PART I

METHOD
CHAPTER I

INTRODUCTION

If the method be but true,
Ours shall be the truths we woo.

I — THE FOUNDATIONS OF PSYCHOLOGY.*

Of late it has almost become the fashion to assume that the foundations of psychology are firmly laid, and that all that remains is to work out problems of secondary importance. It is argued that we have now only to apply the knowledge which has been gained, and to occupy ourselves with an exhaustive examination of the psychology of the child, of races, of animals, and so forth. If this be so, the reader should find in this book a methodical restatement, a dogmatic exposition of the established body of psychological conclusions. Should he expect that, he will be disappointed. According to my interpretation of the data, the ship of psychology is still in mid-ocean, still at the mercy of storms of doubt, still without chart or compass, and still far from port. I maintain not only that the elementary principles of psychology have still to be established; but I believe that, from the scientific point of view, no serious attempt has yet been made in that direction.

So daring an assertion necessitates a prolonged defence. When a literature is so voluminous as is that of psychology — when Americans, Englishmen, Frenchmen and Germans are vying with each other in the production of learned treatises, it seems almost madness to suggest that the scheme of operations is strategically suicidal, and that nothing but a retreat to the base, and a new plan of campaign, can ensure success. However, such is my contention, a contention which, in the interests of science, I feel bound to make and to substantiate. Grave as is my task, its gravity is yet exceeded by its unpleasantness. One shrinks, and never ceases to shrink, from the unwelcome duty of sounding a retreat. The

* "'Not Descartes, nor Malebranche, nor Locke, nor Berkeley, nor Hume, nor Leibnitz, seem to be acquainted with this word" (Boirac, article "Psychologie," in La Grande Encyclopédie, 1800).
heart almost fails when one has to announce to others that the news of victory which we all greeted with joy, is void of truth. Yet, while destructive criticism may give rise to bitter disappointment, we endure it because of its ultimately beneficial effects.

In accordance with the only justifiable mode of procedure, I shall attempt to make good my contention by an appeal to history. First, we will dwell upon the history of the famous doctrine of the Association of Ideas—a doctrine which, while generally correct in its contention that every given idea is connected with the idea which preceded it, is, as I hope to show hereafter (secs. 88-92), quite in error when it reasons backwards, that the likeness between two ideas makes the one follow the other, since, as I hold, relevancy to a topic determines which, if any, of the part-ideas shall be developed.* In some form or another, Associationism was recognised, there seems little doubt, from the days of Aristotle right through the Middle Ages.†

It was Hobbes who in more modern times explained the flow of thought by having recourse to an associative principle. He held that one particular thought or portion of thought followed another because antecedent and consequent formed originally part of one continuous state, and for no other reason. To him, however vaguely he stated it (Leviathan, 1651, part 1, ch. 3), the principle of the Association of Ideas offered a complete explanation of consecutive thought. We are not, in this section, interested in the truth, or otherwise, of this supposed key. It need only be observed that there is no evidence that either Aristotle or his followers, or Hobbes, made an exhaustive study of the subject, for the purpose of either discovering or verifying the explanation. We are nowhere led to believe that these thinkers, for instance, endeavoured to take note of an entire section of thought lasting for a minute or so, and then applied their theory. Nor have we any reason to think that they examined a large number of examples in such a manner as to exclude prejudice and to make their conclusions comprehensive. Nor can it be said that they verified their results experimentally. Their method was the same as that of the geographers and naturalists of the Middle Ages, and completely unlike that of the school of which Galileo was one of the early champions. It is, indeed, an abuse of terms to call by the name of method a procedure from which all orderliness is missing. In the one case, the observer follows vague suggestions which are not verified. In the other, he pursues a method which has taken centuries in the shaping, and which is almost wholly secure

* In this section I am perhaps unduly severe towards the Associationists. While it is true that they offer no explanation of the flow of thought, it must yet be borne in mind that their principle of contiguity forms a valuable basis for such an explanation. The woodiness and hastiness of their statements, however, called for censure. (See secs. 90-1.)

† See Hamilton, Note D** in his edition of Reid’s Works; Stump Robertson, article “Association,” in Enc. Brit.; Volkman, Lehrbuch der Psychologie, 1894-5, pp. 452-5, note 4; Mervoy, L’Association des Idées, 1864; and Ferri, Psychologie de l’Association, 1883.
against superficial reasoning. So long as science was left to Divine Philosophy, progress remained an impossibility, because of the absence of objective tests in the mode of inquiry. Science became fruitful when she evolved a method. The grounds on which she based it, we shall discuss later on. Suffice it to have shown that Hobbes and his precursors possessed no warrant for their conclusions. If they were right, theirs were courageous guesses, and nothing more.

Locke, in his *Human Understanding*, 1690, bk. 2, ch. 33, gave birth, in an incidental way, to the phrase Association of Ideas. He employed it in explaining certain obstinate and injurious associations which men, if they were careless, were likely to form. The theory plays otherwise no part in his system, and his short and casual reference makes it undoubted that he arrived at his conclusions by no scientific route.

Hobbes and Locke having watered the plant, it began quietly to grow. When Hume referred to it (*A Treatise on Human Nature*, 1739, bk. 3, part 1, sec. 4), it was already attracting a good deal of attention. The great and genial sceptic professed to expound a principle which was to the realm of thought—to psychology, what gravitation was to the material universe—to physics. Contiguity or proximity in place or time, resemblance, and cause and effect, were the three masters of the ceremonies, who allotted to each unit its position in a train of thought. Given these, and we had an explanation of the procession of things or mental phenomena, such as was established in regard to gravitation by Newton. These principles, it was held, were elemental, and could not be reduced to other principles; and one must admit that they were eminently plausible, especially when illuminated by means of a multiplicity of examples which led the student in a predetermined direction, and kept him from searching for instances of a conflicting character.

One single illustration must suffice. On a windy day I look at a field of barley, and observe how the blades bend in the eastern portion, and how, in succession, the contiguous blades, right to the western portion, also bend. Does it follow of necessity that the bending of one blade caused the next one to it to bend, and so on till the last of the series had bent? We know that this presents a false explanation; and hence we may assume it at least as possible that contiguous mental changes are not due to contiguous association at all.* The chief point, however, is that Hume's ably urged assertions were speculative and not scientific. He applied no canons in which we have confidence.

Meanwhile Hartley was pondering over the same problem. In 1749, ten years after Hume's treatise had appeared, he published his *Observations on Man's Frame*, which is an elaborate physiological vindication of Associationism, based on a theory of vibrations. He went further than Hume and allowed only for contiguity, or proximity in space and time. The student will observe, as he proceeds, how sadly ignorant we are at

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dthis late date, of the more intimate processes of the central nervous system. Hence Hartley's physiology was conjectural, and that is all one can say of his psychology. There is everything to indicate that he did not follow a scientific mode of procedure, as we understand it to-day, and that whatever his success, he only sought for proofs in the scholastic fashion.

The physical sciences were, at that period, progressing by leaps and bounds, conquering realm after realm of nature. The pace was so swift that men felt assured that the millenium was near. It was then that James Mill published his work, An Analysis of the Human Mind, 1829. In this work, the elder Mill seemingly applied the scientific method to psychology; and he is not the first, if Ophelia is to be believed, whose practice was not consistent with his profession. According to him, the firmness of the associative process depended on frequency of impressions in conjunction with vividness (i.e., p. 83). He has been, and has remained, the Associationist par excellence. Yet, correct as he was in breaking with metaphysics, more correct than many later psychologists, he nevertheless mistook philosophic naturalism for scientific method. One might almost say that he discovered nothing; that he verified nothing; and that he only elaborated in a speculative fashion a speculative system. Though I agree that his work is profoundly stimulating, his conclusions were yet far from being in advance of those of Brown's, published some five years previously.

After Mill the elder, came a deluge of writers who laid more or less stress on Association, their explanations differing in detail only. Thus Hamilton (d. 1856) tells us (Lectures on Metaphysics, 1877, ii, p. 238) that “‘those thoughts suggest each other which had, previously constituted parts of the same entire or total act of cognition,” and that this law is “an ultimate fact” (ibid, p. 240). Shadworth Hodgson (Time and Space, 1865, pp. 266-8) contends that in any state of consciousness, the uninteresting part fades, and the interesting portion develops, thus forming a train of thought. Bradley, again (Logic, 1883, p. 278), argues that “any part of a single state of mind tends, if reproduced, to re-instate the remainder; or any element tends to reproduce those elements with which it has formed one state of mind.”

John Stuart Mill boldly pressed the notion into philosophic service. Nevertheless, with characteristic hesitancy, he admits (Footnote in his father’s Analysis, 1869, ii, p. 71) that “the highly interesting idea of the end in view . . . determines, within certain limits, which of the ideas associated with each link of the chain shall be aroused and form the next link,” a passing admission, which, if consistently elaborated, would have revolutionised the Associationist position. He also, like Hamilton, quotes to a similar effect, Cardaillac, Études Elémentaires de Philosophie.

Bain, in his Senses and Intellect, 1894, following Hartley, Brown and James Mill, again elaborated what had already been over-elaborated. Without being, unfortunately, one whit less speculative than his predecessors, he set himself more clearly to determine the supposed laws of
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Association. Leaving aside compound and constructive association, and
premising that he rejects, as derived, the law of contrast, the two remaining
laws read as follows:—Law of Contiguity: "Actions, sensations, and states
of feeling, occurring together or in close succession, tend to grow together
or cohere, in such a way that, when any one of them is afterwards pre-
sented to the mind, the others are apt to be brought up in idea" (p. 341).
Law of Similarity: "Present actions, sensations, thoughts or emotions
tend to revive their like among previous Impressions or States (p. 486)."
Yet even Bain casually allows, without recognising the radical nature of his
admission, that recency and interest may influence the course of association
between one antecedent and its consequent. He says, for instance: "If
historical events have been recently in my mind, the events referable to
this locality are suggested" (p. 595).

According to Spencer (Psychology, 1890, i, pp. 269-70) "the funda-
mental law of association of relations, like the fundamental law of
association of feelings, is that each, at the moment of presentation,
aggregates with its like in past experience." This, however, he explains
(i, p. 416) "is the law of intelligence in the abstract," and "the change which
actually takes place is the resultant of many tendencies acting together."

James (Psychology, 1890, i, p. 566) says similarly that "when two
elementary brain processes have been active together or in immediate
succession, one of them, on reoccurring, tends to propagate its excitation
into the other," a thesis which he further develops on p. 567. Baldwin
(Senses and Intellect, 1890) likewise gives his assent to the principle.

To return to England. Sully joins the chorus (Human Mind, 1892).
Lloyd Morgan (An Introduction to Comparative Psychology, 1894) adopts
the principle in a slightly modified form. He writes: "The recurrence of
a will be followed by the recurrence of l under similar marginal conditions"
(p. 72), and "only under similar marginal conditions will the impression a
suggest l" (p. 72). He defends his slight heresy by saying that the first
law of motion—viz., that a body, if left free, will move at a uniform rate
and in a straight line,—is true, though bodies are never left free, for-
getting that the marginal conditions are, according to him, essential. Stout
(Analytic Psychology, 1896, and Manual, 1898-9) accepts the principle in
a modified form. In one place (Manual, 1898, p. 74) he states, how-
ever, that "each phase of the process before the end is reached is in-
complete, and tends by its own inherent constitution to pass beyond itself."

We have seen the manner in which Hobbes and Hume and their pre-
decessors attained their results. The later English writers, including the
latest, have pursued the same policy. Not one of them seems to have
thought of testing the doctrine, as men are accustomed to do in scientific
inquiries. From first to last, scarcely veiled speculation determined the
opinion of English and American psychologists in so important a matter.
A belief thus based can claim no respect on scientific grounds. It may be
true or it may not be true; it certainly is not proven.

If we turn to Germany, we find that Herbart (Lehrbuch, 1816, p. 69),
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the founder of an important school, as well as Beneke, adopted Associationism. According to the Herbartian school, presentations hinder or facilitate others coming into the foreground of consciousness. The Herbartians possess an elaborate mechanism, reminding one of pulleys, levers, crowbars, cranes, and what not, for the transporting hither and thither of presentations. Of science there is not a grain. Like Hans Andersen's tailors, the presentations appear to be busily engaged, but produce nothing. Such huge treatises as those of Volkmann, a follower of Herbart, and Lipps, a follower of Beneke, form the completest indictment of present-day psychological methods. Lotze, again (Microcosmus, 1885, i, p. 215), says: "That a newly-produced impression revives the forgotten idea of a previous and similar one, or recalls it to consciousness, is the simplest of the universal laws that regulate the course of memory." In Germany, then, we find, to a large extent, a similar state of affairs to that existing in England.

