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COMMON PRINCIPLES
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by

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To

W. S. & D. T. S.

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PREFACE

If we are to judge by the frequency with which the word is used, psychology is to-day one of the most popular of the sciences. A psychological novel, a psychological "fact" or a psychological experiment excites interest and respect. It might therefore be thought that psychology was a science deriving its authority from a theory, or group of theories, so widely accepted as to be virtually acknowledged as laws, that is as generalizations about mental phenomena carrying a high degree of predictability as to future phenomena or providing satisfactory explanations for a large number of otherwise uncorrelated mental happenings. A glance at psychological literature, however, would seem to suggest rather an opposite conclusion, namely that the popularity of psychology represented an acknowledgement of the importance of the study and a wish that it might have such a well-founded discipline.

In general biology we find the theory of evolution, variously interpreted perhaps, but nevertheless universally accepted. If we ask psychologists for their analogous, basic, psychological theory, however, we are offered (with equal confidence!) conditioned reflexes, *Gestalten*, unconscious sexuality, "purpose", glands of internal secretion, or "faculties". Each school has its facts, many of them unequivocal ones, and strongly in favour of the theory proposed: each, then, is probably expressing truth up to a certain point and not error. On the other hand, when general application is given to these theories, that is, when attempts are made to comprise all mental life in the formulae, irreconcilable differences of opinion arise. Two statements cannot both be true and yet conflict one with another. We must, therefore, assume either that the general explanations of only one of these schools are true or else that they are all inaccurate. In the former case, the true generalization which excludes the others must also eliminate the explanations for narrow groups of phenomena from which the rival generalizations were derived and which had seemed valid before being

generalized. If this logical outcome were to be faced, we should have to deny, perhaps, that the behaviour of many animals at the breeding period was influenced by an endocrine cycle or, on the other hand, that children shewed "conditioned reflex" responses. The view that all the generalizations as yet achieved are inaccurate would seem to lead to less unhappiness and we are probably safe in adopting it.

If a basic psychological theory has yet to be framed, there may be two reasons for this. Either the phenomena considered by psychologists belong to categories that really are unrelated, or else they are related but the vocabulary useful in one category is useless in another. The latter seems the more promising view and the primary object of this book is a suggested vocabulary, a vocabulary in which principles common to reflex action, to instincts and habits, to conscious and unconscious mental activities may be expressed.

In any discussion about nomenclature there is always some peacemaker who remarks with sententious finality that it does not matter what labels we use, what we want is facts! There is probably no more pernicious enemy of science than this man. It is undoubtedly true that a rose by any other name would smell as sweet (except to a suggestible subject!), but if the name is to have any classificatory or theoretic implication it is practically of as much importance for the development of science as is the phenomenon it labels. If we called a rose "beetroot", there would be an end of botany. This pacifist would probably have affirmed that it did not make any difference whether a chemist said that in combustion phlogiston was given off or oxygen added: combustion was combustion no matter in which terms it was described. Yet the simple translation of "addition or loss of phlogiston" into "loss or addition of oxygen" in the description of chemical actions paved the way for the development of modern chemistry. For most of the phenomena observed at the phlogistic period one was as good a formulation as the other; but the implications of the two vocabularies were momentous and opposed.

So long as scientific terms are words and not mere letters or numbers they are bound to carry some implication. In my present venture there is one implication that runs counter to current prejudice. In spite of the criticisms of the more philosophic biologists and physicists, the creed of the modern scientist is materialistic either in its formulations or tendency. That is, he

looks avowedly for an explanation of all problems in terms of physico-chemical laws or else is trained to think only in such language and is thus unable to domesticate an alternative view. Yet, unfortunately, I am bound to propose a heresy. The principles which I detect, and hope to demonstrate, as common to mental behaviour of various types and to the physiology of the nervous system are immaterial agencies. If these were put into terms of physics and chemistry, the correlation, essential for any broad biological theory, would disappear. So I propose to assume that immaterial agencies, which I call patterns, do guide, and in that sense control, the physico-chemical processes involved in all living. Since the book is about patterns it would be foolish to explain them here but I may remark that their operation is analogous to the activity of the navigator who brings a vessel from port to port. Each single movement of the ship is adequately expressed and accounted for in mechanical terms but the voyage as a whole is the work of the navigator. In other words, the pattern theory is not a spiritistic one that assumes an appearance of energy from an extra-material sphere, but merely claims that exhibitions of energy in the biological field may be given their direction by agencies not subject to physico-chemical laws.

