ANIMAL DRIVE AND THE
LEARNING PROCESS

AN ESSAY TOWARD RADICAL EMPIRICISM

BY

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VOL. I

WITH A SUPPLEMENTARY ESSAY ON

THIS MATERIAL WORLD

BY

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NEW YORK
HENRY HOLT AND COMPANY

To

MORTON PRINCE

FOREWORD

THE following essay on Animal Drive and the Learning Process was originally planned as a little tribute to my revered counsellor and friend, Dr. Morton Prince, on the occasion of his seventieth birthday anniversary. The subject, however, was more comprehensive and more exacting than I at first imagined. The birthday anniversary passed, and even several. And then, on the thirty-first day of August, 1929, Morton Prince himself passed beyond the reach of any tributes of friendship and admiration.

I now put out this first volume of the essay with a sad sense that it is belated: yet with the single satisfaction that the greater part of the manuscript, in substantially its present form, was in Dr. Prince's hands before his death.

As the subtitle indicates, it is an essay toward Radical Empiricism. And Radical Empiricism is a name which the late Professor William James gave to a way of thinking; a way of thinking which aims to escape, both in philosophy and in psychology, from the absurdities of subjectivism and any form of psycho-physical parallelism. In a notable essay, 'Does Consciousness Exist?' published not very long before his death, William James declared his conviction that 'consciousness,' the metaphysical entity, does *not* exist; that it is merely the last lingering echo of the primitive ghost-soul. Conscious phenomena of course exist, he said, and the problem of cognition exists, but not mental *substance,* that hypostatized fossil of all the concrete dynamic processes of knowing, feeling and willing which has so misled philosophers and psychologists these several hundreds of years. This means, and I believe that it so meant for William James, that conscious phenomena are to be explained entirely, without reserve or residue, in *physical* terms, and specially of course in the terms of physiology. In a meas-

ure he had already done this in his great work, 'The Principles of Psychology': yet, as he himself at several points reminds us, he did not there succeed in banishing the 'subjective' *sui generis* completely. Later, towards the end of his life, he was even more firmly convinced that 'consciousness' must be abolished.

William James did not live to apply his doctrine of Radical Empiricism in a detailed way to the problems of psychology. We shall never know just what picture he would have given us. We do, however, know that the ghost-soul of metaphysical dualism would have been completely eliminated. The following essay on Animal Drive is an effort to carry on in the spirit, at least, of Radical Empiricism.

As thus conceived, the task will lead us straight into physiology. If the 'materialism' which this seems to imply is at first disconcerting, it is to be remembered that from birth to death we live in the material world and at every moment depend on it for our breath and sustenance. So that philosophers and others who have sought to discredit or to deny the reality of the physical world, have merely performed a little tour de passe-passe or légerdeplume. The argument which is supposed to emancipate us from the 'trammels' of the physical world is in its logical structure simple and from Hume to Vaihinger unvarying. In the second volume of this essay there will be occasion to exhibit this bit of 'logic' in detail, and there the reader shall judge whether it is more than the merest puerility.

In the minds of all serious thinkers at the present time the traditional alignment, in which clodhopping materialists stoned with their rude slings the true children of light, the idealists, who replied with deadly lightning from on high, is a long since forgotten episode. The enemies have become friends and have intermarried. The physicists, although by descent materialists, are now imagining for themselves a physical world that is lighter than air, lighter even than the ether, as light in fact as empty space itself and seemingly identical therewith: while the children of the children

of light, inexpert in feats of panmathematism, incapable in fact of thinking dynamically at all, are still conceiving their world in terms of substance, 'mental' substance to be sure, yet a substance which as clearly spells 'materialism' to the contemporary panmathematist as theories of the atom and of physical shock ever spelled materialism to the foretime subjectivist.

As an approach to this singular realignment of intellectual allegiances Professor Harold C. Brown has very kindly consented to the reproduction of his essay, 'This Material World,' as a supplement to the present volume. For this realignment raises an issue that we must eventually face, and Professor Brown's presentation of it admirably introduces and perhaps even more complements some considerations to which we shall be led, more particularly in the second volume, in connection with the relation between *mind* and *reality*. As at the outset we are brought face to face with the question, whether a 'radically' empirical and physiological psychology is necessarily 'materialistic,' so at a later point we shall need to consider the very categories, 'mind,' 'matter,' and 'reality,' as psychological phenomena. The answer to the former question must wait upon the answer to the latter. The study of the conscious process (psychology) and the study of the knowing process (epistemology) cannot be kept apart if both are taken seriously. It may appear in the course of our study, why tradition has separated them. I am greatly indebted to Professor Brown for permitting his essay to be included in the present volume.

My most grateful acknowledgments for valuable suggestions and criticisms are due to Professor Herbert S. Langfeld, Professor William Morton Wheeler, Dr. Curt P. Richter, and Mr. George X. Bernier. They have generously assisted in this study of animal drive and the learning process.

E. B. H.

Princeton,
August, 1930.

I believe that thought is so little incompatible with organized matter, that it seems to be a property thereof. . . .

Given the least principle of motion, animated bodies will have all that is necessary for moving, feeling, thinking, repenting, and in a word for conducting themselves in the physical realm, and in the moral realm which depends upon it.

Let us then conclude boldly that man is a machine, and that in the whole universe there is but a single substance variously modified.

—J. O. DE LA METTRIE, *l'Homme Machine*, 1748.

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ANIMAL DRIVE AND THE
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PHYSIOLOGY VERSUS VERBAL MAGIC

IF one inquires what it is that moves an animal to action, one finds that two sorts of answer are current. In the opinion of some persons, the chemical energy derived from food is the source of all activity; in the opinion of others, the source of action is feeling, emotion, desire, or something of that kind. And further, in the case of animals that enjoy the benefits of domestication there is a tradition that they are driven by the lash. Now food and desire are so clearly unlike that one is led to wonder whether the truth can possibly be divided between them. If *desire* is the motive power, can *food* have any place in the scheme? Or again, if *food* is the source of energy, what can be the place, or indeed the meaning, of *desire?*

Since no man or animal continues active for very long without the replenishment of food, so that even the staunchest advocates of feeling or desire consent to eat as often as the rest of us, we must conclude that food, at least, is indispensable. And food is not incompatible with the stimulating prod. With these two items well in mind, let us consider, as in the eighteenth century La Mettrie considered, the general problem of animal drive. What impels man and other conscious animals to action? And then, what will account for the manner of their action?

The history of psychology discloses various attempts to answer this question. Most of these, in the earlier periods, try to find the source of action in some power of the soul, that is, in a 'faculty'; and in such faculties as those of feeling, desire, appetition, reason, and others; but most especially in that faculty which is so clearly nothing but another name for the phenomenon to be explained—the

'faculty of conation or will.' There have been efforts to analyze this conation or will, and almost always into strictly 'psychic' terms. Thus we have theories of volition couched in terms of conscious *fiat,* innervation feeling, motor image, or pleasure and pain; and they are often presented with a flourish of physiological terms to insinuate that the data of physiology have been duly taken into account. But in fact, if it were a question of any competent or serious study of it, all such theories leave the physiological organism, which is after all the actor, virtually ignored.

At the present time all 'faculties' of the soul, if presented in that guise, are deemed old-fashioned, and the more active discussions of animal and human action center round other, apparently different, categories, such as 'instinct' and 'purpose' and, one might add, *Gestalt.* For instance; man is impelled to action, it is said, by his instincts. If he goes with his fellows, it is the 'herd instinct' which actuates him; if he walks alone, it is the 'anti-social instinct'; if he fights, it is the instinct of 'pugnacity'; if he defers to another, it is the instinct of 'self-abasement'; if he twiddles his thumbs, it is the thumb-twiddling instinct; if he does not twiddle his thumbs, it is the thumb-not-twiddling instinct. Thus everything is explained with the facility of magic—word magic.

In view of the general contempt in which 'faculty psychology' is held, it is remarkable that anybody can fail to perceive that 'instinct' as here employed is merely a synonym for 'faculty.' Yet so great is the reifying power of words that even in so flagrant a case as this the mere name of the phenomenon is accepted by many persons as the *vera causa.* One could have hoped that Molière, in the seventeenth century, had given the *coup de grâce* to such verbal tomfoolery, in that familiar passage where the candidate in medicine says: "I am asked by the learned doctor for the cause and the reason why opium induces sleep. To which I reply, because there is in it a soporific

virtue whose nature it is to lull the senses." [1]   Molière has several passages in this vein.   And at about the same time Spinoza called attention to this same fallacy in his criticism of the 'will' and other faculties.   There is in the mind, he said, "no absolute faculty of understanding, desiring, loving, etc.—these and similar faculties are either entirely fictitious, or are merely abstract or general terms, such as we are accustomed to put together from particular things. Thus the intellect and the will stand in the same relation to this or that idea, or this or that volition, as 'lapidity' to this or that stone, or as 'man' to Peter and Paul." [2]   H. Vaihinger (1924, Teil II., § 14) has exposed this methodological vice most lucidly.   It is further illustrative of this point that at the present time almost any animal activity which can be named, tends also to be named as an 'instinct' by some one or other.   Dr. Ernest Jones (1924) has recently cited these two absurdities—the 'instinct for parliamentary government' and the 'instinct for salmon fishing.'   Thus every action, if only it is named, at once explains itself.

The case of a psychology of 'purpose' is not different. To hope to explain the operations of the body or the mind in terms of purpose, is merely to adduce as an explanatory category a faculty of purposing.   And this appellation will do no explaining, even though purposiveness can in fact be correctly predicated of many human and animal activities. Since this pathetic subservience to words, this mistaking them for causes, shows no sign of abating, it may be permissible here to examine such a process a little more explicitly.

Thus if one names the class of all plane figures that are bounded by three straight lines 'triangle' one has gained a convenient designation for a large number of plane figures. If now one learns that all these figures have the sum of their interior angles equal to two right angles, and then asks why

[1] *Le Malade imaginaire* (*troisième intermède*, Bachelierus speaking).   I understand that Prof. Stout has cited the passage in this same connection.
[2] *Ethics* (translation of R. H. M. Elwes) Part II., Prop. XLVIII., Note. See also Spinoza's *Abhandlung von Gott*, Theil II., Kap. XVII. (*end*).

this is so, it is possible to reply, "because all these figures *possess triangularity";* and "this must be so, because if you destroy the *triangularity* by adding a fourth side the internal angles will no longer equal two right angles." And the chronic verbalizer, who smothers in his own words and quite forgets the objects which he is talking about, can adduce a hundred other equally cogent 'arguments.' Sane folk know, however, that the constant total magnitude of the internal angles of all triangles is explained by the well-known theorem of Euclid, and not by any 'property of triangularity.'

Or again, I referred above to 'the reifying power of words.' That *names* a psychological phenomenon which may be said to underlie the logical fallacy which we are now considering: or it may be said to be the psychological aspect of this fallacy. That matters little. But if I supposed myself to have really explained the fallacy by adducing the 'reifying power of words,' I should be committing that very fallacy. Why the taking of words for things leads to fallacy may be partly clear from the illustrations I have given: but why words tend to be taken for things is what would still need to be explained (*cf.* Pareto, 1917, pp. 686-7, 691). Towards this I have done nothing, since my present purpose is merely to name a phenomenon, give illustrations of it, and to submit that it is, as logic, fallacious.

The point is that we cannot intelligently employ words if we forget for an instant the actual objects which the words *mean.* The objects and situations to which the words refer is that with which the mind should primarily deal if it would think coherently. And personally I dissent from the view that thought is impossible without language: it would be quite as true, I suspect, to affirm that thought is impossible with language.

Whether the *Gestaltpsychologie* is to be merely one more 'faculty' psychology seems still to lie on the lap of the gods (*cf.* Spearman, 1925). The protagonists of this movement seem to be tending to treat each 'configuration' as an en-

telechy, instead of analyzing it; and this is to fall at once into the old verbal magic. On the other hand, the *Gestalt* psychologists have a most salutary bent toward experimentation, and that of a high order.

It may be said that there is one modern movement, Behaviourism, which successfully avoids any form of the faculty-entelechy pitfall. I quite agree that it does so. And it has done very much to clear the atmosphere of such miasmas: but, as it seems to me, at a singularly high price! For so far as I understand Behaviourism, it flatly denies the existence of all psychological problems; whereas the animal movements present a great number of psychological problems. Now exorcism by verbal denial is a form of word magic that seems to me more primitive and rather cruder than the other. But perhaps not all behaviourists intend to deny the existence of mental phenomena: I do not know. At any rate, such studies as I have been able to make in psychology and in physiology have not brought me into very immediate rapport with either Behaviourism or the Psychology of Configuration.

But I have hinted sufficiently that the problem of animal motion, whether physiological or psychological, has not been and will not be solved by the time-honoured device of word magic. And the purpose of the present essay is not to criticize other psychologies, past or present, but rather to present the outline of a non-faculty psychology in terms wholly of physical and physiological processes. For I am convinced that these are the only terms in which we can find the solution of any of our psychological problems. If we are to understand motion, we must look of course to that which moves; and for our present problem this is always, and can only be, the bodily members of a living organism.

GROWTH AND LEARNING

IF it is the energy of gasoline or other fuel which drives the internal-combustion engine, it is as clearly the chemical energy of food taken into the body which somehow drives the animal organism. So long as psychology persists in disclaiming responsibility for describing the connection between the activities of an animal and the food which he has swallowed, it will of course continue to find the problem of animal drive vexatious.

Further, it is clear that the processes of nutrition, growth, and repair of the tissues in this engine of ours are singularly veiled from our mind's scrutiny. They elude introspection. On the other hand, the processes of sensory stimulation and of motor activity, which from the earliest times have been recognized as inseparably connected with the mental, are, biologically speaking, of a precisely opposite nature: they tear down tissue which the process of nutrition has built up (cf. Verworn, 1913, pp. 91-4; Beritoff, 1924 a, p. 45). Sensory excitation means katabolism in the excited substance and a release of energy; nerve transmission is a katabolic process, though a very economical one; and muscular contraction, most of all, is a destruction of substance and a conversion of the energy so released into work. This contrast between anabolic process and katabolic process, between the accumulation and the spending of energy, gives a significant hint of the distinction between biology and psychology. For some reason mental phenomena are found only where there is an organism whose stored-up energy can be released, by stimulation of sense-organ and nerve, to produce muscular contraction and therefore some sort of motion. Or again, it is in every case an an-

imal body, nourished by food and so maintained in readiness for being stimulated and for actively responding, which is the seat of mind.

Now we shall better understand the organic mechanism of the body if we glance, at least, at the process of development which, from a fertilized egg, has brought the body into being.

In the development of organisms we are accustomed to distinguish rather sharply between organic growth and a subsequent process, that of learning, and to consider the former as an unfolding of 'potential' characters already *contained or preformed* in the fertilized ovum, the environment merely permitting or at most 'favouring' this eclosion, and the latter process as a series of modifications or 'deformations' (Le Dantec) that are impressed on the organism wholly from without, that is by environmental factors. But this view comes from a too summary and abstract consideration of the process in question, and from that insidious linguistic artifact 'potential.' No 'potential character' ever *is* 'already *contained*' in anything: and the notion of potentiality, wherever used, is a mark of finalistic thinking. The contents of the germ-cell are not potential characters at all, whether bodily or mental: they are actual proteins and other substances, and to call these substances 'potential' this or that is to flout the truth. As Verlaine (1922, p. 15) has said, there are no 'innate characters.' So that those persons who imagine that in the fertilized egg they are dealing with 'potential' or 'congenital' albinism, cyclopia, or lethal factor, with agoraphobia, herd instinct, or mathematical endowment, will in the end of course have to give place to more observant investigators who can recognize a carbohydrate or amino-acid when they see one. It is deplorable that such gross superstitions survive in modern biology and psychology. And the confusion is as disastrous for any sober study of heredity as it is for other departments of biology or psychology.

If without this finalistic bias we consider the fer-

tilized egg, we realize at once that its growth is not a mere process of unfolding from within. It does not grow, nor does it even continue to live, unless it is steadily assisted from the outside by various 'vital conditions' (Verworn, 1899, pp. 348-351) such as warmth and moisture, in some cases light, and always by specific nutritive substances in the surrounding medium. As the late Félix Le Dantec never tired of insisting, every living cell is in active relations with its immediate environment (whether an inorganic environment or one of neighbouring cells) : exchange of energies and of substances is at every instant indispensable to the life, growth, and every other function of every cell.

If now we consider 'normal' growth and function, we find that it is not a mere 'unfolding.' Specific external conditions contribute as essentially to determine the specific result as does the original content of the cell. As Prof. C. M. Child (1924, p. 239) has said, preformistic theories "lead us to a conception of a normal pattern, which is predetermined and innate, that is to say inherited, while modifications of that pattern represent effects of individual reactions to environmental factors. From the physiological viewpoint, however, the normal pattern is no more and no less preformed, predetermined, or hereditary than any modifications or abnormal patterns. . . . On the other hand, the evidence forces us to the conclusion that all [organismic patterns] are likewise behaviour patterns, that is, their realization is not an autonomous action of a protoplasm, but the reaction of a protoplasm of specific constitution to an environment." To the same effect Dr. R. S. Lillie remarks (1923, pp. 38-9: see further pp. 4-7, 25-47) : "It has been shown that constancy of development in any particular species requires constancy in the external conditions . . . in all cases the co-operation of definite 'internal' and 'external' factors is necessary to yield the final result." Or similarly Prof. Weldon, as quoted by Dr. H. C. Bastian (1910, p. 57: see further Bastian's Chapters III., IV., and V.) :

"Hence for normal development and perfect embryogenesis a very constant and 'correct' environment is necessary." Or again, M. Etienne Rabaud (1911, pp. 32-3) : "In so far as these [the environmental conditions] remain constant, the various phases of development remain likewise constant, and this constancy even gives to the processes that appearance of end-to-be-attained which has deceived many an excellent observer."

Prof. Lillie (1923, p. 31) has particularly well indicated, it seems to me, the correct scientific position regarding that which the original constituents of the fertilized egg contribute toward the larger pattern of the organism. "The essential fact, requiring physiological explanation, is that each individual animal or plant resembles, structurally, chemically, and physiologically, other individuals of the same species, while differing from those of other species. As already indicated, the physiological basis of this specificity is to be sought in the specific nature of the chemical processes by which the organism is synthesized. We find in fact that a chemical specificity, corresponding to the specificity of the organism as a whole, is exhibited by its constituent proteins, and apparently by these compounds alone. . . . [p. 38] certain proteins with a basic or fundamental relation to the organization of the species may be chemically identical in adult and germ; and they may even be distributed spatially in a similar way in both; *e.g.* with reference to the main axes. In this sense a chemical continuity between germ and adult may exist, corresponding to the morphological continuity. At present, however, we are completely ignorant regarding the details of this correspondence and can only await the results of further investigation."

Yet this chemical specificity is not of itself sufficient to ensure the specific and 'normal,' or indeed *any*, growth and function of the developing individual;—the environment must be very 'constant and correct.' We must here bear in mind the fundamental fact of developmental me-

chanics: that the phenomena of structure and function **are,** in living organisms, never independent of each other. As Prof. C. M. Child (1921, pp. 114-5) has admirably expressed it, "development is a process of functional construction, that is, beginning with a given structure and function, the continuance of function modifies the structural substratum, and this in turn modifies further function and so on." The notion of growth as a mere unfolding of potentially contained characters is to be abandoned. And further, the process of functional construction which is so largely sustained by outside, environmental agencies *is not different in principle* from the process that we call 'learning.' Growth (in so far as it is organic development, and not mere increase of volume) and learning are one continuous process, to the earlier phases of which we give the one name, and to the later (including adult) phases we give the other. The increase of volume is peculiarly a matter of anabolism, chemical synthesis probably effected by organic catalyzers or enzymes (Troland, 1914, 1916, 1917); while developmental growth or learning is probably, as we shall see, mainly the work of katabolic processes which **are** initiated by external stimuli.

THE PATTERN OF THE ORGANISM

As cell-division and growth proceed in the fertilized egg an organic pattern begins to emerge, which develops eventually into the active organism. And the mode of this development will never be explained by even the most earnest speculations on preformism, heredity, or mneme: for here is a process going on which will be understood only if it is directly and attentively observed. Happily, from Wilhelm Roux [1] onward there have been a few serious students of experimental embryology and the general field of developmental mechanics.[2]

The organic pattern is clearly different from the pattern of the protoplasm that is contained in the cell, as also from that of the molecules within the protoplasm. The notion that this larger organic pattern is somehow wrapped up in the nucleus or cytoplasm of the fertilized egg is not held by those investigators who are studying experimentally the actual process of development. From them we get a different picture. One of the first features to emerge in this organic pattern (whether in plant or animal) and beyond question the most fundamental structural feature of the adult organism, is one or more *axes*. In plants the (usually vertical) bud-stem-root axis, in most animals the head-tail or apico-basal (or sagittal) axis is the primary structural axis, and the first feature of organic pattern to appear. Later on, other axes (radial, bilaterally symmetrical, dorso-ventral) make their appearance. Child, in the two works

[1] Roux's *Einleitung* (1895) will still repay careful study. There is an admirable English translation of it, by Prof. W. M. Wheeler (1895).

[2] Some of the important investigators at the present time are Bok, Child, Coghill, Detwiler, Harrison, Herrick, Huxley, Ingvar, Kappers, Spemann.

cited, has collected a large amount of evidence, from many
investigators, proving that in very many cases at least the
primary, apico-basal axis and the secondary axes appear in
the growing embryo *only as an organic response to external
influences*. And, as Verworn (1913, pp. 91-4) has pointed
out, such external influences are largely stimuli to katabolic
process in the organism. In some cases no apico-basal axis
will appear at all in the absence of external influence; or
the place of its appearance can be governed by external
influence; an axis that has begun to appear can be made to
disappear, and a differently located axis to appear by ex-
ternal influence; axes that are anomalous both as to number
and position can be produced by external influence; and in
practically every case the length of axes and their rate of
growth can be governed by external influence.

The external influences here in question are very various,
but in general they are the same that have been found, in
other branches of biology and physiology, to *excite* or *stim-
ulate* living tissue: that is, they are light, heat, electric-
ity, chemical substances, etc. And "excitation, however
brought about, appears to be essentially an increase in the
rate of living, or at least in the fundamental energy-liberat-
ing processes [*i.e.,* katabolic and most often oxidative]
concerned in the life of any particular protoplasm" (Child,
1921, p. 16).

Now the external irritant acts at the surface of the egg
or embryo, and the excitation is transmitted through the
protoplasm inward. And such an excitation at the surface
transmitted inward, with diminishing intensity, constitutes
an *axial gradient*. If the excitation and the resulting axial
gradient are for some time continued, changes are produced
in the protoplasm along the line of the axial gradient, which
are irreversible: and it appears that in this way the first
feature of organic pattern, the apico-basal axis, originates.
"The apical region or head of the organism arises from the
region of greatest activity or highest metabolic or oxidative
rate, the 'high end' of the major or polar gradient" (*ibid.,*

p. 25). There is good evidence that the other axes (radial, or medio-lateral and dorso-ventral) arise in exactly the same way. And furthermore, very many other features of the organic pattern seem to originate with the appearance of such an axial gradient (*ibid.*, pp. 47-9; *cf.* Coghill, 1923). Thus, "In a particular environment a certain organismic pattern may appear highly stable, but experimental changes in environment may show that even its most general features are readily modifiable by many factors. This is strikingly true in the case of the sea urchin. The normal pluteus presents a very definite, highly constant pattern, but through experimental conditions affecting the height, slope, etc., of the gradients, this pattern is modifiable to such an extent that if we did not know the origin of the forms thus produced, we should never recognize them as belonging to the same species, or in the more extreme cases, to the same class, or perhaps even the same phylum as the normal larvæ" (Child, 1924, p. 247).

"Concerning the existence of the axial gradients," says Child (1921), "and their significance in development there can be no doubt" (p. 85). "I believe, in short, that the interpretation of axiate pattern in terms of excitation and transmission and the more or less persistent developmental protoplasmic changes which result from it is not only the simplest interpretation possible on the basis of our present knowledge but the interpretation which accounts most satisfactorily for all the known facts" (p. 87). For a more popular account of axial gradients see Child, 1925.

Studies in embryology and specially in experimental embryology are of course much more difficult in the case of viviparous animals, so that we have fewer data as to the origin of organic pattern in these higher forms. Here clearly evolution has provided a most 'constant and correct' environment for the earlier developmental stages of the egg, and it is not to be wondered at that the organic pattern of the individual is highly stable, that is, it generally turns out 'normal.' It is fair to believe, until there exists some

empirical evidence to the contrary, that the organic pattern in these cases originates on the same general principles as in the better known forms which are lower down in the animal kingdom; that is, that the development, like the *known result,* is axiate and that the axes arise through axial gradients produced by exciting agencies external to the embryo. If this is so, then in general, as Child (1924) says, "the individual from its beginning represents a series of reactions to external factors of a particular protoplasm" (p. 229): "the organism has evolved primarily, not as a morphological structure, but as a behaviour mechanism in the broadest sense" (p. 236). "Organismic pattern then appears to be a behaviour pattern in a protoplasm" (p. 10).

Dr. L. Verlaine (1922, pp. 10-11) has expressed a similar view. "A unicellular individual acquires, in the course of its existence, the characters morphological or functional that were possessed by its predecessor, for the same reasons that this predecessor himself had previously acquired them. But, in order that a child or other organism may inherit the characters of its parents, it is not enough that the fertilized egg which produced it be identical with those from which its parents emerged: it is further necessary that the child be placed in those special conditions which are indispensable to the full realization of its specific potentialities. . . . the problem of the inheritance of specific characters reduces itself, for multicellular as well as for unicellular organisms, to explaining the mechanism of life, of that acquisition of specific characters which is repeated in each new generation, and the cause of heredity should be sought first of all in the constancy of the environment." [3]

"Viewed in this way the whole course of development is a process of physiological learning" (Child, 1924, p. 249). The general scheme appears to be that anabolic, or chemically constructive, processes are carried on by catalytic

[3] Verlaine's entire article, which is on 'The Inheritance of Acquired Characters,' is of interest in the present connection.

