity of organic sensation. Some of us think that a given affective process is coextensive with consciousness; others maintain that consciousness may be a mosaic of affections. Some assert that the feeling element is effective for association; others deny it this effectiveness. Some find the best illustrations of the law of contrast in the sphere of feeling; to others, contrast may itself be a feeling. Our facts are few, our laws dubious. Surely, it is time to gird up our loins and make serious business of these affective problems.

I have insisted on the paucity of the experimental articles upon feeling. I do not, by this, mean to accuse experimental psychology of idleness or neglect: Lehmann's two books would save us from such a charge, if we had nothing else to offer. But these two books are characterized by their reliance upon the expressive method —a method which, as you are aware, has stood in the forefront of many recent discussions. I have been at the pains to make out a complete table—complete, that is, so far as I was able to make it complete—of the results obtained by the method of expression. There is much to be learned from them. But I can not believe that the method will help us very greatly towards an affective psychology. The organic reactions which the expressive method registers are closely interwoven and interdependent, and the task of differentiation presents difficulties which, if not insurmountable, have at least not yet been surmounted. I am disposed to think, *e. g.*, that the plethysmograph, as a differential instrument, is doomed to disappear from our laboratories. The sphygmograph, and especially the pneumograph, hold out better hope; but I doubt if, at the best, a differentiation of affective qualities is to be expected from them. From the method of suggestion, which really takes us over into social psychology, I expect still less. There remains, at present, only the method of impression, which has done good service in a limited field, and which should be capable of modification and expansion. However, I am fortunately not called upon here to propose methods of work, but only to indicate problems. And the facts and laws of the affective life, the life of feeling and emotion, form one of the largest and one of the most insistent problems of modern experimental psychology.

(3) *Attention.*—The prominence given to the state of attention is characteristic of experimental psychology, as contrasted with the empirical psychology of associationism. It is, indeed, one of Wundt's greatest services to the new psychology that he early divined the cardinal importance of attention in the psychological system, and began that series of experiments of which we can by no means see the end to-day. For I imagine that we must all admit, if we are honest with ourselves, that the body of

facts at our disposal, large and varied as it is, is yet not adequate to a theory of the attentive state. We must know more of the constitution of the attentive consciousness, and of the mechanism of distraction; much remains to be done before we can settle the vexed questions of the distribution of attention; we must work out, experimentally, the relation of attention to affective process; even the familiar problems of the range and duration of the attentive state are—well, are still problems. I am not sure that we shall not have to manifold the study of attention, as we have that of memory; and to speak in future of the facts and laws of visual attention, auditory attention, and so on, instead of taking 'attention' as a single state. I am certain that we must have a more specialized psychology of the great variants and resultants of attention—a specialized psychology of expectation and habituation, of practise and fatigue.

If, then, I have seized the situation correctly, we have in these three fundamental departments of psychology three problems of different orders, the solution of which calls for a diverse endowment of psychological skill and insight. There is an outlying group of sensations that can, we must believe, be successfully attacked by the analytic methods which have been successfully employed in the other sense departments. The experimental study of the affective processes calls for a much greater gift of originality and constructive imagination; we have to shake off literature and tradition, and to begin almost at the beginning. In the case of attention, we have to push on and make progress along paths already marked out but insufficiently explored.

What holds in this regard of the attention seems to me to hold also (4) for that mixed medley of formations which we include under the general term *perception*. I wish that we could banish the word 'perception' to the special limbo reserved for unregenerate concepts, and could put in its place a round dozen of concrete and descriptive terms! But it has, so far, held its own, and I can hardly avoid its use. We know, now, a great deal about tonal fusion, about space perception, about rhythm—if rhythm be a perception; we know something about time perception. You will, however, agree with me that no one of these topics is a closed chapter. I see no very pressing problem, as I look over the field; but I see, in every quarter of it, good work that needs doing. I am sorry if this opinion appears indefinite; it is the opinion that I have come to after a study of more than a hundred and fifty articles that deal with perception in the five journals referred to just now: and I can not make it more definite without going so deeply into detail as far to exceed the time allotted to me.