A preface is not the place for the exposition of the argument, but, rather, gives the author an opportunity of preparing the reader for what follows. Patterns represent properties and relations—pure abstractions—and yet I am gravely proposing to treat these abstractions as if they had some kind of existence apart from the material phenomena in which they are detected and as if they determined in some way the nature of these phenomena. This is a doctrine likely to meet with prejudice and is, at best, difficult to understand. It seems to fly in the face of common sense. Lest the reader should therefore discard it out of hand, it may be well to point out here that this is precisely the method and theory of mathematics, the utility of which cannot be gainsaid no matter what may be thought of its sanity.

In mathematics properties of number and relationship in time and space have been *abstracted* from the material phenomena in which they are displayed; these abstractions have then been dealt with as if they had an existence separable from their material representations. The combinations and interrelationships of number and dimension, possible and impossible, have been studied

from the standpoint of logic (not as derived from experience) and in the end a system of laws, ever growing, has been erected that claims to have universal reference. The practical results of this method are better recognized popularly than is its "irrationality". In fact there lies in this discrepancy what is perhaps the most monstrous paradox of modern times. The contemporary citizen accepts as having material reality what can be shewn to follow mathematical laws, laws referring to relationships so abstract that they could exist if our material universe disappeared!

To the ordinary man the abstraction involved in using the formula "four times two is eight" is so habitual that he does not realize that he is invoking agencies that, although they are immaterial, regulate some of the properties of material objects. When, having counted the total number of cows in two lots of four each and found them to be eight, he predicts that two tricks of four cards each will contain eight cards altogether, he is unaware that what he has done is to abstract a property of a group of cows and apply it to a group of cards. To a hypothetical savage who cannot count this would appear as highly potent magic. The formula $4 \times 2 = 8$ seems rational to any of us, but when its method is put into non-numerical terms, it looks like madness—for what have cows to do with cards? Yet these terms do not misrepresent the method as the following example will shew, an example for which I am indebted to Professor G. I. Taylor.

He was anxious to learn about the distribution of stresses within a steel bar when it is twisted. For this he had an equation, but it was impossible to solve it using only mathematical symbols: some actual measurements were required. Obviously he could not measure anything within the steel bar without disturbing its structure. It happens, however, that the distortion under air pressure of a soap film stretched over a hole whose contour is the same as that of a cross-section of the bar could be expressed in the same equation as that for the distribution of strains within the bar. Now this soap bubble is available for measurement. So he measured the relative heights of such a bubble at various points and from this was able to tell what the strain would be at any point within the steel bar. Such a procedure flies right in the face of common sense. If proposed to a layman by one who was not a mathematician, it would be regarded as madness, for there is no similarity whatever between the physical properties of soap films

and steel. To the non-mathematical it is as reasonable as weighing the ink used in drawing a picture of the bar in order to solve the above problem. To the mathematician, however, the physical properties of substances which give material representation of an equation are quite irrelevant. So long as the properties he is interested in are present, all others can be neglected just as we neglect the colour and habits of cows when counting the number of them in a herd.