(especially autocatalytic) action.[4] These processes, strictly chemical of course, may be considered *the essentially vital* processes (Troland, 1914, 1916, 1917). Here the various catalyzers, or enzymes, contained in the germ-cell are perhaps the *more* active and specific agents although the environment must supply favourable vital conditions (warmth, light, etc.) and such raw substances as the enzymes in question can utilize (fairly specific requirements, both). Whereas the *organization* into organic pattern of the substances so created appears to be effected by a quasi-opposite process. Environmental forces impinge on the living unit, stimulate the built-up substances to katabolic, or energy-liberating, activity, that is, to 'function'; and the energies freed in this functioning influence the next succeeding phases of anabolism.[5] For 'function' commences, not with the first pulsation of the heart or with some visible twitch of muscle, but at least as early as the first cell-division or indeed earlier. Hence it is that the organism is inevitably more or less 'adapted' to its environment (*cf.* J. S. Huxley, 1924), and this adaptation will frequently *appear* to be 'teleological.'

Too much of organic structure has been attributed to 'growth,' as an unfolding from within, and too little to 'learning,' that is, to the effect of environment. The *structure* of the organism, the organic pattern, is in considerable part acquired through the influence of external stimuli; these latter producing excitations that are transmitted through the chemically formed but organismically unformed tissue of the embryo. Nevertheless one sound distinction remains, as already indicated. All anabolic processes are doubtless carried on and regulated by the *enzymes* contained in the fertilized egg; and these, I believe, are the sole basis, in any true sense, of heredity. For growth these enzymes require material (food) from the environment;

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[4] A catalyzer is a substance which by its presence accelerates a chemical reaction, but itself appears to remain unchanged. A catalyzer that is organic in substance is called an enzyme.

[5] Excitation or stimulation is always, probably, a release of energy.

yet they *select* from the material available and they determine *how* this shall be synthesized into higher organic compounds (anabolism) to make the *substance* of the organism. But this raw substance acquires ('learns') organic pattern by an inverse process, a katabolic one;—stimulation from outside, axial gradients, organic axes, organs. The two processes are always proceeding simultaneously. And this would be a sound physiological distinction between growth, as increase in volume, and learning; or, one might say, between mere life and function. We shall find that the field of psychology lies on the side of developmental learning or function.

Thus food substance synthesized by enzyme action and quickened into function (katabolic activity) by the release stimulation of external irritants forms the living organism. And this, whether embryonic or adult, whether 'growing' or 'learning,' is at any given moment merely in some one phase of that *continuous process* of development which ends only at its death.

THE PATTERN OF THE NERVOUS SYSTEM

THE view which I have quoted from Prof. Child rests on a considerable body of empirical data, derived from many sources. The view gains weight from the fact that there can now be little doubt that the pattern of the nervous system, at any rate, develops in precisely such a way. The primitive neuroblasts develop into neurones by growing out along axial gradients. Since even the simplest sensori-motor arcs consist of three threadlike neurones in series, and since each of these first appears in the growing embryo as a tiny round cell, a neuroblast, which slowly grows in length, like the rootlet of a plant, eventually to attain a functional connection with an (also growing) end of some other neuroblast, it became clear to students of developmental mechanics that the doctrine of 'preformed' nerve pathways left something to be desired. The 'preformed' pathways were in process of formation, and the 'inherited' sensori-motor connections were as yet unconnected. How could these growing neurones form sensori-motor chains that would ever function in any useful or significant way? Here would have been an opportunity for 'heredity' to appear right under the microscope.

The elder His, Cajal, and other early students of hodogenesis observed that the growing neuroblasts, as they pushed their way through the embryonic tissue, looked much like the growing roots of plants. And these are known to turn, as they grow, away from light, toward moisture, toward some and away from other chemical solutions, and round obstacles (such as stones). The growing rootlet is guided in its course by these several tropisms. It was natural to conjecture that the growing neuroblast was

similarly guided. Indeed, neurones were *observed* to grow round obstacles (such as cartilaginous tissue) and often to thread their way *between* other cells. Also the tendency of some neurones to grow directly into muscle cells, and the affinity of others for ectodermal cells, suggested chemotropism. Such conjectures were not far astray. All chemotropism depends on concentration gradients, and such concentration gradients probably play a part in the development of organic pattern.

But chemical concentrations, like all chemical processes, are closely bound up with electrical phenomena. And the most recent and most successful investigations in this field have been concerned with electrical gradients. It is found that both the axone and the dendrites of the growing neuroblast are guided in their growth by electrical gradients: they exhibit a clear galvanotropism. We have now, owing largely to the investigations of Drs. Kappers and Bok, two definite laws of neurone growth. "Bok found that when a bundle of unmedullated nerve-fibres grows out and passes neuroblasts on its way, these neuroblasts become activated by that bundle and send forth an axon in a direction perpendicular to the activating bundle, apparently growing in the same direction as is taken by the current that irradiates sideways from the growing bundle" (Kappers, 1921, pp. 132-3). That is, axones grow directly *away* from a kathode pole (*i.e.* 'stimulofugally,' or as it is sometimes put, 'concurrently'). This is Bok's law of Stimulogeneous Fibrillation. It has been confirmed experimentally by Ingvar (1920) with serum cultures of embryonic tissue (*cf.* also Detwiler, 1921; 1923 [a], p. 377; 1923 [b]). The second is Kappers' law of Neurobiotaxis, which, briefly stated, is that dendrites grow *towards* an active neurone or nerve bundle (kathode pole), *i.e.* stimulopetally and contracurrently, provided that the neurone from which the dendrite grows and the neurone towards which it grows are in excitation simultaneously or in close succession (Kappers, 1917, p. 266). The cell-body itself may eventually migrate in the

same direction as its growing dendrites. This law has also been confirmed (Kappers, 1922). Kappers (1917; 1919, p. 53; 1921) has remarked that the law of neurobiotaxis begins to look like the psychological law of association. And we shall see that with a very small additional consideration as to the direction in which the excitations flow, it *is* the psychological law of association.

The two laws just stated seem to be, in this broad way, definitely established. Various more detailed points, such as the precise movement of ions at the boundary membrane of the growing nerve tip (a question, indeed, which involves the general theory of excitation and transmission as well as that of growth) are still under discussion. But as Kappers (1921, p. 146) has remarked, in an undogmatic spirit, to Child, "the chief point is that we agree that electrical potentials are the moving factors. As far as concerns their details the neurobiotactic phenomena will offer still many difficulties and controversies, the whole problem being a very difficult one." [1]

So too in regard to the whole development of the egg, there remains much that is ambiguous, much that is under dispute, and much that is still unexplored. And if I seem to have made a long digression into this field, it has been with two aims in view. First, to show that the facts so far as known go to prove that the development of the egg is one continuous process from the outset, and is guided very largely by the principle of excitation from external agencies and transmission of the excitation along axial gradients: of which stimulogeneous fibrillation, neurobiotaxis, and (as we shall presently see more fully) psychological association are but special phases. I hope to show that *no* independent new principle enters in to interrupt this continuity of development even in the adult stages of life, and that this same developmental process in its later phases *is* mentation and consciousness. It is not, then, a digression to glance

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[1] There are two excellent papers on morphogenesis in the nervous system, by Professors C. J. Herrick (1925) and S. R. Detwiler (1926).

briefly at the earlier phases of a process which in its later, adult phases is the very basis of psychology. Certainly, too, it is fair to invite those persons who believe in a non-physical 'subjective' entity (the 'mind' or 'consciousness' of dualistic philosophy) to consider carefully the developmental process so that they may tell us just where they suppose that this novel entity begins to intrude.

Second, I have wished to advertise the forgotten fact that there is such a process as ontogeny. For it *is* a perilous insouciance with which many biologists and the majority of psychologists rattle on about ancestral habits, original nature, innate tendencies, inherited instincts, innate ideas, engrams, mneme and so on (or the geneticists about heredity units, determinants, ids, genes, and factors [2]), while refusing even to glance at the process of ontogeny; where all these things, if they have any existence at all, should be in evidence. It is fair to invite these persons to study ontogeny and to discover there, say, an 'inherited instinct.' As Prof. Lowie (1923, p. 298) well says, "Since we are interested in establishing the existence or nonexistence of *innate* differences, the influence of training and other non-congenital factors, all of which for convenience' sake we may call environmental, must be eliminated. The light-heartedness, not to say unscrupulousness, of many writers on this point is appalling."

Mais, revenons à nos moutons. The law of neurobiotaxis *is* undoubtedly very close to being a physiological law of association. We must remember, however, that when the neurones have developed sufficiently to function as a synaptic nervous system, nerve-impulses will pass only from the axone of one neurone to a dendrite of another, that is, will pass only towards a muscle; presumably because the synapse is, demonstrably, a 'one-way valve' (Sherrington, 1906, pp. 38-42; Forbes, 1922, p. 373). There will be a directionality of function. If we combine this consideration

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[2] For a fair idea of the antics of contemporary geneticists I would refer the reader to the criticism by Dr. É. Rabaud (1921).

with Kappers' law of neurobiotaxis (growth of dendrites) we get, as I believe, the definitive law of association. And precisely this, as it happens, has been independently discovered by Dr. Pavlov, in what he calls the 'Conditioned Reflex.'

THE CONDITIONED REFLEX

THE statement most nearly approaching a definition of the conditioned reflex which I have found in the writings of Pavlov, is the following (1923 [b], p. 33) : "If any casual [or 'indifferent'] stimuli happen, once or a few times, to accompany stimuli which elicit definite inborn reflexes, the former stimuli begin of themselves to produce the effect of these inborn reflexes. According to the particular sequence of conditions, associations are inevitably formed in an orderly fashion. In this way we have every reason to consider associations as true though acquired reflexes, and we have the incentive to study them purely physiologically. We designate the two sorts of reflexes, and the stimuli by which they are elicited, as unconditioned (inborn) and conditioned (acquired), respectively."

Of course many reflexes are 'inborn' in the sense that the organism has already acquired them before it emerges from the egg or the womb. But before this, Pavlov believes that reflexes are definitely inherited. Here he is merely influenced, I believe, by the current superstition, as one can see from his easy habit of calling unconditioned, *i.e.* inborn, any reflex which an animal happens to possess at the moment when Pavlov commences conditioning it to new stimuli. We have just seen why no reflexes are inherited. Nor is it necessary that the 'unconditioned' reflex should have been learned prior to birth: a stimulus can be conditioned to a previously conditioned reflex, as Pavlov himself undoubtedly holds (1923 [a], p. 359). The essential point about the conditioned reflex is that it shows under what conditions new reflexes are acquired or, more specifically, how a stimulation whose afferent or sensory path has so far no

central connection with a motor path, acquires such a connection.

In one respect I think that Pavlov unduly limits the scope of this 'conditioning' process. He says (1923 [b], p. 33) that after extirpation of the cerebral cortex no further conditioning of reflexes can take place; and, if I remember correctly, Pavlov's original name (1898) for conditioned reflexes was 'psychic' reflexes. It may well be that in adult animals, and such have been commonly used in the experiments, it is difficult to effect new conditionings after extirpation of the cerebral cortex (especially if optical or auditory stimuli are used!). But the close relation between the law of neurobiotaxis and that of the conditioned reflex, and many other considerations, make it altogether unthinkable that the principle of the conditioned reflex applies only to the cerebral cortex: and Beritoff 1926, pp. 389, 394-5) has found experimentally, in pigeons, that the cortex is by no means essential for the conditioning of responses.

From a reading of Pavlov's experiments, particularly his earlier ones (1898), and in substantial agreement with the definition given by J. S. Beritoff (1924 [b], p. 111), I should formulate the principle of the conditioned reflex as follows (*cf.* Pavlov, 1923 [a], p. 359; 1923 [b], p. 33) :

I. If one afferent (*i.e.* sensory) path already has connection with a motor path of discharge, then another afferent path if stimulated simultaneously, or nearly simultaneously, with the first afferent path will tend to acquire the same motor path of discharge.

II. After some repetitions of this simultaneous excitation, a stimulation of the second afferent tract alone will suffice to innervate the same motor path (*i.e.* to produce the same muscular contractions).[1]

This law of the conditioned reflex contemplates the nervous system at a time, obviously, when it is so far developed that some afferent impulses can pass through the central

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[1] An admirable summary of work on the conditioned reflex has been published by C. L. Evans (1925, pp. 328-360).

nervous system and out along motor nerves to muscles. (I am not imputing any special utility or significance to such reflexes.) Now by that time the neuroblasts have become neurones, and they may be divided into three classes: neurones that run from the external surface of the organism to and into the central nervous system (afferent or sensory neurones) ; those, of various lengths and very varied distribution, which lie wholly within the central nervous system (central neurones) ; and those which have one end inside the central nervous system while the other extends out to and into some muscle (motor neurones). Any impulses traversing these neurones will have to come from outside stimulation and along afferent neurones.

But these neurones are not connected end-to-end in any 'preformed' chains (though their general topography is not entirely random since even their earliest growth has been determined by electrical gradients), and any impulse, in order to pass from the end of any one neurone to any other, will have to travel as best it can through a region of non-nervous tissue. Such regions are called regions of decrement, synaptic regions, or synapses, and it has been found (Sherrington, 1906, p. 39; Forbes, 1922, p. 373) that the nervous impulse can pass through such a synaptic region in one direction only, from the axonal termination of one neurone to the dendritic termination of another. That is, the synapse is a 'one-way valve.' All nervous impulses then, starting, as was just said, from external stimulation and along afferent neurones, will pass (and according to all indications quite *at random*) from an afferent axonic arborization to a dendrite of some other neurone, then from the axonic ending of this to a dendrite of another, and so on: but they will come sooner or later to some muscle, because it is the *axonic* endings of motor nerves that have grown to and into muscles.

If now we add this fact of the general *sensori-motor direction* of impulse-flow to Kappers' law of neurobiotaxis (that "dendrites grow towards an active neurone or nerve

bundle provided that the neurone from which the dendrite grows and the neurone towards which it grows are in excitation simultaneously, or in close succession") we shall see that neurobiotaxis is the underlying histological aspect of all reflex conditioning. Inasmuch as every afferent impulse spreads more or less widely as it traverses the central nervous system, the afferent impulse which is to be conditioned will somewhere come to a synaptic region which forms a part of the sensori-motor tract along which the 'unconditioned' impulses are travelling to a muscle. At this point of conjunction the conditions requisite for neurobiotactic growth are realized, and dendrites (on the motor side) of the synapse of junction will be stimulated to growth, contracurrently, toward the terminal arborizations of the neurone on which the to-be-conditioned impulses are arriving. This dendrite growth will tend to reduce the extent of non-nervous, and more resistant, tissue across which subsequent nerve impulses arriving on the same 'to-be-conditioned' path must pass in order to reach the motor side (dendrites) of this synaptic region. Thus the resistance will be reduced which this synapse interposes between the 'to-be-conditioned' impulses and the already established motor outlet of the 'unconditioned' impulses. We shall presently see that the only *original* 'unconditioned' impulses are such as make their way through the central nervous system and out to muscles *at random;* corresponding to the picture of general random movements in the fœtus.

In my opinion this growth of dendrites under the stimulus of nerve impulses is the sole basis of learning. And here a further point is worth noting. Any, even random, nerve current arriving at the terminal arborizations (telodendria) of any neurone may succeed, if it is intense enough, in crossing the synapse and stimulating dendrites of other (and with reference to this synapse, relatively motor) neurones. In this case, as in the case previously considered, the requisite conditions for neurobiotactic growth are realized, and the resistance which this synapse

offers to the passage of subsequent impulses will be lowered. In this way the resistance of any synapse is lowered by use, or in other words the neurobiotactic growth of dendrites explains the familiar phenomenon of habit.[2]

While the conditioned reflex has recently been given a conspicuous position in physiological and psychological discussions mainly through the investigations of Pavlov and his associates, Pavlov is not actually the first to have observed the phenomenon. The principle of the conditioned reflex was described, and illustrated with a diagram, by William James (1890, vol. II., pp. 584-5), who referred (p. 583) to the general tendency of afferent impulses to spread over into any simultaneously active motor paths 'hypothetically' as a process of 'drainage.' And under that caption the phenomenon has met with considerable subsequent discussion (cf. Sherrington, 1906, pp. 200-3; Bayliss, 1915, pp. 424-5).

As we have seen, the conditioned reflex is only a special complication of the law of neurobiotaxis; and this by reason of a definite motor reference or specification which is not contained in Kappers' present formulation of neurobiotaxis nor, on the other hand, in the familiar psychological law of association. I hope to show that the principle of the conditioned reflex is the true psychological, as well as the physiological law of association: that it is, indeed, the conditioned reflex which brings *mind* into being. As Pavlov (1923 [a], p. 359) has said, the conditioned reflexes "are being acquired all during the life of the individual, and *are the education and the development of the individual*" (italics not in the original). It is to be freely conceded that mind or consciousness is a process, an activity of the body, and not a ponderable or static substance: if that is any solace to those persons who insist on the "immateriality of the soul."

[2] It is to be noted that the direction of dendrite growth is always counter to the direction of impulse propagation. Prof. K. S. Lashley (1924) dissents from the view that synaptic resistance is lowered by the passage of nerve impulses.

So far, as to animal drive, we have seen that any organism is undoubtedly propelled in the strict sense, but not steered, by the food energy which is stored in its tissues, especially in its muscular tissues. The stored energy is released by forces of the environment stimulating sense-organs which in turn excite the nerves and muscles. This *release* of stored energy is 'drive' in the looser sense; as when a man drives a horse. We are really prodded or lashed through life. But what steers us? The development of the mechanism in which this energy is released and the means by which it is released are now our problem.

RANDOM MOVEMENTS

WE now leave behind the earlier phases of ontogeny, and commence our further study with that phase in which the organism is just far enough equipped to enable Pavlov's law to operate. This will be, of course, when at least some of the afferent nerves possess irritable sensory organs and at least some of the efferent nerves have already made their connection with muscles. It is generally agreed among embryologists that at this moment of the ontogeny most if not all of the synaptic connections between neurones have yet to be made; that is, that few, and more probably no, complete reflex paths (*sensu stricto*) from a sense-organ to a muscle as yet exist. In other words, at this moment (in mammals this is several weeks prior to birth) a pressure on the eye-ball is just as likely to produce (if it produces anything) a twitch of the arm, hand, leg, or toe, as it is to produce a movement which in any way refers to the eye: there being no established synaptic paths in the central nervous system, the afferent impulse passes diffusely along whatever lines of least resistance it finds. Hence it is that even after birth, although not a few reflexes are by now established, yet by far the greater part of a baby's motions are *random* writhings, wrigglings, and twitchings (Preyer, 1885, p. 453). The evidence of engrams, inherited instinct, or mneme (R. Semon) is singularly scanty. Here is the (very conservative) statement of Prof. J. B. Johnston (1906, p. 220) : "Whether any regularity in the relations of these neurones is constant in the species and is inherited from generation to generation is unknown. It seems more probable that these neurones offer a relatively indifferent material in the embryo, providing for the diffusion of im-

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pulses from segment to segment and from one side to the other, and that definite paths for impulses are set up chiefly as the result of the experience of the individual." [1]

Similarly, and with special reference to the absence of myelin sheath at this stage of development (third to fourth month in the human embryo), Dr. Minkowski (1922, p. 753) remarks: "Since the fœtal spinal cord is so constituted, it is easy to see that excitations arriving from the periphery, finding here no definite conduction paths in the form of differentiated neurofibrils, of myelinated nerve-fibres, or of individually insulated neurones, will naturally spread more or less in all directions and give rise to diffuse and variable reactions; as we actually find to be the case with young fœtuses." (See also *ibid.*, p. 724.)

Precisely because no connections between afferent, central, and motor neurones are 'preformed,' or established by 'heredity,' the earliest movements of organisms are utterly random movements. The movements become biologically useful, significant, or 'purposive' only very slowly. The human infant at birth exhibits mostly (but not entirely) random movements: and some trace of random movement can be detected at least as late as the tenth year; and indeed, in a restricted and minuter sense, throughout life. Dr. Minkowski (1921, pp. 1111-2) gives the following picture of the movements of human embryos from about the second to the fifth month of intrauterine life. "The head turns to this side and that, is lifted and lowered, the trunk bends and straightens out, the extremities are lifted or fall back, they bend or unbend, swing out from the body or back to it, or twist in one direction or the other. These movements are slow, unsymmetrical, arrhythmic, unco-ordinated,

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[1] Similarly in an amphibian (amblystoma) Prof. G. E. Coghill (1924, p. 64) finds that the central nervous connections are the last to be made. "It is known (Coghill, '14) that the afferent system on the one hand and the efferent system on the other are structurally ready to function for a considerable time before response to tactile stimulation occurs. Such response begins when the floor-plate cells establish a commissure in the vicinity of the fifth and seventh nerves and opposite the fifth myotome."

diffuse and unformed, of small amplitude, and there is a tendency for the member to return to the position from which the movement started: the movements may involve a single joint (for instance, the shoulder, neck, hip, or knee) or several joints, a single member or several members at the same time: sometimes it is the proximal portion of the limbs that is most active, and sometimes the distal end: in one case (a fœtus 8.5 centimetres long) I even saw little movements of the fingers, individually, and a rapid trembling of the thumb; in another (fœtus of 20 centimetres) also movements of the fingers. If one were to characterize these fœtal movements by a clinical neurological term, I think one might well call them *choreiathetose*. From time to time, specially if one applies stimulation, these mild little movements are interrupted by others more sudden and rapid, more intense and of greater amplitude, a more *choreiform* movement: such movements become more frequent in the older fœtuses (fourth to fifth month), whose movements also tend to take on, momentarily at least, a more regular character, the two arms or legs moving more or less co-ordinatedly, together or in alternation." On stimulating the fœtus, "one observes usually, and more pronouncedly in the younger individuals, no isolated reflexes merely in the limb stimulated, but *motor reactions which spread to the other limbs and to the head and trunk, and which are extremely variable.*" (P. 1113.) "In general, one may say that *every part of the integument can serve as a point of stimulation for very various motor reactions, near and remote, and tending more or less to spread through the entire foetal organism.*" (See also Minkowski, 1924; 1925; 1928, pp. 534-556.)

All observers agree that the early movements of any embryo are unco-ordinated, and that a large part of the movements of the human infant after birth are random movements (*cf.* W. Preyer, 1885, p. 453; M. Sherman, 1929, pp. 144-5). We are in a position to see why this is the case.

At this stage of its development [2] the nervous system consists, first, of afferent fibres attached peripherally to sense-organs, which go to the central nervous system (brain or cord) and there end without definite functional connection, as yet, with other neurones; second, of central neurones (later to be called 'connection fibres' or 'association tracts') lying wholly within the central nervous system but as yet unconnected at either end with other neurones; and third, of motor neurones starting without definite connection with other neurones in the central nervous system and going out to muscles. If now any sense-organs are far enough developed to function and so to send impulses along afferent neurones, these impulses on reaching the afferent nerve termini (in the cord or brain) spread quite fortuitously in the intervening tissue and continue along those neurones whose dendritic endings lie nearest. Of course it is accidental what muscles such impulses eventually arrive at. Hence random movements.

Clearly the nervous system at this stage is not so very different, functionally, from the primitive nerve-net of the very humblest marine animals. It allows afferent impulses to diffuse or spread so widely that the possibility is open for *any* sense-organ to acquire functional connection with *any* muscle. And this remarkable equipotentiality is not

[2] Such a 'stage of development' is, strictly speaking, somewhat of an abstraction; as is, say, the familiar notion of a single 'reflex arc.' Central connections between neurones are made at various periods of the development, earlier here and later there. The true picture is of the utmost complication: such factors as the early autonomous rhythmicity of muscles (Minkowski, 1923, p. 479), the steady multiplication of internal chemical processes with the resulting gradual establishment of 'transportative correlations' (Child, and others), temporary or 'scaffold' nerve paths, complicated sequences in the development and myelination of nerve tracts, and the different times at which sense-organs become irritable—are only some of the factors which complicate the actual picture. Here as elsewhere we are forced, in describing, to simplify. And the same is true of the next step in development; which is, as we shall see, that process of neurobiotaxis, reflex-circle, and the conditioned reflex in general, which in the later half of the fœtal life and thereafter comes more and more into prominence, and which is, as I believe, the final principle of development and of learning.

entirely obliterated even in the adult mammalian nervous system. As Sherrington (1906, pp. 145-6) has said, "each receptor stands in connection not with one efferent only but with many—perhaps with all, though as to some of these only through synapses of high resistance." Or Magnus (1924, p. 25) : "In the condition of strychnine poisoning every afferent sensory path is in functional connection with every motor nerve, and the central nervous system presents itself as a diffuse net in which incoming excitations can go to all of the motor centres." Strychnine is a drug which lowers the resistance of synapses to the passage of nervous impulses.

Engrams, or special pathways for nerve impulses, are inscribed in this diffuse network by a lowering of the resistance of some, as compared with other, synaptic junctions. Though the electro-chemistry of this process is imperfectly understood, the fact is sufficiently attested that every passage of a nervous impulse across the junctional tissue between two neurones (the synapse) lowers the resistance of that tissue to the passage of all subsequent nervous impulses.[3] There is every indication that this lowering of resistance is an irreversible process.

Three points call for mention before we go on to consider how the patterns, of the engrams to be inscribed, are determined: which latter is one of our most serious considerations, since it is precisely here that the 'causal processes' in a world of centimetres, grammes, and seconds determine all of the features of that pretendedly different, the 'mental' world; just here that emergent evolution produces a new level, a transformation from the 'mechanical' to the 'psychical.'

The first point is, that if there are no definite sensorimotor paths in the nervous system, there are no reflexes,

[3] *Cf.* M. Verworn, 1919, p. 62: C. J. Herrick, 1924, p. 119. Prof. K. S. Lashley (1924) has raised objections to the view that synaptic resistance is lowered by the passage of nervous impulses. One is led to infer (p. 369) that these strictures, to which I by no means subscribe, are intended to include Dr. Kappers' law of neurobiotaxis.

no instincts, and no ideas. Yet so the embryo, even the human embryo, finds itself. And John Locke's doctrine of the *tabula rasa*[4] rests on solid embryological as well as psychological grounds. The chemical energy, derived from food, which is to be the animal drive, is being stored in sense-organs, nerves, and above all in muscles. But this raw material of the will is so far unorganized. As yet no 'soul' is there.