We can speak a little more concretely of (5) *recognition, memory* and *association*. Association was, at first, handled in rather stepmotherly fashion by experimental psychology. Of late years, however, we have come to see the importance of detailed analyses of the associative, as also of the recognitive consciousness; we have, I think, finally broken free from the traditional schemata, and are approaching the problem with open minds. Something has already been done; much more remains to do. The experimental study of memory was begun, by Ebbinghaus, rather in a practical or psychophysical than in a psychological spirit. In the development of the work since Ebbinghaus, we can trace two tendencies: a tendency towards psychological analysis of the memory consciousness and the explication of the psychological laws of memory: that on the one hand; and on the other, a tendency towards the application in practise of psy-

chological results. While, now, I take the recent experimental work on memory and the associations involved in memory to be work of a high order; and while I believe, in particular, that certain of the methods employed are a valuable addition to our psychological repertory, I can not but think that the two tendencies just mentioned have not been kept as distinct as they should have been, and that experimental psychology has suffered in consequence. We can hardly hope to get a psychology of memory and association on the ground of Reproduktionstendenz and Perseverationstendenz: we can hardly hope to get practical rules, if they are what we want, out of the published studies on economy of learning. The Tendenz-concepts are psychophysical, and tend to cover up the complexity of actual experience; the practical studies are made under conditions widely remote from those that obtain in ordinary practise. Let us realize that we may attempt here any one of three distinct problems. We may aim at a psychology of memory and association; *i. e.*, we may seek to record our experience, to trace the introspective patterning of the memory consciousness. We may aim at a psychophysics of memory; *i. e.*, we may try to establish formulæ akin to the well-known formula of Ebbinghaus' 'Gedächtnis,' which represents retention as a function of time elapsed. Or we may aim at an applied psychology of memory; we may work out, experimentally, an art of acquisition. I do not say that an investigation into one of these three topics will throw no light on the other two; on the contrary, I have already insisted on the value of indirect results in psychological enquiries. But in our thought, at any rate, the three problems should remain separate and distinct. They offer, without doubt, a wide field for future research. I would suggest, though with all reserve, that the psychological study of memory and association may, in the long run, help us to clear up the much-disputed question of the subconscious. There are, as you know, experimental psychologists who work simply in terms of introspection and of physiological process; there are others who interpolate between these terms an unconscious or subconscious mentality. I can not go into detail; but it seems to me that, if these differences of opinion can in any connection be brought into the laboratory for adjustment, it is here, in the investigation of memory and association, that we may hope to introduce them.

I come next (6) to *action*. You will remember that, in its early years, experimental psychology was much concerned with the psychophysics of action; indeed, the problem of the 'personal equation' is a good deal older than our laboratories. This interest has never flagged. If we have not heard so much of late about reaction experiments, we have heard a great deal about the psychophysiology and psychophysics of voluntary movement. And I think that we can leave those things to take care of themselves; we may, without any question, look to the next few years for improvements of technique, for revision of numerical determinations, for recasting of theories. That work is under way. What I should like now to emphasize is the need for investigation of the more strictly psychological kind. Our knowledge of the action consciousness is still very schematic, very rough, in part very hypothetical. It has been recognized for some years that the reaction experiment may be turned to qualitative, *i. e.*, to analytical account; but so far more use has been made of this idea in laboratory practise than in research. We must start all over again, and take the action consciousness seriously. I once made a sort of reaction experiment of the setting-up and

taking-down of an inductorium; the student made the manipulations continuously, under time control, and gave his introspective record at the end of each experiment. We worked at the problem for a year, only to learn that we had been too ambitious; we had, as even with experience one is apt to do, underestimated the complexity of consciousness. At the same time, we decided that the problem was soluble; we gathered in a good store of introspective results, even if they were too individual, and too discrete, to be employed for generalization; with more time and more observers, or with a simpler set of voluntary movements for study, we should have accomplished something for psychology. I regard such studies as those recently made on the control of the retrahens of the ear, or on the control of the winking reflex, as extremely promising in this field. At any rate, whether we work from the classical reaction experiment, or whether we take voluntary movement under more natural conditions, the problem is quite definite: we must submit action to an introspective analysis as detailed and as searching as that to which we have subjected perception.