Paulhan (L'Activité Mentale, 1889), in France, with his Systematic Association, has broken through the tradition. He rightly holds that actions are determined by ends, and that each fraction of a train of thought is not determined primarily by its predecessor, but by the end in view. In a word, association is systematic or topical, and not atomic. To this view Ladd (Psychology, Descriptive and Explanatory, 1894) subscribes. Largely as I agree with Paulhan, I still wish to insist that there is no sign that he deduced his theory from scientific observation or has verified it in fact.

Taking Associationism as a whole, omitting for the present the experimental school, one feels justified in maintaining that its procedure has been unscientific, and its results, as will be shown, unsatisfactory. So far the foundations of psychology remain to be laid. (See sec. 90a.)

The most striking exemplification of unscientific method is offered in the case of Habit. Locke tells us (Human Understanding, 1690, bk. 2, ch. 22, sec. 10) that the "power or ability in man, of doing anything, when it has been acquired by frequently doing the same thing, is that idea we name habit." And in another place he compares the acquirement of habit to a path worn smooth, saying that repetition of action has the same effect on the brain. (Ibid, bk. 2, ch. 33, sec. 6.) This hazarded conjecture, utterly unsubstantiated, has been the first and the last word of the psychologists on the subject. As any well-known work on physiology will show, our knowledge of brain processes is unspeakably inadequate to permit us to make such an assertion. When we come to psychology, we find no other confirmation than popular rumour might lend; there is not even the faintest suspicion of scientific caution. Everybody acknowledges what has not even been tentatively examined. So complete has been the acceptance of this theory that the leading psychological journals, such as Mind (omitting my own contribution), The Psychological Review, The American Journal of Psychology, Brain, and Philosophische Studien, have not a single contribution on the subject, whilst the Revue Philosophique had one article twenty-six years ago. This is not owing to indifference. James and others insist upon its importance at some length, and there are few psychologists who pass it by.
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Yet the feeblest attempt at investigation would have shown the difference between complacent popular acquiescence, which selects some superficial aspect, and scientific comprehension, which points to the general facts. The complex nature of habit the student will find exhaustively discussed in ch. 3.

As a third illustration in point, one may take Attention. The subject is part of the time honoured stock-in-trade of psychology. It is also one which has been seriously studied; and the failure in this case has, therefore, not been so complete, though even here a certain degree of attention has been generally mistaken for attention, as such. Had there been only a consistent and thorough neglect of data other than those arrived at by direct study—a neglect of metaphysics, of irrelevant theology, of slipshod theory, of commonplaces, we might be near the end of its problems instead of near the beginning. The trouble has been that theories have so darkened the minds of students, that the light of the most powerful intellects scarcely shed a glimmer along the path. By means of almost superhuman efforts a trifle was gained here and there, and these trifles tended more to bewilder than to encourage. The primitive tools worked wonders. Taking a general survey, however, the subject is still buried in obscurity, though the outlook is not so unpromising as in the two other cases. As I hope to explain in ch. 2, attention is not an intermittent process requiring strain, but it is activity itself from the point of view of the direction in which it is engaged.

The three illustrations I have given—Association, Habit and Attention—are, I believe, typical of the present-day state of reflective psychology as a whole. They justify the contention that the scientific foundations have still to be laid.

When one turns to those who emphasise the physiological aspect, scholars such as Maudsley (Physiology of Mind, 1876), Lewes (Problems of Life and Mind, 1874), and Bastian (The Brain as an Organ of Mind, 1880), one is even more disappointed. I fully appreciate their insistence upon the neural aspect of thought, with which their work began and ended; but their positive results were most meagre, since they tried to establish a fictitious psychology by means of an admittedly fictitious neurology.

The work of the psycho-physical school will be examined in sec. 8.

What is the nature of the method which I have called unscientific and barren? It may be illustrated as follows. Owing to a new departure in psychological thinking some fresh problem has arisen, say, the problem of effort. Men ask themselves accordingly what is effort, and in asking recall what seem relevant illustrations. These illustrations, being what is uppermost in thought, display some particular and striking characteristic, and men then turn over the old clothes of memory for proof. At the same time they more or less carefully note this or that particular case of living effort. Meanwhile (sec. 160) the suggested solution attracts as much what is favourable to itself as it prevents from developing what is disadvantageous to the theory, while the situation is aggravated by the fact that the actual things observed are illegitimately suggested by the hypothesis. Thence follows some chance conclusion which, psychologically considered, appears firmly based. As against this method, I would advise the exhaustive study of a large variety of facts, with no anxiety—until we have proceeded far—to obtain conclusions. We would apply impartially such rules as shall eliminate any bias (sec. 136), and we would lay no
stress on anything recollected unless it be something due to careful examination, and unless we have, by rule (sec. 136), exhausted all the relevant material to be found in the memory, the stores of which are useful for conduct, but not for eliciting truths. Such a method, I hold, is scientific, fruitful and well-based. Such a method, I claim, is the quintessence of the scientific method as applied in the physical sciences. Instruments and mathematical treatment may supplement it, but not displace it. These latter only give polish and precision to the great truths otherwise obtained. [Let the student observe that the mere neglect of metaphysics or theology will not assist him in the discovery of truth.]

2.—The Use of Hypotheses.

In his *Comparative Psychology*, Lloyd Morgan tells the world that "Psychologists make, or should make, no claim to any monopoly of knowledge in the subject they study; their province is mainly to systematise that knowledge" (p. 44). Fortunately for us, this author's practice is not in accord with his theory, for his conception of focal and marginal consciousness forms a valuable contribution to psychology. Nevertheless there is incalculable mischief in his assertion, however hedged round. What would be thought of a physicist or an astronomer who mainly systematised knowledge without seeing that it was gathered at first hand by competent specialists or by himself, or who gave a *locus standi* to "the plain man of shrewd insight"? The idea is monstrous. A psychology which mainly busied itself with systematising the conclusions of "those who are not professed psychologists" might as well relieve Sisyphus of his task. The one is as likely to be successful as the other.

Underlying the statement I am criticising, there is an unpleasant truth. Unhappily, psychologists have been too anxious to systematise that which they had not previously examined. They leaned fatally to the opinion that truth could be sifted from popular notions as is sand by means of a sieve. The student must recognise once for all that if he is to be on the scientific plane he must make a claim on behalf of psychologists to a "monopoly of knowledge," and that he must not attempt to systematise what has not been procured through the application of scientific methods. It has been the bane of psychologists that they have tacitly assumed that facts of consciousness do not require to be collected with the disciplined care which other sciences employ. I have advisedly said "tacitly" assumed, because few men have spoken out boldly as Lloyd Morgan does. The mischief has lain in unthinkingly proceeding along the wrong path, in giving elaborate explanations of popular fictions, and in not deliberately recognising that in psychology, as in physics, unbridled speculation is criminal waste.

It is always dangerous to make unqualified statements. Instead of condemning speculation outright, it would be perhaps better to pronounce sentence against it when its excursions are not rigorously limited. Few can object when "speculation is but the play of the imagination along the fringe which borders our knowledge" (Lloyd Morgan, *Comparative Psychology*, 1894, p. 323). But then we must insist upon a reasonable interpretation which shall not make the fringe equal to the robe to which it
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belongs. Speculation thus interpreted is not only innocent; it is an imperative necessity. If men formed hypotheses about the things on the borders of knowledge, they would never go far astray, and often return laden with trophies. Let me, however, define this term. “By an hypothesis we understand an assertion which is employed as a principle of explanation, though its correctness is yet unproven” (Volkmann, Lehrbuch, 1894, i, p. 103). I should say, then, that accepting the above definition, hypotheses are legitimate when they apply to the fringe which borders our knowledge. In this limiting their uses one seems not only to be cutting at the root of ordinary psychological speculation, but to be contradicting weighty authorities. According to Jevons, study without hypothesis is not scientific. “The true course of inductive procedure is that which has yielded all the more lofty results of science. It consists in Anticipating Nature, in the sense of forming hypotheses as to the laws which are probably in operation; and then observing whether the combinations of phenomena are such as would follow from the laws supposed” (Principles of Science, 1877, p. 509).

The fallacy underlying the preceding statement must be exposed. Where, the student should ask himself, do we find our hypotheses? Do they come to us as divine intuitions? Can we obtain them, or any new truth, by a course of abstract reasoning? Or is it not rather that we act on the basis of one observation, and note “whether the phenomena are such as would” be in accordance with that observation? This must obviously be the case, for no compounding of zeros can force the gates of nothingness. If that be so, Jevons' statement implies that there are no hidden facts which are not amenable to casual and superficial observation. That, as we shall see, is an indefensible position. There is no guessing which can take us farther than the fringe which borders our knowledge. We may set up the wildest theory, and yet it will be composed of what is known, its extravagance arguing no fresh knowledge. Take, for instance, a passage from Volkmann, which well illustrates the length to which fanciful speculation or adaptation may go: “The presentational mass, which is helplessly sinking, meets with the freely-rising apperception mass, is gripped and firmly held by that, and placed before the ego as an object to be viewed” (Lehrbuch, 1895, ii, p. 206). This pretentious psychic mechanism to which we are here introduced is but a copy of material mechanisms, and the hypothesis underlying its use implies that analogy holds the key to the whole realm of the unknown, while the truth is that the master facts of a science are due to deliberate research. It is for this reason that one can scarcely detect a single sound brick in the elaborate Herbartian structure—an sophisticated observation of a scientific type has been scouted, and hypotheses were powerless to discover the new facts.

If my interpretation be correct, the progress of science should bear me out;* as indeed it does. The serious study of fact is continually going hand in hand with tentative speculation “along the fringe which borders

* See, however, Rigg, The Place of Hypothesis in Experimental Science, 1887.
our knowledge." Workers here and there wrest from nature trivial secrets. These secrets accumulate, and men now and then tentatively combine a few of them. As the store of knowledge assumes considerable proportions, so larger and larger generalisations are ventured upon in every direction. At last, when general statements of a far-reaching nature abound, men rightly venture to speculate as to the broadest generalisations possible. Already the chief notions are in the air; already those engaged in the search feel that they are approaching a solution. After many minor attempts some one, a Newton or a Darwin, sometimes comes to the front and completes the structure. As knowledge progresses, new speculations and observations "along the fringe which borders our knowledge" are readily suggested and easily verified. No stupid guess is possible to the man who has ascertained facts to go upon; and when he goes astray, the clashing of his theory with his existing store soon brings him back to the right path. Progress, though slow, is hence certain and reliable when men shun the use of large hypotheses. In the absence of well-ascertained facts, everything is changed. A guess, under these conditions, has no inherent plausibility, and what is as bad, any attempt at verification in an unexplored realm can only end in failure either acknowledged, or, as is more usual, disguised. Our intellectual sense of equilibration forsakes us when it has no general facts to assist it. Thus large hypotheses in neurology are not only worthless, but vicious, because it is only by the accumulation of facts and generalisations that solutions of neural difficulties can be reached at all. Sciences cannot be, and never have been, guessed at. Jevons' mistake was a plausible one. In the physical sciences so huge a body of organised observations has been accumulated that the guesses of men of learning are at once permissible and easily verified. Where, on the other hand, as in psychology, the stock of genuine observations is infinitesimal, these guesses will be unreasonable, and their verification will consist of an immensely protracted process, equal to the building up of the science itself. Indeed, Jevons supplies his own antidote. He says of the alchemists, of whom Newton was one, that "Many of them were men of the greatest acuteness, and their indefatigable labours were pursued through many centuries. A few things were discovered by them, but a true insight into nature now enables chemists to discover more useful facts in a year than were yielded by the alchemists during many centuries" (ibid., p. 505). The alchemists, like many present-day psychologists, indulged in large hypotheses, and large hypotheses are useless, are impedimenta in more senses than one.