Second, the spreading of nervous impulses, so conspicuous in the embryo, remains to a great extent through life. Never does an afferent impulse, though it start from a single sensory cell, proceed through the nervous system without irradiating widely and altering the tonus of many groups of muscles. This fact, if fully realized, alters considerably our familiar and hitherto dominant conception of the single unifibrillar reflex arc as the 'physiological unit' of sensori-motor process. It makes necessary, I believe, an extensive modification in the theory of tropisms, and a complete revision of our notions regarding 'brain localization' and 'psychophysical parallelism.' This spreading of nerve impulses toward the motor field seems not to have been fully recognized until it was discovered that muscular, *i.e.* contractile, tissue becomes differentiated in the embryo before either irritable (sensory) or conducting (nervous) tissue (*cf.* Parker, 1923, p. 31). And this neglected priority, phylogenetically and ontogenetically, of movement over stimulation promises far-reaching consequences in both neurology and psychology.

Third, I shall in the succeeding pages consider, so far as the *topography* of the nervous system goes, hardly more than the one fact that it allows any sensory cell to acquire functional connection with any motor cell. This is, of course, very much to oversimplify the picture. And yet this

[4] Locke's happy use of a Latin expression makes us almost forget that Thomas Hobbes, thirty-nine years earlier, wrote as follows: "For there is no conception in a man's mind, which hath not at first, totally, or by parts, been begotten upon the organs of Sense" (*Leviathan*, 1651, Part I., Chap. I.).

one topographical feature is so important that it will probably suffice for the confessedly general purposes of this essay. It is interesting to recall that this very feature is the basis of a diagram of nervous ganglia and nervous systems which Herbert Spencer gave in the second edition of his *Principles of Psychology* (1870, vol I., pp. 527-9). The spread of sensori-motor impulses has hitherto been thought to be due to association establishing new connections between central nerve 'centres,' and so providing more collateral motor outlets for afferent impulses. From our study of random movements, however, we see that afferent impulses spread diffusedly from the very beginning, and that the work of association on the contrary is to *narrow down* the range of motor outlet, leading afferent impulses along more definite paths of specifically lowered resistance. We have now to consider what happens to the original diffusion of impulses and the resulting random movements. All learning, we shall find, is based on random movements.

THE REFLEX-CIRCLE (BOK)

As soon as the embryo manifests movement, although inherited neural engrams are lacking, and the existing nerve tracts have developed only under the influence of functional gradients, Pavlov's law of the conditioned reflex can begin to operate; the diffuse, random activity of the embryo, and not 'heredity,' being the indispensable condition for the formation of synaptic connections, or preferential pathways, in its central nervous system.

The first results of the working of Pavlov's law are what Dr. S. T. Bok (1917) has called 'reflex-circles.' [1] Let us consider *any* muscle *at a moment when* a nervous excitation, seeking some outlet of least resistance, purely fortuitously finds its way into the motor-neurone of this muscle. The muscle contracts,—a random movement. But now something happens which is *not* random. Every muscle has sensory organs embedded within it, its proprioceptors, which are stimulated, probably by mechanical pressure, when the muscle contracts. And so the sensory cells of this muscle (of its tendons also and of the joint involved) are now stimulated and send an excitation along their afferent

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[1] As with the conditioned reflex, William James has also in this case anticipated later 'discoveries.' The principle of the reflex-circle is described on page 582 of the second volume of his *Principles* (1890): and James himself refers to Pierre Janet as having used the principle to explain catalepsy. If I continue to speak of the laws of Pavlov and Bok it is only because these investigators have significantly elaborated points to which James and others gave but passing emphasis.

Dr. Bok finds the principle of the reflex-circle first exemplified in the process by which a motor neurone gains connection with its muscle. Here I either do not follow, or else do not altogether agree with some steps in his argument. But however matters may stand with regard to its applications, Dr. Bok has excellently described an important principle, the 'reflex-circle,' in the article above cited.

nerves to the central nervous system. But this excitation arrives only a second or two after the above-mentioned random impulse has found, or while it is still finding, outlet from the central nervous system into the muscle. Therefore by Pavlov's law (or equally by the law of neurobiotaxis) the incoming excitation will find outlet along the tract just used by the random impulses, that is, will go back to, and will further contract, the very muscle from which it came. Thus the afferent neurones from this muscle will begin to acquire, and after a few repetitions of this process will acquire, a synaptic connection with the motor-nerve which goes out to this same muscle. A reflex-circle is established.

Now in course of time what has happened to this (*any*) muscle, will happen to all the muscles: fortuitous impulses will sooner or later reach them all, and in all of them such a reflex-circle will then be established. The result is that in every muscle a contraction tends to re-enforce and to perpetuate itself: and this is, of course, the 'circular reflex' which Sherrington (1909, p. 109; 1915, p. 191) has described as the basis of 'postural tonus.' Thus the random writhings of the fœtus are turned to excellent use.

But the principle of the reflex-circle goes much further and is, in fact, of capital importance for the learning process at large. Suppose, for instance, that owing to anatomical configurations, the random contractions of a muscle stimulate other sense-organs than those within the muscle itself. Consider a random impulse reaching the flexor muscle of a finger. In the fœtal position the fingers are often closed over the palm of the hand, and the least random flexion of a finger will cause it to press on the palm. Then (what is not random) afferent impulses ('tactile') from the two surfaces in contact (palm and finger) will be sent back to the central nervous system, where by the principle already cited they will find an outlet in the motor paths that were just now excited, that is, those of the flexor muscle of the finger in question. When this has

happened a few times (as it is bound to happen) the reflex-circle will be established; and then a pressure stimulus on either palm or finger will cause the finger to flex and so to close down on the object that caused the pressure. Such is the origin of the 'grasping reflex,' which is so useful through all the later life. This reflex is regularly established before birth.

With Pavlov's law and the external anatomy of a baby in mind the reader can discover for himself the genesis of many of the earliest reflexes,—lip closure, jaw closure, 'extensor-thrust' when the bottom of the foot is touched, knee-flexion when the posterior surface of either lower or upper leg is pressed, adduction of the legs and arms, and many others.[2] The principle is the same whether before or after birth. I will give one more illustration, from the post-natal period.

It is well known that infants which are born deaf do not learn to speak even though their vocal organs be absolutely normal (Preyer, 1889, pp. 42, 98). This is because a prerequisite to the acquisition of speech is the establishment of reflex paths from the ears to the vocal organs, such that a sound received at the ears causes the vocal organs reflexly to reproduce that sound as closely as their anatomical structure permits. These indispensable reflexes are established inevitably if the infant's audition is normal. For, as is well known, its random murmuring, cooing, babbling, and other more strenuous vocalizations are, during certain of its early months, well-nigh incessant. It exercises *at random* its entire articulable gamut. Now each sound as it is produced stimulates the child's own auditory apparatus, if this is intact, and each such auditory excitation finds motor outlet in precisely that set of the vocal organs which has just made that very sound: and which will now make it again (a reflex-circle). Hence the infant's persistent reiteration of any sound which it has made (*cf.* Humphrey, 1921). So, little by little, if the child has normal hearing

[2] For further illustrations see Bok, 1917; also Minkowski, 1922.

it becomes able to repeat also those same articulate sounds which *other persons* utter to it; and is well on the way to speaking.[3] Of course this principle is the basis of all onomatopœia; and indeed Prof. J. M. Baldwin (1895, pp. 130-4) has called it the principle of 'simple imitation.' The reflex-circle in fact gives rise to a general law of iteration: A child will repeat any of its own random acts provided that this action (simultaneously) stimulates, howsoever indirectly, any of its own sense-organs (and also, of course, that no other reflex steps in to interrupt).

The 'reflex-circle' always starts with an aimless, chance innervation of muscle, but ends with a reflex established which is always a response that intelligibly refers to the stimulus, and one that is often distinctly 'purposive.' It seems to me obvious that by its very nature every case of reflex-circle will be a reflex which causes the organism to get *more* of the eliciting stimulus; and I am astonished that Dr. Bok (1917, p. 298) takes the exactly opposite view. "The reflex reaction," he says, "according to the reflex-circle must weaken or strengthen the perception of the reflex-stimulus. In the first case the consequences of that stimulus upon the animal are always diminished by *avoiding* it, and in the second case these consequences are very often diminished by *opposing* the stimulus, in other words the reflex-circle determines that the reflex reaction in the majority of cases diminishes the influence of the reflex-stimulus"; and "thereby each reflex reaction affords a chance to escape the so frequently detrimental influences of the outer world, or to wage war against them." Dr. Bok even believes that another principle must be sought to explain why the organism ever responds so as to receive more of an impinging stimulus (p. 299). Since, however, the afferent half of any reflex-circle is a sensory impulse excited directly or indirectly by the previous action of the efferent (motor)

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[3] Perhaps the partisans of inherited engrams will tell us why mere *deafness* should so abolish the "congenital speech engrams" in children with perfect vocal organs and of normal intelligence.

half, it must regularly, in re-enforcing that action, prolong
and re-enforce itself; it can never 'weaken' the incidence
of the stimulus. I cannot agree with Dr. Bok's contention:
although it is true that the reflex-circle provides a broad
basis of 'outreaching' reflexes on which, later, avoidant and
many other reflexes will be erected. If a reflex which acts
so as to get more of the stimulus happens thereby to thrust
the stimulating object aside, this is an accident of the ob-
ject and the external situation, but not a property of the
reflex. Nor would reflex-circles become established if most
objects yielded readily to the impact of random movements.[4]

By and large, certainly, the reflex-circle principle equips
the organism from an early period of its life with an over-
whelming number of reflexes which go out to meet the stim-
ulus, get more of it, repeat or reproduce the stimulation;
which are in short adient.[5] Were this not the case I should
incline to doubt the great importance of this principle. For
it is observable that the fundamental character of the nor-
mal organism, both in infancy and in adult life, is an out-
reaching, outgoing, inquiring, examining, and grasping one.
And on the other hand, the infant is in fact peculiarly help-
less in face of noxious influences; and it seems to me that

[4] Of course any outreaching (as, say, grasping or kicking) reflex can
later be utilized in disposing of an annoying stimulus. This will happen
specially, I think, in a more developed stage of the intelligence, when
some conception of objects and of their spatial relations has been acquired.

[5] Since there is no satisfactory adjective already in use to characterize
these responses which give the organism *more* of the stimulus, I shall
adopt the very apt term *adient*, which has been kindly suggested by Prof.
H. C. Warren. The immediate effect of an adient response, then, is to give
the organism *more* of the stimulus that elicits the response; and of its
opposite, the avoidance or *abient* response, the immediate effect is to give
the organism *less* of the exciting stimulus. The important distinction be-
tween these two types of response has received due recognition only in
the discussions of tropisms and taxeis. The ideas of reflex-circle and
adient response have been used by Prof. J. M. Baldwin (1895, p. 133),
but like Bain, Spencer, and most psychologists, he obscures the matter
at once by hypotheses involving 'pleasure and pain.' Further, avoidance
responses are too often not distinguished from aggressive reactions, both
being considered 'defensive' and therefore 'biologically equivalent.' It
is amusing to note that *adeona* and *abeona* figured among the petty
Roman 'gods' (Pareto, 1917, p. 86, footnote).

most if not all of its avoidance reactions appear only later.[6]
So characteristically is the infant positively thigmotactic,
cuddlesome, clinging and confiding, the child curious, imi-
tative and venturesome, the youth inquiring, full of initia-
tive and heedlessly aggressive, the adult (now at last more
cautious, but still) forward-looking, forward-pressing, ac-
quisitive, and predatory—that in common speech the very
word 'responsive' is never used for an avoidance response.
Without this underlying positive responsiveness of the or-
ganism, the 'instincts' of imitativeness, curiosity, acquisi-
tiveness and the character of general initiative could never
arise.

Lastly, while Bok's reflex-circle and Pavlov's law of the
conditioned reflex, with their derivative principles, all pre-
suppose purely random movements, they do not involve the

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[6] By avoidance I mean immediate retraction from the stimulus, so that
the stimulation ceases.   I do not mean defence (*Abwehr*) reactions,
which are aggressive and not retractive.   It may be that some avoidance
responses are developed during intrauterine life, but there seems to be
scanty evidence of this in the reports of most observers.   Diffused, non-
specific, or random responses (crying, squealing, etc.) even to overstrong
stimuli are reported more frequently than anything that resembles direct
avoidance.   Yet it must be admitted that there is some evidence of
specific avoidance.   Thus Dr. H. H. Lane (1917, p. 26) reports of three-
day-old rats that, "Pricking of the *hind feet* resulted in a violent attempt
to tuck them under the body."   Prof. G. E. Coghill (1924, p. 41) finds
that the first responses of amblystoma, in the 'early flexure' stage, are
predominantly *away from* the side touched.   Dr. C. P. Richter, who has
made extensive observations on cat, dog, and rabbit fœtuses enucleated
several days before birth, informs me that he has observed unmistakable
avoidance responses in these fœtuses, though such responses are less fre-
quent and perhaps rather less striking than the outgoing responses.   Most
emphatic of all, Prof. Stewart Paton (1926, p. 448), citing work of his
own and of Wintrebert, states that "this 'avoiding' reaction seems to be
common to all embryos and is the first pronounced overt movement to
follow external stimulation": and Prof. Coghill (1926, pp. 51-52) finds
that in general early reflexes are mainly avoidant.

There is doubtless a question here which calls for further study.   It is
not inconceivable that avoidance responses of the type, and in the way,
that will be described in a later section should develop even in the intra-
uterine period.   Meanwhile, I can say that in the young of such mam-
mals, including infants, as I have had an opportunity to observe, definite
avoidance responses appear conspicuously later than definite outgoing
responses.

'trial and error' which are so frequently mentioned in connection with learning. No 'errors' are here made; nor without the fundamental, outgoing character of organisms, as above described, would very many 'trials' ever be made. The cases of 'trial and error' must be later complications. And I see nothing in the principles so far discussed that will immediately and directly account for avoidance reflexes. It is for these, I believe (in disagreement with Bok), that we must seek a new principle.

CIRCULAR REFLEXES AND PROGRESSION: INHIBITION

CIRCULAR reflexes, made by a functional connection in the central nervous system at the synaptic regions between the central termination of the afferent nerve coming from the *proprioceptors* of a muscle, and the central end of the motor nerve which goes to that same muscle, are formed as we have seen, on the reflex-circle principle, before birth in mammals, and for all muscles. Such reflexes are the first step in synaptic learning, and are perhaps as clear a case as we have of a distinct stratum of reflexes upon which other reflexes are erected.

Their first action is to give *tone* to the entire organic musculature. If the two nerves (sensory and motor) and the muscle involved are alive and nourished, a circular reflex is bound to keep itself going: save for outside interference (inhibition) the muscle cannot altogether relax. It is found experimentally that if the afferent nerve of the arc is cut (the muscle 'deafferented') the muscle loses tonus at once (Sherrington, 1909, p. 155). If the motor nerve of such a deafferented muscle is stimulated, the muscle contracts sharply and falls back as promptly to the limp condition; its action is 'dead beat.' Whereas if the afferent nerve is intact, the contraction is sustained longer; the relaxation is more gradual and sometimes marked by a decreasing series of contraction renewals ('after-discharges') ; and at the end the muscle retains a mild contraction tonus. Without these circular reflexes our movements, even the voluntary, would have that *staccato* jumpiness that we see in a child's marionette. Thus, whatever impulses are sent to the muscles from 'higher centres,' the circular reflexes impart a *legato* quality to all movements.

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Under normal conditions the circular or "proprioceptive reflexes tend especially to be tonic in character" (Sherrington, 1909, p. 155) ; that is, moderate in intensity, long sustained and singularly unfatiguable. These properties, so unlike those of the ordinary, phasic muscular contraction, have led to a conjecture that tonus and the tonic contraction involve a nervous current distinctly different from that which produces the phasic contraction.[1] But the simpler conjecture seems still to be the more likely, that tonus and tonic contraction are maintained by a slow rate of nerve pulsations of the usual all-or-none type (Denny-Brown, 1929), a rate at which the process of repair is able to keep up with the expenditure of energy.

One is led to ask, since the circular reflex paths automatically re-enforce their own activity, why they do not keep all muscles at maximal tension until a state of complete chemical exhaustion is reached instead of remaining, as they normally do, at a merely tonic intensity. A similar query arises in the case of postural or plastic tonus, where the same proprioceptive or circular reflexes are believed to maintain the weight of the body or limbs in any position to which they are brought, without either further lifting this weight against the force of gravity or yet allowing it to sag under that force. Simple as these questions seem to be, I do not know that any completely satisfactory answer has as yet been found. Since both plastic tonus and the ordinary tonus of rest are appreciably reduced under rather mild conditions of fatigue, it might be conjectured that trophic factors play an important part in these phenomena. Analogously to many chemical processes, it may that sensory and muscular tissues in the state of maximal nutrition (trophic repair) are hypersensitive to excitation, and that

[1] Presumably this nerve current would resemble the 'more primitive' type which is said to be found in the autonomic, or involuntary, nervous system. This current is thought to be non-pulsating, relatively slow to reach a maximum and to subside, never very intense, perhaps not following the all-or-none law, and practically unfatiguable. As Prof. E. D. Adrian (1918, p. 46) has suggested, this problem is not settled.

this lability falls off rapidly under stimulation (as in most *sensory* tissues seems to be actually the case). If now the self-re-enforcement of proprioceptive reflexes (automatically mounting tonus) is a function which increases more slowly than the chemical lability (sensitivity) of the sensory (or motor) tissues involved decreases, then a point of equilibrium would be reached at which the actual tonus (postural or resting) would be a function of the vascular process of tissue repair.

The proprioceptive reflexes, which maintain the tonus of rest and postural tonus, are only one type among the many reflexes which, developed according to the reflex-circle principle, tend to maintain themselves without mounting to an immoderate degree of tension. In all these cases, the factor of sensory adaptation which we have just considered probably plays its part. But another factor, inhibition or more specially the mutual inhibition of reflexes which are in their action opposed, soon becomes prominent: so prominent indeed that inhibition, rather than sensory adaptation, is generally taken to be the main factor that moderates all self-re-enforcing reflexes. Thus James (1890, vol. II., p. 583) has said, "We should all be cataleptics and never stop a muscular contraction once begun, were it not that other processes [motor processes are meant] simultaneously going on inhibit the contraction. Inhibition is therefore not an occasional accident; it is an essential and unremitting element of our cerebral life."

On a topic so intricate and so much disputed as is this of inhibition one speaks with some hesitation, but on the whole it seems to me that James' conception of inhibition as "an essential and unremitting element of our cerebral life," a conception with which probably most physiologists would agree, rather tends to exaggerate the rôle and to obscure the actual significance of this important function. It seems to me, rather, that v. Uexküll (1929, p. 762) has found the key to the matter when he declares that animal organisms "are constructed on the principle of the antagonism of

muscles," and implies that inhibition is found only in con-
nection with such antagonism.   Be this as it may,[2] it cannot
be doubted that an inhibitory influence arising from the
simultaneous excitation of antagonistic muscles often im-
pedes or cuts short the action of self-re-enforcing reflexes.
Almost all animal movements depend, as v. Uexküll sug-
gests, on the to-and-fro play of limbs, and two muscles
whose several actions are to flex and to extend any joint
are antagonistic muscles.   The earliest and simplest case
in which an interference between two such muscles occurs,
is the simple continuous to-and-fro motion of a limb (joint).
Such motions are the basis of walking and almost all move-
ments of progression: and if all extraneous nerve impulses
are excluded, they are maintained in steady rhythmical
alternation purely by the proprioceptive circular reflexes
of the muscles and joints involved.   We have first to con-
sider the anatomical pathways (canalizations) here in-
volved, and secondly the dynamic factors (nerve currents).
Practically all the muscles of the body are arranged in an-
tagonistic pairs such that if one member of a pair, on con-
traction, flexes a joint, then contraction of the other mem-
ber will extend the same joint.   From this anatomical ar-
rangement it follows that the contraction of a muscle will
to some extent mechanically stretch the opposite, antago-
nistic muscle.   Now v. Uexküll (1904; 1909) has shown that
such passive stretching of a muscle excites it to contraction.
Some of its proprioceptor organs are mechanically stimu-
lated by the stretching (Liddell and Sherrington, 1924,
1925; Magnus, 1924, p. 43) and these then activate some
of its circular reflex paths and contract this, the antagonis-
tic muscle.   Thus in the absence of reflex impulses from
independent sources, a sort of balance of tone is brought
about between the muscles of every antagonistic pair.   And
yet both muscles might, and apparently would, be contracted

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[2] We shall consider the phenomenon of inhibition in greater detail in
Chapters XVIII. and XIX.

to the utmost degree: and the joint controlled by them would then become rigid.

The observed fact is that this balance is a very labile equilibrium in which neither muscle, in the absence of outside impulses, attains any extreme degree of tension. If now we consider these two antagonistic muscles M and M', their respective motor nerves m and m', their (proprioceptive) sensory nerves s and s', and the joint which these two muscles control, at the early period when their proprioceptive reflex-circles (*i.e.* their circular reflexes) are being organized (canalized) from random movements, in the way that has been already described, we find this situation: as soon as either circular reflex, say m M s, begins to function enough to move the joint and so to stretch the opposed muscle M', afferent impulses from M' will begin to reach the central nervous system on nerve s'. (In general, of course, spinal nerves are here involved, nerves m and m' leaving and s and s' entering the cord at the same, or at closely adjacent, spinal levels.) Now usually the circle m' M' s' will be as far developed (canalized) as the circle m M s, so that the impulses coming in on s' will find a *motor* outlet through m' to muscle M'. *But,* since in this case m M s is in action, the afferent impulses on s' will, on the reflex-circle principle that is now familiar to us, to some extent at any rate find a *collateral* motor outlet through the now active motor neurone m to muscle M. In other words, the afferent nerve from the proprioceptor organs of either muscle, while discharging into the motor nerve of this same muscle will also gain a collateral outlet into the motor nerve of the antagonistic muscle. It seems improbable that the two discharge pathways s' m' M' and collateral s' m M (or s m M and collateral s m' M') will ever come to have the same degree of lowered resistance: the circular path s' m' M' (or s m M) will keep well ahead of the collateral path s' m M (or s m' M') in permeability if only because the stretching of an antagonist muscle is conditional on, and secondary to, the (earlier and intenser) contraction of an

agonist, and it is only stretching of the antagonist that tends to develop the collateral pathway.

I should place no great reliance on a conclusion that is so schematic and deductive were it not that several eminent physiologists have reached a closely analogous conclusion, arguing deductively to be sure but from quite different data, from the phenomena namely of reciprocal innervation and movements of progression (Descartes, *cf.* Bayliss, 1915, pp. 493-496: Sherrington, 1900, p. 842; 1905 [a], p. 286; 1906, p. 201: Fröhlich, 1909, p. 98: Lucas and Adrian, 1917, p. 94: Forbes, 1921, p. 304; 1922, pp. 397, 411: Brücke, 1922, pp. 52-55: Cooper and Adrian, 1924, p. 77: Fulton, 1926, pp. 339-341). In short, it seems to me that Bok's principle of reflex-circle yields a significant hint as to the origin and the structure of what Sherrington has called 'motor half-centres,' that is, the pairs of motor centres in the spinal cord which innervate pairs of antagonistic muscles.

The anatomical arrangement of half-centres appears to be simply that the sensory nerve from each of two antagonistic muscles is connected by a synapse of low resistance to the motor nerve which returns to (and innervates) its own muscle, and by a synapse of appreciably higher resistance to the motor nerve of the antagonistic muscle. In the dynamic functioning of half-centres it is observed that as each of the two antagonistic muscles contracts in turn, and the limb alternately flexes and extends, each muscle after reaching its maximal contraction relaxes and becomes (for at least a moment) toneless while the other muscle moves the limb through the opposite phase of the reciprocating motion. The relaxed, toneless muscle is for the moment *inhibited*. This is probably ontogenetically the earliest case of inhibition. I believe that motor half-centres are the typical location and that reciprocal innervation ('algebraic summation') is the typical function of all reflex inhibition. We must inquire into the nature of this inhibition before we can understand the dynamics of the reciprocating mo-

tion which is automatically maintained by two antagonistic muscles and their motor half-centres.

Notwithstanding that the phenomenon of inhibition is still much debated among physiologists (*cf.* Beritoff, 1922; 1924 [a]: Forbes, 1922; Brücke, 1922; Howell, 1925; Sherrington, 1925), yet there is one view, or at least one point, which seems to me almost if not quite settled by the weight of evidence: this is that inhibition ('reflex inhibition') is a phenomenon of *interference;* analogous to the interferences that are found in physical phenomena. This view was suggested by H. E. Hering (1902, pp. 516, 529-530), was advocated by Fröhlich and Verworn (Fröhlich, 1908 [a] and [b]: Verworn, 1913, pp. 220-221; 1914, p. 194), and has been, it seems to me, amply confirmed by the nerve-muscle investigations of the last fifteen years. The definitely proven fact is that if nerve impulses crowd on one another's heels in too rapid succession, all are extinguished. Adrian (1924, pp. 411-415) conveniently calls this 'overcrowding.' It can be demonstrated on a single nerve fibre by stimulating with small induction shocks at such a rate (between 500 and 1000 per sec.)[3] that each new excitation comes before the nerve has recovered from the relative refractory phase that was set up by the preceding excitation (Cooper and Adrian, 1924, p. 77). It then cannot excite a nerve impulse (negative wave) but it does renew the refractory phase. Thus a

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[3] These figures are taken from Cooper and Adrian (1924, p. 77): "the smallest interval between successive impulses set up in a nerve by direct stimulation lies between .001 and .002 second." If the refractory period of nerve is about .001 sec., running up to .00225 sec. on some nerves, and under fatigue is still longer (Field and Brücke, 1926), the frequency for overcrowding could never be above, and would tend to be below, a value ranging between 444 and 1000 per second. But *before* pulsations on a nerve fibre are totally extinguished they are so *reduced in intensity*, by crowding, as to be unable to pass a region of decrement (synapse, myoneural junction). Adrian (1924, p. 414) states that if the pulsations are not at least .004 sec. apart, they will be extinguished at the next region of decrement: this corresponds to a frequency of 250 pulsations per second. In view of the great difficulties that are encountered in measuring nerve pulsations, all figures are doubtless subject, at the present time, to some correction.

prolonged state of refractory phase can be maintained on a nerve by a rapid series of excitations of which only the first one will travel along the nerve as a normal impulse (negative wave) : and so overcrowding can occur on a single nerve fibre.