I have put off (7) *imagination,* because I am a little afraid of the term. It is a word which, like perception, I should be glad to see discarded from the vocabulary of experimental psychology. I think that we employ it more vaguely even than we employ perception; and I think that the future will substitute for it a number of descriptive terms. If we begin with the elementary process, the image itself, we must plead ignorance on two fundamental points: whether image quality is coextensive with sensation quality, and whether image difference is adequate to sense discrimination. If we go to the other extreme, and regard imagination as the general name for a group of typical formations—as a concept coordinate with memory—we must surely say that experimental psychology is, as yet, hardly over the threshold of the subject. We know, perhaps, how to set to work: some investigations have been made, and some hints toward method have been given; but, in the large, this chapter of experimental psychology remains to be written.

(8) Of the more complex *affective* formations we can say but little until we have a better psychology of feeling. No doubt, there are certain problems in the psychology of sentiment, and more especially in that of the esthetic sentiments, that can, within limits, be handled without regard to the ultimate categories of feeling. I should, however, consider these limits as very strictly drawn. (9) For the higher *intellectual* processes we have, I think, three sources of knowledge: direct experiment—that, as you know, has been well begun,—the indirect results of experiment upon sensation, and Völkerpsychologie. I am inclined to lay great stress upon the second of these sources. Experimental psychology has often been reproached, on the one hand, because it devotes most of its time to sensation, and on the other because the results of its dealings with the higher processes are jejune and meager. To the former charge I plead guilty, in so far as we have avoided the affective problems, though this neglect is not at all what the framers of the accusation have in mind. And even so, I might offer in extenuation the experimental work upon attention. But this apart, I think that experimental psychology is justified in its choice of topics. The only way to catch the higher intellectual processes in course of formation is to work from the periphery, by way of the sense organs. It is when we are working with tones, or with lifted weights, that the amazing diversity and complexity of judgment becomes apparent. If, on the con-

trary, we take any one of these higher processes full-formed, and attack it directly, we are very likely to find that the vehicle of the mental function is extremely simple; there is a law of reduction, running all through mind, whereby a highly complex formation tends to degenerate, to reduce to a stereotyped simplicity. It is, to my mind, a distinct merit of experimental psychology that it has brought to light this meagerness of content in the examination of 'higher' mental functions of an habitual order; and it is a healthy instinct that sends us back and back again to the channels of sense, as we seek an appreciation of the fulness and richness of the mental life. I may add, though I say this a little hesitatingly, as a merely personal impression, that the introspective attitude of the observer seems to me to be more nearly normal, less artificial, in cases where the avowed object of experimentation is comparatively simple. If you are asked overtly to grapple with a complex psychosis, you are likely to brace yourself to the task, to put on an armor of preconceived opinion; if the psychosis meets you unawares, finds you off guard, the facts will have their own way with you. A distinguished English psychologist once declared that it is futile to attempt the problems of recognition by way of rotating discs of black and white sectors. I should say, on the contrary, that these discs are, in principle, the very best means to an understanding of the higher intellectual formations.

As for the ultimate goal of experimental endeavor, I suppose that we may call it (10) the problem of *consciousness*,—not in the sense in which that problem is understood by the theorist of knowledge, but in this sense: that, as hitherto we have analyzed and traced to their conditions certain mental processes, of lesser or higher degrees of complication, so now we analyze and trace to their conditions total consciousness, given in varying states and constituted of various formations. The difficulty of this problem is enormous. Only those of you who have attempted it, in one case or other, for yourselves, who have discarded classificatory terms, and faced the living facts; only these, even of experimental psychologists by profession and training, can form any proper idea of its difficulty. It is a problem for which we are not yet ripe. We can approach it only by way of theories which we know to be inadequate, and by help of hypotheses which we can not substantiate by facts. But it is the problem towards which we are trending, and the road to its solution lies, as in my judgment all such roads in our science lie, not through brilliant suggestion and ingenious forecast, but through patient and steady work. This work must be in part the work of experimental psychology, as we are here interpreting that phrase; in part the work of what is called individual psychology—though, indeed, from perception onwards, the difference between these two departments of psychological investigation is simply a difference of accent. Or, to put the matter concretely, we must work not only with the doctrine of states of consciousness, comparing experimentally the attentive and the inattentive, the hypnotic and the dreaming, all sorts of normal and abnormal states of consciousness, but also with the doctrine of conscious types which we owe (and the debt is great) to the psychologists of individual variation.