I, therefore, side with Bacon (d. 1626) who says: "The subtilty of nature is far beyond that of sense or of the understanding: so that the specious meditations, speculations, and theocrites of mankind are but a kind of insanity, only there is no one to stand by and observe it" (Novum Organum, ed. 1893, bk. i, x); and who draws his admirably prophetic picture of some modern thinkers: "When any one prepares himself for discovery, he first inquires and obtains a full account of all that has
been said on the subject by others, then adds his own reflections, and stirs up, and, as it were, invokes his own spirit, after much mental labour, to disclose its oracles. All which is a method without foundation and merely turns on opinion ” (ibid, bk. 1, lxxii). Bacon has been roundly attacked for his views, which are all too sound. Even Newton has not escaped chastisement. His celebrated pronouncements against hypotheses are discounted on the assumption that he himself gained his great successes by their means. The censure on him is wholly inapplicable, for his great generalisation was no more self-evolved than that of Darwin. No man of his attainments could have been far wrong in his surmises, and the accumulations of organised knowledge were so extensive that verification was within easy reach. Newton focussed the learning of his time. When he goes beyond that, as in his conjectures concerning general stellar problems, he is only a wordy theologian. Hence, regarding the term hypothesis as implying a supposition which is not preceded by exact study, we can heartily agree with Newton’s strong and unmistakable language. “Whatever is not deduced from the phenomena, is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy. In this philosophy particular propositions are inferred from the phenomena, and afterwards rendered general by induction” (Mathematical Principles, trans. by A. Motte, 1729, ii, p. 392). Compare this hesitancy with the impatience of a modern writer: “Give us theories, theories, always theories!” (Baldwin, Mental Development, 1895, p. 38); or compare it with the speculative deductions of the learned Whewell: “...an Art of Discovery is not possible. At each step of the progress of science, are needed invention, sagacity, genius—elements which no Art can give. We may hope in vain, as Bacon hoped, for an organ which shall enable all men to construct scientific truths, as a pair of compasses enables all men to construct exact circles” (Philosophy of the Inductive Sciences, 1847, p. viii. See also his Novum Organum Renovatum, 1858).

The essential thing for the student to remember is that the chief facts of every science are only obtainable by the close observer after laborious research.* Let me give one illustration. What are dreams? Seeming to resemble the waking imagination, we forthwith guess that they are the result of vivid imaginings. Quantities of books had been written on the subject, based on speculation and occasional observation. Yet proper light only began to be thrown on the problems of dream-life when men methodically and for a considerable period observed their own dream-states. Excepting such contributions as those of Giessler and Schwartzkopff, most of the books are almost superfluous. Let me now state the dream facts as they appear to me (ch. 10). (1) The muscular and sensory tones are lowered, and the position of the various parts of the body is unknown (sec. 19, 1st and 2nd conclusion). (2) The characteristic pictures spring usually out of the

*Men are sometimes said to stumble upon important discoveries. In such cases it is the preparedness of the discoverer, which accounts for the discovery.
dark or closed-eye field of vision, and are not imaginings at all. (3) Most of the dreams are initiated and maintained from without, resembling waking life and not waking imagination. (4) The amount of possible effort is largely reduced, whence follows confused thinking, (5) impossibility of strenuous thought, (6) inappropriate recollection, (7) a strong tendency for appetites, expectations, doubts, hopes and fears to actualise themselves, (8) the continual creation of a setting to each picture, giving it an air of reality, and (9) the fact that the happenings are compounded out of most recent events and those immediately passed. Thus points (2) and (3) argue afferent or outer influences, and the other points follow from the lack of strenuousness. Now, by what rational calculus could one have jumped from imaginative seeing to the retinal pictures which are plainly due, in part at least, to new circumstances. And, suppose we had not previously ascertained the fact, how could we have discovered the arbitrary creation of settings to the dream-pictures? The two essential facts of dream-life are afferent, or outward, influences and a drop in strenuousness. Yet the waking imagination has nothing to do with either of those factors: it tears away from outer impressions and implies considerable force of thought. Where, we may ask accordingly, lies the difference between the methods of ancient philosophy and those of current psychology? And who would ever confound scientific with philosophic procedure? The traditional philosophic method is as barren as the syllogism which it has produced. It may, therefore, be laid down for the student’s guidance that scientific progress depends on gaining new classes of facts, that such can only be acquired by painfully close and methodical observation in the first instance, and that they are not obtainable by employing hypotheses which go beyond well-ascertained facts and established generalisations. As Tyndall puts it: “The thing to be encouraged here is a reverent freedom—a freedom preceded by the hard discipline which checks licentiousness in speculation” (Scientific Use of the Imagination, 1872, p. 33).

Adam, L’Imagination dans la Découverte Scientifique, 1890; Boirac, La Méthode Expérimentale, 1898; Naville, De la Place de l’Hypothèse dans la Science, 1876; Naville, Les Conditions des Hypothèses Sérieuses, 1877; Naville, Les Principes Directeurs des Hypothèses, 1877; and Venn, The Use of Hypotheses, 1878.

3.—Approaches to the Study of Psychology.

To understand the human frame, we require to know its constitution to the minutest part, together with its reactions when stimulated: a microscope and a dissecting knife, with a battery, might be an adequate equipment for this task. As matters stand, these tools are found to be inadequate, and secondary means are, therefore, resorted to in addition. We study the development of the embryo, animal characteristics, evolutionary traits, and cases of disease or malformation. We also stain the tissues with preparations which affect only certain parts; we cut nerve bundles, as we might cut strings, and notice which of the nerves degenerate as a consequence; and we experimentally alter or remove structures or parts of structures.
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With no understanding, or next to none, of actual structure, the secondary means would be of less than doubtful value. Vague and diffuse observations would of necessity be followed by vague and diffuse conclusions.

So with psychology. In the absence of a power of self-observation, advance would be barred, for secondary means can only be helpful when the primary approach is not virtually closed. If a direct approach be impracticable, we shall be compelled to rest satisfied with vague hints. Yet, taking a broad view, our aim is to ascertain the whole edifice of thought by experimentally looking within, or, as it is called, by introspection. Not until this approach is found to be demonstrably inaccessible like the centre of the earth is now, may we think of applying other methods. Most thinkers have, however, despaired of obtaining a satisfactory peep into the thought jungle, and have consequently advocated secondary means of attaining their object. One of these means which Herbart eloquently advocated, the use of hypotheses, I have already examined and rejected. The others are as follows: retrospection; the comparative study of children, races, animals, criminals, the insane, hypnotised and diseased persons; as well as the examination of products such as facial expressions, monuments or books. As I believe that introspection is eminently practicable, I consider the other means to be of secondary importance, and shall refer to them only incidentally, if at all.

4.—INTROSPECTION.

The difficulties of introspection were insisted on at an early date by Hume: "It is remarkable concerning the operations of the mind, that, though most intimately present to us, yet, whenever they become the object of reflection, they seem involved in obscurity; nor can the eye readily find those lines and boundaries, which discriminate and distinguish them. The objects are too fine to remain long in the same aspect or situation; and must be apprehended in an instant, by a superior penetration, derived from nature, and improved by habit and reflection" (Inquiry, 1747, sec. 1. See also Hume's Introduction to his Treatise on Human Nature). If the objects "must be apprehended in an instant," reasonable observation is out of the question; but we are convinced that absence of adequate introspective training accounted for Hume's opinion.

To one hypothesis we owe almost the entire neglect of introspection. Auguste Comte (d. 1857) maintained that "the affective functions, and yet more the intellectual, exhibit . . . . this particular characteristic, that they cannot be observed during their operation, but only in their results" (Positive Philosophy, 1875, tr., i, p. 382). This belief, never thoroughly tested, pervades most criticism. Herbart* (Psychologie, i, p. 206), says: "Do you intend to observe yourself passively, so as to clearly perceive what is

*For accounts of Herbart and Herbartians, see Stout, The Herbartian Psychology, 1888; Stout, Herbart compared with the English Psychologists, 1889; Stout, Herbart's Disciples, 1899; Ward, article "Herbart," in Enc. Brit., 1886; Ribot, La Psychologie de Herbart, 1876. See also my account of Herbart in sec. 80.
proceeding within? Only the sooner will everything that was to be seen, become dark, and very soon the spectator will only face himself and his own attitude.” Again, in his Lehrbuch, 1816, p. 56, he says: “That which we are specially anxious to observe within ourselves, becomes confused during the inspection.” If Herbart be correct, then self-observation becomes useless; but here, too, we see that the existence of an indivisible ego is implied, for why otherwise should not one state be able to exist alongside of another state. Herbart’s disciple, Volkmann,* takes sides with equal boldness. Introspection “pre-supposes a breaking-up of the observer in a subjective portion which observes and an objective portion which is observed... Outside its range fall all those phenomena which, like passion, arduous thinking,... and attention, pre-suppose the undivided concentration and devotion of the whole process of presentation” (Volkmann, Lehrbuch, 1894, i, p. 41). And again, “the effort of the observer appreciably alters the object to be observed... The more seriously we wish to observe ourselves, the less do we find to observe” (ibid, p. 42). Thus Waitz (Psychologie, 1849, p. 673), another Herbartian: “The keenest self-inspection reveals only what is past.” So Nahlowsky (Das Gefühlslieben, 1862, p. 6), still another Herbartian: “As long as one is subject to a certain feeling, it is impossible to attend to it; we become acquainted with it only through memory.” Thus Brentano, Psychologie, 1874, pp. 35-6: “Objects which, as the saying is, are perceived without us are amenable to observation... To objects which are perceived within us this procedure is totally inapplicable.” In a similar vein Wundt (Grundriss, 1896, p. 25) says that “the intention of observing, which must exist in all exact observation, materially affects... psychic processes.” Why, in the nature of things, this should be, he leaves unexplained. Kant (Anthropologie, 1800, p. x), who, like Herbart (Psychologie, i, p. 233, and other places), confounds self-observation with morbid self-consciousness, writes: “The man who desires to explore his inner life puts himself in a critical condition, especially where the feelings are concerned, i.e., when his impulses are in action, he cannot observe himself, and when he observes himself, his impulses are at rest.” Ebbinghaus (Psychologie, 1897, i, p. 57) tells us that self-observation “cannot clearly and objectively apprehend the things towards which it is directed; it unavoidably displaces and falsifies them.” Joél (Lehrbuch, 1896, p. 10) says that his remarks do not imply “the impossibility of introspection, but only a difficulty, now more, now less, prominent in certain cases.” Thus Heinrich, Die moderne Psychologie, 1899, p. 97: “In self-inspection we can only note that which, on the ground of a hypothesis, we wish to observe; for introspection is made possible only by the reproduction of the phenomenon; and in reproducing we are always determined by a settled opinion.”

Maudsley, who cannot be suspected of possessing metaphysical bias, also reasons against introspection. “To direct consciousness inwardly to

* For an account of Volkmann, see Whittaker, Volkmann’s Psychology, 1890.
the observation of a particular state of mind is to isolate that activity for
the time, to cut it off from its relations, and, therefore, to render it unnatural.
In order to observe its own action, it is necessary that the mind pause from
activity; and yet it is the train of activity that is to be observed. So long
as you cannot effect the pause necessary for self-contemplation, there cannot
be a sufficient observation of the current of activity: if the pause is effected,
then there can be nothing to observe; there would be no consciousness,
for consciousness is awakened by the transition from one physical or mental
state to another" (Physiology of Mind, 1876, p. 17). To which we reply
that the dilemma does not exist for the skilled observer. Hamilton is
unambiguous. "Before we can observe a modification, it is already
altered; nay, the very intention of observing it, suffices for the change. It
hence results that the phenomenon can only be studied through its remini-
scence" (Metaphysics, 1877, i, p. 379).

Ward is equally unfriendly, as is Stout. The former speaks of "... the
very obvious fact that our powers of attention are limited, so that we
cannot alter the distribution of attention at any moment without altering
the contents of consciousness at that moment," implying, as I understand
him, that each observation occupies the whole field of attention or that the
connection between what co-exists is organic (Psychology, 1886, p. 37).
The latter says: "It has been maintained that all so-called introspection is
in reality retrospective. On this view, the modifications of our conscious-
ness vanish on being noticed, so that we do not apprehend them until they
are past. We shall see later on that there is sufficient justification for this
doctrine" (Analytic Psychology, 1896, i, p. 13).

The remaining English psychologists whom I shall quote, write in
a similar strain. Sully says: "The very directness of the inspection gives
rise to special difficulties. For all accurate and scientific observation
requires a certain aloofness of mind and absence of all but a purely
scientific interest in what is observed. When, however, we are called on
to observe our own mental states we cannot put ourselves into this cool
scrutinising attitude. The same person whose mind is agitated by a
passion is required to dispassionately inspect its characteristics. Thus in
the very process of observing we necessarily change the phenomena to be
observed" (Human Mind, 1892, i, p. 16. See also his Illusions of
Introspection, 1881, pp. 11-8). John Stuart Mill, as usual, is non-committal,
saying that at all events retrospection is possible (Auguste Comte and
Positivism, 1865, p. 64). Lloyd Morgan is also a hostile witness:
"Directly we begin to examine and measure any part of the margin, it
thereby ceases to be marginal and becomes focal" (Comparative
Psychology, 1894, p. 19). "We cannot examine the physical wave as it
passes; we can only endeavour to focus it, or its constituent parts, in the
mental vision, as it was when it was passing" (p. 20). Again, "Introspec-
tion always deals with past experience. ... Introspection is thus always
retrospection" (Psychology for Teachers, 1894, pp. 83-4).