Conditions are somewhat less favourable for impulse conduction at the synaptic regions and at the (myoneural) junctions between motor nerves and their muscles: so that, for instance, a rapid series of impulses that are not too frequent for the nerve to transmit, will be so impeded at synapses and at the myoneural junction (Cooper and Adrian, 1924, p. 77) that they reach the muscle, if at all, only as a relatively slow series of impulses, and the muscle will contract only very moderately.   At synapses, moreover, nerve impulses are not merely impeded, *i.e.* subject to decrement, they are also accumulated, in a form somewhat analogous to an electric 'charge' (Sherrington, 1921), and this charge it is which serves to stimulate the dendrites of the next-lying neurones and so to transmit the nerve impulses onward.   Since at synaptic regions two or more neurones can discharge their impulses simultaneously, these impulses summated in the synaptic charge may produce a charge so intense as to stimulate the dendrites of the succeeding neurones not merely at a very high frequency, but even at a frequency which will inhibit those neurones by overcrowding.[4]   Thus a nerve current arriving at one of these regions of decrement, if feeble, may be unable to establish a synaptic charge of sufficient intensity to stimulate the next-lying dendrites; if the current is intense, it may make its way across without appreciable reduction; and if several intense currents converge (simultaneously) they

[4] This sketch of the dynamic process at synapses would be disputed *in toto* by not a few physiologists: yet I believe it to be the picture that is indicated by nearly all the relevant investigations which have accumulated in England, Germany and America since Sherrington first called attention to the special properties of the synapse (*cf.* Sherrington, 1905 b pp. 814-816; 1906, p. 14).   This picture will be discussed in greater detail, and with a fuller documentation, in Chapters XVIII. and XIX.

may summate in a synaptic charge so intense as to produce on the onward-lying dendrites only inhibition by overcrowding. Synaptic regions, including of course all motor half-centres, and perhaps *myoneural junctions*, are unquestionably the significant points in the central nervous system so far as the phenomena of reflex behaviour are in question.

We are now in a position to understand the reciprocating mechanism in motor half-centres. We were considering the case of a joint with its pair of antagonistic muscles and their (proprioceptive) circular reflex connections, and we saw how as a consequence of the stretch reflex (and the reflex-circle principle) the proprioceptive afferent *from* each muscle gains collateral access to the motor nerve that leads to the opposed muscle. Thus when one of these muscles contracts, and then stretches its antagonist, its own contraction (subject, if intense enough, as above described, to *an eventual self-inhibition at its motor half-centre*) will presently be inhibited (still sooner than otherwise) by impulses coming in along the collateral path from the proprioceptive afferent of the antagonistic (stretched) muscle. But when the first muscle is thus inhibited (relaxed) and *the circular reflex of its antagonist has been set in action* by stretching, this second muscle now unopposed will continue to contract. Then in precisely the same way, the contraction of this second muscle will eventually be inhibited by overcrowding and the first muscle will again be set to contracting. And so on. Thus the joint in question will be alternately flexed and extended automatically, until such time as fatigue of the synapses and other tissues or until impulses from extraneous *sources step in to interfere.* And such rhythmical movement of joints is the basic element in all movements of progression.

Thus Kappers' law of neurobiotaxis, or, if you prefer, Pavlov's law of reflex 'conditioning,' as applied by Bok in *his principle of reflex-circle, throws* some light on the origin and nature of 'reciprocal innervation' (*i.e.* 'antagonistic innervation') and movements of progression. The account

above given helps to confirm, perhaps to amplify, recent discussions in this field (Forbes, 1922, pp. 396-9; Brücke, 1922, p. 52; Cooper and Adrian, 1924, pp. 77-8).

We should consider here, how far these proprioceptive circular reflexes and half-centre pairs of such reflexes that lie so as to actuate opposed phases of joint movement depend for their functioning, and how far they do not depend, on impulses from extraneous nerve pathways. As Exner (1894) remarked, such an automatic mechanism as that of walking requires to be *started* from outside, but once started will maintain itself in action until interfered with from outside. A nerve current from some other part of the central nervous system must start the reciprocating action by imparting a first decisive motion to the limb. It may be that the sense-organs on the sliding surfaces of the joint concerned are stimulated by this first swing of the limb more intensely than they are stimulated in any postural tonus, and that it is this stimulation which is necessary to change the postural tonus of a joint into reciprocating motion of that same joint. The half-centre reciprocating mechanisms are at any rate self-actuating when once started (so long as the blood supplies them with sufficient fuel), and they are to a considerable extent organized, in mammals, before birth. T. Graham Brown (1913; 1916), for example, has observed rhythmical progression movements in the hind limbs of very young cat embryos. And similar observations have been reported by others.

The picture must not be left over-simplified, however. Even the simple case of reciprocal innervation and inhibition at one joint, presents complications that are by no means fully understood. There are, for instance, several classes of proprioceptive organs that are stimulated by the movement of a limb, and therefore several classes of proprioceptive circular reflexes are set in function. The proprioceptors stimulated in a muscle by its own active contraction are probably not those which are stimulated when the same muscle is passively stretched, nor yet (as Sher-

rington suggests, 1909, p. 153) those stimulated when it is 'passively relaxed,' *i.e.* its two ends approximated without active contraction. Certainly in passive and active stretching the tendon and the contractile part of a muscle seem to be differently susceptible, for proprioceptive impulses from tendons are not demonstrable under certain conditions where we might expect to find them (Asayama, 1916; Liddell and Sherrington, 1924, 1925; Hansen and Hoffmann, 1920). Sense-organs histologically demonstrable in the sliding surfaces of joints, and believed to be proprioceptors, are still as to their exact function rather obscure. The possibility that fatigue may reduce the conductivity of synaptic regions, the possible contribution of fatigue organs (chemical receptors) in the contractile part of muscles, the disputed problem of 'lock' mechanism in muscles (Bayliss, 1915; Denny-Brown, 1929, p. 294), and the part played by receptor organs (notably in the soles of the feet) that in walking are differentially stimulated by pressure (Bremer, 1929)—are further complications that must be taken into account.

With these cautions in mind we can proceed a little further in our consideration of movements of progression. The strictly spinal animal (cat, dog) can "perform movements with a high degree of perfection" such as the scratch reflex, extensor thrust, alternating movements of progression, etc. (Magnus, 1925, p. 341) ; these are therefore spinal neuro-muscular mechanisms, independent of the cerebrum and mid-brain and of the sense-organs (notably the ocular and the vestibular) that are connected with the mid-brain. The extensor thrust, an important component of progression movements, is a reflex 'co-ordination' of the extensor muscles of fore- or hind-limb: and it is a simple product of 'functional construction' (reflex-circle). In the developmental stage when early random movements are building reflex-circles (as previously described) a random contraction of any muscle that *extends* any joint of a limb is likely to cause the cushions of the paw to press against

surrounding tissues. It follows that eventually a pressure on the cushions of the paw will produce a reflex contraction of any, that is to say of *all* the *extensor* muscles of the limb. So simple a 'co-ordination' as this is organized almost *pari passu* with the several reflex-circles themselves. (The grasping reflex is an exactly similar co-ordination of the *flexor* muscles of the fingers.) Thus when a 'spinal' quadruped is placed squarely on the ground the weight of the animal produces pressure stimulation where the four paws touch the ground, and each of the four legs exerts its extensor thrust against gravitation. Thus the animal *stands*. Such an animal, however, lacks the 'righting reflexes,' and if it once loses its balance cannot right itself (Magnus, *loc. cit.*). It is not without interest, here, that Sherrington has distinguished the 'anti-gravity muscles' as constituting in some respects a distinct functional class (*cf.* Richter and Bartemeier, 1926).

In view of the variety of gaits (the trot and the gallop, for instance) that many quadrupeds can assume, it is obvious that there is no one, exclusive way in which the walking co-ordinations of quadrupeds are developed. Functional construction offers several possibilities. Let us consider first the alternating progression movements of the two hind- (or fore-) limbs: the type found in the walk of two-legged animals and in the trot of quadrupeds. If in the womb both feet are to some extent pressing against, and pressed against by, the wall of the womb, a random extensor thrust of one limb will diminish the pressure of the womb on the contralateral foot. This will tend to weaken the contralateral extensor tonus, by diminishing one of the sources (plantar pressure) that feed it (the other source being proprioceptive). Thus, by the principles so often adduced above, flexion of one hind-limb will slowly acquire a reflex co-ordination with extension of the other, and extension of one with flexion of the other; the sense-organs involved being the proprioceptors of all the muscles in question and the plantar pressure organs of both feet. And

as the reciprocating movements of progression (alternating extension and flexion) become organized in the two hind- (or fore-) limbs, the rhythms of the right and left sides will be reflexly co-ordinated, with an *opposition of phase.* It is not surprising that co-ordinated progression movements have been observed in the foetal cat.

In the situation just described, extension of either hind-limb will cause pressure of the animal's hind, dorsal surface against the wall of the womb. This establishes a reflex-circle such that pressure on the hind, dorsal surface causes reflex extension of both hind-limbs: similarly, *mutatis mutandis,* of the fore-limbs. Thus quadrupeds, of several species at least, if placed flat on their backs so that the entire dorsal surface is under pressure, exhibit marked extensor tonus in all four limbs. This reflex is well-marked in such dogs as I have observed, and collies frequently sleep, for short periods, in that position: it can be easily demonstrated in cats, though here it is sometimes masked by other reflexes: and I believe that it is observable in horses.

Now this developing co-ordination of the two hind- (or fore-) limbs, extension with crossed flexion succeeded by flexion with crossed extension ('trampling'), is assisted after birth by the weight of the body as the animal stands. If either limb relaxes its extension, the contralateral limb receives more of the weight of the hinder half of the body, and the increased plantar pressure heightens its extensor tonus. But will extension and flexion ensure progression?

If we take a young kitten, perhaps seven to ten days after birth, and place it carefully and squarely on its four legs, it usually stands almost still for a few seconds. The simple extensor tonus of the four limbs seems to be prepotent both over phasic flexion and extension of the single limb and over the bilateral co-ordination of flexion and contralateral extension. As a first sign of these breaking through, the extension of one of the four limbs is seen to relax, and the paw is presently lifted slightly from the floor. There is a slight contralateral extension. At this juncture I have

never observed trampling movements to set in *without* progression. (An adult cat or older kitten, if very happy, may trample with the fore-paws or even with fore- and hind-paws, without progression.) The reason probably is that full extension of the hip in bipeds, and in quadrupeds full extension of the hip and tarsal joints, thrust the leg slightly backwards, and therefore if the animal is standing thrust the body forward. This forward thrust of the body need be only very slight since walking is a 'regulated falling' and the weight of the body will cause it to fall forward if the feet of the (two) supporting limbs are even slightly to the rear of their points of articulation with the body. (I must leave the question open, whether full extension of the fore-limbs similarly involves a rearward thrust of those limbs or whether the forward component of progression is contributed at first by the hind-limbs alone, and is only secondarily learned by the fore-limbs.) And so the kitten creeps slowly and somewhat haltingly forwards. The co-ordination is rapidly perfected. So far as I am aware, a purely spinal reflex co-ordination for walking or running *backwards* is unusual among vertebrates; and this was to be expected if the extensor thrust necessarily includes a backward thrust of the limb extended.

We could go on, along exactly the same lines, to consider the further correlation that is effected between the hind-limbs and the fore-limbs; the mechanical factors being here the weight of the body and the practically rigid connection between the hips and the shoulders. But we are reaching the point where quantitative relations become paramount: nor do I wish to pursue deduction to the point where it might fade into mere conjecture. I have presented the above considerations mainly in order to illustrate further the principle of reflex-circle and the principle of functional construction. The two together suggest a way of looking at, and a way of looking for, the phenomena of spinal reflexes. Of the investigations with which I am familiar, the recent work of R. Magnus and A. de Kleijn comes perhaps

nearest to exemplifying this method. The diametrically
opposed method consists in sectioning or otherwise muti-
lating some part, selected at random but most meticulously
measured and quantified, of the cerebrum, mid-brain, cere-
bellum or cord, and thereafter studying "under the most
rigorously exact conditions" whatever may be left of reflex
conduction. It is the standard academic method and can be
practised by any one who enjoys mutilating animals and
whose intelligence suggests, as a scientific method, arbi-
trarily to create a functional chaos in order to measure
minutely the results. This method has yielded many myths,
as for instance 'cerebral localization,' and has assisted us to
our present complete ignorance of the physiological func-
tions of both the cerebrum (Goldstein, 1923, p. 38; 1927)
and the cerebellum (Magnus, 1925, p. 352). There has
been, of course, much intelligent study of reflex conduction
after operative intervention; but there has been much of
another sort. And far too generally experiments have been
planned and the results interpreted on the implicit assump-
tion of distinct and separate 'reflex arcs': whereas in fact
every nervous system, whether 'tubular' or synaptic, is
merely a modified 'primitive nerve net.' [5]

As to co-ordinated movements of progression and any
other at all co-ordinated reflexes, I would remind the reader
once more of the probably very great functional importance
of the stretch reflexes. Von Uexküll (1904) has himself
suggested that they are the basic source of animal rhythms.
Further, as Magnus (1924, p. 49) has said, "every distinct
body posture corresponds to a distinct distribution of reflex
thresholds in the central nervous system," and this latter
largely controls the fate (augmentation, diminution, or in-

[5] The theory of tropisms by its tacit assumption that every 'reflex arc'
is isolated from every other, remains a somewhat trivial contribution to
the understanding of animal behaviour. Such an assumption is the more
remarkable if we reflect that most biologists have at least heard of the
'primitive nerve net.' Of those investigators of tropisms whom I have
read, Professors H. S. Jennings and P. Hachet-Souplet (1912, pp. 52-74)
seem to realize the falsity of this assumption.

hibition) of all reflex impulses that are set up by newly arriving external (or internal) stimuli. In every distinct body posture the muscles that are *stretched* have, since their proprioceptors are more active, a lowered reflex threshold; that is, the stretched muscles are the most ready to contract. This *Dehnungsgesetz* of v. Uexküll has been admirably elucidated by Magnus in his *Körperstellung* (1924, pp. 26-30, *et seq.*).

Any immobile body posture, including the state of 'complete repose,' is physiologically a dynamic equilibrium; where possibly the energy expended is limited by the rate of repair. All movements are superimposed on this equilibrium, and disturb it. They are produced by impulses from many sensory (afferent) sources, which both diverge and converge as they traverse the synaptic regions of the central nervous system, and which finally converge on motor half-centres and the final common paths, *i.e.* motor neurones (Magnus, 1924, pp. 43, 49, *et passim*). At each pair of half-centres there appears to be a sort of 'algebraic summation' of the arriving motor impulses, and a movement of the member which it governs, in one direction or the other (or on occasion reciprocating motion, or inhibition, etc.), follows.

Two further points may be mentioned in passing. One often meets the statement that the 'higher' spinal, and especially the cerebral, levels exercise an inhibiting influence over the 'lower' neural levels. This, by some philosophers, has been overemphasized in order to make it appear that some 'higher,' immaterial, subjective or spiritual influence holds in leash the 'lower' instinctive tendencies. This is such nonsense as philosophers delight in. A neural level may be called 'higher' if it is higher from the ground in an animal that stands erect (as the ape or man), or it may with some show of reason be called 'higher' if it is the central terminus of afferent fibres from the *distance-receptors*. Otherwise the term 'higher' is mere deceptive rhetoric. The anatomically higher levels do sometimes inhibit the

anatomically lower levels; but just as often and as characteristically they augment the lower, or are themselves inhibited by the lower (Goldstein, 1925, pp. 370-1; 1926, p. 107). Meanwhile the lower levels are busy augmenting and inhibiting one another. On the whole, as Sherrington (1909, p. 155) has said, "proprioceptive reflexes [the 'lower'] normally fuse with other reflexes as adjuvant to them." (See also Sherrington, 1906, p. 139.)

The second point:—Clinical literature abounds in statements that this or that early reflex 'disappears' at about so many weeks after birth; and such language is very misleading, for no reflex ever really disappears. Many early reflexes can at first be elicited immediately on applying the appropriate stimulus: whereas later, when more reflexes have been acquired, the interplay between the reflexes becomes more complicated, and some are liable at any time to be non-elicitable, being momentarily inhibited or masked by the antagonistic activity of others. It is only in this way that a reflex 'disappears.' [6] Further, this growing complexity of interaction, making prediction less easy and making reflex action *seem* more capricious, has lent a specious plausibility to the favourite philosophical text that the mental life is not subject to the "laws of mechanical causation."

In fine, we have seen that circular reflexes, the earliest product of the reflex-circle principle, constitute the broad basis on which all further neuromuscular action is erected. The circular reflexes of the hip, shoulder and limb muscles commence very early to gain collateral connections with one another (both homolateral and contralateral) and so become organized into mechanisms for the reciprocal innervation of antagonists, alternate flexion and extension of single limbs, correlated progression movements of the two

[6] I do not see how clinicians can speak so loosely of the 'disappearance' of early reflexes when they constantly use the knee-jerk and several others as a test of normal reflex conduction, even in aged patients. The grasping reflex and all the others that are normally present even at birth continue to be used through practically every moment of an individual's waking life.

fore- and the two hind-limbs, and for the correlations be-
tween the fore- and hind-limbs. All these progression re-
flexes are purely automatic (*cf. e.g.* Beritoff, 1923, p. 268),
and of the central nervous system they involve only the
spinal cord. A dog or a cat deprived of both cerebrum and
mid-brain is still capable of alternating progression move-
ments (Magnus, 1925, p. 341). To set this mechanism in
motion, the voluntary innervation, as Preisendörfer (1920)
has said, gives only a general outline, so to say, of the re-
quired movement: the finer adjustment is reflexly contrib-
uted by the proprioceptive apparatus. The 'voluntary in-
nervation,' as Exner and Mach long since observed, merely
starts or stops the reflex mechanism of walking.

CHAPTER IX

EQUILIBRATION AND POSTURAL TONUS

ANOTHER extensive group of reflex impulses consists of those which are initiated in those sense-organs that are stimulated, directly or indirectly, by the pull of gravitation. This is the system which, together with the underlying proprioceptive reflexes, gives to the organism its most pervasive and steadying postural tonus.[1]

In the words of Magnus (1925, p. 340): "Nervous impulses, which can influence posture, arise: (1) From the labyrinths, a double sense-organ: the otoliths reacting to changes in position, the ampullæ of the semicircular canals to accelerations; (2) from the proprioceptive sense-organs in muscles, joints and tendons; (3) from exteroceptive nerve endings of the body surface, chiefly from the pressure sense-organs, which are stimulated, if the body touches the ground; (4) from teleceptors, reacting to distance stimuli, such as the eye, the ear, the nose."

Receptors of Magnus's second class, the proprioceptive sense-organs, are as we have seen stimulated, and thus circular reflexes are started, whenever muscles, by impulses from whatsoever sensory sources, are contracted. The proprioceptors are also active in maintaining normal muscle tone and, on occasion, in executing movements of progression. Now it is to be noted that the pull of gravitation must exert an influence on any reflex that involves *lifting* (the

[1] The most valuable single work on this action system is R. Magnus's *Körperstellung* (1924). A popular summary of the subject is the same author's Croonian Lecture of 1925. C. L. Evans (1925) has reviewed the subject concisely. Two other summaries, more technical and in German, will be found in the first 29 pages of Rademaker's work on the Red Nucleus (1926), and the first 67 pages of the monograph by H. Hoff and P. Schilder (1927).

body, a limb, or an extremity), which it does not exert on other reflexes. The energy required to lift a weight is of course greater than that required to move the same weight horizontally, and in the lifting, a stronger innervation is doubtless required. The muscles used in lifting are both contracted more forcibly and stretched more intensely; and the proprioceptors that are stimulated by contraction and by stretch will contribute more towards effecting and maintaining the lift than such proprioceptors will contribute towards a horizontal movement. The phenomena of plastic postural tonus seem to indicate that this proprioceptive increment is sufficient to maintain the lifted member in position even after the original innervation has ceased (*cf.* Sherrington, 1909, p. 156). This postural reflex mechanism is a further instance of adient reflex-circle since the reflex tonus perpetuates the stimulus (the downward pull of the lifted member) which maintains the reflex tonus. In this way the organism exhibits a sort of alacrity in any movement which opposes the pull of gravitation; and I am tempted to believe that this accounts, in part at least, for the tendency of young and well-nourished creatures to leap, bound, hop, skip and dance.[2]

On the other hand, reflexes that lower the trunk or limbs miss this proprioceptive re-enforcement. But they are, of course, eased by the direct pull of gravitation on the member lowered; which is physiologically an utterly different form of co-operation. Possibly the slow lowering of a limb requires nothing more than the gradual inhibition of the proprioceptive tonus that has been holding the limb up. I am unacquainted with any investigation into the reflex innervation of lowering movements. In movements that are chiefly horizontal, as often with the human arm, it seems that the mere weight of the limb is automatically counterbalanced by the proprioceptive plastic tonus. In any case,

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[2] Alexander Bain (1864, pp. 256-7) has mentioned the 'craving' of well-nourished muscles for exercise, and classed it with hunger, thirst, etc., as an appetite.

and this is the main point, the vertical component of every movement of non-aquatic vertebrates is qualified, if upwards in one way, if downwards in a different way, by gravitation: so that to this extent the organism is always oriented towards the centre of the earth, a sort of vertical geotropism.

Magnus's third class, external sense-organs that are stimulated by pressure as an animal stands or lies on the ground, is a source of reflexes which are once more, so far as I can learn, purely and obviously *adient;*—further cases of Bok's reflex-circle. Thus, if a quadruped is standing with equal weight on all four legs (a posture which is itself equalized adience of the four limbs) and one lifts one of the hind-legs, then this leg *once it is off the ground* immediately flexes (and presses but little on one's hand), while the other hind-leg actively *takes* the full weight of the hind quarters by an intenser reflex contraction of its extensor muscles. A momentary interruption of the balanced adience of the two legs allows the latent adience of the undisturbed leg to assert itself more markedly. Similarly, if one lifts the fore quarters well up in the air, and so throws the main weight of the animal on the hind-limbs, these will then stiffen in their extensor muscles. The animal 'braces itself' against gravitation: adience to pressure stimulation on the soles of the feet being necessarily a resistance to gravity (or abience from the centre of the earth). This extensor reaction of the hind-limbs is sometimes masked by a very different reflex, possibly from the otoliths. Again, in lifting one hind-leg alone, as before, after the first flexion of the lifted leg and the extensor thrust of its contralateral, the lifted leg will be felt to push gently back adiently toward the hand that holds it. In the end somewhat more or less than the weight of the lifted limb will usually rest on the hand, and the remaining weight of the hind quarters on the extended contralateral limb. (Of course these reflexes cannot be observed if manipulation of the animal excites any sort of fear reactions.) When an animal starts to walk or run, the limb

which is least supporting the weight of the body, is least adiently engaged, will take the first step forward (*cf*. Magnus, 1925, p. 345). Or again, if the animal is standing equally on its four feet, any moderate contact on either flank will elicit a return pressure (adience) from that flank; and if the animal's general physiological condition is quiescent and relaxed it will lie down on the side stroked, continuing its adience to the hand by adience to the surface on which it now lies. (It is worth noting that much of the meaning of the word 'caress' lies in the adient responsiveness which a light tactual excitation evokes.) It is clear that reflex standing and reflex lying are both cases of automatic adience: the location of the impinging stimuli, and their relative intensities and extents, determine which reflex group will be set in action.

Reflex standing and lying, however, are only two minor aspects of equilibration, a phenomenon which depends primarily on the sense-organs which Magnus gives as his first group. These are the labyrinth and semicircular canals of the internal ear. The former consists of two sense-organs, the saccule and utricle, each of which is provided with an 'otolith' that either rests upon, or (in other positions of the head) depends from, a sensitive surface. The *weight* of the otolith is the stimulating agency, and its pressure or pull excites the sensitive surface in a slightly different way for every different *position* of the head in space. It is not a mere metaphor to call these organs distance-receptors, since they are stimulated (though indirectly) by the centre of mass of the earth. The three semicircular canals are stimulated by any *motion* of the head, whether of rotation or of translation.[3] And the agency of stimulation is a liquid which by its *inertia* presses against a bundle of hair-like, sensitive fibres projecting from the wall of each canal.

Further than this, the physiology of these five organs

[3] *Cf*. A. de Kleijn and R. Magnus (1921 b and c). Previous writers have held, and not implausibly, that motion of translation stimulates not the canals but the saccule and utricle.

(in each ear) is but imperfectly understood. They are very minute and very inaccessible, and it is not even known more *precisely* how they are stimulated. The recent investigations of de Kleijn and Magnus have led the latter even to suggest (1924, pp. 527-534) that possibly the saccule and utricle are autonomous sources of steady afferent impulses, and that the effect of gravitation (for this or that position of the head) is to diminish, as in damping a vibrator (in these or those groups of nerve fibrils), the otherwise steady nervous flow. This conception suggests the early view of J. R. Ewald, but the idea that these sense-organs independently (and incessantly!) generate nerve impulses offers difficulties. Most, and so far as I recall all, other authorities have believed that the pull of the pendant otoliths, in these organs, *initiates* the afferent nervous impulses, and that the varying directions of pull (for varying positions of the head) sufficiently vary the afferent pattern. De Kleijn and Magnus (1921 [a]) find indeed that the otoliths produce most effect when pendent from, and least effect when lying upon, the sensitive maculæ acusticæ. Furthermore, the supposed stimulation of the semicircular canals by inertia of the contained liquid has its puzzling aspects. In view of these uncertainties it is not remarkable that the way in which the reflex paths innervated by these sense-organs, are first connected in the central nervous system cannot be traced so readily as in the previously considered cases of reflex-circle.

As to the reflexes themselves, according to de Kleijn and Magnus (1921 [a]), the otolith of the saccule, on each side, produces tonic reflexes that keep *the head vertical, i.e.* that compensate for any movement of the body which might bring the head away from the vertical position. These reflexes may involve muscles of neck, trunk, and limbs. The same otoliths produce analogous compensatory movements of the eyeballs, tending to keep the lines of regard fixed even though the head be raised, lowered or tilted. None of these succeed always in being complete compensations (Angier,

1904). According to the same authors, the otolith of each utricle produces tonic extension of the limbs. Analogous compensatory reflexes are initiated from the semicircular canals when these are stimulated by motion: or more exactly, by positive or negative acceleration. There is, further, a series of 'righting reflexes' starting from the same sense-organs, acting on the body and limb muscles, and tending (if other sources of innervation do not antagonize this) to bring the animal to the normal, symmetrical, standing posture.