So I finish the first part of my review. If I have omitted anything of consequence, or if I have seemed to do injustice to any department of work, I must ask for pardon and correction; I have spoken with the utmost possible brevity. My own habitual thought in experimental psychology is positive, not negative; that is, I am accustomed to look upon our problems rather as con-

tinuations of work already begun than as gaps and lacunæ in our system of knowledge. I could wish that it had fallen to my lot to address you in this positive way, to show what experimental psychology has done, how in the past few decades it has changed the face of systematic psychology, rather than to insist upon the tasks that still lie before it. I have, however, tried to be entirely honest; I have, I think, rather exaggerated than concealed our deficiencies; and I would have you remember that this definite formulation of things to do presupposes and implies that much has been done. When Wundt wrote his famous essay 'Ueber die Aufgaben der experimentellen Psychologie,' the problems that loomed before him were the psychophysics of sensation, the analysis of perception, the time-relations of the higher processes. To-day, the list is longer and the range wider. But it is only because we already possess that we can say, in such detail, what still needs to be added to our possessions: in which fact let us take encouragement.

I pass, with some diffidence, to a consideration of wider issues—of those extensions of the experimental method, proposed or attempted, of which I spoke at the beginning of this address. Most psychologists, I take it, would agree that the picture I have drawn of experimental psychology in what has preceded is drawn too narrowly. The title of psychologist is, indeed, given at the present day to two distinct types of scholar. On the one hand, we have the psychologist as I have represented him: a man keenly interested in mind, with no purpose beyond mind; a man enamored of introspection; a man to whom the most fascinating thing in the universe is the human consciousness; a man to whom successful analysis of an unresolved mental complex is as the discovery of a new genus to the zoologist or a new river to the explorer; a man who lives in direct companionship with his mental processes as the naturalist lives with the creatures that are ordinarily shunned or ignored; a man to whom the facts and laws of mind are, if I may so put it, the most real things that the world can show. On the other hand, we have men to whom mind appeals either as a datum or problem, or both, to be dealt with by philosophy, by theory of knowledge and theory of being; or as a natural phenomenon, something that must be taken account of whenever life is taken account of, in evolutionary biology, in anthropology, in medicine, and where not. Of the psychologists of this second order, the philosophers, you will say, do not concern us. Yet they do, somewhat. I suppose that all sciences—certainly, all young sciences—are liable to be told by well-wishers that they have mistaken their work; that they would advance more quickly, and more solidly, if they would put off their present business, and settle down to this or that suggested problem. At any rate, experimental psychology has always received such hortation from friendly philosophers. If, now, I have ignored this advice, it is not from lack of gratitude, but simply because, after consideration, I have come to believe that experimental psychology knows what she is about, and can walk without assistance. Outsiders, we are told, see most of the game. I venture to urge that the insider better knows how the game is to be played.

We are left with the two opposed types: what shall I term them?—the inner and the outer, the subjective and the objective, the narrower and the broader. What, then, of the outer, wider, objective problems of experimental psychology?