Turning to América, opinión is also almost unanimous against intro-
speculation. James, in his characteristic style, says with regard to thoughts and feelings: "Whilst alive they are their own property; it is only post-mortem that they become his [the psychologist's] prey" (Psychology, 1890, i, p. 189). And Baldwin: "All our mental states are rendered more intense by the attention: consequently as soon as the state observed comes within the range of fruitful observation, it is changed, both in its own integrity and in its relative importance in the mental life" (Senses and Intellect, 1890, p. 10). So Titchener: "Direct introspection—observation of a process which is still running its course—is, as a matter of fact, entirely worthless; it defeats its own object" (An Outline of Psychology, 1896, p. 33).

Egger, in a dissertation on internal speech, pronounces against introspection and for retrospection. "Instead of observing directly our present condition," he says, "let us interrogate our memory" (La Parole Intérieure, 1881, p. 79). Paulhan (La Perception Inté rne et la Psychologie, 1888) sails round the subject, enlarging upon the possibilities of error. Rihgt (Psychology of the Emotions, 1897) speaks of introspection, "always an uncertain guide which leads us but a little way" (5, vi).

The army of the faithful is very small. Lewes (The Study of Psychology, 1879) seems to support the possibility of introspection unconditionally. Ladd and Münsterberg are, generally speaking, favourable. The former sensibly remarks that "the risks, limitations, possibilities, and proper uses of introspection in psychology can only be made known in connection with the development of the science itself" (Psychology, 1894, p. 15), while the latter rightly urges that talent, training and appropriate knowledge are requisite (Über Aufgaben, 1891). Yet, however, in another place Münsterberg writes: "To direct the attention, or the will, to our volitions, would mean the possession of a doubled self-consciousness, and is, therefore, a complete inner contradiction. Psychological analysis is, in consequence, restricted to the memory pictures of inner processes" (Die Willenshandlung, 1888, p. 57). Lipps (Grundtatsachen, 1883, pp. 10-1) rebuts the attacks on self-inspection in a general way. Beneke holds views as strong as mine, though one cannot detect any attempt at realising those views: "As for the objection that self-observation is impossible, it can be made only by those who have never seriously set themselves the task" (Neue Psychologie, 1845, p. 15. See also the preface to Beneke's Lehrbuch, 1845). So Robertson (Elements of Psychology, 1896, p. 14) says: "Without making light of the difficulties attending introspection, we may therefore rest satisfied that there is no reason why it should not, when properly conducted, lead to results of a purely scientific character." But this is a mere theoretical plea. Similarly Bailey (Letters, 1855-63), especially in his Third Series, pp. 1-13, argues in favour of introspection as against Comte. No application, however, is made by Bailey of the introspective method.

We have heard the witnesses for and against. Almost without exception, the testimony, directly or by implication, against introspection is crushing.
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My reply is as follows: retrospection is, of necessity, introspection, and if, therefore, introspection be impossible, our minds are absolute blanks.* All that can be said in favour of the former is that memories are less vivid (Münsterberg, *Ueber Aufgaben*, 1891, p. 171). On the other hand we must note that memory is but a poor copy, most of the details disappearing, and the very faintness being as often as not an obstacle to observation. Observe yourself, for instance, when you are being surprised, and compare your notes with those gained from a previous recollection of a state of surprise: the difference is that between poverty and wealth, while, at the same time, prejudice is more likely to alter what is faint than what is vivid. Psychologists speak of obtaining furtive glances; but if we reflect that only sustained and close observation is ever of use, we shall learn that these glances have no scientific value.

Let us now meet the chief charge by an illustration. Listening to a light conversation, I, at the same time, study the cover of an art journal and also tap the table; but, on another occasion, when I attend to a difficult argument, all my activity is absorbed in the one task of comprehending what is put forward. Attention, then, need not be directed to one object alone. Indeed, as we shall see in ch. 2, normal attention represents a stable quantity, and in proportion as an object requires less than that quantity, so we attend in two or more directions. To the attention it is indifferent whether we attend to five separate objects or to one. Hence, as we become absorbed in one direction, all other things fade—we hear nothing, we see nothing, we feel nothing, we think of nothing but what we are absorbed in. Suppose now that I am looking at the reading lamp before me. As my whole attention is not required to perform the feat, I can still hear the fowls running to be fed. Now it does not matter to the attention, as such, what I do as long as I do not leave it unoccupied. Further, the mere shifting, as such, of one portion of the contents does not necessarily interfere with the other portions. I can be attending to one object continuously while changing the other contents. The various unrelated objects of thought, to all intents and purposes, are like so many logs of wood lying at a distance from one another and removable separately. Only one condition must any second object of thought fulfill, it must not make such a call on our attention as shall interfere with the first object of thought. Let us apply this. To attend to a trend of thought or action is possible in so far as the second trend of thought does not, owing to its volume, affect the first one. Since, then, attending to the object of our attention, with those who are trained, requires the veriest trifle of exertion, it follows that the artificial objections against introspection fall. There is scarcely a passion, so wild, or a dream so subtle, that a trained psychologist cannot collectedly turn round and with freedom inspect the related process.

This is not the place to go into fine details. The student, who takes his

* "A mental state that is past is a mental state no longer, and to be unable to know it as present until it is past is to be unable to know it until it is non-existent and is as such beyond being known at all" (Hodgson, *The Adversaries of the Sceptic*, 1901, p. 41).
study seriously, and who knows that without effort nothing worthy is achieved, will persist, according to instructions, until he can walk about the thought smithy as self-possessed as the physicist in his laboratory. Great authorities who, like great mountains, echo each other, must not impose upon him. He must test, and not slavishly obey.

In view of Wundt's criticism (Selbstbeobachtung und innere Wahrnehmung, 1887) of Volckelt, it will be well to fix the meaning of the term Observation. For instance, a speech repeated by the phonograph in my hearing, while I was absorbed in thinking out some problem, never existed for me, that is to say, if I had a perfect memory and deliberately recollected the period during which the instrument was at work, I should not be able to recollect the speech. Again, the speech repeated by the phonograph in my hearing, while I was not otherwise engaged, did exist for me. A perfect memory would prove that. However, lacking an unexceptionable memory, I may have forgotten what I heard. In the latter case the final effects of hearing and not hearing are equally negative, though there was observation of the speech in one instance and not in the other. If we add, then, to the ordinary observant state an implicit desire to remember what is observed, and set the phonograph at work again, the effect will be that I not only heard a speech, but that I also am pretty certain to remember it. The presence of the implicit desire, however, gives rise to no important modification, i.e., if a perfect memory reproduced both sets of observations, the differences would be insignificant. Accordingly, scientific observation always looks beyond the moment, and carries with it, therefore, the implicit desire spoken of. Now what is true of observation holds of introspection. I may observe comparatively little; I may observe much and forget it almost instantly; or I may observe as well as remember, because there is an implicit desire to do so. Hence both in outer and inner observation the implicit desire to remember is essential for scientific purposes. To deny that we discriminate is to assert that nothing exists. If, then, discrimination be a fact, it becomes a question whether the implicit desire to remember introduces a fatal factor in introspection. My contention is that it does not; and that a perfect memory would reveal nothing appreciably different from what is actually disclosed. There is only one danger we must guard against. When we attend to a slightly discriminated state, say to what lies in the margin of vision, we must not assume an attitude which would transform it into a highly discriminated state, as by focussing an object. We must remain unbiassed spectators, a thing not at all impossible. Indeed, the slightly discriminated state may be produced experimentally just as much as the opposite state. Our conclusion, then, is that while discrimination, inner and outer, varies, the implicit desire to remember what has been discriminated, or to observe what is being discriminated, introduces no disturbing factor in the case of a skilled observer.*

5.—Practical Psychology.

In the physical sciences it is considered essential that a student should be experimentally trained. One who only knows chemistry from books, or even from observing demonstrations, is not regarded as seriously acquainted with it; and one who is ignorant of practice is generally not considered as trained in that subject. What we have just said concerning the physical sciences must in the future hold of psychology. A student not trained in psychologising or in performing introspective experiments, is a superficial amateur.

The method-in-chief which the psychologist has to employ is that of introspection. The student must, therefore, practise that art until he has fairly

* See Volckelt, Psychologische Streitsfragen, 1886, pp. 8 ff. Volkelt's theory is that self-observation consists in involuntarily consulting the memory contents (ibid., p. 12).
mastered it. Ordinary scientific observation is unquestionably beyond the uninitiated.* This is also true of self-observation. At first we must expect the student to be helpless and inefficient. Only with time and practice will his ability to grapple effectually with psychic facts assume any considerable proportions. Like every discipline, psychology has its own difficulties which the student must overcome. Failure at first should no more discourage or dishearten him than the corresponding failure to ride a bicycle easily and gracefully when mounting one for the first time. He must judiciously practise until the so-called impossibilities, of which we heard so much in the last section, become commonplace realities.

"Be normal" in your psychologising, is the supreme rule. To show nervousness, to become excited, to be full of anxiety, to wonder, to doubt, to desire, are states which the student must ignore. Ordinary attention is liable to these very freaks, and passes beyond them by dint of intelligent practice. Average individuals go about their affairs without becoming morbidly self-conscious and agitated. In a similar manner, agitation in self-observation argues the apprentice eye. We must become accustomed to turn inwards with as little ado as when turning outwards.

Let us leave generalities. As I am writing at the present moment I do not, as far as the writing is concerned, in the slightest measure feel excited or confused. If I shut my eyes, as I have done just now, the writing ought to proceed with no more transformation in the process than is implied in the absence of sight. Any excitement or change in the attention is of the evil one, and the most delicate instrument should scarcely record any modification. Mechanically the eyes are closed, and mechanically we proceed. After some practice it should be difficult to tell which out of a number of short lines were written with eyes closed or eyes open.† On the intellectual side, too, the severest scrutiny should reveal only serene peacefulness. [Repeat this experiment a number of times, and record results.] Thus with walking. At any convenient moment I shut my eyes and walk along as if they were open. My thoughts also keep unchanged, and there is altogether no alteration except as regards the absence of sight and its results. Should I be self-conscious and find normal thought difficult, or should I detect that I am different, it would be a proof that my experiment has been a failure and that I require further practice. [Test experimentally.] Thus with eyes open, after due exercise, I attend to what can be observed as regards the process of walking: how I lift the legs, how I put them down, and the sensations connected with these acts. If I am thoroughly trained, there will be no normal feelings suppressed or added to, and there will only be noted what a perfect memory of the normal process would redevelop;

* "Observation is not, like perception and sensation, something that comes to man of itself; witness the fact that there are countless numbers who never reach the point of observing the phenomena of outer existence" (Beneke, Die neue Psychologie, 1845, p. 15).

† Bain (Senses and Intellect, 1894, p. 348) incautiously observes: "When we make our signature without seeing it, the execution is very faulty." [Test this.]
All accounts which disagree with this, stamp the narrator as a beginner, for development or suppression of ideas should be entirely in our power. Having several times observed myself walking, and having written down the observations, I repeat the experiments with eyes shut. The closing of the eyelids, experiment has taught us, makes no difference. Walking for about ten yards in a perfectly normal frame of mind, I learn that my walk is not in the straight line which marks open-eyed walking. Attending to the reasons, with eyes closed, I find further confirmation of the same fact. How often, indeed, do we not talk in the most natural manner to our neighbour, while all the time observing the effect of our speech, or attending in addition to something else. Irrelevant modifications condemn the observer at once. [Observe yourself while walking with eyes closed; try and practise varied distances, directions, speeds, obstacles, etc.]