There is also another important series of reflexes, similar in type to the foregoing though sometimes antagonistic in sense, arising from the proprioceptors of the neck muscles; so that any movement of the head on the shoulders changes the distribution of tonus throughout the body and limbs, and may change the whole posture. These reflexes are of both the compensatory and the 'righting' types. There is some indication that these neck reflexes originate in part if not wholly through conditionings (in the Pavlovian sense) to other reflexes (cf. Beritoff, 1916). And there can be little doubt that a wide range of interconditionings (between very early, I do not mean 'innate,' postural reflexes and a great variety of simultaneously stimulated sense-organs), as well as a wide range of mutual antagonisms (functional interferences), render the study of postural tonus exceedingly difficult.

As an illustration of the integrated working of these reflexes Magnus (1925, p. 345) gives the following: "Suppose a cat is standing in the middle of the room, and a mouse is running on its right side along the wall. The optic and acoustic stimuli act on the telereceptors of the cat's head, and make it turn the heavy head to the right. [Adience of the eyes and head to distant objects; and in psychology called 'attention.'] By this the centre of gravity of the fore part of the body is displaced to the right. At the same time tonic neck reflexes are evoked, by which the vertebral column is curved and the right fore-limb strongly extended,

so that it carries the weight of the body alone and prevents it from falling. The left fore-limb has nothing to carry, and in harmony therewith this limb relaxes under the influence of the tonic neck reflex. At the same time the distribution of excitability in the motor centres of the spinal cord is rearranged by the turning of the neck, so that, if for some reason running movements begin, the limb which has no static function will always make the first step. In this way the moving mouse impresses on the cat through the mediation of tonic neck reflexes an attitude, by which the cat is focussed towards the mouse and made ready for movement. The only thing the cat has to do is to decide: to jump or not to jump; all other things have been prepared beforehand reflexly under the influence of the mouse, which will be the object of the resulting jump." (By the cat 'deciding,' in the last sentence, Magnus doubtless means merely further reflexes manifesting themselves in the cat whose postures he has been describing. If later the cat 'decides' to jump, or clean itself, or go to sleep, it will *still* be acting reflexly.) "The cortex cerebri," Magnus later remarks (p. 349), "evokes during ordinary life a succession of phasic movements, which tend over and over again to *disturb* the normal resting posture. The brain-stem centres will in the meantime [tend to?] *restore* the disturbance and bring the body back into the normal posture, so that the next cortical impulse will find the body prepared to start again." And once more (1924, p. 619), "Such is the groundwork on which the cerebral cortex, as on a piano, plays the most complicated melodies, according to laws which are in part known, and which henceforth can be studied from new angles of approach." [4]

On the motor side the reflexes which we have been studying enlist "the musculature of the entire body in one common function" (Magnus, 1924, p. 618), which is to orient the body with regard to gravitation, to equilibrate the or-

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[4] See also in this connection Sherrington, 1908 a, p. 562; Preisendörfer, 1920; and Hansen and Hoffmann, 1920.

ganism. If, for instance, *other* stimuli impel it to some
course of action, the postural tonus or equilibration mech-
anisms reflexly guide every movement *so far as gravitation
is concerned;* that is, the organism succumbs neither as a
whole nor in any part to its own weight. If other stimuli
impel the organism to assume a certain posture, that pos-
ture is reflexly maintained and often for an astonishingly
long time; that is, postural tonus is also on occasion a very
'plastic' tonus (*cf.* Sherrington, 1909). If the head is tilted
to one side, or forward or backward, the two eyes compen-
sate by reflexly moving in their sockets and tend to pre-
serve their previous fixation and their normal position with
reference to the vertical (though this latter correction may
fall short of being total: *cf.* Angier, 1904, and others).
Whatever else the organism may be doing, it automatically
by equilibration, by plastic maintenance, and by compen-
sation, resists gravitation and preserves an orientation with
reference to the centre of the earth.

Tonic labyrinth reflexes in limbs are found directly after
birth in cats, rabbits, and guinea-pigs. Tonic neck-muscle
reflexes in limbs are present directly after birth in cats
and guinea-pigs, in dogs after two days. Standing reflexes
are remarkably well developed in all these animals at birth
(Magnus, 1924, p. 711). But in the human infant this
action system is very imperfectly if at all developed at birth,
and here we can readily observe the gradual encroachment
of postural tonus as a *steadying* factor on the hitherto rela-
tively uncontrolled and unoriented movements of body and
limbs. A little child that is learning to stand erect and
walk, easily loses his balance because the necessary postural
reflexes are not yet firmly established. If any sudden stim-
ulus comes from outside, the weak postural reflexes are apt
to give way (inhibition by interference), and the child falls
down: after more practice he responds less impulsively,
more restrainedly, to a sudden impinging stimulus. And
the finished expert in this line, the tight-rope walker, re-

sponds not detectably to such stimuli even though they may be intense.

All of the postural tonic activities so far discussed are, according to all observers, purely reflex: they go on unimpaired in an animal from which the cerebral cortex has been entirely removed. According to Magnus (1925, p. 342) the highest centre concerned in their production is the red nucleus of the mid-brain; even the cerebellum seems not to be involved (p. 352). This fact becomes the more impressive when we consider that it is this function of equilibration, more than any other aspect of its behaviour, that gives to a young organism the appearance of acting autonomously and 'as a whole.' Nor is this appearance altogether illusory. Gravitation is an ever-present stimulus, and practically no movement can be successfully carried out without due reference to it. And although this 'reference' is a purely automatic, reflex one, yet such unremitting adjustment of all members to the downward pull of gravity is a truly unifying element in an animal's behaviour.

The maintenance of postural tonus against gravitational (or other) pulls is a physiological *activity* [5] as truly as is the execution of a movement. The chief difference between the two is in the amount of physiological energy expended. [6]

[5] Whether in the higher animals there exists, as a limiting case, a 'lock mechanism' such as has been reported in molluscs, whereby a posture can be maintained indefinitely *without* expenditure of energy, is a question which has hardly been answered definitively at the present time. Nor is the question of supreme importance here, since we now know definitely that in the higher animals, at any rate, tonus is ordinarily maintained by nerve currents.

[6] The view that some sort of tonus, notably 'plastic tonus,' is also provided for in the skeletal musculature by unmyelinated fibres from the autonomic nervous system, and by a 'different kind' of nervous impulse, is much disputed. The evidence for this view, though not negligible, seems at the present time to be far from conclusive. Two notable papers (with bibliography) *for* the view are by the late Dr. J. I. Hunter (1924 a; 1924 b). If verified, this further source of tonus will be an interesting (and not altogether unwelcome) complication. The position taken in the text above is the generally accepted and amply documented one, and it is not likely to be invalidated by the discovery of *further* sources or 'kinds' of body tonus.

And in the experimental studies it has not been found feasible or significant to draw any sharp distinction between tonic postures and postural movements: thus Magnus, for instance, naturally and properly uses one or the other term indifferently. For it requires merely an *increment* in postural tonus, as when tonic impulses from different sources converge on one muscle or group of muscles, to produce a postural movement.

We have seen that postural reflexes originate in a great variety of sense-organs and terminate in a wide range of muscles, none of the body musculature being exempt from tonic impulses from one source or another. These postural reflexes sometimes converge on the same muscles and so work synergically, often producing movement, and in other cases they may go to antagonistic muscles and so inhibit one another. As Magnus (1925, p. 344) says: "The general rule is, that every group of muscles reacts to the algebraic sum of stimuli arising from the labyrinth and neck receptors." Indeed the general rule of *algebraic* summation seems to hold for the entire range of reflexes (Sherrington, 1905 b, pp. 809-810; 1908 b, p. 578; Holmes, 1923, p. 177). This fact, as well as their practical unfatiguability, previously mentioned, seems to indicate that postural nerve impulses probably consist of individual pulsations that succeed one another at a relatively low rate of frequency; for it is known that when the total frequency at a synapse comes to be around 250-300 per second, algebraic summation no longer holds and the too frequent pulsations begin to inhibit one another by 'overcrowding.'

This reflex orientation of the body, whether in motion or at rest, against the pull of gravitation is the first great steadying factor that appears in the movements of the young of higher organisms. And this alone gives to the young creature a considerable appearance of integration, of autonomous 'control' (as it is confusedly called) and even of intelligence. Such equilibration when combined with au-

tomatic movements of progression, described in the preceding chapter, yields the substantial foundation for all conduct (*cf*. Lashley, 1923, pp. 342-3).   Yet this mechanism is, in its anatomy, a purely reflex adience to the ever-present pull of gravitation.

THE EDUCATION OF SENSORY SURFACES

IN the preceding two chapters we have seen in outline how the organism, from being a merely squirming unit of animal life, becomes responsive to the most unremitting force in its environment, gravitation. It has become 'alive to' that aspect of the world in which it is to live. The slow transformation of its random movements, that crude exhibition of the organism's merely animal equipment, its physiological energy, into motions that more and more definitely and more and more discriminatingly respond to particular features of the world about it will constitute the history of its life. This process of learning to move more definitely is the animal's education, all life being education. It knows, of course, no completion. And the random element in the activities of animal or man is never wholly left behind. So far as may be, we must trace the successive steps.

The following paragraphs are a deductive application of the principle of reflex-circle to the young organism with its sensory surfaces and its random movements. As before, it is a question of how its sensory surfaces gain definite, in place of random, motor outlets: for they do not inherit any definite connections in the central nervous system. The argument here is not guesswork; but neither is it, except at certain points, observation. And I offer it for what the reader may deem it to be worth. Personally I believe that here physiology affords some little light on a very old problem,—that of the origin and nature of tactual and other 'local signs.'

If a random muscular contraction at the hip or shoulder moves a leg or arm as a whole, that aspect of the limb's sur-

face which moves foremost, *i.e.* the *front* surface for the supposed movement, will most frequently strike foreign objects and so receive tactile stimulation. Further, a strip of skin running the length of the limb (in a diagram drawn in the plane of movement it would be the *front profile* of the moving limb) will usually receive the hardest impact, while the surface on either side of this strip will in general merely graze (a milder impact) any object that lies in the way of the limb movement. From our familiar principles it follows that in time a pressure applied to any point of this front profile strip will cause the limb (as a whole) to move toward the pressure (adiently) : while a pressure applied on either of the side surfaces (lying more nearly tangential to the considered plane of movement) will innervate the limb (but less intensely than in the previous case) to move still in the same original plane: though this is now more or less *sidewise* to the direction of pressure on the surface now stimulated. Points on the rearward aspect of the limb, receiving no stimulation, acquire no motor connections from the particular random movement with which we started. Thus in time all of the pressure sense-organs on one side of the arm or leg acquire some degree of connection with the motor path for this particular movement at shoulder or hip; the paths of lowest resistance being from the front profile strip to the muscles in question, those of somewhat higher resistance from the regions at either side, and of still higher resistance from the two tangential profiles.

We supposed but one particular random movement, while in fact all movements that are anatomically possible are persistently being made. For every possible movement there will be somewhere on the limb a corresponding front profile strip, lateral strips, and two tangential or edge strips that barely graze objects as the limb moves. Eventually every point on the limb will have figured many times on a front profile strip, and many times on lateral and tangential strips. As a consequence, a pressure stimulation applied at

right angles to the skin surface at *any* point on the limb will innervate a considerable motor pattern: first and strongest, an innervation to swing the limb directly toward and against the stimulus, then, less strong innervations to swing the limb obliquely to the stimulus.[1]   Diagrammatically, in a plane passing through the point stimulated and at *right angles* to the axis of the limb, these directions are the directions of all the radii lying in the quadrants on either side of that radius which, starting from the axis of the limb as centre, passes through the point of stimulation.   The two limiting directions (and those of weakest innervation) swing the limb neither towards nor directly away from the stimulus, but out to one side or the other from under it.   Since these two movements, as well as the lateral components of all the oblique movements, are opposed one to one, they cannot be executed and the arm will move only directly up against the stimulus.   It is also fairly safe to say that these antagonistic innervation components will meet and extinguish one another *in the nervous system,* without producing any actual, opposed muscular tensions.

Similarly, if we draw our diagrammatic section through the point of stimulation, not transversely to the axis of the limb but parallel with this axis, we should find besides the innervation to swing the limb directly back against the stimulus (as before), oblique innervations with a component of motion that is parallel to the axis of the limbs. Since the movement of the limb along the line of its own axis is anatomically difficult and restricted, the innervation components acquired for movements in this direction will be relatively meagre.   Such movements do occur at random, and such innervations are bound to develop, as for instance, on the forearm surfaces when the elbow is partially flexed and the shoulder joint is active.   Yet the 'education' of the skin for longitudinal movements of the limbs will lag behind that for transverse movements.   Here too, antago-

[1] It matters not what the muscle groups are, nor how many, that participate in making these movements.

nistic innervation components will neutralize each other before they reach the muscles.

Now all these antagonistic innervations, though not very effective when a single point is stimulated, are there for purposes of mutual inhibition or re-enforcement when many sensory points are simultaneously stimulated, that is when the afferent (stimulus) pattern is more complicated. The broad general rule is that innervations that are antagonistic will cancel one another, unit for unit, and all others will re-enforce one another. Therefore whatever area on the limb be stimulated, the bundle of innervations (diverse in direction and in strength) excited by the stimulation of *each* pressure point combines with the similar bundles from all the other points stimulated, and the result is a swing of the limb as a whole in the direction of (*i.e.* towards) the main resultant of stimulation. We shall have often to speak of *allied* and of *antagonistic* reflexes, and it is well to remember that the stimulation of a single point already excites a sheaf of reflexes of which some are allied and some are antagonistic: though *all* of these may be more or less habitually masked by other reflexes.

We have considered the education of the sensory surface of a leg or arm only so far as this is accomplished by random movements of the hip or shoulder joint. And the limb possesses other joints. Random movements of the elbow joint, for instance, provide a further education to all sensory surfaces lying distal to this joint (except surfaces between the fingers), though they *do not undo* any of the shoulder joint education: and they contribute no further education to the sensory surface of the upper arm (*i.e.* proximal to the joint). Unlike the shoulder, the elbow is a hinge joint so that for random elbow *flexion* the *palmar* surface of the forearm and hand is the 'front' surface, which is most liable to receive stimulation from collision with the environment: while for elbow *extension* the volar surface is 'front.' The result of elbow-joint education is that a

stimulated point or area on the palmar surface of the fore-
arm or hand, while exercising its innervation of shoulder-
joint muscles (as already described), will now *also* inner-
vate the elbow-joint muscles to flexion (adience) : and stim-
ulation of a volar surface, besides innervating the shoulder
muscles, innervates the extensor muscles of the elbow.   An
interesting complication is here introduced by the fact that
the radius bone of the forearm is physiologically capable
of sliding about the ulna as axis: and random contractions
of certain muscles will produce this movement (pronation—
supination).   If the reader will follow out the necessary
consequences of this, he will readily see that any pressure
on forearm or hand will, in addition to its already described
effect on the shoulder and elbow, also produce a movement
of pronation or supination *if* such movement will give the
stimulated surface *more stimulation* (adience).

Now the sensory surfaces of the hand receive yet addi-
tional education, according to the same fundamental prin-
ciples, from random motions of the versatile wrist-joint and
of the carpo-metacarpal joints; the phalanges again from
the metacarpo-phalangeal joints; and the outer phalanges
from any phalangeal joints to which they are distal.   Sim-
ilarly for the thigh, lower leg, foot and toes.   Thus each
pressure-point on the surface of a limb is the point of stim-
ulation of a spreading sheaf of muscular innervations, and
these the more numerous and varied in proportion to the
number of joints lying between it and the hip or shoulder.
For two further reasons all the innervations produced by
the stimulations of a point on the skin are apt to be more
intense the more *distal* that point is: first, distal points
have always the greater momentum in any random move-
ment (being farther from the axis) and therefore their
impact in any collision with outer objects gives a stronger
afferent impulse and so a readier *Bahnung* into the motor
channels then in use; second, the more distal points are
more motile and therefore collide more *frequently* with

outer objects, which further lowers the resistance between them and their motor connections.[2]

This motor education of limb surfaces presents at once a parallelism with our tactual consciousness of space. It has long been known that the accuracy with which a touch stimulation can be localized by the person touched and the ability to discriminate two near-lying simultaneous pressures *as two* (two-point limen, Weber's circles) *vary directly with the motility* of the surface touched (Vierordt, 1870; Paulus, 1871; Riecker, 1873; V. Henri, 1898, pp. 123-5). Thus touch discrimination is finest on tip of tongue, finger tips and tips of toes, and decreases progressively towards the base of tongue, the shoulders and the hips. It is poor on the surface of the trunk, and specially so on the back and near the median line both ventral and dorsal. On the upper- and fore-arm, thigh and lower leg there is but little reflex movement in the direction of the limb's axis, and in this particular direction the discrimination is less keen (Ebbinghaus, 1902, p. 443: Ponzo, 1911, p. 13; 1912) : so that Weber's 'circles' are on most parts of the limbs ellipses of which the long axes lie parallel to the axis of the limb. In short, the two-point discrimination is less acute in that direction along which the sensori-motor education of the touch spots has been less adequate. This parallelism can scarcely be without significance. The favourable influence of practice on cutaneous localization (Judd, 1896, pp. 451-2) further argues that the consciousness of 'tactual' space depends on the motor education of dermal sense-organs. Thus there is considerable empirical

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[2] It is worth noting that the motor education of sensory points which are similarly located, say those on the palmar surface of the forearm, is to some extent similar. Stimulation of these points excites *in part* the same muscles, though in differing degrees. And this motor response is more nearly identical as the sensory points are more nearly adjacent. For this reason a stimulating object drawn along the skin excites a motor response which as a whole changes only gradually; and if, as I shall try to show later, consciousness is only that which a stimulus causes the organism to *do*, it becomes clear why the felt location of the object changes only gradually, *i.e. pari passu* with the changing response.

support for Prof. J. Peterson's contention (1926) that the conscious 'local signs,' so called, are 'orientation tendencies.'

In the foregoing we have considered afferent impulses from stimulation by pressure only. Warmth and cold spots doubtless acquire motor connections in the same general way. But pain stimuli, diffuse warmth and cold, deep-lying sensibility (fasciæ?), and as we have already seen the afferent impulses from muscles, tendons and joints, would all require special consideration. This brief account will have sufficed, I trust, to show that there is a motor education of sensory surfaces. And I know of no other physiological interpretation that can be put on the familiar expressions 'education of the senses,' 'sensory training,' etc. This education becomes increasingly interesting in the case of such sense-organs as the eye and ear, which possess a special *musculature of their own* in addition to their capacity for gaining motor outlet into the general musculature of the body.

The point which is important to notice at present is that this motor education of sensory points is *limb-wise,* and not in terms of this or that muscle: it is in terms, that is, of any physiologically possible *adience* of the motile member. The process and the resulting functions, which would be quite unintelligible if viewed as a "co-ordination of single reflex arcs" or if described, even exhaustively, in the disjunct distributive way of most anatomists, take on significance when we realize that every sensory cell is the starting point for a diverging pencil of motor innervations which spread widely, along paths of greater and lesser resistance, to many and to any portions of the animal's musculature. And hence of course, every muscle is the apex of a converging pencil of innervations which originate from any whatsoever of the animal's sense-organs (*cf.* Sherrington's 'final common paths'). In other words, the organization of nerve connections has here reached a new 'transformation level.'

This education of sensory surfaces (and sense-organs) to

a universal adience, so that stimulation of a sensory cell may elicit adience from any the remotest member, is, I presume, an important aspect of what is called 'integration' of the nervous system. And the remark of Prof. Beritoff (1922) might be recommended to psychologists as an heuristic principle, "that the central activity resulting from a peripheral stimulation is not limited to any one part of the central nervous system but spreads to almost every part, from the spinal cord to the cerebral hemispheres" (p. 412), so that "the experimenter, when he applies some special stimulus, is only adding a new stimulation to the many that were functioning before" (p. 425). The experimenter is adding a diverging pencil of innervations which will reenforce some parts and antagonize or inhibit other parts of the already active motor pattern. Clearly there is bound to be, regularly, *an extensive play of muscular tonus* as well as of overt muscular contraction. This point is of importance if we are to understand, subsequently, several of the 'psychic' phenomena.

This reduction of general random movements to local adiences goes on through the entire organism. Every sensory cell gains, and continues to gain, as its education, connection with many paths of communication in the central nervous system. There result preferential pathways of diminished, yet widely differing degrees of resistance from every sensory cell, and spreading to any or all muscles and muscle groups, howsoever remote anatomically from the sense-organ and howsoever widely separated from one another; the choice and distribution depending on the position of the sense-organ, the random movements that are anatomically possible, and the *obstacles* that are encountered in the environment as the random movements are made. That is, the muscles with which the sensory cell (or surface) gains connection are all muscles of the body that can in any way assist in thrusting just that particular sensory cell (or surface) towards the object that stimulates it, that is, in giving the cell *more* of the stimulation. This is adi-

ence. And thus in time the blind and aimless writhings of the organism, the random lungings of its limbs, and the random restlessness of its smaller members, are gradually replaced by a condition such that any stimulation or any combination of stimulations will elicit as much adient movement as the nature of the stimuli and the anatomy of the organism will admit of.

When I first became convinced by the evidence that every afferent impulse spreads through the central nervous system along ever diverging paths, and realized that the doctrine of single reflex arcs must be abandoned, I was puzzled as to how in that case an organism could ever execute any movement requiring accuracy. I seemed to feel myself armed with an ancient blunderbuss, and attempting to take exact aim at a mosquito. Here I was still a victim of the vicious old doctrine, and my quandary was partly an artifact. The accuracy that sets the problem is the accuracy, such as it is, that one can see in the performance, and this is a transaction between an object that stimulates and a sensitive surface which responds when it is stimulated; but only *if* it is stimulated. If the stimulating object is massive no great nicety of response is required, nor is it attained. But if the stimulating object is a fine point or edge it can often play rather nicely on the somewhat blundering response mechanism. Usually the external setting for accuracy is a yes, or no situation, as in aiming a rifle: one gets the near gun-sight in line with the point aimed at, and holds this as well as one can; then the off sight is in the same line or it is not, and if not, it is on the right or left or above or below (yes or no, each time) ; then one corrects, as well as one can; and recorrects, as well as one can; and then since nothing of all this can be held rigid, one waits in order to shoot as nearly as one can when the three cardinal points chance for a brief instant to lie exactly in line. And after all only the exceptional person ever does this very well. Such accuracy of movement as there is, even in human beings, depends mainly on the fineness of the stimulating

object and on the fact that the sensitive elements of all sensory tissue are themselves minute points. The actual accuracy of our movements is often overestimated.

The object guides. In the direct manipulation of tangible objects these serve immediately as obstacles to our random and wayward movements: and from such positive obstacles we receive our strictest and sanest training. Where these are not encountered we wander at once, and certainty can be regained only by introducing some device, such as the gun-sights, that brings us again under the *positive* guidance of external objects. Thus in weighing something on a precision balance, as the reading point oscillates over the graduated scale it is the zero point of this latter that serves as the positive check on our movements. In default of such a check, as I aim to show more fully later, we stray almost instantly into vagary; and return to fact only when, and *if*, we encounter again some positive obstacle.

Now as a sensory cell acquires its diverging sheaf of preferential neural pathways, so *pari passu* an afferent impulse originating in that cell will cease to spread to any except these preferential (*i.e.* less resistant) paths. Random movements cease because definite movements have been learned. The learning is based on random movement: but the learned movements are adient. And as the motor education of a limb is limb-wise, and the limb's adient activity obliterates all indication of the separate participating muscles, so the education of the whole is ultimately organism-wise; all members and parts, either by their participation or by their inhibition, being subdued to a general adience of the whole towards all save, as we shall presently see, 'overstimulating' objects. I suppose that this organization is most clearly to be observed at the period of puberty in mammals, when the body is in vigorous working order, its basic adient training practically completed, and when neither wisdom nor weariness has as yet set in to obscure the picture.

The wide spreading of the single afferent impulse pro-

duces a picture of nerve activity so intricate that it could hardly be mapped out. And this complication is vastly increased when not one, but very many and various sensory cells are simultaneously stimulated (total afferent pattern) and in an ever shifting succession. But while this process *as neurogram* becomes more hopelessly complicated, it becomes *as response* or behaviour, on the other hand, ever more definitely and exactly adient, more amenable to description, more 'intelligible': just as the somewhat orderly movement of molecules and atoms in a chemical process is easier to describe than would be such chaos as a 'Brownian molecular movement.' In the latter, indeed, as in the random squirmings of a very young mammal, one feels that there *is nothing* to describe. With the adient organization of the infant's sensori-motor tissues comes intelligibility, for us; and for it, as we shall later see, *intelligence.* In view of this hopeless intricacy at the neural level, but relative simplicity and 'significance' at the behaviour level, such familiar sayings as the following are silly and, when carefully examined, untrue: "If we could have a really complete record of the processes that have taken place in the brain-cells of the Rt. Hon. William Ewart Gladstone, we should have the only adequate history of his life." It is not to be wondered at that those philosophers and psychologists (and neurologists) who seek to locate consciousness in, or to 'correlate' it with, some part of the central nervous system, find themselves hopelessly entangled. They are making a blunder with regard to transformation levels.

THE CHAIN REFLEX

A FURTHER phenomenon which, as heretofore, comes about purely as a complication of principles already familiar to us, is the 'chain reflex'; a term which was first used, I believe, by Exner in his work on the *Physiological Explanation of Mental Phenomena* (1894). Anticipating a stage of nervous integration that is rather in advance of our discussion, we might illustrate the chain reflex as it appears in an adult animal.

A house dog let out in the morning usually romps away to his favourite post or tree, smells carefully of all sides of it, micturates on it, and then looks about; he spies the tree where the old cat is apt to sit waiting for robins, so he bounds off thither; having driven the cat up the tree, he takes an olfactory survey of the spot, and then observes that the cross cow is tethered in the field halfway up the hill, so he dashes on to give her a disturbed ten minutes; when tired of this he stops to look about, and his eyes alight on the swimming-hole down at the foot of the hill, so away he bounds again and does not stop until he is in the water; emerging from this he shakes himself, rolls on the grass, and dashes off on some further enterprise. Clearly the pursuance of one quest brings the animal into a situation where some new stimulus incites him to a new activity. This is diversion.