Let us be clear, first of all—the matter admits of no hesitation or compromise—that the experimental psychology of the normal, adult, human mind must take the

form that I have described—the form of introspective analysis. I have little sympathy or patience with those experimentalists who would build up an experimental psychology out of psychophysics and logic; who throw stimuli into the organism, take reactions out, and then, from some change in the nature of the reactions, *infer* the fact of a change in consciousness. Why in the world should one argue and infer, when consciousness itself is there, always there, waiting to be interrogated? This is but a penny-in-the-slot sort of science. Compared with introspective psychology, it is quick, it is easy, it is often showy. We have been a little bit corrupted by the early interest in psychophysics; or perhaps, more truly, we have not all learned instinctively to distinguish between psychophysics and psychology proper; and so we are apt to take the tables and curves of reactions for psychological results, and the inferences from them for psychological laws. Now the results, where they are not purely physiological or anthropometrical, are psychophysical results. As such, they have their usefulness; and the psychological laboratory is their right place of origin. But there is no reason why one should gain psychological credit for them —still less for erecting a speculative psychology upon their foundation. This mode of psychologizing is inherently as vicious as any of the constructive modes of the older psychology, the psychology before experiment. Historically, it has proved disastrous;* it falsifies problems and obscures real issues; we must set our faces against it now and for all time. How, indeed, shall one call a man a psychologist who deliberately turns his back upon the one psychological method, in the one field to which that method directly applies? There is no excuse, in psychology, for the neglect of introspection, save the one—and that must be demonstrated—that introspection is impossible.

Having said this much by way of preface, I may take up the further question. We can hardly open a magazine nowadays without finding applications of the experimental method beyond the limits of the normal, adult, human mind. In animal psychology, in child psychology, in various departments of mental pathology, the experimental method is employed. Even the conservative *Studien* contains articles on the state of sleep and dreaming, and Wundt has looked more favorably upon experiments under hypnosis since they promise to confirm his theory of feeling. Experiments on children and animals have for some years past occupied the attention of leading American psychologists; work on child psychology is characteristic of the *Année psychologique,* and is being published more and more freely by the *Zeitschrift;* you all know the avowed purpose of Kraepelin's 'Arbeiten.' I need not multiply references. Wherever psychological interest has gone, in these fields, the experimental method has gone with it. Sometimes the particular experiment is borrowed forthright from the normal practise of the laboratory, sometimes the procedure has been recast to suit the novel problem; sometimes the experimental method is taken seriously, employed with care and knowledge, sometimes it is thrown in as a makeweight, without responsibility or understanding; sometimes it is praised, sometimes decried. All this is natural. The important thing for us is, I think, the recognition that the experiments are a part of 'experimental psychology,' in the sense of this paper, and must be taken account of in any general review

* Is proof needed? Think of the early work upon the just noticeable difference, upon the simple reaction, upon the 'time sense'; or think of Wundt's current discussion of Weber's and Merkel's laws!

of the problems of experimental psychology. The psychologist of the laboratory is apt to emphasize the crudity and roughness of the work, and its neglect of introspective control; the psychologist of the clinic or the schoolroom or the animal room is apt to consider his colleague narrow and his colleague's work finical and meticulous. The transcending of this difference, the reconciliation of these views, I take to be a very real problem for experimental psychology—though a problem of a different order from those that I have been discussing. And I suggest the following points for your consideration. First, one can not be too nice or too careful in experimenting on mind. There is no such thing as over-refinement of method.* Let those who doubt this fact read Martin and Müller's 'Unterschiedsempfindlichkeit'; the more delicately one analyzes, the more subtle does mental process reveal itself to be. Galton's questionary results on visualization are psychology, and valuable psychology; but they are also pioneer psychology. Now, the pioneer may pride himself on his work, but not on the roughness of his work. When the laboratory psychologist smiles at the charcoal sketches of objective experiment—well, he does wrong to smile, because honest work should not be laughed at; but he is right in his conviction that the details are all to come, and that the simplification of the lines means overhasty generalization. Mind is, so to say, our common enemy; and the laboratory psychologist learns, by dearly bought experience, not to underestimate his opponent. Secondly, I would remind you that, after all, objective work in psychology must always be inferential; introspection gives the pattern, sets the standard, of analysis and explanation. If we interpret the animal mind by the law of parsimony, our only justification is that introspection discovers the reign of this law in the human consciousness; if we subsume the evolution of mind in the animal series to the principle of natural selection, our only justification is, again, that introspection discovers the working of this same principle in our own case. As I put it just now, there is but one excuse for the neglect of introspection in psychology, and that is that introspection is impossible; but even here our neglect is methodical only, and does not—must not—extend to interpretation. These things have been said so often* that they have become commonplaces; but even a commonplace may be true—and it makes a difference, too, whether the truth be urged with polemical or with friendly intent. I should like to see more cooperation between the alienist, or the student of comparative psychology, and the laboratory psychologist; quite apart from practical results, such cooperation would be of great advantage to the psychological system. We can hardly hope —this point should be borne in mind—that the two interests, the objective and the subjective, will be combined in the same person. When one has once stepped inside the ring of the normal, adult consciousness, there is very little temptation to step out again; the problems that I listed a little while ago are enough to occupy several generations of workers, and the fascination of the work is like the fascination of the mountains or the sea. And if one begins from the outside, with the child or the animal or the abnormal mind, there is little