In the light of the mathematical analogy, we are in a position to understand why it is expedient to discuss biological patterns in terms of the immaterial. Numbers we call digits because our forefathers counted on their fingers. Our children still begin their primitive arithmetic by counting fingers or other serial objects: this is the easiest way. But how far does it go? We quickly pass to simple generalizations about numbers and dimensions. Supposing we had to cling to their primitive material expressions in thinking about them, how cumbered we should be! If south were the position of the sun at noon, if horizontal meant a relationship to the horizon seen or imagined, if vertical were a position of balance in standing, how laboured our orientations would be! By generalization and abstraction we are emancipated from the material and thus gain control over it. But this control is limited to the range of the properties that are abstracted. Within the limit of finite numbers it is, humanly speaking, absolute. Six eggs added to six eggs will always make a dozen. Enough is known about electricity—another abstraction, by the way—for what used to appear only as a thunderbolt to become a slave of man. Less has been discovered about heredity, yet the breeder can produce some varieties with reasonable certainty. The behaviour of man is studied without reaching relevant abstractions, or, at least, these abstractions are not taken seriously, and so human behaviour is still sufficiently uncontrolled for both soldiers and pacifists to exist.

Mathematical abstractions, which are for the layman mostly mysteries, are accepted as impalpable and dealt with as abstractions. Mathematics have advanced because the laws governing the properties abstracted have been regarded as *sui generis*. A three-cornered sheet of iron has many properties. A geometrician treats it as a surface, then idealizes its shape, and with these two abstractions generalizes about triangles. The theorem that the three angles

of a triangle together make two right angles holds for any flat surface and what the material may be that is co-extensive with the surface is irrelevant. On the other hand the metallurgist is indifferent to the shape of the sheet of iron but is interested in its texture, hardness, etc. He too, however, is able to put many of his abstractions into mathematical terms. But the worker with biological material finds relatively few phenomena that can be translated adequately into such language. In fact the arguments of the vitalist school as to the existence of non-material factors have never been answered except by citation from the materialistic creed. Whenever we meet with a process that is characteristic of living matter, we know it to be such from the very fact that it displays something unpredictable from the laws of Newtonian physics. These vital properties should be abstracted and treated as such. They then become, in my nomenclature, patterns—psychological, physiological, developmental and so on.

It appears to be obvious that, if patterns in the biological world could be formulated and their laws studied as mathematicians have done with those for number and dimension, biology would advance as have the physical sciences. This assumption, however, is probably not justified in the present stage of human mental development. The basic units with which the mathematician works are simple and stable. The biological sciences, on the other hand, are engaged in studying quality rather than quantity, something much more difficult to formulate. Moreover, since the essence of life is adaptation, the reactions of living matter are never stable: patterns are changing, slowly in the sphere of bodily form as exemplified in evolution, and rapidly in the sphere of behaviour. For these two reasons, he who tries to abstract the principles that underly and control living processes arrives at units that are neither simple nor stable: his task is, from the outset, more difficult than that of the mathematician. It is one that calls for a facility in dealing with abstractions such as the mathematician and the philosopher enjoy and, at the same time a familiarity with phenomena unknown to either of these savants.

These accomplishments are rarely combined so I cannot refrain from emphasizing that—apart from the facility or clumsiness of the style—any argument such as I am attempting is going to be followed easily only by those who are both sympathetic with its tendency and familiar with its mode of thought. How readers

may vary in these two respects is shewn in the criticisms of the manuscript I have been privileged to receive from three scientists of note. The first is an unusually competent biologist. He complained that the general treatment was philosophic rather than strictly scientific; that he had difficulty in understanding my general position because he could not conceive of a pattern without a physical basis: "Because certain phenomena are *not yet* adequately explained on a mechanistic basis is no good reason for assuming that they are immaterial". In contrast was the point of view of a man recognized as an authority on the higher functions of the nervous system as revealed in both psychological and physiological investigation. He criticized the anti-mechanistic argument as being over elaborated because "I don't suppose any sane person thinks that 'mechanistic' theories, in our present state of knowledge, explain the phenomena". A middle position was assumed by one whose name is known in every anatomical and physiological laboratory the world around. He expressed himself as out of patience with the current materialistic assumptions because, he believed, they were sterilizing research. Yet he found my manuscript hard reading; this, he politely concluded, was due to his unfamiliarity with any type of thinking that was not materialistic in its subject matter. He wanted to think in terms of abstractions but found it arduous. For whom should I write? If these three learned men are typical, I have small hope of my presentation being wholly acceptable to any one and to many it may be intolerable.