Now, when let out on the following morning, the dog is apt to follow exactly the same course unless he encounters irresistible new distractions in the form of cats, birds, or the like. The phenomenon begins to look interesting *only* when we notice that even if the cat is *not* under the tree, nor the cow in the field, the dog will still go the same round,

smelling about under the old cat's tree and in the field just where the cow had been tethered. This shows that activities once unrelated have become linked together in a certain sequence. The word 'memory' is merely a 'faculty' name for this connection, while for an explanation of it we must refer again to Pavlov's law.

Let us consider the dog at the moment when, having driven the cat up her tree and while he is sniffing about on the ground beneath, he spies the cow. The visual stimulus of the cow is cue for a chase (adience), that is, it opens those definite motor paths. But the olfactory, tactile and other stimuli of the spot where the dog is standing have been and are still playing into his nervous system, and these will now find motor outlet into the newly opened efferent paths, the motor set for chasing the cow. After a few repetitions of this situation, according to the second part of Pavlov's law, the ground beneath the tree, as a stimulus pattern, will send the dog chasing off to the field even in the absence of that other (visual) stimulus, the cow. So the dog will proceed from the cat's tree to the cow's spot in the field even when the cow is not there. In this way an excursion that has first been made in separate stages, of which each has casually broken in and interrupted the preceding, tends to establish itself as a fixed routine. Diversion has become concatenation. And such concatenated activities are readily observable in all young mammals.

This is, of course, a case of association. But Pavlov's law enables us, as the psychologists' law of association did not enable us, to tell exactly what is physiologically associated with what.[1] In all cases of association the primary mechanism is that an afferent path gains connection with, outlet into, some efferent (motor) path.

In a similar way when one plays a musical instrument from printed notes, the notes at first guide the eyes and the

[1] It is interesting to note, however, that in 1898 Prof. E. L. Thorndike (1898, p. 71) had the insight to assert that, "The groundwork of animal associations is not the association of *ideas,* but the association of idea or sense-impression with *impulse.*"

eyes guide the fingers. But the sound of one chord comes
to the ear as the fingers are preparing (from the visual cue)
to strike the next chord, and so the auditory impulse gets
its motor outlet into the paths for producing the next fol-
lowing sound; and the auditory impulse from this sound
goes over in turn to the motor paths that produce the next
following sound; and so on. Therefore, after a few repeti-
tions the player is able to dispense with the printed notes
altogether, and to play the composition 'by ear.' But more
than this, the striking of one chord, besides causing a wave
of auditory impulses to flow back to the central nervous
system, also sends back a wave of afferent impulses from
the proprioceptors of the contracting muscles, these pro-
prioceptor organs being stimulated directly by the contrac-
tion of the muscles. These afferent 'kinæsthetic' impulses,
no less than the simultaneously arriving auditory impulses,
find motor outlet into the muscles that strike the next
following chord. And so, just as the performer can dis-
pense with printed notes and play the composition by ear,
he very soon could play it, if he became deaf, by his kinæs-
thetic sense alone; the afferent impulses from his proprio-
ceptors sufficing to link each movement to the one next
succeeding. Thus a person who is thoroughly familiar with
the plan and interior furnishings of a building, is able to
walk through the building blindfolded or in complete dark-
ness without colliding with walls or furniture. As James
(1890, I., p. 115) has said in his discussion of 'concatenated
habits,' "In action grown habitual, what instigates each new
muscular contraction to take place in its appointed order
is not a thought or a perception, but the sensation [I should
say, *afferent impulse*] *occasioned by the muscular contrac-
tion just finished.*" In as much as the concatenation or
chain reflex is capable of being carried by afferent impulses
from proprioceptors alone, we are able, even in the absence
of external stimuli, to run through in 'imagination' (or
'memory') any journeys to which we are habituated. All
concatenated activity begins to look like 'conduct,' and in

fact all of our habituated activities, such as manual skill in art or craft, general bodily co-ordinations, and the daily routine of life, depend largely on nicely adjusted chain reflexes. But I need not dwell on what James has so well described in his chapter on *Habit* (referred to above).

Our definition of the conditioned reflex (Chap. V.) stated that if one afferent path already has connection with a motor path of discharge, then another afferent path if stimulated simultaneously, *or nearly simultaneously,* with the first will tend to acquire the same motor path of discharge. The expression 'nearly simultaneous' needs further explanation. Pavlov (1927, p. 27) and his associates, especially Feokritova and Krestovnikov, have believed that if the two stimulations are not actually simultaneous, then the stimulation or afferent impulse which is to be 'conditioned' (*i.e.* is to acquire a new motor outlet) must *precede* in time the impulse which already has a motor path of discharge; and that if it comes only after the latter impulse has ceased, it will not acquire a new motor connection (*cf.* Evans, 1925, p. 340). The validity of this statement is open to question (Humphrey, 1928, pp. 426-8) and Pavlov himself, in at least one place (1926, p. 57 [2]), has stated that the conditioned reflex can be formed even when the unconditioned stimulus precedes the stimulus which is to be conditioned. More recent investigations by Beritoff (1926, pp. 388-9) and Switzer (1930, pp. 76-7, 89-92) indicate that the stimuli may be given in either order: at most it may be said that the conditioned reflex is somewhat more difficult to establish, and is perhaps less permanent, when the 'unconditioned' precedes the to-be-conditioned stimulus. If one considers the question in connection with the law of neurobiotaxis and Sherrington's theory of synaptic charge (the histological and electrochemical aspects of reflex con-

[2] The sentence here in question (in Volborth's German translation) is replaced by one of quite different import in Gantt's English translation of the same ('Thomas Huxley') lecture (Pavlov, 1928, p. 93).

ditioning) it seems more probable that Beritoff's and Switzer's contention is correct.

In all the preceding discussion I have adduced only cases in which the two afferent impulses were, for a few seconds at least, simultaneous. But the circumstance that the afferent impulse which is to acquire new motor connection *may precede* (or follow) the action with which this impulse is to become associated, gives greater scope for the organization of reflex chains. It means that a stimulus given at one moment (and withdrawn) may still become associated with an activity that is not even commenced until some time later. And this time, which one might expect to be of the order of magnitude of seconds, Pavlov and his associates give as 30 minutes or more ('long-trace reflex': see Anrep, 1923, p. 407; Pavlov, 1927, pp. 40-2). Yet nerve impulses do not *wait*, and the phenomenon in question certainly implies that the afferent impulse that comes first is in some way held up or perpetuated until the second impulse traverses the nervous system. So far as I know, the observed facts of nerve conduction suggest two possibilities. The first is that a nerve current, after it has passed, leaves behind, at synaptic regions, a temporary condition which is analogous to a state of tension or 'charge.' Prof. Sherrington (1925) has put forward such a conception: and although I do not know of experiments that directly demonstrate the phenomenon, the conception certainly has empirical support. More recently Levin (1927) has reported a phenomenon in crustacean nerve which he calls "retention of action current." In general, the idea that the passage of a nerve current leaves synaptic regions in an altered condition (resulting ultimately in a slightly lowered resistance at these points) is, and I think should be, widely accepted (*cf.* Herrick, 1924, p. 119). The second possibility, that a nerve impulse may be perpetuated at points in the central nervous system by an uninhibited *circular reflex* (perhaps of low intensity), may also account for Pavlov's long-delayed conditionings: it will be of interest to us in another connection

('ideation'). If, according to the first suggestion, the passage of a nerve current leaves synaptic regions in a temporary state of tension, this condition can hardly be supposed, I think, to last more than a few seconds or, at most, a very few minutes. I believe that conditionings which are produced when the two afferent impulses are separated by only some seconds are to be explained in this way.

However that may be, the important point here is that the afferent impulse which is to be conditioned to a new motor path may precede the impulse that is already able to innervate that path, by *several* seconds (possibly even a few minutes). For this fact explains a property of chain reflexes which would otherwise be hard to understand. Namely, in the illustration given of playing a musical composition 'by ear' (where the links of the concatenation are auditory and proprioceptive afferent impulses) one sees at once that every note in the musical scale, and indeed most chords, recur in hundreds of different musical compositions, and therefore in any musical person almost any chord would soon become reflexly concatenated with a large number of chords each of which would be, in one composition or another, the next chord to follow. Such a concatenation would assist no player or singer to keep true to the musical sequence of any one composition. But since in fact not merely the single chord preceding, but all the chords (or notes) that have been struck in the several seconds preceding, gain at least some connection with the paths that carry the next succeeding motor impulse, the concatenation is much more specifically determined. The return afferent impulses from a considerable number of preceding notes or chords constitute a *total afferent pattern* (not merely one or two afferent currents) each contributing its quota to the one efferent pattern that will produce the particular next, the 'right,' note or chord. Each component of the afferent pattern has, it is true, its *other* motor connections, but these are scattering, partly inhibiting one another and seldom summating as the impulses summate for the one 'right'

movement. The specificity of the afferent pattern, and *ipso facto* of its efferent convergence channels, is further enhanced by the fact of sequence: the residual synaptic charges left by the notes that have been earlier produced being undoubtedly fainter; and fading eventually to a mere vestige. Thus not the note or chord but the musical 'phrase,' and in musicians I believe the longer musical 'passage,' is that which *carries* the definite concatenation.

Yet this specific linkage not infrequently fails, and there are places in many musical compositions where an amateur whistler or singer is liable to go over unintentionally to some other 'tune.' The linkage is practically sure to fail, of course, if a performance is interrupted; the performer must 'go back,' sometimes to the very beginning, and commence again. Now the *really* 'total' afferent pattern is even more than the afferent return impulses from the musical phrase just executed, for these are but added, as Beritoff would remind us, to many other stimulations from quite other sources that were playing on the organism before: these if persistent and even though invariant come in time to contribute their quota to the afferent-efferent pattern. Thus it may happen that when one is in a certain building, or a certain town, or when with a certain person, or group of persons, or at a certain time of day, or season of the year, one will invariably find oneself humming (or 'thinking') a certain musical air: because this was first learned under those 'associations,' *i.e.* those stimuli. But to pursue this further would lead us into the larger field of dynamogenic or cross-conditionings.

On the other hand there probably are simple chain reflexes where the proprioceptive afferent impulse from one movement is the immediate and sole cue for the next movement. Perhaps the peristalsis of the gullet and that of the intestines are such simple chains. Both are learned very early, are deeply 'canalized,' and their afferent paths have few (if any) connections into extraneous motor channels: in fact the intestinal peristalsis is generally believed to be

a strictly 'local' reflex, *i.e.* having no connection with the central, or with the autonomic, nervous system.

By 'reflex path' we mean one or more sense-organs connected with afferent fibres (afferent pattern), these with further transmission-fibres, and eventually with various efferent fibres (efferent pattern) which lead to and are connected with muscles. A 'chain reflex' is, as we have seen, a series of such reflex paths which are functionally related because the final action of one stimulates (or perhaps indirectly causes to be stimulated) the starting points of the next; and so on. And the work of establishing reflex paths is a matter of lowering the resistance to transmission, in the central nervous system, from certain afferent to certain efferent paths. There is every reason for believing that resistance is lowered where the resistance chiefly is, namely, at the small gaps (synapses) where one neurone or bundle of neurones terminates and other neurones begin; in other words, that the central functional connections are made at synapses: and further, that the passage of a nervous impulse across a synapse sets up some process of dendritic growth (*i.e.* neurobiotaxis) which gradually and only very slightly lowers the resistance of that synapse to the passage of subsequent impulses. But this result is cumulative. If, then, conditions such as those which I have been describing favour the passage of nerve impulses, for a few times, from a certain sense-organ (or group of sense-organs) to a certain muscle (or group of muscles), a reflex path of lowered resistance is established from that sense-organ (or group) to that muscle (or group). This process is called *canalization*. Henceforth, as we have seen, any stimulation of that sense-organ will lead to the contraction of that muscle: and this, of course, is the basis of 'habit.' In canalization we find in fact, as we should expect, that the first impulses which pass a synaptic resistance are those that produce the deepest effect, and this (resistance-reducing) effect diminishes progressively as the synapse continues to be used. There are good grounds for believing too, as indeed many

analogies would otherwise suggest, that the resistance-reducing effect is much greater in very young, and especially in fœtal, tissue; and that it progressively decreases with advancing years. It seems improbable that synaptic resistances can ever be *in*creased: that is, habits once formed cannot be unmade. Thus Freud has amply shown that all habits that are acquired during the very early months or years, are fixed for life. They may be to some extent overlaid later, and so *apparently* reduced, if circumstances, accidental or deliberately corrective, persistently impose other habits; that is, if they succeed in establishing connections of *even lesser* resistance from the sense-organs in question into other and quite different motor channels. Deep and early canalization is one of the two essential conditions that determine which shall be, in the life of the individual, the *prepotent* reflexes. (And the other essential is an extensive, as well as early, cross-conditioning: but of this, later.)

The reflex paths so far discussed and especially the chain reflex paths are the anatomical basis of many rather simple and elementary habits. Vastly more complicated habits are built up on these, but in this process the fundamental principle of resistance diminished through use, remains. So that in the last analysis all the activities of an organism, including the highest mental achievements, are *reflex;* and, as is well known, if the eliciting circumstances (stimuli) are repeated the activities become habitual. Now the familiar name for the simpler modes of reflex behaviour is 'instinct,' and since it was used to designate these phenomena long before the current theories *about* instincts came into existence, I shall feel free to employ the word without implying any part of any superadded theory. The instincts are not sharply and compartmentally sundered from one another, nor from the higher activities: they are learned and not 'inherited': since this learning is very gradual, they are not "correctly performed the first time": and they are by no means "identical in all the members of

one species." The three phrases in quotation marks con-
stitute the current 'definition' of an instinct. And it is not
always that one can hit on a 'definition' for a phenomenon
in terms of three predicates no one of which is true of the
phenomenon to be defined. From our discussion so far it
will have become clear, I trust, why the notion of 'instinct'
is of no very great systematic importance: nevertheless the
name is convenient enough as a general designation for the
rather simpler habits, mostly learned early and therefore
well canalized as engrams, of living organisms. Where the
activity is consecutive enough to be called instinctive con-
duct, chain reflexes are generally involved.

AVOIDANCE RESPONSES: TRIAL, ERROR, AND SUCCESS

IN the several cases of reflex-circle and its more complicated manifestations which we have so far studied, we have seen nothing that resembles a true avoidance response. The closest approach thereto would be a case in which a strong adient response against some unstable object chanced to push the object out of range (to upset it, send it rolling, etc.) so that it no longer stimulated the organism. This result, however, would be due not to the organism's adience but to the unstable shape and location of the object. Moreover, incidents of this sort lead, in the organism's course of development, not to avoidance but to a very different type of action—aggression. The genesis of true avoidance responses must be sought elsewhere.

In regard to avoidance reflexes a certain distinction must be made at the outset. What sort of stimuli, namely, *does* the young organism learn to avoid? They would seem to be the harmful or noxious stimuli, save that at this stage of development the organism has no means for distinguishing the 'harmful.' So far as I can discover the facts are as follows:—Every organism *at first* responds positively towards (*i.e.* so as to get more of) *any* stimulus. If, however, the stimulation is, or becomes, very intense, the transmitted excitation 'spreads' (Sherrington, 1906, pp. 150-2) in the central nervous system, overflowing into diverse and random motor channels, and the young organism is thrown into a state of more or less general and inco-ordinated activity; it wriggles, writhes, howls, and so on. In fact any muscles may become involved; and if the stimulation becomes strong enough, general motor spasms will ensue.

But some one of these energetic though random movements
may carry the organism out of range of the stimulus. Then
the stimulation ceases, and so will the organism's move-
ments. This is a process which we have not met before, and
which is called trial, error, and success. There has been a
considerable disturbance throughout the neuro-muscular
system of the organism, but one last, clear thing results.
The motor paths left 'open' are the ones that were last used,
those that carried the organism out of range of the over-
stimulation: and such a stimulation will almost always have
done enough injury to the sense-organs involved (or to the
tissues surrounding them) for afferent impulses, milder
but of the same quality and over the same paths as before,
to continue to pour in from the injured region. These, then,
will get their motor outlet into this last-used, relief-giving
group of muscles, and perpetuate their contraction. For
some minutes, and in extreme cases for hours, after such
an episode the organism remains in an intense tonic posture
of aversion to the overstimulating object. Thus an avoid-
ance reaction is canalized.[1] Of course such very strong
stimuli are in general the harmful ones; their 'intensity,'
as Sherrington (1906, p. 228) has said, 'constituting their
harmfulness.'

The 'openness' of the last used motor path, a physiological
condition which was mentioned in our explanation of Bok's
reflex-circle, can be readily understood in the light of Sher-
rington's theory of synaptic charge. Those synapses which
are left partially charged from the recent passage of nerve
currents, are of course (for some seconds, at any rate)
more ready to transmit further nerve impulses. They are,
so to say, 'set' or 'open.' And further, so far as these set
or open motor paths are motor half-centres and final com-
mon paths, their openness will be indefinitely prolonged by
the proprioceptive return impulses (circular reflexes) from

[1] In the young of human beings parental care sets bounds of modera-
tion to this process; but ought never to forestall it altogether, that is,
ought not to prevent a *moderate* overstimulation by the object.

the muscular contractions recently innervated. Thus after an overstrong stimulation and a successful escape through random trials, the final posture of avoidance will tend to be maintained (long after any residual synaptic charges would have been dissipated) both by the impulses which continue to come in from the seat of overstimulation (injury currents) and by the proprioceptive circular reflexes which have been set in action.[2] The posture of avoidance will probably actively persist until it is definitely inhibited by antagonistic impulses arriving from extrinsic sources.

It can be observed that when the random movement which accidentally brought avoidance is not allowed to perpetuate itself for a little time and so to become canalized, an avoidance response is not learned. Thus to 'baby' a child, that is, to rub or caress it briskly after every trifling mishap so as to make it stop crying and 'forget,' is to retard and sometimes to prevent its acquiring the simplest and most indispensable avoidance responses. On the other hand, if one has inadvertently hurt a child or domestic animal one has only to caress it briskly, and immediately, if one wishes the sufferer *not* to acquire an avoidance response toward oneself. Here the caressing evokes adience to oneself, which directly antagonizes any avoidance posture. He will then 'cherish no resentment,' but on the contrary will come to one for comfort whenever he is hurt by another person or by accident.

This process of avoidance learning can be easily watched in the very young human infant when it is overstimulated: and I have been amazed to see how helpless it is, how at random it responds, under overstrong stimulation. Of

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[2] There can be little doubt that the set or open condition of motor paths is the physiological basis of that phenomenon which James (1890, vol. II., p. 583) called 'drainage.' I shall, in later chapters, refer to a similar phenomenon as 'motor set.' In view of this residual openness of just-used *motor paths* it seems that Beritoff's contention (*contra* Krestovnikov) is the more probable, *i.e.* that in the conditioning of responses the to-be-conditioned stimulus can *quite* as well *follow* as precede the 'unconditioned' stimulus (Beritoff, 1926, pp. 388-9).

course the happy accident that its random writings *do* carry it out of range of the harsh object, is in no wise guaranteed: hence the need, during the early weeks and months, of unremitting maternal watchfulness. I may be in error, but I have yet to learn that the human infant at birth possesses a single avoidance reflex, though it already has a fair equipment of somewhat wavering adient responses. It is really a pity that 'inherited engrams' do not attend to this so all-essential matter of avoiding harm. Had I been the Creator, less 'parliamentary government' and 'salmon fishing,' and more instinctive avoidance of harm, would have been my thought.

Learning by trial-error-and-success is very different from the previously considered cases of learning by reflex-circle, although the physiological principle involved is the same in both cases (neurobiotaxis or Pavlov's law). Every case of reflex-circle starts with a random efferent impulse, which produces a movement of some motile member, which in turn produces a stimulation of some sense-organ and thus an afferent impulse which, lastly, now finds motor outlet in that very efferent path with which the process started. Whereas learning by trial-and-error [3] starts with intense afferent impulses from an external stimulation, which overflow into a variety of motor paths, and which cease only when some one of the various random movements made carries the organism out of range of the excitant. The milder impulses which continue to come in from the irritated sensory surfaces re-enforcing (by Pavlov's law) the connection between their afferent path and the open motor paths leading to the movement that brought cessation (avoidance). The end is always defined in advance, as it were, so that although the organism 'tries' and succeeds or fails quite blindly, this mode of learning has an inherently teleological or finalistic aspect.

This natural 'end' that so terminates random activity will

[3] For the sake of brevity I shall use the more familiar expression in place of the more complete "random-trial-error-and-success."

often *appear* to be an end sought rather than an end avoided. Thus the hunger stimuli drive an animal to general motor restlessness. A completely inexperienced animal thrown on its own resources would simply perish; but happily he has generally learned the simpler co-ordinations of walking and eating *while still under maternal care*. Now when hungry, he wanders about largely at random until he chances upon food. He eats, and the arrival of food in his stomach stops the hunger contractions and so terminates the animal's restlessness. It is a strictly mechanical process, which nevertheless looks very 'teleological'; and it is primarily a process of avoidance, which nevertheless looks like one of seeking. Hunger contractions (or some similar internal source of *restlessness*) are the cause of the process and are the definite thing avoided, while to an outside observer the animal appears to be 'seeking food.' And at a later stage of learning he will in fact seek food:—a more complicated process. The use of random trial-and-error in cases of apparent seeking and of avoiding has been described, of course, by many biologists and psychologists.[4]

Like learning by reflex-circle, the trial-and-error learning of avoidant responses continues to some extent through life. As the former, by its very nature, gives the animal *more* of any stimulus, so the latter, starting with a motor restlessness (overflow) and ending only with the *cessation* of that restlessness, is bound to be a process of avoidance.

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[4] Very notably by Prof. H. S. Jennings in several of his well-known publications. The picture of learning as a 'sifting process' from among random movements has been well presented by Prof. E. G. Dexter (1902) under title of "The Survival of the Fittest in Motor Training." The crux of the matter lies in finding what constitutes 'fitness,' why and how the 'fittest' movement becomes so re-enforced as to survive. The pleasure-pain theory, adopted by Dexter, is not a fundamental explanation (*cf.* S. J. Holmes, 1923, pp. 164-179). Prof. J. B. Watson's explanation (1914, pp. 262-8) in terms of 'frequency' of the fit response would seem to be more to the point save that, puzzlingly enough, he denies (p. 259) all random movements, and appears to deny even trial-and-error learning. "There is," he says, "no formation of new pathways" (p. 259). The process of learning by trial-and-error presents many intricate problems, some of which we shall meet in later chapters.

But trial-and-error appears in several disguises, as we shall see, and more often than not has an outward aspect that hides the actual mechanism.[5]

After the first few weeks of babyhood the gradual acquisition of more, and more nicely directed, adient responses begins to transform the picture of avoidance learning; and by the second or third year it is so completely transformed that an observer who had not witnessed the earlier picture might conclude not only that no trial-and-error is involved in the acquisition of avoidance responses, but even that these (abient) responses are quite as original and 'instinctive' as are the adient responses. If an infant so young that it has developed but few adient responses, happens to encounter a lighted candle, it will undoubtedly acquire (if it is not burned to death) an avoidance reaction of a sort: and in the way that I have above described. But this reaction will be so crude as to be often a non-purposive or sometimes even a counter-purposive acquisition. Thus in a special case, the motor activity at the moment when the infant happened to escape the direct stimulation of the flame, may have been backward curvature of the spine, flexion of one leg and extension of the other, perhaps adduction of an arm, and shrieks (for these two are motor) ;—a distinctly random picture. And when again the infant gets even a mild heat stimulation on the part of its anatomy that was burned in the former experience, or a visual stimulus of the colour of flame, or any other stimulation that had been a prominent component of the total sensory pattern in the former experience, it will be thrown into the same postural activity as before. The mother will cry 'spasms' and the father, 'fits'; the family physician will prescribe his favourite and now famous remedy for 'acidosis'; and the psychologist from the municipal Bureau of

[5] One is tempted to think that the reflex-circle mode of learning is in general more economical than the trial-and-error mode: but such an evaluation would mean little unless the value of the results achieved in the two cases could also be taken into account. Probably a study of the rôles which they respectively play in life's journey would be instructive.

Public Health will pronounce it a striking case of 'congenital fear,' and will instruct one of his district investigators to pry into "the ancestry of this case. Sure to be a morbid history somewhere up the line. And see to it that you find it." She does so. In short, such an 'avoidance' acquisition is so little specific as to the stimuli that elicit it and so ill-adapted as a means of avoidance, that it is not recognized as purposive conduct at all. And so far from advantaging the child, it is an unfortunate engram for the child to carry through life. Learning has here produced a trauma: as so often happens.

Except as the negation of a fairly precise adience, the avoidance will have no precision: as in the illustration above. Similarly the process of birth is said, and plausibly, to involve a considerable shake-up in an infant's sensory pattern: practically every sensory cell in the infant's body that is far enough developed to function, is now stimulated as it has never been before, and many of them are stimulated harshly. This of course produces a great outburst of chaotic motor activity, and such activity has no ulterior meaning or significance. But since the psychologist feels called upon to assign to it some name from his own jargon he calls it 'emotion,' or 'fear' or 'anger': and of course it is *strictly* 'inherited.' It is really nothing of the sort: it is merely violent, random movement. And since an organism in the moments of birth, howsoever overstimulated, cannot perform *all* movements at once, it performs *some* movements: that is, its birth posturings and writhings are some definite postures and movements from among all those that are organically possible to it, where by 'definite' I mean, merely, definable in terms of certain muscles and muscle groups. For brevity let us call these its birth postures. Now these birth postures can become in the minutes of birth all too deeply canalized, and what is worse, they are 'conditioned to' practically all of the infant's sense-organs. It is no wonder, then, that in the succeeding weeks and months these birth postures are often rearoused: and espe-

cially, that they tend to appear when the infant encounters any sort of overstimulation. With their low resistance threshold they tend to become the general, first-line overflow channels. In this way, although without significance at first, such postures and movements (or surviving components of them) may acquire significance, may come to mean that the individual is 'inwardly perturbed.' Since the birth postures are of course not identical in any two individuals, they soon come to be sensed by others as 'characteristic' of the individual.