When we first attend to a habitual action there is a tendency to confusion. Thus, when I first attempted to do so deliberately—being under the influence of the current psychology—I felt convinced that it was impossible. Practice, however, soon disclosed the fact that nothing was easier. The student should be able to observe any habit of his to the minutest detail without interfering with the habitual action itself. There must be no change or confusion, and there need not be. With many habits it is easy to test whether attention thereto perceptibly interferes with the process. At all events, while at the beginning there is hopeless chaos, at the end the practised intellect detects no change. Some games illustrate this gemmation of the attention. Try to tap the table with one hand while stroking it continuously with the other, and do the first more quickly than the second. Or perform the experiment so that the chest shall take the place of the table. [Repeat.] In these instances, a little practice will demonstrate that what could at first not be done at all, can be done later on with ease. Redistribution of the attention over a limited area is alone necessary in self-observation, and can become as easy of accomplishment in the one case as in the other.

A favourite illustration of the drawbacks of introspection is that passion excludes calm self-observation; that, in short, you are either in a passion or you observe. Strange to say, the truth seems to lie in the opposite direction, for a passion is never a momentary fact, and is, by its nature, far-reaching in its bodily effects, while it hardly ever has complete hold on us. Since self-observation requires but a modicum of attention, there is nothing easier than to observe oneself when excited. For example, walking through the fields, it happens that I am startled by a covey of partridges suddenly rising within a few feet of me. Forewarned, I instantly pull out my ready note book and write down the various changes I undergo from the very beginning till the subsidence of the upheaval. Since I cannot write as quickly as I observe, it only requires repetitions of the exciting occurrence to embrace its many aspects. Again, for functional or other reasons, I feel in a boorish mood. There is nothing easier in this case than to sit for hours together, if one wished, writing down the exact
condition one is in. Thus, however annoyed I am, I find no difficulty in analysing my condition. Wherever self-observation cannot reach, there analogy at once fills in the outlines. It would be tedious to pile up examples where verification is readily accessible. [Examine, making use of note book, cases of surprise, mood and passion.]

Introspection must never, unless deliberately required, change the relevant state of thought. I say advisedly “relevant”, because some redistribution must take place. Usually it will be sufficient, if our attention, say, to a mood, excludes the ordinary noises we should otherwise be aware of. There is not a spark of truth in the statement that there is such a thing as “a state of mind” in the sense of an indivisible whole which constitutes the field of thought at any one time. We might as legitimately speak of “a state of Europe,” meaning thereby that when some one in London has the toothache, every one everywhere else in Europe is profoundly modified in his whole being. The residual, illicit effects of introspection, as of observation, may be ignored without any danger.

“Be normal,” is one rule of self-inspection; “Be minutely observant,” is another. Not only should we insist that the observer shall not interfere with the observed state; but it is also necessary that he shall train himself to observe the veriest trifles. He should give ever fuller and more accurate accounts of what he sees in thought land.

Much of what is called introspection is merely conscientious observation. It is the power of discovering all sides of a subject and unearthing what is hidden instead of heaping up instances to support stray notions. Not the difficulties of introspection, but the absence of method is responsible for psychological vagaries. Indeed, the distinction between introspection and observation is not scientific, for the worlds of mind and matter are one in the final analysis. (Ch. 8.)

6.--DETAIL AND GENERAL FACT.

In sec. 2 we condemned the free use of hypotheses, and we insisted upon an exhaustive study of detail. Yet a very little reflection shows that details may be collected ad infinitum, without furthering a science. It is easy to imagine a novelist making a life-study of Drink. He draws, perhaps, most harrowing or most amusing pictures. He gives us many situations and scores of divergent characters. One man is made generous by drink; another mean; one persists in giggling, another becomes morose; one is loquacious, another taciturn; one is made sleepy, another lively. The observable situations are also inexhaustible, and innumerable are the note books which one observer could fill with his observations of the concrete consequences of drink. There is, perhaps, no crime under the sun which is not chargeable to it, and hundreds of thousands of homes have been turned by it into so many hells. Yet any conceivable array of books treating of these concrete facts leave the scientific observer as such uninterested. He wants something else.

Particular facts, in science, are a means to an end. It is only because they help us to arrive at general facts, or simplified statements, as
Mach would say; that they are of importance, science being the shorthand of knowledge. Hence a collector is not necessarily a man of science, though a man of science is, of necessity, a collector. A student, then, who is a close observer and nothing else, is not on the scientific plane at all. His observation must serve a purpose. He must collect with some end in view, with the notion of attaining to, or helping others to attain to, general facts—facts which widely hold.

Let us again examine our typical drink problem. I wish to understand the nature of drunkenness. Preliminary to any generalisation whatever, I examine a number of instances, ignoring the general memory contents. In each case I pass by what is plainly incidental, and note what repeats itself under varying circumstances. I want as many facts as possible, so that any generalisations I venture on are quickly verified or checked. I also demand many diverging instances, so as to determine by an after-appel to memory what is special and general. One evening—it is a bank holiday—I am returning to town by rail. [Observe some such case.] Opposite me sits a middle-aged man who is the worse for drink. As he holds the glass, his hand shakes; so his other hand trembles as he pours out some of the liquor. He puts down the glass in a position which allows it to tumble over at the first jolting of the train. He does not recollect where he placed the bottle a minute previous, nor who has the glass.

I am aiming in this typical example at general facts, at general antecedents. So, with exact details to help me, I begin. His hand shakes. Is he unsteady on his legs? Can he carry his body or his head properly? Is there any portion of his body which shows steadiness? I now draw up the tentative statement that every one of his movements lacks firmness. "What is the reason of it?" I ask, looking at the disgraceful scene. I decide that his muscles are affected owing to an abnormal state of the nerves. Reasoning must, in this wise, proceed from step to step in a graduated fashion. Naturally I first referred to the body and its parts. Having settled that portion of the whole man, I make another move. "How does drink affect his reasoning, his judgment, his sensibility, his memory, his powers of attention, his discrimination?" The progress from one part of the body to every one of its parts; from thence to one part of thought and then to every other, should be a fixed custom. At every stage the student should have general rules for his guidance. What, then, is the conclusion as to his brain? It is that, as in the body, there is a tendency towards fitfulness and prostration. And have the peculiar physical and neural states a common factor which shall account for their likeness? Most probably; the nervous system being generally affected, brain as well as body suffer throughout and equally so. The last query should, of course, also rise mechanically on all appropriate occasions. But what seems true of this person now, would it be generally true of him? And what holds good of him, does not hold good of every one without exception? And do only drunkards behave like that? And what is the precise point of drunkenness where such behaviour ensues? And what
is the particular process by which intoxicants produce the result? And what practical conclusions can we draw? Every one of these queries should arise unerringly, and does so arise with the trained thinker. The method of eliciting general facts must not be left to chance suggestions.

We see now that while duly appreciating an intimate acquaintance with details, our aim nevertheless lies beyond them. Without abundant details we should blunder repeatedly. Suppose I know a drunken person. Every few steps he takes he makes a little jump; aware of his condition he wisely refrains from speaking; and occasionally he bursts into a towering passion. The little jumps, the taciturnity and the wildness, would never suggest that the whole of the nervous system is in a quite abnormal state. The longer I study such an instance by itself, the further I stray from the truth. A large quantity of varied detail is, therefore, an essential as a preliminary to reasonable generalisation. Again, the conclusions we deliberately arrive at are, in their nature, different from those of the market place. As in the first inquiry we had comprehensiveness exclusively in view; so here we aim directly at definite generalisation. Amusement, interest, prejudice, are naturally absent. We do not hazard reckless maybe's, which we do not take the trouble to verify; but we think of what is reasonable, and rigorously verify our conclusions. In this light the work done by the current psychology, still leaving aside the experimental school, resembles to some extent scientific procedure, as the crow with its few peacock feathers attached resembled a peacock. So blinded are we by an old tradition that we do not see the gulf which as yet separates psychology from the established sciences.

The method above described precludes "licentiousness in speculation," though it allows of "a reverent freedom." Such problems as the psychology of Western nations, the psychology of war, or the psychology of a certain man's character whom we for the first time meet, we shall of necessity pass by as being for the moment beyond us. Only that which requires limited attention can repay study. When the majority of elementary notions have been ascertained, we can, with boldness, as in physics, proceed to unravel larger and larger issues. Arrived at the stage when physiological material abounds, we may freely venture to put on the seven-league boots of speculation. At every point then happy guessing will be possible, and verification easy. Until that time sails into sight, we must pay almost superstitious homage to minute details and cautious generalisation.

7.—Systematic Observation.

We have seen that hypotheses or general speculation are of little use in a new science. We have learnt that introspection, normal and minutely observant, is the avenue by which to approach our subject. Lastly, we have tried to weigh the value of details. We shall now urge that useful observation must be systematic, or pursued according to a defined plan.

Sir Michael Foster's Physiology is a splendid illustration of the rigorous application of deliberate method. Take, for instance, the question of
cutaneous sensations or feelings. An average individual, and also an
average psychologist, accepts the popular conclusions as to touch, pain,
and temperature feelings. He then aimlessly speculates concerning them.
If he, perchance, as an act of supererogation, makes half-a-dozen special
observations, and performs an experiment or two, he thinks he is worthy
of admiration. Let us compare such a method with that referred to in
Foster. By systematic observation every part of the bodily surface is
explored, and that most thoroughly. In this fashion differences of
sensibility of various parts to touch, say, are determined. The observa-
tions are repeated on the same individual and on others, and nothing is
set down as generally true to which there attaches the faintest suspicion.
Anxiously the slightest hint is watched which shall throw more light or
which shall qualify the observations made. The examination is then con-
ducted under new circumstances. Perhaps a part is diseased or insensible,
and we note the differences. We also systematically increase or decrease
the pressure. Then we experiment with a view to seeing whether
direct contact with the nerves which convey skin or cutaneous feelings to
the brain yields the same results. As such contact gives rise to pain, and
not to feelings of touch, we ask to what factors the different effects are
due. Then we separate feelings of touch from feelings of pressure,
temperature and pain. We, therefore, start a fresh series of systematic
observations, more prolonged than the first series. In each instance every
point of the skin is carefully and repeatedly tested, and the results are
again put to the test as in the case of touch. We thus determine variations
of different classes of sensibility, in the same individual under different
circumstances, and in different individuals. We find that different parts of
the body are much less affected than others by irritation; that touch alone
is felt in some parts; that some "spots" are sensitive to cold alone,
and others only to heat; that under certain conditions we may be sensitive
to cold and not to heat, and vice versa; that, in short, feelings of touch,
pressure, heat and cold, and even sub-classes of these, may be observed
independently. (Foster, Physiology, part 3, 1897, ch. 2, sec. 9.)

In the above we have judgment entered in our favour, and directed
against current psychological methods. Here is systematic and close
observation, and cautious progress towards large generalisations. No
proud hypothesis is postulated; but the careful investigation proceeds
from step to step. Most of the results thus achieved, revealing as they do
what is inaccessible to passing observation, are beyond the power of the
keenest speculative intellect.

The student must particularly note the method, and apply it himself
always, without exception. First, one portion of a problem should be
studied thoroughly. Then, we must remember that since what is true of one
thing is not necessarily true of any other, observation must be extensive;
that the very opposite may be true; that the same thing may be true of
disparately unrelated things; that the same thing may not always be true to
the same degree; that change of circumstances may make a crucial difference;
and, with these facts in view, we must find out in what direction any discoverable differences may lie. Our scientific mood must be so familiar with these cross-questions, that no problem for solution should ever suggest itself without our being prepared and able to apply the most rigorous standards. One psychological issue after another should be attacked in this manner. Mere reflection or recollection, unconnected with detailed and systematic analysis, should not even be thought of. We must examine the thing itself to the minutest fraction, repeatedly, and under every variety of circumstance. We must provisionally hold that the suggested solution is wholly or partly incorrect, that its opposite is true, and that it is true of other things also. Only by strenuously applying such canons as tests and precautions, shall we be worthy servants of science.

I reproduce from Foster the interesting example of the nature of skin or cutaneous feelings. His Physiology is full of records of examinations as beautiful as that. It was thought at one time that the cortex, or rind, or surface of the brain was the seat of intelligence and volition. Observers had discovered, for instance, that on application of an electrode to a certain portion on the surface, a particular movement was initiated. The theory seemed proved until some one cut away the brain in slices, and found that the surface could be removed without doing away with movements.* Touching in an appropriate manner a certain area of the brain of the frog, an area very ill-defined, we learn that motion of the shoulder results. Touching other similarly ill-defined areas, we discover that every class of motion can be produced automatically. Having abundantly verified this fact in frogs under various circumstances, we proceed to examine pigeons, then rabbits, then dogs, then monkeys, and then men. In systematic order we examine animal life, and find that the irritation areas become more defined as we rise higher in the scale, being most defined in man. (Ibid, ch. 2, sec. 7.) It must be noted that the examination does not proceed at haphazard, and that often the placing of undoubted facts in a certain order tends of itself to elicit general truths.