* A method may be too refined for the man who is using it, or for the problem upon which he is immediately engaged. But these are different matters.

* In saying them, from the 'narrower' point of view, I am, of course, hoping for similar cautions (at any rate, for varied advice and information) from the more 'objective' psychologists. What they will have to tell their colleagues of the laboratory, I do not know; but I have no doubt that it will be worth listening to.

likelihood that one can breathe the confining air of the laboratory, or that one will presently limit one's range of interests to oneself. Partly it is a matter of temperament, partly a matter of chance introduction or of continued occupation. The two types of psychologist are distinct: all the more reason that they should work in harmonious cooperation.

I hope that, in this latter portion of my address, I have not traveled too far out of the record. Some men have problems thrust upon them. And, after all, if what I have said contributes ever so little to the furtherance of mutual aid and the increase of mutual esteem, as between psychologists of different camps, I may hope for forgiveness, even though I have exceeded the letter of my instructions. Now let me briefly summarize what I have said. I began, you will remember, by pointing out that, above and apart from the many special problems of experimental psychology, there lies the great problem of self-definition, of the range and scope of the experimental method in psychology. Then, under the headings of psychology proper and of psychophysics, I called your attention to a series of laboratory problems that, more or less insistently, more or less immediately, call for solution. Whatever else experimental psychology may be, I said, these issues are issues of experimental psychology. Incidentally, I deprecated any departure, at the bidding of philosophy, from the straight path of psychological investigation; and I deprecated also that neglect of introspective control in psychology which has been the besetting sin of many whose direct interest lies in psychophysics. I then went on to include in experimental psychology the more objective applications of the experimental method in child psychology, in animal psychology, in abnormal psychology. It was not my province to detail the special questions in these fields; they form the topic of other addresses in other sections. But I should regard as incomplete any review of the problems of experimental psychology which omitted reference to them. Their consideration helps us to attack that first problem of definition, clarifies our method, and furnishes an opportunity for the give-and-take of criticism and encouragement. We can not afford to misunderstand one another, as we can not afford to waste our time on unreal and constructive problems. The work presses; the rule of work is definite and unmistakable; there is room in the workshop for all sorts and conditions of men. I do not think that the outlook of any science could be more hopeful; I do not think that we need fear a lessening of that quiet enthusiasm which, from the first, in the beginner as in the mature student, has been the salient characteristic of the experimental psychologist.

E. B. Titchener.

SCIENTIFIC BOOKS.

An Introduction to the Theory of Mental and Social Measurements. By Edward L. Thorndike. New York, The Science Press. 1904. Pp. 212. 8vo. Price, $1.50.

In this book Dr. Thorndike has undertaken to explain the 'meaning and use' of recent contributions to statistical theory 'in common language to a common-sense thinker.' "Knowledge will be presupposed of only the elements of arithmetic and algebra. Artificial symbols will be used only where they are really convenient." In order are discussed: Units of measurement, the measurement of an individual and a group, the causes of variability and the theory of probability, the arithmetic of calculating central tendencies and variabilities, the transmutation of measures by relative position into terms of units of amount, the measurement of differences, changes and relationships and the use of tables, reliability of measures and errors of measurements.