It is into such fortuitous overflow channels as these that any overstrong stimulation which a young organism encounters, is almost certain to go. And this will never lead to avoidance responses that are adapted with precision and nicety to the varied situations which are encountered. But as the child's adient co-ordinations and the reciprocating mechanism of motor half-centres are gradually perfected, the picture of avoidance learning undergoes an evolution. And one can observe, in the child's successive experiences, the prompter avoidance of overstimulation and at the same time the dwindling number of random movements. Let us return to our illustration of the child and the candle flame,[6] assuming now that the child, say one year old, is able to bring its hand fairly directly to the bright flame which it sees. Its adient hand will move toward the source of heat somewhat lurchingly, since vestiges of random movement are still evident in every response which it gives. As the hand comes close to the flame the beginning of nervous overflow from the heat stimulation may appear in a little jerk of the other arm or a changed facial expression (this last may be secondary to an altered breathing). The hand will probably continue to move, though perhaps more slowly, until it comes into the flame. Then the overstrong stimulation produces various random writhings and *among them* a movement which appears to be a *direct pulling back*

[6] It is a situation which I have touched on elsewhere (Holt, 1915, pp. 69-74), but in no thoroughgoing way.

of the hand. One is puzzled as to where this withdrawal movement, apparently unlearned, has come from.

When the child is, say, three years of age, the picture is further perfected even though it is unascertainable that the child has had significant experiences with overstrong stimuli in the meantime. The child now carries its hand not very rapidly and at a fairly uniform speed toward the candle flame. At a little distance from it the movement of the hand comes slowly to a standstill and is resumed in the reverse direction, as withdrawal. The warmth of the candle has caused no injury to the hand, *apparently* no overstrong stimulation and nervous overflow, and no sign of random movement or other inco-ordination.

The secret of this reversal of the hand's adient motion lies in the more complete development of the child's motor half-centres. In Chapter VIII., on circular reflexes and progression, we saw how the proprioceptive afferent neurones coming from any muscle, besides having a motor connection back to this same muscle, acquire also a collateral connection with its antagonistic muscle. This collateral connection (canalization) lies presumably in the (second) synaptic region preceding the final common path (motor-neurone) to the muscle in question, and constitutes the motor half-centre of that muscle. As the hand of the child moves adiently toward the candle flame, if the collateral connections from the motor half-centres of the muscles which effect this adient movement to the motor neurones of the antagonistic muscles are fairly well canalized, then as the warmth stimulation from the candle (which is assisting to innervate the adient movement) becomes more intense its *first path of overflow* is into those antagonist muscles which will *reverse* the motion of adience. This is the phenomenon of 'reflex reversal' which has been described by v. Uexküll (1929): it may also be taken as an illustration of the statement previously quoted from Magnus (1924, p. 26), that in every distinct body posture the muscles that are *stretched,* since their proprioceptors are stimulated by the stretch and begin

to innervate them, have a lowered reflex threshold: they are, of all, the most ready to contract.

Thus as soon as the reciprocating movements of progression are established, in a child or other young mammal, through the perfecting of motor half-centres, the motor channels that are most ready to receive any nervous overflow are no longer the paths for birth postures or other motor chaos pertaining to early infancy; unless, that is, some unduly severe experience has so deeply canalized certain of these motor paths that they take any and every nervous overflow: as sometimes happens. In our illustration of the three-year-old child, as its hand gradually approaches the flame and the afferent impulses from warmth stimulation begin to 'overflow,' the channels that are most 'open' to receive this overflow are the collateral motor connections into all muscles that are *antagonistic* to those now used in adience: and so, with no appreciable trial or error, the adient motion slows down and stops, and the exactly reverse motion is started, before the hand comes actually into the flame. In this case avoidance has hardly to be 'learned,' since it is an intrinsic function of the mechanism for the reciprocal innervation of antagonistic muscles; which latter however was learned, as I have shown, by reflex-circle. So avoidance is to this extent a consequence, although indirectly, of the reflex-circle principle. This is another illustration of more complicated reflex functions depending on the simpler and earlier reflexes.

I believe not only that avoidance responses are acquired more tardily on the whole than adient responses, but also that a tolerably *well-adapted* avoidance response can be acquired only on the *basis* of some previously acquired adience, and that the precision of the former will depend on the precision of the latter. We can now see why there should be a tendency among observers to place the acquisition of avoidance no later than that of adience. It is in fact not usual to discover a young mammal, unless it be *very*

young, in the throes of learning avoidance by a process that is conspicuously one of random trial, error, and success: they seem to have the gift of avoidance 'by heredity,' as soon as they can creep or walk. They do have it in so far as, and no further than, they have learned proficiency in adience: and lacking *that*, the 'inherited instinct of self-preservation' is unfortunately 'latent' and remains so. It may seem far-fetched to call this later phase of avoidance learning a process of trial-and-error: yet I do incline so to call it, for the reason that the later phases, of easier and more precise avoidance learning, are quite continuous with (and merely a refinement of) that early blundering learning of avoidances which is unmistakably a process of trial-and-error. In the older individual an increasingly strong stimulation *still* produces nervous overflow, but it happens that now the nearest and most open motor channels to take this overflow lead at once to the 'successful' trial. Yet if this first 'trial,' say the direct withdrawal of the hand, does *not* suffice (as it may not if instead of a candle flame an open fireplace is in question) the overstrong stimulation continues to overflow, spread, or 'irradiate' (Sherrington, 1911, p. 145; Beritoff, 1922, p. 412; Minkowski, 1924, p. 257; Magnus, 1924, p. 25) to other motor channels which, in a three-year-old child, are still largely 'random'; all too plainly so. And in an adult where adience is still further perfected, so that *all* the muscles of the body which can in any way contribute 'synergically' to any given act of adience have become 'conditioned' to do so, this irradiation is seen to engage serially and progressively all such muscles of the body as can in any way contribute to avoidance of the over-strong stimulus, each of these muscles being the antagonist of a muscle which had been employed in the preceding act of adience. And still in extreme cases we do see the picture of random, 'unco-ordinated' action (contortion, howling, spasms, etc.) even in an adult. In short, withdrawal is always due to nervous overflow, but the promptness and

deftness of it is in proportion to the precision and completeness of adience which the individual has acquired.[7]

It is clear that if an infant is allowed freely to handle and explore, developing its adience anywhere and everywhere, it will *pari passu* develop an equally adequate and competent 'faculty' of abience and self-preservation. Here the parent need and should have but two cares: he should be watchful to prevent any damage to the child's tissues or bodily frame and, for a different reason, to prevent destruction of valued property. Beyond this it is of the utmost importance that a child's spontaneous adient explorations

[7] There is, I suppose, a 'locus' or range of situations (*cf.* Holt, 1920, pp. 371-5) within which it is methodologically justifiable to employ the concept of 'chance,' a concept which is implied in the notion of 'random trial-and-error.' Chance or accident is of course a 'subjective category,' a mere name for our human inability, in certain circumstances, to discover a cause or to predict a result (*cf.* G. Just, 1925, pp. 4-5, 9). Yet in a given universe of discourse many a fact or event is so irrelevant to the discussion in hand that one chooses, and properly, not to examine it very closely: it is 'fortuitous,' 'accidental,' 'random.' Thus, for instance, the unorganized muscular activity of a fœtus is strictly enough caused by the chemical exchanges between its various tissues, stimulation from the maternal tissues surrounding it, etc.; but such factors are so irrelevant to the purpose of our present Essay that these fœtal movements are, properly enough, 'random.' Yet the more I have tried to study into the learning process, the more I seem to find, unless I delude myself, that 'random trial-and-error' evaporates into mist, as it were, and discloses what it had previously hidden—a clear picture of mechanical causes and effects: precisely as in our last illustration of the hand avoiding the candle flame. This is not because the trial-and-error notion is 'wrong'; it is an immense advance over the silly teleological, preformist and hereditarian notions which in the minds of all serious thinkers it has displaced. It merely does not advance far enough. 'Heredity' is a name for complete ignorance; 'random,' for partial ignorance. But it happens too often that that which was ignored as being 'random' is exactly that which, when examined, solves the "problems presented by trial-and-error learning" (*cf.* Kuo, 1928, p. 428). More concretely and more appositely to our present discussion, I have seemed over and over to find a general substratum of adience functioning underneath 'random trial-and-error'—as in the case above of the more precise avoidant reflexes. I believe that in practically all learning by trial-and-error, the 'trials' are supplied by adient reflexes. We shall find another instance of this when we come to Prof. Washburn's theory of learning (Chapter XVI.). Meanwhile we are still ignorant regarding many processes, and still remain largely, I fear, in the 'random trial-and-error' stage.

shall be as unrestricted, as diversified, and as incessant as possible, and that he encounter all the common objects which are capable of stimulating overstrongly.

In confirmation of what has been said of avoidance responses, it is perhaps worth noting that in an individual whose development has proceeded without mishap no single sense-organ (unless the 'pain' receptors) when mildly stimulated replies regularly and prepotently with an avoidance. That is, no sense-organ is actually conditioned (canalized) primarily to an avoidance reflex.[8] But precisely this *can* happen; and the result is unfortunate, the more unfortunate the more precise the individual's adience was when such stamping in of the avoidance reflex occurred. Thus if a child in its second year, say, rather blunderingly creeps or stumbles into a fire and is seriously burned, its chaotic movements to get out will be rather well stamped in by the impulses that continue to come into the central nervous system from the injured sensory surface. As a result, even a mild stimulation of any part of this sensory surface will tend to elicit rather general reflexes that will be crudely avoidant, or at any rate not freely adient. This will be interpreted as 'fear.' Hereafter, and perhaps throughout the life of the individual, the part of the body involved will 'flinch' or 'wince' from even the mildest stimulation. This is unfortunate, because this (sensory) part is henceforth disqualified from contributing its full share to the adient activities of the individual, and further, it will impede them by interposing at times utterly malco-ordinated and meaningless inhibitions. But all this, in the case

8 Whether, however, and to what extent the sense-organs may in the end become conditioned to immediate withdrawal remains a question. From the principle of the reflex-circle it seems probable that after an overstrong stimulus has effected an adience reversal, it would commence to acquire more direct motor outlet (not through the collateral from the half-centre of the adient muscle, but directly) to the half-centre of the (antagonist) muscle which effects the withdrawal. This is an important question, but I am unacquainted with any experimental investigations which would seem to throw light on it. It may be related, too, to the still more difficult question of the sensory 'qualities.'

assumed, is so general and non-specific as to be seldom disastrous, though it is unfortunate.

If, however, a child of five years or more, with nicely developed hand and arm adience, is so unfortunate as to put its hand into a steel trap or some piece of machinery, then even though the hand be not seriously mutilated, it gets so profoundly canalized for avoidance that the whole child will flee from any mild contact on its hand, and the entire arm in question will be for a long time (perhaps for life) a non-participating member. I have seen a child habitually carry such a hand behind its back for years: and this not for concealment since the hand was not disfigured. Even if, later, hand and arm participate once more (through counter-conditioning) their education is sadly retarded, their retraction habit is conditioned to a wide range of irrelevant stimuli, and there are paths of ominously deep canalization ready to receive all nervous impulses that overflow. Here the disastrous results seem to be more concentrated than in the previous case.

Such is the physiological mechanism of many 'psychic traumata': the child last mentioned, if it suffers some other disasters, may easily present as an 'hysterical stigma' or 'transfer' a useless right arm. We shall see later whether this will help us to understand 'mental repressions.' But many of us come through the ordeal of childhood more or less maimed in this way; that is, by acquiring to some extent definitely canalized avoidances, sense-organs which upon *mild* stimulation excite a prepotent withdrawal from the stimulus. This is the exact antithesis of a normal, healthy development. Apparently those who have suffered more of such maimings than the average, become the 'introverted personalities.' They are handicapped for life.

For exactness, we must note that there is one class of receptor cells which *probably* are normally conditioned to direct avoidance. These are the free nerve terminations which lie for the most part deeply embedded in the dermis, the so-called 'pain spots.' They are, as any text-book of

physiology tells us, so deeply embedded that under ordinary. circumstances they receive no stimulation whatsoever. On the other hand, if they are *reached,* as by agents that puncture or destroy the epidermis, their threshold of stimulation is very low, and they readily set up afferent impulses so intense as to spread more widely than most, *i.e.* to 'overflow.' Neurologists confirm this last item by saying, a bit crudely perhaps, that impulses from 'pain-receptors' travel along the grey matter of the spinal cord. I suspect that normally these organs become definitely canalized, or conditioned, to responses that withdraw the point stimulated from the stimulus. We see at once how this would come about: they are also the only external receptors which, owing to their protected location, are probably not conditioned to any form of adience.

In view of what we have so far learned about adience and avoidance, I believe that we may safely frame the following tentative and partial picture of neuro-muscular integration: for 'heuristic purposes' as a philosopher would say, or, more exactly and frankly, in order to save ourselves from bewilderment. Every sensory cell is originally connected by one or more afferent fibres with the central nervous system. This latter has originally no preferred paths of lower resistance, but diffuses any afferent impulse (if it is strong enough) in all directions impartially, transmitting it finally to any and all muscles. As a result of random movement every sensory cell becomes connected by paths of lowered resistance (canalized) to all muscles of the body whose contraction will thrust the cell nearer to that which stimulates it (adience). Connections that are earliest made and most frequently used are the most deeply canalized. True avoidance reflexes are not, save as traumata, directly canalized (except from free nerve terminations). At any given instant, many stimuli are playing on the organism, determining posture or reflex action, or starting chainreflex action: some muscles are in steady (mild) contraction because their circular reflexes are not inhibited: other

muscles are in special readiness for contraction because they are stretched and therefore (by their own proprioceptors) already subinnervated. The impulses converging on a muscle from all sources summate to produce intenser contractions, unless the total number of pulsations per second is high enough to cause 'overcrowding,' and inhibition of this muscle. The organism appears to act 'as a whole' to some extent because every sensory cell is connected with all the muscles that can act *for it* adiently, hence synergically.

As we proceed in this Essay we shall be able more and more to spare ourselves much of this neuro-muscular intricacy—so long as we are able to go back to it when we need to. For this mechanism produces as a synthetic result novel phenomena which we can deal with as such. Of this, adience of the organism, and avoidance, are two instances.

ECHO AND IMITATION

BEFORE proceeding to further problems connected with learning by trial-and-error, we shall probably do well to consider two or three less intricate phenomena. All the later stages of learning (in youths and adults) involve, so far as I can discover, trial-and-error, and this is always fundamentally a process of avoidance or escape from some physiological annoyer: though the restlessness in question may be of the attenuated sort which we call discomfort, agitation, bewilderment, dilemma, or the urge to solve a problem. But since a creature cannot escape from one situation without arriving at some other, the fundamentally avoidant nature of the process is regularly masked by an appearance of 'end sought' or (later still) complicated by the fact that an 'end' is in fact sought. The learning process in infancy and early childhood is simpler: it is merely the slow development of general adience on the reflex-circle principle, with trials-and-errors only in connection with avoidance learning.

Mention has been made in Chapter VII. of a general law of iteration, whereby a child will repeat any of its own random acts provided that this action stimulates, directly or indirectly, any of its own sense-organs; and of course this law depends directly on Pavlov's law. Now the same principle goes further, and its next phase is one which we might call the mirror, or the echo principle.[1] We have already seen how the child comes to say back the very sounds that are spoken to it; as also, and for the same reason, does a parrot. Now the ear or any other sensory organ will, as

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[1] *Cf.* 'echopraxia,' 'echophemia,' etc., as used in psychiatry. Either optical or acoustical figure is inadequate, for the phenomenon calls in play any or all of the senses indifferently.

we have seen, co-operate in this way with any motile member of the body. Suppose that in its random movements the infant happens to bring its hands together with a smacking sound, but happens not to have watched the motion with its eyes. The auditory stimulation will of course find its motor outlet in that clapping motion. And one may watch the child clapping its hands over and over, clearly listening each time for the noise, but not as yet watching its hands with its eyes. If at *just this* stage another person claps his hands, either in front of the child or in an adjoining room if still within hearing, the child will clap. But if the other makes the same motion directly in front of the child but *silently,* the child will not clap. This stage, however, is very brief for presently the child happens to *see* as well as hear itself clap; and thereafter either the sight or sound of another's clapping will cause the child to mirror back the same. And sight and sound together will work synergically to re-enforce the response.[2] Or again, a mother may smile and smile to her baby without provoking a response. But if, when among its random movements the child has got its own lips into the smiling position,[3] the mother will *then* smile back to it (assuming that the child sees her) the child will soon learn to smile back to her.[4] Since most mothers do return the infant's random 'smile,' most infants learn truly to smile: but not all. For some mothers (or nurses) are not responsive. And it is here the *responsiveness of the mother* which develops the responsiveness of the child.

[2] Of course another child may happen first to *see* itself clap, and then vision will participate in the echo mechanism before audition. Or again, both vision and audition may be involved from the first. The order is fortuitous.

[3] Which I suspect to be the position of rest after nursing. See also S. Freud (1920, p. 44) and W. Preyer (1884, pp. 217-8): but Prof. H. M. Kallen is convinced, from observations of his own, that some other factor than mere rest is involved in this posture of the lips. For the child of course this posture has not as yet the significance of a smile.

[4] I am told that this principle has begun to receive the attention of trainers of animals.

The mirror or echo principle can be broadly stated thus: —a child will learn to echo back any action of another provided that another's performance of the act stimulates any of the child's sense-organs at a moment when the child is engaged in a (random) performance of the same act.

It will be seen that the echo principle is only a special case of what I have called the principle of iteration (just as this, in turn, is a special case of reflex-circle), the case, namely, in which a random act of the child stimulates another individual to respond with a similar gesture ('imitatively'), which latter if it stimulates *any* sense-organ of the child will incite it to repeat (or prolong) its original (random) action. In general, of course, any action which a child iterates by himself he will usually also echo after any other person. Now, as in the case of uttering random sounds, any child will sooner or later perform at random nearly all the movements and simpler combinations of movements which his bony and muscular anatomy makes possible. Therefore in so far as those around him are responsive to his movements, he will come to be responsive to theirs. This is one of the reasons why so much in the behaviour of a child, and even of animal pets, is a direct reflection of the deportment of the older persons with whom he comes in contact. He learns to treat them as they have treated him. This does not account for the sympathetic responsiveness that is presupposed in the older individual; but it does show why such responsiveness, once acquired by a family or a race, tends to be transmitted from each generation to the next.

Any presupposed 'sympathetic responsiveness' might, for our present purposes, be summarily ascribed to the 'simple imitation' mentioned in Chapter VII.: though it also originates otherwise. Simple imitation comes about because the individuals of all social species often do the same thing at the same time. Thus if even two animals are feeding together the sight of the other in the act of eating becomes a sensory cue that will excite the food responses of either.

And since any animal *hears* himself eat, the same characteristic sounds made by any other individual of the species will *ipso facto* arouse him to that line of activity even if he sees no food and has never before eaten in company with another animal. Such 'simple imitation' arises in a thousand ways among animals: and of course the life of school children, drilled and trained in groups, abounds with situations that develop this tendency (*cf.* Humphrey, 1921).

But children will echo the actions and 'dispositions' of those around them even if these latter do not appear to respond imitatively to the children. For of all the activities random or other, of children, many go unnoticed by their elders, others attract attention because they either please or displease the older persons. When the latter are pleased they respond in some way (from an almost imperceptible, indulgent smile to open praise or the award of favours) and this response, whatever it is, becomes the cue that stimulates the child to repeat whatever it had just done. It will presently be repeating the act in order to 'show off' or court favour. In this way the elders, often unconsciously, exercise a considerable selection over the actions that are to become habitual in the children. Now the point is that the childish acts which the elders thus encourage (by which the latter are 'pleased') are those which the latter 'understand,' that is, which the latter are themselves more or less set to perform. *Du gleichst dem Geist den Du begreifst.* This will become plainer when we deal explicitly with the subject of 'empathy': for empathy, which is the very basis of 'perception,' is, as we shall see, a more or less concealed imitation. It is a motor synergy in the parent, an attenuated non-overt imitation, that causes him to 'notice,' to be 'pleased' by, and to encourage *certain* of the child's performances. Thus it is that in the course of time children seem to absorb the habits and general motor dispositions of those about them; specially of their parents.

This too is of course ascribed to 'heredity.' Everybody knows that, for a rather obvious reason, fathers are gen-

erally more responsive to their girl babies, and mothers to their boys. The result of this is that in their early years little girls tend more to take on (echo) the ways of the father and little boys those of the mother. Hence the persistent delusion that, "mental inheritance runs from mother to son and from father to daughter." Unfortunately children 'inherit' much from nurses, chauffeurs, and the movies, but 'scientific experts' are not interested in these collateral lines of inheritance.

Parents also notice and respond to a child's actions of which they do not approve. Here too the parent imitates, but mostly only with that minimum amount of visual empathy which is requisite for merely perceiving what the child does. And here the parent must be cautious how he responds. If one is definitely trying to train a child or animal, one must be careful at any moment when the organism is doing something which is *not* to be encouraged, either to give the organism no stimulation at all or else to give it so strong a stimulation that the afferent impulses will have to *spread* into a variety of other (random) motor channels. The latter, which is the 'corporal punishment' method, is a blunderbuss business though perhaps necessary at times: it does discourage the action in question (though only temporarily) by breaking up the motor posture and substituting therefor a sort of motor chaos: but it contributes nothing toward forming any desirable habit, and it produces a very undesirable motor shake-up. Any frequent resort to corporal punishment spells failure. It is shrewder to give the erring organism no stimulation at all: but it must be *no* stimulation. If when a child is tearing the lamp-shade or tipping out the waste-basket one says, "Don't do that, darling," the auditory stimulation (by Pavlov's law) goes over into the motor paths *then active* and becomes at once a positive cue for tearing the lamp-shade, etc. One can test the truth of this. On the morning after such an incident, and at a moment when the child is innocently ruminating on cabbages and kings, if you will say, "Don't do that,

darling," you will have the pleasure of seeing the child march straight over to the lamp-shade and proceed to tear it (looking slyly at you, the while). How *could* the little imp know what it was that he was not to do? How, indeed!

This is the easiest and surest way to develop obstinacy and perverseness. The don't-do-that mother is the one who comes to mothers' meeting with the question, "Can some kind friend present not help me with my dear little Effie? In spite of the tenderest care at all times, she seems determined to do just the thing which I ask her not to do." And the good vicar, ever faithful in attendance, delivers a brief but eloquent homily on 'original sin,' closing gracefully on a note of redemption and hope. The vicar's taste is flawless. Then the visiting psychologist from our State University delivers his professional cough and a brief but inspiring discourse on 'inherited tendencies.' His diction is almost over-learned, but he concludes with one very practical and acceptable suggestion: "Is not your husband, Effie's father, a very stubborn man at times, Madam?" (General applause.) Then Mrs. Titwillow rises to say, "Though I am so unfortunate as to have no dear little ones of my own —the greatest of griefs to my dear husband and myself—I feel sure that if our dear sister, Effie's mother, could be invariably tactful and wise with little Effie, she would have no further trouble. For surely God has intended all of His little ones to grow up virtuous and beautiful in His Own blessed sunshine." There is then a further, more general, exchange of word salad, the meeting closes, and everybody goes home exhilarated. Little Effie remains obstinate.

Of course, no child must go on tearing the lamp-shade. Wails or no wails, it must be summarily removed from proximity to the lamp. This saves both the lamp and the child, for the undesired activity of tearing the lamp-shade requires the conjunction of child and lamp-shade for its successful continuance. The fact is that the instinct to explore is bound, in the early years, to lead to the dismemberment and destruction of any fragile object that the child can get

at: yet the tendency must be cautiously dealt with for it is adience itself, and this must not be discouraged. Furthermore, if the child is tactlessly punished, a love of destruction for *its own sake* is developed and fixed for life: this is one of the most familiar by-products of injudicious training. For the child *echoes,* as surely as a mirror reflects, all that comes to it and if the parent shatters its proclivities by harsh punishment, it will visit the same devastation on its pets, playmates, and inanimate surroundings; and later, on the parent too. Common sense would suggest, since the playthings of parents are not suited to the uses of infancy, that the child be kept out of any Louis Quatorze drawing-room, and be provided as far as possible with implements and accessories which are so simple and substantial that he will not easily demolish them. Moreover, when he has broken something, it should *not be replaced nor the fragments removed* for several days at least. The child will presently, often surreptitiously, try to piece them together. And with that the period of 'wanton' destructiveness begins to draw to a close.

To return again to the general principle of echo, it is clear that, over and above the uniformity of human anatomy and its terrestrial setting, the carriage and deportment of each new generation is bound to resemble (echo), and so to perpetuate, the idiosyncrasies of the preceding generation in the same geographical vicinity. This similarity becomes closer as between the members of one community, closer still within one social set, closer yet between the members of one family, and, as a rule, closest of all between identical twins. This similarity of motor habits, or of 'action systems' as they are sometimes called, is of course the basis of *sympathy;* whereby one person's posture, gesture, or pressure of the hand is automatically responded to by a more or less identical posture, gesture, or hand-pressure of the person *vis-à-vis.* In sporadic cases such closeness of motor *rapport* can go to incredible lengths, and gives the participants a sense of organic continuity of per-

son, and immediate community or even 'identity of soul.' And this for the best of all possible reasons—c'est ça précisément que *veut dire* 'l'identité de l'âme.'

Looking in the opposite direction, that is at the feebler grades of sympathy, we recall that it has often been pointed out that our sympathy with other organisms is directly in proportion to their similarity of structure with ourselves. We have most sympathy for other human beings, considerable sympathy with the vertebrates (except snakes and eels) and especially with the higher vertebrates, some sympathy for Insecta, less for Crustacea and spiders, and practically none for molluscs and worms. Snakes and eels, although vertebrates, move much like worms, and our anatomy does not lend itself to mirroring their motions.

Like all phenomena that depend on Pavlov's law, any case of echoing requires a little preliminary period of learning before the echoing is manifest. So that one who would train a child or an animal by this principle must not expect an instantaneous result; one may have to wait for a week, or perhaps for a month, before the result appears. But there is an amusing and instructive little game that can be played with a child, say of five years, that illustrates three points that I would here bring out. At a moment when the child is in good humour and not keenly bent on any special activity, one stands up in front of the child smiling, and copies whatever movement the child happens first to make. The child will almost infallibly (smile and) repeat his movement. One repeats after him. He will repeat again, and with each repetition the child's movement becomes more energetic and longer in sweep. This will easily go on until for the older person it has become monotonous. In time the child too will tire of this motion, but he likes the game, is keyed up by it, and he will almost always make some other motion probably more gesticulatory: looking as if he expected it to be repeated. If one repeats it, a new sequence is set up; and so on, indefinitely. In this way I have seen a child worked up to an actual frenzy of ex-

citement (and delight). Now when the game is in full
swing, if the older person introduces a new movement the
child will almost infallibly *imitate* it, and this will start a
new sequence. I recommend this game to any parent, for
when the parent has acquired this good-natured rapport
with the child, he will be pleased to find that the child has
acquired an equally good-natured rapport with the parent:
which will last unless it is discouraged. When any one
can so easily get a child or animal into this eager and
almost frantic mood for emulation (within due limits), it
strikes me that any one is a fool, and the dupe of his own
brutishness, who neglects this means and uses a club in
order to get 'obedience.' This last is always an imitation
of obedience.