Qui s'excuse s'accuse. I have been trying to explain why the reader is likely to make heavy weather in voyaging through the following pages and have written as if the difficulties lay wholly in the subject matter. This, however, is not the case and no one is more aware of it than myself. I suppose that everyone who speculates at all feels vastly inferior to his ideas. At least I know it to be true of myself. To realize that one has stumbled on formulae which, if applicable in one field, must hold for the whole territory of biology with equal validity (else the basic hypothesis is unsound), makes the discoverer sympathize with a mystic after his experience of universality: it is something he can neither describe nor understand himself. True abstractions in the present stage of human evolution defy clear formulation simply because they are abstract. It is the rarest of mortals who does not intuit, or

"feel", abstractions rather than "know" them. For one who has had no philosophical training to claim that the importance of his theory is equalled by the lucidity of his style would be ὕβρις the enormity of which would shock him more than it could any Philistine critic. For my theory I will not apologize, but I do crave indulgence for the inadequacy of my expression.

If the reader's effort in following the argument be not intolerable, it is because I have been favoured with patient and painstaking criticism. For two successive years these formulations have been tried out in "lectures" to more advanced students of psychology in Cambridge. A lecture often consisted of five minutes of exposition and then fifty-five minutes of criticism and discussion. Under this fire unwarranted assumptions have been blown up, awkward facts reconciled, and hazy statements clarified. Foremost among the critics has been Mr F. C. Bartlett, for whose precision of attack I have as great a fear as I have gratitude for his patience and interest in attending the classes. I also owe much to two undergraduates, Mr Gregory Bateson and Mr R. W. Pickford, particularly the latter.

My debts are not fully acknowledged in recognizing these personal favours. In the present volume I am discussing problems successively in the fields of psychology, neurology and general biology. I cannot claim to be expert in any one of these subjects, for, as a specialist, I am only a psychiatrist. Consequently, my data are largely second-hand. Were it not for several classical works, the labour of gathering material, both relevant and accurate, would have exceeded my energy. Three of these must be mentioned specifically. First and foremost stands Sherrington's *Integrative Action of the Nervous System*, one of the monuments of modern physiology, distinguished alike for its originality, the soundness of its theory and the accuracy and wealth of the data recorded. In the field of animal behaviour I have drawn largely on the experiments of Koehler as described (so graphically) in his *Mentality of Apes*. Finally from Child's *The Origin and Development of the Nervous System* and *Physiological Foundation of Behaviour* I have found a key to much recent work in experimental biology and read a stimulating theory. These three authors have, of course, furnished introductions to literatures that have taken me far afield, but I have purposely reduced my references in the text so far as possible to books of this order. If it be desired to

pursue reading in any of these fields, references will be found in the writings which are mentioned. The recently published work of Parsons, *An Introduction to the Theory of Perception*, can be warmly recommended as a guide to all the worth-while literature pertinent to the form and function of the nervous system.

Finally, a word should be added as to the general philosophic implications of my pattern theory. My education has never led me through the mazes of philosophy and I am as ignorant of its technical terms as I am unfamiliar with the habit of thought of the professed philosopher. Consequently, I am really incompetent to carry my generalizations beyond the range of the biological sciences, and for the few words in the last chapter in which a pattern metaphysic is suggested I beg the indulgence granted to a tyro. In particular, I must hasten to disavow any claim for proprietorship therein. It is quite possible that, when "patterns" are given universal reference, they merely repeat some well-known metaphysic. Specifically I suspect that Whitehead in the concluding chapters of his *Science and the Modern World* proposes a view that is analogous to, or identical with, what I am trying to express, but I am too innocent of mathematical philosophy to follow his argument with understanding. So I may be saying something quite different and should have no right to claim so great an authority as his in validation of my speculations.

J. T. M.

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