Or consider again the example of brainless animals. A frog deprived of its cerebrum will only move when it is stimulated from without. Under such stimulation, if continuous, it will behave almost as intelligently as an uninjured frog. So profound is its want of initiative, however, that, if close round its body a circle be drawn with chalk, it will, we are assured, die on that spot without any attempt at movement unless stimulated. This statement must nevertheless be qualified, for the operation involved injury besides that intended. Immediate results, Foster ever and ever again urges, must be discounted. The "deficiency" phenomena are not likely to be pure at first, and the longer the animal lives after the operation the more important, therefore, become the observations. Hence we find that initiative, in the absence of any growth of the cerebrum, is, to some

* This experiment should certainly have been made at the first, if such experiments be allowable at all.
extent, restored when the animal has, after some months, completely
recovered from "shock" and other irrelevant injuries. It is, therefore,
proved that initiative is not solely dependent on the cerebrum, and that
motion is also not exclusively connected with the cortex. Yet once more
we cautiously climb up the animal ladder, and find that while injuries, as
we ascend in the scale, become more and more immediately fatal, initiative
grows more independent of the cerebrum. (Ibid, ch. 2, sec. 4.)

Take again the case of ingoing and outgoing, or afferent and efferent,
nerves. Cutting through a bundle of spinal nerves, as we might cut through
a rope, we notice that some degenerate and others do not. Those which
degenerate downward, we are tempted to consider as fed from above, and,
therefore, carrying messages upward; and those which degenerate upward,
we consider as giving rise to movements and other changes in lower centres.
However, if we carefully examine the matter by the methods which we have
insisted upon, we find that the problem is by no means so simple. It
seems as if there were relays of nerve fibres; then fibres—internuclear or
commissary—which connect the various parts of the spinal cord horizon-
tally and longitudinally; and, lastly, stimulation may proceed physiologically
along the grey matter. Instead of single nerve wires connecting skin with
cortex and cortex with skin, Foster continually insists that the true process
which ends in motion and sensation, is one of complex elaboration, utterly
unlike the single wire system which we have assumed. (Ibid, part 3, passim.)
Here, again, we learn the folly of paying heed to large hypotheses, and the
absolute necessity of proceeding by systematic examination. A child
could make as a good a guess at the nature of the Himalayan flora as we at
the nature of cerebration or thought.

The preceding references to Foster's Physiology—which must also serve
as a very brief account of the more important facts of brain physiology—have
made clear what we mean by systematic observation. However, the student
of psychology must not only know, but be skilled. He must methodically
learn to apply what he knows. It would be well for him to practise on a
large scale, unless he has done so already. For instance, to start with extra-
psychological examples, he might, from minute observation, give the com-
pletest general account of which he is capable of the history of a dandelion
or any other flower he chooses, from the moment it is discovered above
ground to the time when the wind has scattered the seeds. He should
examine samples in all sorts of places, high and low, shady and sunny, cold
and warm, wet and dry, windy or sheltered, at different times of the day or
the season, and in various soils. Every kind of variation should be noted;
and, if time permits, comparisons might be made with like and unlike
species. [Its large size and its interesting transformations make the dandelion
peculiarly suited for study.] Or the student might be inclined to write
an essay on stamens and pistils, on sepals, on corollae, on leaves, and the
like. A limited task, efficiently carried out, is the ideal for practice in
general skill; an intricate problem is beyond the beginner. Or the student
may watch a thermometer, a barometer, or the movements of the sun,
the moon, the wind, or the clouds. Or he may take account of the transformations which one particular bush undergoes in a twelvemonth, and possibly generalise tentatively at the same time as to our common flora. As explained more especially in sec. 136, he will examine concrete facts; he will choose a simple problem and give his whole attention to it; he will proceed methodically, and reason as boldly and as systematically as his facts allow.

I have referred to brain physiology, botany and some facts in physics. The application to psychology is no less in place. The study of a science whose facts we merely store as we usually do the data of geography, does not cultivate the judgment: physical science thus taught is not to be compared in its effect with the benefit derived from a classical education where skill is constantly required in the interpretation of an author. In psychology, too, the appeal must be to the student’s judgment rather than to his memory.

Let us examine some psychological problems in the light of the principles I have endeavoured to expound in this section. Everybody knows, we are told, what is pleasure and what is pain: they are elemental facts which admit of no explanation. Apply now the rules of investigation which have been referred to. First, we go to the facts. We do not recall illustrations, which might or might not be the result of bias; we observe at first hand. Observation, again, must not be without guidance. We observe pain after pain in mechanical succession; every pain as it occurs; and not only one here and there, where subjective selection may play some part. We observe for as long periods together as possible. Our experiments are as minute and as guarded as were the experiments about skin sensations already dealt with. We notice kinds and degrees of pain, and try to define them accurately. We learn what are the most regular accompaniments or signs of pain. We compare our painful states with other states where pain appears to be absent or pleasure present. We are on the lookout to see what pain has in common with other states, such as sensations, emotions, feelings of doubt or touch, inclinations or disinclinations, habits, and effort. Of all our observations we keep full notes. As facts repeat themselves, so we tentatively, but none the less boldly, suppose them to be general facts.

At last no new classes of facts seem to come forward, and to go on, is to be thrashing chaff. We proceed now to set down those features which were repeated oftenest, and we arrange our material, with our tentative minor conclusions, systematically. Is it then true, we ask ourselves, that in pain we always tend to turn away from the object which causes it, and that in accordance with the degree of pain? And is it correct that in nothing else do we tend to turn away from an object? Is pain an elementary fact, not to be defined, always recognised with certainty as soon as met with, and never confounded with anything else? The answers to these questions the student will find in ch. 6. Here I insist that even a tentatively correct reply must be based on an examination such as I have fore-
shadowed. Observation must be systematic and exhaustive if we are to offer any solution at all. No new problem is so simple that method can be dispensed with.

Let us take another case. James tells us that in certain critical instances our conscious wills can, by means of a special effort, add to the sum of physical energy existing. Just as a flash of lightning comes from cloud-land, so occasionally a bottle of extra energy is poured into the stagnant world pool.* What is effort? we ask ourselves, and proceed to observe systematically every kind of effort. We examine methodically phases from the clearest to the vaguest, from the most obvious to the least obvious. On every possible occasion, and under every conceivable circumstance, we attempt to trace its origin, its nature and its effect. We note how far its own states resemble one another, and how far these resemble other states which are not usually classed as efforts. We observe how far they universally prevail, and how far there are other forces. Having conscientiously examined the material, we pronounce judgment either for or against James. At the same time we have probably been able to find a more or less permanent basis for that phase of thought which we call effort. Examination of facts is absolutely essential: the cleverest reasoning in its absence is only a jugglery with words; the most neatly written essay, with its firstly to Xthly, is as unsatisfactory as an imaginary fortune. All the quoted arguments against introspection have no more solidity than the reflection of a fortress in a looking glass.

Every one of the questions treated of in this volume should be examined systematically. "I can, therefore I must," is even more imperative in scientific inquiry than in moral conduct. When it is asserted that attention is a casual process (ch. 2), or that action may be divided into habitual and non habitual (ch. 3), or that ideas are of such and such a nature (ch. 4), or that conscious process should be divided from not conscious process (ch. 8), we must in each case fall back on our first principles of investigation. When once a number of problems of crucial importance has been disposed of, sufficient facts will have accumulated to allow of a quicker procedure in psychology. Until then, oceans of ink imprinted on balls of paper as big as the sun, will not advance psychology. The scientific method varies in kind from current psychologising, and hence the two are incommensurable.

In connection with every problem or assertion, the student must have recourse to systematic observation. He should put a query against every psychological statement, however emphatically put forward. He must let no supposed truism pass, and no maxim or axiom should be left unchallenged. Fiction has often been so cleverly attached to fiction that the student is likely to be deluded into seeing an imposing edifice where there are only bits of coloured glass. A true generalisation is soon verified.

* So also Ward, Naturalism and Agnosticism, 1899.
INTRODUCTION

8.—Quantitative Psychology.

The senses of sight and sound have long been favourites with physicists. In Germany, Helmholtz, and in England, Tyndall, are best known as having devoted some of their genius to the task of resolving problems of this class. Physiologists, in the same manner, have not been remiss in dealing with the instruments of sensations. I have already referred (sec. 1) to the speculative attempts of Maudsley and Lewes, among others, to interpret thought in neural or brain terms. Further investigations since their time have been satisfactory, but extremely slow. The perusal of a work such as Foster’s (Physiology, part 3, 1897), or Ferrier’s (The Functions of the Brain, 1885), leaves one with the impression that the possibilities are great, but the results small. The neural facts known, form, as yet, an insufficient basis for any wide conclusions. However, it is in neurology, or brain science, coupled with introspection, that our hope ultimately lies. Psychology, jealously separated from physiology, as we shall abundantly see in the following chapters, cannot supply us with a consistent account of the facts of mind.

There was but one step from probing sense problems to exploring quantitatively some of the simpler sensations and images. Following Fechner, Wundt, about a generation ago, in a large work, Physiologische Psychologie, was the foremost in popularising such inquiries. His laboratory in Leipzig became a centre of interest and the type of many which were to follow, till now both Germany and America boast a number of psychological laboratories. Since Wundt’s time, also, books on the subject have been on the increase, whilst a multitude of articles and essays have seen the light. Such publications as Philosophische Studien, Zeitschrift für Psychologie, The American Journal of Psychology, and The Psychological Review, are largely devoted to the interests of Quantitative Psychology or Psycho-Physics.

The object of the quantitative school is to examine psychological facts experimentally. For this purpose laboratories are fitted up, containing the necessary appliances. All the customary scientific checks are employed. The number of experiments is recorded, as well as the number of persons experimented upon. The time taken by each experiment is determined by electric clocks which generally mark thousandths of a second and which are usually stopped by pressing a button on which the hand already rests. In short, these experiments are distinguished by an ingenuity and a care which is scarcely exceeded in physical inquiries.

We cannot determine the current of thought by withdrawing or adding to it half-pints or spoonfuls, nor can we yet ascertain the length of a string of ideas with a tape measure. Psycho-physicists, therefore, approach their subject tentatively. Here is a simple experiment. It is arranged by a mechanical device that a sound shall be heard or a sight seen by one prepared for the task. The subject of the experiment or, shortly, the “reagent,” as soon as he hears or sees the special signal, stops the electric
current which his fingers control. By repeated experiments with many persons we gain at last the knowledge of what average time elapses between a sound or a sight and its apprehension. Following scientific method, we find accordingly the experiments varied, account being taken of different ages and races, different times of the day, and different seasons. So also the reaction time is determined when the "reagent" is smoking or under the influence of intoxicants, or hashish or morphia. Each of these sets of experiments is carefully recorded, and naturally presents great difficulties. What is true of sight and hearing holds, of course, of the other senses, including pain and the so-called muscular sense.

Mere recognition of what is expected is the simplest form. The sensations are now tested as to their intensity (or obviousness), and especially as to the smallest added degree of an impression which is immediately perceivable; or the effects of fatigue are studied in this connection; or we learn how far a preceding sound affects vision, and vice versa. Assuming that we have exhausted repetition, obviousness, quickness and circumstance, we proceed to more complex experiments. The "reagent" is told that it will be either a colour or a sound. Here the preparedness is divided between two possibilities, and the results are noted. Then the changes are rung on this set of experiments as on the preceding set.

We come next to more extensive discrimination experiments. The "reagent" is informed that he is to tell the capital of any country that may be named. Or he is asked to tell the country when its capital is given. Or he is requested to give the name of a great man, a great poet, a great painter, when he is asked. In each case the "discrimination period," the time it takes to discriminate, is determined. Obviously, experiments in this direction admit of endless complexity, and become progressively more difficult.

Experiments are of various kinds. A number of writers have sought to determine the sleep curve, i.e., how far during the night the depth of sleep varies in an average individual. Ebbinghaus, again, made experiments with nonsense syllables, so as to determine, in its purity, the nature of memory (sec. 135). So Miss Calkins has carefully tabulated the facts of a series of over three hundred dreams (sec. 228), while various experimenters have made a variety of experiments to elucidate the process of attention (ch. 2), the capacities of school children, and racial differences.

I welcome the quantitative method as such. If it can best elucidate the problems of psychology, it must take the first place. One does not know what it may accomplish in the future; but up to the present, after a generation of toil, its many solid achievements have scarcely touched the borders of psychology proper. It has thrown no light whatever on any of our chief problems. It deals with borderland affairs which apparently yield no glimpse of the far interior. The facts of psychology seem so varied that once psycho-physicists forsake the frontiers, endless discussion instead of fruitful research ensues. It is perhaps the absence of continuity in the investigations that is to blame. Experimenters take up subject after subject,
when perhaps some years spent by one person in studying one aspect, such as memory, attention or dreams, might end in valuable contributions. Another fault lies undoubtedly in the too ready acceptance of the chief propositions of reflective psychology. Associationism is tested as if it only required exemplification or correction, and attention theories are unsatisfactorily dealt with in the same manner. The quantitative method itself is overdone. Ebbinghaus' huge inquiry as to memory (sec. 135) settles practically nothing. The simple factors he experiments with—nonsense syllables—no more give a satisfactory solution than an examination of pinches of powdered bones would yield the key with which to unlock the secrets of the central nervous system. In my opinion a broad-based inquiry into what we actually do remember and forget, and the circumstances which favour the one and the other, would have yielded more promising material. So also with the question of dreams. To tabulate dreams according to the hours when they are supposed to have occurred, to divide them into reasonable and unreasonable ones, to tell us what relations and persons were seen, as Miss Calkins does, is to supply us with meaningless figures. Giessler's detailed non-quantitative analysis towers far above such mechanical figure-work. Finally, as to Thorndike's very interesting experiments with animals (sec. 233), we may observe that valuable as are some of the results, suggestive as are others, yet the restricted method employed entailed as many fallacies as a non-quantitative analysis in that direction. Nearly every point is vitiated by the absence of an appropriate background of general fact. Much again rests on indefensible assumptions which the inquiry hides rather than exposes. A few months' careful observation of a healthy ape, or, in default, of an average dog and cat, should have resulted in wider and less disputable conclusions. Lastly, when we come to directly observed trains of thought, the quantitative method sheds no light whatever. A book, such as Scripture's, makes us feel that psycho-physics will not have the last word to say in matters psychological; and Wundt's latest edition of his large Psychology only confirms us in our conclusions.

On examining the psycho-physical literature two failings become specially prominent. There is a superstitious belief in the magic of figures, a belief not to be found in the physical sciences.* To an outsider it seems that judicious observation and chance experiment could settle with comparative ease many of the questions which demand mountainous labour from the figure school. Or we may say that instruments would be better used after other methods have prepared the way. At all events, the second failing illustrates the first. Psycho-physicists are now insisting that figures, uninterpreted by the state of mind of the "reagent," are not to be relied on. We are thus completing a circle. First, men rushed to figures because introspection seemed unreliable; and now self-observation is

*I highly esteem figures which prove something or which can be utilised theoretically; but I am at a loss to understand the special dignity which figures and rows of figures possess in the eyes of some men" (Lipps, Grundzüsse, 1885, p. 421).
demanded to give a meaning to the figures. Systematic introspection must, therefore, ere long, be generally acknowledged as essential to psychologising.

We conclude, then, that there is still room for a method different from that of the speculative or the quantitative school.

After reviewing what is practically the whole field of psycho-physics, my misgivings as to its value have grown stronger than ever. The method seems fundamentally wrong. A problem is posited, reduced to its simplest form, and then rigorously tested. It appears to me that the simplicity is in every instance non-existent. It is a hypothetical simplicity, a simplicity suggested by surface knowledge. The primary facts of a science, I cannot help thinking, must be obtained by tentative and resourceful trials, and are not obtainable by stiff laboratory experiments.

The two chief works of the quantitative school are Wundt’s Grundzüge, 1893, and Munsterberg’s Beiträge, 1889-92. A simple exposition of psycho-physics will be found in Scripture, The New Psychology, 1897. See also Bain, Introduction and Psychophysical Experiment, 1893; Bettmann, Beeinflussung einfacher psychischer Vorgänge, 1895; Binet, La Nature en Psychologie Individuelle, 1898; Cattell, The Psychological Laboratory at Leipzig, 1888; Cattell, Mental Tests and Measurements, 1890; Cattell, Mental Measurement, 1893; Fechner, Elemente der Psychophysik, 1860; Heinrich, Physiologische Psychologie, 1899; Henri, Les Laboratoires de Psychologie Expérimentale en Allemagne, 1893; Scripture, Psychological Measurement, 1893; Titchener, The Leipzig School of Experimental Psychology, 1892; Titchener, A Psychological Laboratory, 1898; Titchener, The Equipment of a Psychological Laboratory, 1900; Titchener, Experimental Psychology, 1901; and Wundt, Über psychologische Methoden, 1881.

9.—Experimental Introspection.

The experimental method is rightly esteemed to surpass the method of simple observation, for there is something exhilarating in grasping the gate of time, and swinging it to and fro at our pleasure. In observation we have to wait months and years to observe a fact, when in experiment we can often produce it as soon as thought of. In observation we are dependents whose desires may never be satisfied; in experiment, we are masters whose word is decisive. Wherever, therefore, experiment can be applied, it will be folly to continue with simple observation, the latter being to the former, as “moonshine unto sunshine, and as water unto wine.” An observational science is, for these reasons, generally viewed with suspicion, while the use of experiment adds dignity and inspires confidence.

What marks an experiment? Professor Titchener says: “An experiment is a trial, test, or observation, carefully made under certain special conditions: the object of the conditions being (1) to render it possible for any one who will to repeat the test, in the exact manner in which it was first performed, and (2) to help the observer to rule out disturbing influences during his observation, and so to get at the desired result in a pure form. . . . Experiment thus secures accuracy of observation, and the connection of every result with its own conditions” (Outline of Psychology, 1896, p. 35). This definition strikes one as unsatisfactory. It savours too much of the psychological laboratory. It resembles dangerously those definitions of religion which, by implication, prove that our pet-
creed has no rival. It would be safer to say, "An experiment is a trial, test, or observation, carefully made... so [as] to get the desired result in a pure form." This, again sounds vague. Stout's definition is no more satisfactory. Experiment, he says, "is only observation under test conditions, deliberately pre-arranged for the purpose of settling a definite question" (Manual, 1898, p. 26). It seems that one may vary conditions at will without having deliberately pre-arranged anything and without one's having in view the settling of a definite question. For instance, in pulling hard at something firmly fixed I notice that I hold back my breath. In accordance with customary method, I try to pull hard while breathing normally. Here, instead of waiting for an opportunity, I create it. In that—in the creation of an opportunity, or in varying and controlling the conditions—lies probably the nature of experiment. Stout's definition applies only to special experiments; since even many of the quantitative attempts start without hypothesis. Most probably, there is no clear line of division between observation and experiment. As the former becomes systematic and varied, so it approaches the nature of the latter.

Experimental introspection is not a subject usually discussed in treatises on psychology. When we are told that self-observation is an absurdity (Comte and others), or that we must become skilled if we are to take a momentary glance at what is happening (Hume and others), it follows that experiment is out of the question. Herbart, indeed, takes high ground. He tells us that "psychology must not experiment with man" (Lehrbuch, 1834, p. 9). Is then experimental introspection impossible or impracticable? We have seen that attention to selected portions of the field of attention is possible to a high degree. Does then the creation of an opportunity, instead of waiting for it, introduce a fatal disturbing factor? Observation replies in the negative. It was as easy to allow breathing to proceed normally in the "pulling" experiment, as to do anything else which one is not accustomed to. Experimental introspection, in short, has certain advantages, but no appreciable drawbacks. As Beneke puts it: "Nothing is falser than the assertion that introspection cannot be assisted by experiment. Not only is such assistance possible, but it offers here perhaps greater scope than in any other department of nature, and that because the necessary control is generally more in our power" (Die neue Psychologie, 1845, p. 21).

Everybody recognises the superiority of experiment over simple observation. The student must, therefore, be prepared to learn that in psychology every inquiry must be experimental. Simple observation is only permissible when, for peculiar reasons, experiment is undesirable or out of the question. The normal procedure, the all but exceptional method, must include experiment. The reader will see that in sec. 7 I applied systematic observation to the elucidation of the problems of pleasure, pain and volition. According to the last ruling, these inquiries should be experimental. Take the
question of pleasure-pain. Instead of waiting, like wall-flowers in a ball-room, we take the initiative. We deliberately heat a glass and gauge our feelings with the changing heat. We deliberately and repeatedly touch the marble or iron on which the sun is striking with its beams. We go to the fire and expose ourselves to its heat, in order to test our powers of resistance, and ascertain the changes in our sensations. We deliberately cool one hand and warm the other to test the relative aggressiveness and diffusion of the feelings of cold and warmth. We pinch our hands till they ache, etc., etc. Thus there is the fullest room for experiment since we need not wait for chance opportunities. We compare experimentally various sensations and feelings with one another, and examine their degrees, their resemblances, their differences and their conditions. We deliberately ignore a so-called feeling of pain which we have deliberately inflicted, or attempt to reduce or increase the pain value by inhibition or by removal of inhibition. In psychological experiment we thus control the conditions, and test our conclusions at our own discretion. Results can in this way be attained which otherwise might scarcely be approachable.

As with pleasure-pain, so with volition experiments. In systematic observation there is only a preparedness to perceive or record facts of a certain order as they arise. In systematic experiment we act on our preparedness: we summon the actors to the footlights.

Instead, therefore, of waiting for volitions to occur, we initiate them, and we also purposely watch whole trains of our actions for any signs of volitions. We deliberately will, and in doing so we make an effort to trace the antecedents of our volitions. If a sensation of sight is perhaps connected with the act, we endeavour to eliminate it, or even instal in its place an irrelevant sight sensation. One after another, we push rudely aside every factor which admits of being so treated. We measure the relation between will, effort and action, finding perhaps in some instances that they have nothing in common. We see whether there are similar states to that of volition, and experimentally test the relationship. In a like manner we examine our volitions during joyous or depressed moods, when we are in robust health, or when we are ill or fatigued, and under other special circumstances. After such an inquiry, and, only after such an inquiry, we may be able to agree with Professor James, or to differ from him. To meet his gratuitous assertion, based on a temperamental view, with one equally gratuitous and temperamental, is to forget that experimental introspection should be the final arbiter. A temperamental psychology is as irrational as a temperamental chemistry.

The rules which we have selected for guidance in systematic observation apply here. An experiment, not protected by method, is a poor instrument of research. Experimentation is an art with canons to be observed, and not the equivalent of the method employed by a careless child. Merely to shift things and to break them, is something different from experimenting. The student must, therefore, see that his experiments are conducted.
on a proper plan, and that they are accompanied by "normal" and "minutely observant" observation (sec. 5).

As I have already indicated, the student should pursue his studies experimentally. To assist him in this, I have inserted in the body of the book italicised remarks in brackets. Every statement of any importance should be at once challenged and tested, or if testing be sometimes inconvenient, the statement should be marked as doubtful. There must be no acquiescence. When experimental introspection has been pursued by many, then, and not until then, can statements of any kind pass unchallenged [Note that all complicated and violent experiments defeat themselves]

I suggest the following experiments or observations, and must remark that repetition under varying circumstances is necessary, and that notes should be taken at the time, of what is observed. Write mentally in characters of various sizes; so also employ mentally printed and sounded characters. Use lips as in vigorous speech, without making any sound, and also observe the various organs employed in speech. Picture to yourself squares, triangles, etc., of various sizes. Observe eye movements in seeing, also movements in walking, running, working, etc. Examine mentally form, detail, many colours as possible; shades of colour, relief, scenes, motion of eyes in watching moving objects. Hold steadily pencil, pencils, etc., in hand, behind the ear, etc., and note result. Recall various smells of things just smelt, lately smelt, and smelt long ago. Describe bodily feelings in sitting (in various positions), standing, walking, etc., and describe what you feel, passively and actively, of feet, legs, back, arm, head, teeth, tongue, separate fingers, etc. Recall in succession relatives, intimate friends, acquaintances, celebrities, movements, houses, cities, events, villages, flowing rivers, mountains. Recall young people, old people, poor people, rich people, and other classes of persons. Write letters, etc., in the imagination, and watch for eye movements; connect thus sounds with the ear, smells with the nose, tastes with the mouth, movement with the muscles, etc. Write, speak, move, distinctly in the imagination, and note rate of progress. Think of two or more colours at once. Think of coloured things, of sounds, tastes, smells, touches, puns. Mentally see things moving; see two things moving in different directions. Think of yourself moving along a room, passage, stairs, hall, street, etc.; also measure distance and time, by feet and seconds. Observe mentally and in motion, train, cab, cart, bicycle. In mentally writing, do you see arms, hands, fingers, pen, paper, characters; do you feel pressure on pen, and do you hear the scratching of the pen? Imagine man, cow, horse, etc., as blue, green, violet, pink, scarlet, etc. Look at some pebbles, etc.; then see whether you can count them mentally. Hear with one ear, both ears, far and near, much and little, different kinds of sounds. Examine degrees of cold, warmth, touch, soft, hard, rough, smooth, pushing, pulling, effort.

Each of the experiments suggested above should be made in the light of the rules laid down in this Introduction. Barely to observe this or that is of no use at all.

10.—Definition.

Volkmann (Lehrbuch, 1894, i, p. 1) says that the success of every scientific enterprise is essentially conditioned by the accurate definition of its aim. On this hypothesis, which is in accord with the current respect for hypotheses, the writer of this work should have had his definition cut in adamant before starting his inquiry; but, as it happens, research and
not hypothesis, determines its contents. Only subsequent to the arrangement of the data is there a possibility of venturing on a definition. The latter embraces the most general facts, and only after we know these is a summing up possible. It is, therefore, not surprising that the definition here put forward, began only to be framed after the ten chapters which follow were completed.

My definition, tentative in its way, is as follows: Psychology treats of the nature and the satisfaction of those distinctive needs which are connected with the central nervous system, and this it treats of in systematic conjunction with the systems of sights, sounds, smells, etc., which are developing concurrently, i.e., psychology treats of the needs which arise out of the relations of the various systems in the organism, and out of the relations of that organism to its environment (sec. 156). The book itself is the most concise explanation of the definition which can be offered. The student will find therein why needs play the part we have allotted to them, why the central nervous system is here coupled with sights, sounds, etc., and why the nature of needs and the method of their satisfaction is the be-all and end-all, the base and the summit, of psychological inquiry.

II.—Literature of the Subject.

The text of the ten chapters which follow, leaving out of account most of what appears in small type, was written away from, and, humanly speaking, independent of, books. It was only afterwards that the usefulness of literary references occurred to me. Accordingly, I determined to make a survey of the whole field of psychological literature. Let me begin by stating what I shall not treat of in this volume: (1) Everything pertaining to, or bordering on, philosophy, including free-will controversies; (2) most of what is related to physiology and the study of the special senses; (3) the mass of that which refers to experiments in reaction time, to the psycho-physical law, and the theory of innervation; (4) nearly everything specialised, such as the psychology of music, or painters, or races; and (5) abnormal psychology, such as is implied in studying insane and hypnotic subjects and cases of aphasia. Those are the studied omissions.

On the other hand, I have attempted to include (1) all the principal psychologists; (2) all the writers of special treatises, such as on memory, attention, etc., which I could obtain; and (3) all the articles in reviews which had a bearing on the subjects dealt with in the body of the book.

The reviews consulted, from the first issue to the end of 1900, or to the last issue, are as follows:—American: American Journal of Psychology; The Psychological Review; The Philosophical Review; and The Journal of Speculative Philosophy. English: Brain, and Mind. French: La Revue Philosophique, and La Revue Scientifique. And German: Philosophische Studien; Zeitschrift für Psychologie und Physiologie der Sinnesorgane; Psychologische Arbeiten; Pfüger's Archiv für die gesammte Physiologie der Menschen und Thiere; and Vierteljahrsschrift für die wissenschaft-
liche Philosophie. In this manner, students will find almost a complete bibliography of such subjects as Attention, Habit, Association, Memory, Feelings, Dreams and Genius.

12. — Psychological Terminology.

In this Introduction I have employed the current psychological terminology. In the sequel, however, it suggested itself as best to introduce, without labouring the matter, a terminology which shall be simple, free from misleading implications, consistent, impersonal, and which shall reflect the systematic conclusions we reach. At the same time I have, by re-defined, been enabled to retain many of the well-known psychological expressions.

The terms chosen indicate degrees of systematic complexity, and nothing besides. They are, therefore, descriptive of the facts, without carrying with them extra psychological implications. Furthermore, the words are well known, easily adapted to psychology, readily admit of prefixes and as readily form adjectives and verbs. The precise meaning given to the terms requires nevertheless some explanation. (1) The word System indicates that psychology, like physiology, deals with determinate complexes, and only with such. (2) The word Integral expresses the lowest form dealt with by psychology; it indicates the simplest whole, a sensation or an image. Integrals are for us the elementary units or integers of thought and action. (3) The word Compound leads us a step further. We have here two or more integrals in intimate union, as in perception. (4) The word Complication marks another form of systems. Here two otherwise unconnected primary or (and) secondary systems appear together, as when the sight of a fire engine is always or generally accompanied by the re-collection of a certain conflagration. (5) The word Connection suggests that with any given system is connected some other system, connection being another general word for consciousness, awareness, knowledge, ignorance, belief, doubt, certainty. (6) Lastly, the word Combination hints at a combination of systems as in a train of thought or action, and it may be divided into sub combination and super-combination, the former referring to partial needs, and the latter to the principal need at any time.

Useful as the above terms may be, they are yet too vague from a scientific point of view; for the meaning underlying any one of the terms underlies all others to some degree. Hence we may, as in sec. 10, describe the rising complexity spoken of in terms of units of various degrees. However, there can be no disputing that the words do reflect increasing complexity.

The prefixes employed are of some importance in showing diversity within unity, and also in preventing too bulky a vocabulary. As to their appropriateness, much may be said for and against. It is assuredly useful that thought and action, memory and activity, should be readily distinguished or combined, and this the prefixes do. Since in thought there
is no new material, we are justified in systematically applying the prefix re, indicating recurrence, to all its phases. And, by contrast, it is not a far stretch to employ the prefix pre to point to those systems which are new, e.g., as we say, arrange, pre-arrange, re-arrange, so we say, combine, pre-combine, re-combine. To indicate imagination and work, we may, in addition, employ the prefix trans, thus trans-combine (primary or secondary action), trans-pre-combine (work), trans-re-combine (imagine).

The general problem of a psychological terminology is far from simple. On reflection one finds that our whole vocabulary reflects the psychology of the past, and that, therefore, nothing less than a total reconstruction of human language can satisfy rational demands. Such a revolution, not an unlikely one, must be, however, the work of centuries and not that of a solitary individual. Suffice it, therefore, that an attempt has been made at linguistic reform and interpretation.

Descartes is responsible for the notion that we must see that our ideas are clear and distinct. Perhaps on account of the turbidity and indistinctness implied in that rule, the cry was re-echoed everywhere, Locke being the principal sinner in England. The question of terminology is fully discussed by Tönnies, Philosophical Terminology, 1899. My own opinion is that a close study of a subject, together with a desire to a mutual understanding among experts, are the chief pillars of a solid terminology. When all is confusion, as in present-day psychology, one smiles as one hears complaints against the varied ways in which words are used. Here are Descartes' four rules, which will repay careful study: "The first rule was never to receive as true anything which I did not demonstrably recognise as such; that is to say, to carefully avoid precipitation and prejudice; and to include in my judgments nothing which did not present itself to my mind so clearly and distinctly that I had no occasion to doubt it. The second rule was to divide every difficulty under examination into as many parts as possible, or into as many as might be necessary for its solution. The third was to conduct my thoughts in an orderly manner, by beginning with the simplest objects and those easiest known, slowly rising by degrees to what is most complex, and postulating an order even among those objects which do not at all naturally follow one another. And the last rule, to make such a complete induction, and to take such a comprehensive view, that I might be sure of having omitted nought" (Discours de la Méthode, 1637, second part). See Gibson, Regulae of Descartes, 1898.

**System.**—Anything given whatever. [To develop].

**A. Primary system.**—Any system referable to the Present. [To pre-develop.]

**B. Secondary system.**—Any system referable to the Past. [To re-develop.]

[In the place of pre and re, primary and secondary can be used.]

**A and B** are again divided into—

**C. Integral** (system), where a system is considered apart from any interpretation placed upon it, as when a coloured surface is seen, without being connected with (say) the name of an orange or a lamp. [An integral, or a sensation or image; a pre-integral, or a sensation; a re-integral, or an image. To integrate, or to sense or image; to pre-integrate, or to sense; to re-integrate, or to image. Integrate also equals to member, to collect.] [Thus also de-develop, dis-integrate, etc., equal to forget.]
D. **Compound** [system], where the opposite to C takes place, as where a certain coloured surface has attached to it the name of a hat or a desk. [A compound, or a percept or idea; a pre-compound, or a percept; a re-compound, or an idea. To compound, or to perceive or ideate; to pre-compound, or perceive; to re-compound, or ideate.] [Combination unit or unit = the unit in a train of thought or action.]

C and D are again divided into—

E. **Elementary** [integral or compound] system, wherein is included every system, except visual, auditory, olfactory, and gustatory systems. [A feeling, elementary sensation, elementary image, etc. To feel; to develop an elementary sensation, etc.] [Summary feeling = the impression an event makes on us. Combination feelings = feelings, such as are implied in doubt, belief, etc.]

F. **Semi-advanced** [integral or compound] systems, wherein smell and taste alone are included. [Semi-advanced sensation; image, etc. To develop a semi-advanced sensation, image, etc.]

G. **Advanced** [integral or compound] system, wherein only sights and sounds are included. [An advanced sensation, image, etc. To develop an advanced sensation, image, etc.]

A to G have as a sub-form—

H. **Transformed** systems, embracing illusion, delusion, imagination and all productive activity. [Illusion; delusion; imagination; production; etc. To be illuded, to be deluded, to imagine, to produce, etc.; or to trans-integrate, to trans-compound, etc.]

I. **Complication** [of systems], as where two sensations or two images, or a sensation and an image, or any two or more systems, are compounded together to form one whole, e.g., when treating of a conflagration I am always reminded of a particular conflagration which I had been a witness of. [A complication; a pre-complication, a re-complication. To complicate; to pre-complicate, to re-complicate.]

J. **Connection** [of systems], as in consciousness, awareness, knowledge, belief, doubt and certainty, where something is connected or linked with something else. [A connection, linking, or chaining. To connect, to link, to chain.] See sec. 99b.

L. **Combination** [of systems], i.e., a train of thought or action. [A combination, or thought or action; a pre-combination, or action; a re-combination, or thought. To combine; to pre-combine, or to act; to re-combine, or to think.] [Combination unit or unit = the unit in a train of thought or action.]

M. **Exhausted** system, i.e., an object or a notion as interpreted in the light of fullest knowledge, e.g., a seen clock and all that that implies, a felt love and all that that implies.

N. **Unexhausted** system, i.e., an object or a notion as immediately given, and apart from all inference, e.g., a clock as just seen, a love as just felt.
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O. Need = absence of functional stability. Satisfaction of a Need = the process of regaining functional stability.

P. Change in systems as need-determined. [Need-determined change, or functional adjustment. Thinking; acting.]

Q. " " self-determined. [Self-determined changes. Aestheticising, imagining, playing.]

R. " " distributed. [Attention; pre-attention; re-attention. Attend; pre-attend; re-attend.]

S. " " disturbed. [Disturbance, or pleasure-pain; opposed disturbance, or pain; semi opposed disturbance, or pleasure; pre-disturbance, or immediate pain, etc. To disturb; to pre-disturb; to re-disturb.]

T. " " organised. [Habit, organised reaction, trend, economisation. Organisation; pre-organisation, re-organisation. To organise; to pre-organise; to re-organise.]

U. " " excited. Acting under excitement. [Excitement; pre-excitement; re-excitement. To excite; to pre-excite; to re-excite.]

V. " " impelled. Acting under momentum. Mood. [Impulsion; pre-impulsion; re-impulsion. To impel; to pre-impel; to re-impel.]

W. " " inclined. Acting under inclination. [Inclination; pre-inclination; re-inclination. To incline; to pre-incline; to re-incline.]

See the Index for the words Explanation, Why, Cause, Effect, Power, Energy, Faculty, Motion, Change, Space, Time, etc.

13.—A BIRD'S EYE VIEW.

We passed in review the various psychological schools: the reflective, the physiological and the quantitative. In each instance it was found that the positive acquirements were scanty in an undue degree, and this was attributed to the fact that psychologists had not put themselves in close touch with the scientific method. Accordingly, it was laid down that in psychology, as in physics, there must be systematic observation, generalisation and experiment, while hypotheses that trespass beyond the fringe which borders our knowledge must be condemned as unscientific. For the guidance of the student the author also added and illustrated what he considers are the methods to be applied in psychological inquiries.