The little game brings up more points than the three that
I wish to emphasize here. Of these three, the first is that
the increasing vehemence of the child's repetition of his own
and the older person's movement is directly due to echo.
Iteration alone often causes a child to repeat its own move-
ment, but I have never seen an iterated movement increase
in amplitude and intensity as it does when re-enforced by
some outside stimulus-cue, as in the game mentioned.

Secondly, when the game is in full swing, and the older
person introduces some *new* motion, the child will copy it.
And it may be that this is a motion which the child has not
previously, by the true principle of echo, learned to imi-
tate. In that case we have here a distinct process of imi-
tation, that is somewhat different from simple echoing. It
is common knowledge that in all animals whose eyes are
motile, the eyes will follow any moving object; this is
adience of the eyes. Now in the progress of our game,
when the child moved, say, his right arm out to the right,
his eyes were led to follow the movement (vis-à-vis) of the
older person's left arm out to *the child's* right: similarly
for movements to the child's left, and up, and down. Thus
the *proprioceptive* afferent impulses (returning to the cen-
tral nervous system) after any movement of the child's

eyes (right, left, up, or down) will get motor outlet into muscles that carry the child's hand or leg in the same direction (right, left, up, or down). But these are the four components of any movement that a person standing vis-à-vis to the child can make in that right-left plane in which each sees the other. Hereafter the child will tend to *imitate* any movement in this plane which he *sees* another person execute. Although this depends on no new physiological principle, but merely on Pavlov's law, it is more than iteration, echo or 'simple imitation': it is empathy [5] or empathic imitation through the mediation, as I believe, of proprioceptive impulses set up by adient or exploratory eye movements. This, it seems to me, is genuine imitation.

Thirdly, our game with the child sheds some little light on the process known as 'leadership.' When the game is in full swing it becomes at times ambiguous as to which individual is leading, and which is copying, the other. Here the *timing* of the movements becomes important. In a rhythm which two persons execute nearly simultaneously, any movement of one person obviously both follows and precedes a movement of the other: but it may follow more closely than it precedes, or *vice versa*. Obviously too the person whose movement most closely *precedes* that of the other person, will be he who most stimulates the other's movements. Introspectively also, I believe, one 'feels' that one is the 'leader' when one's own movement just precedes that of the other, when one has gotten it in ahead of the other. All this is in agreement with the popular notion that the leader always does everything first, always initiates, is the first to suggest, etc. But in this rhythmical game we see leadership reduced to its very lowest terms, and another aspect of it appears which is *not* included in the popular conception, but which *is* present in every case of practical leadership. At any moment, namely, it can become am-

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[5] The excellent term 'empathy,' as an English equivalent of the German *Einfühlung*, was introduced by the late Professor Edward B. Titchener (1909, p. 21).

biguous as to which of the two persons (or parties) *is* leading; and at any moment the rôles of leader and led may be suddenly exchanged. We saw that at the very outset of our game the would-be leader must commence by imitating that which the child whom he hoped to lead, was *already doing*. And the leader's real aim was not to do gymnastics, as it seemed to be, but the quite ulterior one to develop imitativeness in the child. Now all this is invariably found in practical leadership. And it is no accident or 'regrettable exception' that practical leaders are men who have no convictions of their own, always "with their ears to the ground," and all but breaking their necks by looking round to see which way the crowd is going, so as to know in what direction they must next 'lead.' Nor is their own chief aim ever that which they profess that it is.

From the second to the fifth years of life the activities of a child consist very largely of echoing and imitating, though he is still engaged in acquiring further habits of adience and avoidance.

INSTINCTIVE AND APPETITIVE DRIVES

AN organism equipped with adient and avoidant responses, with chain reflexes, movements of progression, equilibration, and the other varieties of adience that we have so far studied, is ready to do much. The neural paths of lowered resistance which mediate these responses are 'engrams': but, as we have seen, they have all been learned and not inherited. They are all reflexes. And with this elementary equipment the animal begins to evince what must be called 'conduct.' It is the various acquired habits of this rather elementary sort, which have commonly been called 'instincts'; and this word is a good one if we remember that it is merely a name for these activities, and that it does not explain their mechanism or their causal origins. These origins we have briefly surveyed in the foregoing pages; so that we are in a position now to speak of instincts and to mean thereby, not faculties, but the activity of reflex mechanisms whose function we can define and whose structure we fairly well understand. In doing this, we shall not be repeating the error of the instinct-faculty psychology, for whatever we try to explain will be explained not by the *name* of the phenomenon but by the physiological mechanism which produces it (*cf.* Craig, 1918).

The instinct psychology, besides treating instincts as explanatory causal categories and as inherited, commits the third blunder of attempting to list them in so and so many 'fundamental' groups. And this is as factitious as would be an attempt to list the 'fundamental' shapes of constellations in the sky. There are no outstanding groups and, as Prof. Dewey (1922, p. 131) has said, no grounds for restricting "original activities to a definite number of sharply demarcated classes of instincts." . . . "For any activity is orig-

inal when it first occurs. As conditions are continually changing, new and *primitive* activities are continually occurring. The traditional psychology of instincts obscures recognition of this fact. It sets up a hard-and-fast preordained class under which specific acts are subsumed, so that their own quality and originality are lost from view" (p. 155). It is not to be wondered at that the 'fundamental instincts' as listed by the various writers on the subject differ so preposterously. Moreover, there is no thoroughgoing uniformity of instinctive behaviour from individual to individual, and so far as there is any uniformity it is merely because the individuals of a species develop under tolerably similar conditions, but the uniformities are absurdly exaggerated by most of the writers (*cf. op. citat.,* Chapters V. and VI.).

As a result of his studies in the psychology of the Hymenoptera Dr. L. Verlaine has invaded the central citadel, as it were, of the 'instinct' psychology; for it has been customary to assert that the insects are so far the perfect examples of distinct, phylogenetically invariant and inherited instincts that they exhibit no other activities. They have been supposed to be incapable, at any period of their lives, of learning; while at the same time the marvellous 'perfection' of their preformed mechanisms ('blind instincts') has been held up for admiration. Speaking of wasps and bees, Verlaine (1925, p. 15) says, "The psychology of these insects differs from human psychology only in the simplicity of its manifestations, although these are far from being as elementary as has been often supposed." . . . "The psychology of the Hymenoptera and human psychology are ruled by the same laws and determined by natural factors. There are not two psychologies, that of animals and that of men, but one general psychophysiology with its manifold and varied aspects" (pp. 18-19). Having insisted (1922, pp. 15-16) that, "There are no innate characters" ("The class of 'innate characters' and that expression itself should be abolished. There exist only acquired characters"), Ver-

laine goes on to say (1925, p. 16) : "The numerous criteria of instinct that have been so far proposed, have all at various times been found gravely defective." . . . "Instinct and intelligence are labels placed by us on groups of phenomena which are separated only in our mind but not in that of the organisms producing the phenomena; and while these phenomena may differ considerably if considered in their final form, they do not so differ when one studies their causation and their mechanism" (p. 19).

It is worth noting, further, that the lists of 'fundamental instincts' tend to be absurdly prettified. So that for the future convenience of instinct-faculty psychologists I will suggest a few obvious additions, all being of course 'strictly innate and inherited':—the instincts of wanton destructiveness (surely an authentic 'instinct' if any is), ostentation, greed, hatred, mendacity, treachery, larceny, cruelty, self-mutilation (apparently an exclusive attribute of *homo sapiens*), lechery and the whole array of sexual perversions. Let these not be neglected when we discuss 'our God-given inheritance.'

Among the instinctive activities many of the older writers, as for instance Erasmus Darwin (1794, vol I., pp. 124-5 *et passim*), drew a distinction which of late has been too often ignored by psychologists. This distinction was preserved by Alexander Bain (1864, p. 255) when he divided the simpler forms of habit into 'instincts' and 'appetites.' Some stimulators, namely, are contained within the organism's own body, and these, being inescapable, force the organism to activities which Bain, following precedent, called 'appetites.' Bain's list is: the appetites for food (including water and oxygen), exercise, sleep and repose, and (developing later) the sexual appetite ("Sleep, Exercise, Repose, Thirst, Hunger, Sex"). It seems to me that there is some question about an appetite for exercise; though Erasmus Darwin (1794, vol. I., p. 250) also mentions a 'propensity' to action due to "accumulation of the spirit of animation." Circular reflexes, as we have learned, when

once stimulated will perpetuate themselves until inhibited by some other reflex, and this gives the appearance of an autonomous 'craving for exercise' on the part of the organism. I suspect this to be the phenomenon which Bain had in mind. An activity like this which must first be stimulated from outside, but which will then continue its own stimulation, is clearly intermediate between an instinct and an appetite; and strictly is neither the one nor the other. To the list must be added defæcation and micturition, and in woman an appetite to give suck to her offspring. This last was included by Darwin.

While biologists and physiologists, in general, recognize the importance of distinguishing appetites from instincts, internally stimulated activities from externally stimulated activities, there are relatively fewer contemporary psychologists who, like Dr. James Drever, realize the significance of this distinction. In a discussion on the classification of the instincts, at the Seventh International Congress of Psychology (Oxford, 1923) Dr. Ernest Jones (1924, p. 229) said: "The main hint Dr. Drever himself gives of a classification is after all into two groups only, the appetitive and reactive ones. The distinction is evidently a valid one, though how useful it may prove is another matter. As a criterion to be applied to the whole gamut of instincts it could be justified only if the difference between the two classes is both fundamental and important. Clinical observations make it doubtful whether the difference is fundamental. Thus many manifestations, for instance most sexual ones, are both appetitive and reactive." Dr. Jones seems to forget that the *reactive* apparatus is of course necessarily the same for both instincts and appetites, namely, the bony skeleton with the muscles that actuate it. How else could, say, the hunger appetite 'manifest' itself? The distinction, however, is assuredly both fundamental and important: fundamental, as the distinction between extero- and intero-ceptor sense-organs; important, because a self-contained (interoceptive) irritation can be allayed only by very specific modes of

response, and not along the ordinary lines of 'avoidance.'
It is precisely, I take it, because the appetite stimulators
always travel with the organism and so continue to irritate
it, that repression of the sex appetite, for instance, is so
dangerous. That Dr. Jones should minimize such a distinc-
tion is rather amazing, but instructive. The whole Freu-
dian psychology, with its Herbartian slant, has in fact per-
sistently (and I incline to say completely) ignored the
physiology which some modern psychologists have so
profitably consulted. It is no wonder, then, that Freudians
are a little hazy as to just what should or should not be
repressed; that, for instance, after discovering that sex
repression is the source of most functional nervous disor-
ders, they can proceed to *recommend* sex repression under
the sanctified name of 'sublimation.' An interesting and
logically necessary corollary of this sublimation doctrine is
that satisfactorily married men and women must be 'cul-
turally' the least effective!

The great point about the appetites is that since their
stimuli are contained within the organism, these cannot be
evaded by any ordinary avoidance response (locomotion,
etc.), but will keep the organism restless until it acquires,
by trial-and-error, very different and often intricate modes
of response which will allay the internal stimulation. The
appetites, when active, are imperative. And any reflex
habits which are acquired as motor outlet for the appetitive
stimuli, which may justly be called 'annoyers' in the physio-
logical sense (that is, stimuli which produce a general rest-
lessness), are bound to be touched off spontaneously and
at more or less regular physiological periods: whereas re-
flexes that are released by external stimuli often go for
years without being activated. Thus an ability to speak a
foreign language (for this also is reflex) may lie fallow for
many years, and gives no trouble; not so an appetite.

While the distinction between instinct and appetite is
solely one as to the source of stimulation, all appetitive

stimuli being resident within the organism, and while this has its implications, yet in both cases alike that which is stimulated and driven to action *is the general motor, and indeed the sensori-motor, apparatus of the organism.* It is not necessary here, nor is the writer competent, to treat of the internal appetitive processes in any great detail; specially since here the vegetative or autonomic nervous *system is more or less involved.* Very broadly speaking, however, three problems are here presented: first, as to the actual appetitive stimuli; second, as to the receptor organs which these stimuli excite; and third, as to the response patterns which develop as outlets for the afferent appetitive *impulses.*

The appetitive stimuli appear to be of two sorts, mechanical pressures and chemical substances. The former occur in the case of 'hollow viscera' (*cf.* E. J. Kempf, 1918, pp. 25-8; G. H. Wang, 1923, pp. 1-25), that is, of secretory and excretory glands which possess regular ducts for the discharge of their contents. The accumulating contents exert a pressure (distention) on the walls of these glands, which *initiates the physiological 'craving' for discharge.* The appetites for defæcation, micturition, lactation, and the sexual appetite appear to depend on such a mechanism. On the other hand the appetites for sleep (repose), oxygen, water, and other foods appear to originate from chemical *stimuli contained,* probably, in the blood. In the case of sleep the stimuli are probably chemical products of fatigue, and particularly perhaps those which the blood has received from fatigued muscle tissue (lactic acid?); in the case of oxygen-need the deficiency of oxygen in the blood leaves an excessive hydrogen-ion concentration, which is the appetitive stimulus;[1] with water depletion the serum of the

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[1] Just possibly, however, it is the depleted air itself which acts as a chemical stimulus. In one place Sherrington (1922, p. 346) speaks of "the delicate mechanism linking the condition of the air at the bottom of the lungs with that particular part of the nervous system which manages *the ventilation of the lungs.*" In this case the 'craving' for oxygen would not be in the strictest sense of the term an appetite. Or again, both modes of stimulation may be operative.

blood carries more crystalloidal substances (salts) than normally, and these are the stimuli of thirst (*cf.* L. R. Müller, 1924, pp. 530-7).[2] These last two cases are conveniently termed 'deficit stimuli.' The appetite for other foods is doubtless due to similar deficit stimuli (Müller, 1924, p. 526) ; and here it may be questioned whether there is not a considerable variety of specific stimuli, as R. Turró (1911, pp. 2-16) believes, corresponding to a deficiency in the blood of various essential food elements: whether, too, these deficit stimuli may not be identical with some of the 'hormones.'

As physiological annoyers or appetitive drives, the deficit stimuli should be capable of stimulating, *i.e.* of initiating nerve impulses in, some portion of the sensori-motor system.   Yet I am not convinced that any of the chemical constituents of the blood (deficit stimuli, hormones) have been conclusively shown actually to excite nerve impulses at any points in this system—although it is a widely accepted view that they do so.   Perhaps the best established case of hormone action is that of adrenaline which, as has been shown by W. B. Cannon and others, in a series of papers, intensifies (sensitizes?) a variety of physiological processes all of which in turn facilitate the (otherwise excited) muscular activity of the organism, and depress digestive and other 'vegetative' processes (*cf.* Cannon, 1915, pp. 36-65; Cannon and Britton, 1927, pp. 460-3; Cannon, 1928).   It seems uncertain whether adrenaline or any other hormone directly excites the central nervous system, though it may well be that this and several other hormones indirectly qualify behaviour in various ways.   Some of the physiological effects even of adrenaline are under dispute (*cf. e.g.* Takayasu, 1916).   And at the present time the functions of the endocrine (ductless) glands are in general too obscure to be brought very definitely into our picture (*vide* Swale Vincent, 1922).

[2] Here too there may be another, more properly external, stimulus— namely, a parched condition of the mucous lining of the mouth and throat (*cf.* Cannon, 1918).

As for the receptor organs, secondly, which the appetitive stimuli excite, they are in the case of hollow viscera thought to be sensory cells lying in the walls of the viscus, which are mechanically compressed (or stretched?) when the walls of the viscus are distended by accumulated contents. It may be questioned, incidentally, whether this pressure as exerted on the sphincter muscles of such viscera, does not primarily assist in keeping these sphincters contracted (stretch or myotatic reflex) ; if so, the evacuation of such viscera would have to be effected by nervous impulses from some other source, such as the pressure receptors in the walls of the same viscus, coming to inhibit the myotatic contraction of the sphincters. Such a reservoir would be 'self-tripping,' while the contraction of the sphincters might still receive extraneous re-enforcement or inhibition from remoter parts of the nervous system. This is partly conjectural; but it seems to be certainly ascertained that when the hollow viscera in question are distended, pressure receptors pertaining to them are stimulated, and so send afferent impulses to the central nervous system, which, if the viscus remains distended, produce a general restlessness of the organism.

The chemoceptors which are excited by 'deficit stimuli,' presumably in the blood, are generally believed to be various 'centres' or motor ganglia in the mid-brain or medulla, each of which 'controls and regulates' such muscular activity as will repair the deficit by which each particular 'centre' is specifically stimulated (Bayliss, 1915, pp. 630-5; L. R. Müller, 1924, pp. 526-537). Thus in a depleted atmosphere an organism will have too little oxygen in its blood and an excessive hydrogen-ion concentration, which latter, according to current notions, will stimulate chemically its central 'respiratory centre' and so produce both faster and deeper breathing. Or again, the temperature of the body is supposed to be regulated by a 'thermal centre' which is directly and selectively stimulated by the temperature (!) of the blood.

This altogether prevalent conception of medullary and mid-brain 'centres' which, as their normal function, are selectively stimulated by various chemical constituents of the blood, and then 'regulate' the various appetitive activities, is supported by experimental evidence, of a sort; and it is doubtless true that there are in the central nervous system fairly definite regions ('centres') which if artificially stimulated will exalt or depress this or that vegetative function. Yet I am obliged to believe that this theory of 'regulatory centres' is mere word magic. The notion of "breathing regulated by a breathing-centre," or of feeding regulated by a hunger-centre, may seem wonderfully adequate provided that one does not see that it is wonderfully and absolutely verbal. It presents the same methodological disabilities as do the notions of talking regulated by a 'speech-centre,' thinking regulated by a ratiocinative centre, and philoprogenitiveness (F. J. Gall) by a bump of philoprogenitiveness. An animal nervous system is a network of conduction paths, where impulses that come in go continuously through and out; and any imputation of 'centres' lying between afferent and efferent paths is, if you consider it squarely, a survival of ghost-soul psychology—which is to say, of theology. To retain the 'centres,' as does the theory under consideration, and cut out the afferent paths by making the 'centres' also chemoceptors, permits the optimist to hope that the centres will 'regulate' everything most intelligently, and that the motor strands therefrom will have been connected (by heredity or Divine Providence, or both) with precisely the right effector organs—the optimist, that is, who is also a mere verbalist.

In the general animal economy the cells of the central nervous system are specialized as conduction tissue, while sensitivity (the initiation of nerve currents), and selective sensitivity to specific sorts of stimuli, are the function of sense-organs. The histology of ganglia of the central nervous system does not suggest their functioning as specific sense-organs; and I am unacquainted with any reputable

evidence that these ganglia differ from one another, chemically or otherwise, as they undoubtedly must differ if they are to have that selective, and very specific, irritability which the theory imputes to them. In short, it seems to me that the safe and sober view here is that the deficit stimuli (chemical) probably exist, that they may (or may not) exist in the blood, that they stimulate specifically different receptor organs, and that at present nobody has the faintest idea what or where these receptor organs are.

Our third point for consideration is the response patterns which develop as outlets for the afferent appetite impulses. The appetites pertain primarily to the internal animal economy, and the first path of motor discharge (path of *lowest* resistance) for any appetitive stimulus is usually on autonomic channels and to effector tissues that are in close relation to the organ from which the appetitive afferent impulses come; giving rise to internal processes of a relatively local character. The case of oxygen hunger will illustrate this. By a moderate deficiency of oxygen in the air some chemoceptors (perhaps in the lungs: *cf.* Sherrington, 1922, p. 346) are stimulated and these afferent impulses find their readiest (most deeply canalized) motor outlet into the muscles that inflate and deflate the lungs, causing heightened breathing. But if the oxygen deficiency becomes more extreme, the chemoceptors in question are more strongly stimulated, and their afferent impulses now *spread* to the general body musculature, and the animal becomes restless. It remains so until its movements bring it into a region of fresher air (or else, until it dies of asphyxiation and exhaustion). If we undertook to study these more or less local appetitive mechanisms we should be led to consider the entire internal (vegetative) economy of organisms; and of course this is not our present quest. It is my belief that all of these internal reflex processes are *learned* from random beginnings, in ways strictly analogous to those which we have studied; but they are learned during the fœtal growth, when function and organ are developing hand in hand.

For, as Prof. Child (1921, pp. 114-5) has said, "development is a process of functional construction." The embryonic behaviour pattern survives in later life partly as the vital organs, and partly as the (now stereotyped and automatic) functions of these organs. (It would be intelligent, I think, to *look* for the appetitive chemoceptors in these vital organs.) But this great industry, largely automatic as it is, is not isolated from the rest of the animal economy. Its afferent impulses normally spread (and when intense spread very widely) into the great skeletal musculature.

The afferent appetitive impulses which spread more widely in the nervous system, and so reach the general body muscles, are of course those which produce the open and obvious behaviouristic manifestations of appetite. And here it is plain, particularly in the cases of the hunger and sexual appetites, that the habits learned under appetitive stimulus are learned by trial-and-error. As Turró (1911, p. 49) has said, all the early impulses of newly-born vertebrates are 'blind' impulses.

The first outward manifestation of any appetite, in young organisms, is general random restlessness. In a totally inexperienced young individual of any of the higher animals the restlessness of hunger would have no other termination than death. But maternal care, guaranteed in the case of mammals by the mother's mammary appetite, carries the young offspring safely on to a point where it has acquired such ('instinctive') activities as locomotion, smelling, looking, listening, seizing, eating and the like. These activities are now the ones which, at the time of weaning, are excited by internal deficit stimuli, and the young animal prowls about restlessly but randomly, smelling, looking, listening, with a very fair chance that one of these adiences will lead it to food. Lastly, the swallowing of food allays, though not immediately, the deficit stimuli, and the hunger cycle is completed. But the appetite is inexorable, and if food is not secured the restlessness becomes more and more ex-

treme, with brief remissions, until finally the animal becomes too weak for further exertions, goes into a state of coma, and dies. The appetites for oxygen and water, depending likewise (in all probability) on deficit stimuli, function in a similar way.[3]

The remaining four appetites (for copulation, lactation; defæcation and micturition) all depend, it is fairly safe to say, on afferent impulses of pressure arising from the distention of hollow viscera, which is due to accumulation of the respective secretion or excretion. They appear to be alike further in that in each case a tonic closure of the viscus by a sphincter muscle (myotatic reflex?) has first to be acquired: and the pressure or distention is superimposed on that. Then, as in the other cases, these afferent impulses reach the central nervous system by paths which have at first no established motor outlets, and so, although the impulses are not at first of high intensity (as they are in the case of 'noxious' stimuli), they are persistent enough to produce a general motor restlessness, which will cease only when (according to the trial-error-and-success pattern) the random activities have brought about a release (inhibition) of the sphincter muscles of the hollow viscus. In all these cases the relief-giving activity is vastly more elaborate and specific than mere locomotion, and the period of trial-and-error learning is correspondingly protracted. This learning, too, is subject to grave idiosyncrasy and mishap.

The sexual appetite is so hedged about by taboos, super-

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[3] Several investigators (Cannon and Washburn, 1912; Cannon, 1915, pp. 251-264; Carlson, 1916, pp. 9-13, 62-71; Richter, 1922; Müller, 1924, p. 526) see in the so-called 'hunger contractions' of the empty stomach the probable cause of conscious 'hunger pangs,' and of the appetitive hunger restlessness. It may be so, but the cause producing the hunger contractions is not, so far as I can learn, satisfactorily made out. And as Carlson (1916, p. 63) has remarked, whatever causes the hunger contractions could just as well cause the hunger sensation (and restlessness) directly. Cannon (1918) and Müller (1924, pp. 530-537) assign an analogous rôle to 'thirst contractions' of the œsophagus, which appear when its mucous lining becomes parched.

stitions, and lies, that any true statement about it will meet with a thousand frenzied denials. So if I say that the sexual appetite, like all other appetites, is imperative and like the others does not brook indefinite delay, there will be loud cries of dissent, to the general purport that "pure thoughts, hard exercise, cold water and prayer" will divert the sexual appetite for a lifetime. To those in whom the sexual appetite arouses 'impure thoughts' I make no doubt that cold water and prayer will prove beneficial. For indeed I suppose that the most, and perhaps the only, impure thought connected with sex is the thought to perpetuate the stereotyped lies about it. But if there is anybody who honestly imagines that this appetite ever has been or ever will be beguiled by any such precautions, he should find his attention instantly claimed by the attractive themes of sexual perversion and the neurotic disorders, and *why they happen*. He will also find it interesting and profitable to study the phenomena of 'cultural' mendacity, and the 'as if' hypocrisies. And yet even the Freudians, who ought to know better, follow the accepted superstitions with their theory of sexual 'sublimation'—a colossal blunder—and the unctuous rubbish about 'cultural' (*kulturelle*) aims. Of course, if by artificial, 'educational' means this or any other appetitive restlessness *is* drafted off into motor channels other than those that appease the appetite, the individual is successfully wrecked.

In short, all of the appetite drives are persistent afferent impulses coming from organs situated within the body, and producing (in addition to visceral and other internal processes) at the outset, that is previously to trial-and-error learning, merely random movements of general restlessness. These impulses commence at a mild intensity, but if the appetite remains unappeased they gain in strength (often with rhythmical remissions of longer or shorter duration) until, unless the animal chances to do something that allays the appetite, he perishes. This chancing is a process of trial-and-error learning, exactly similar to that

by which avoidance reactions to overstrong external stimuli are established; except that in the latter case some form of locomotion is very often that which terminates the annoying afferent stimulation, while the appetitive annoyers are appeased usually only by a more highly differentiated course of action. As in all trial-and-error learning, the 'end' is set in advance, that is, the restlessness of an appetite will not cease until the internal excitation is abolished. Notwithstanding that all conduct which is driven by an appetite, and particularly so in the cases of hunger and sex, has strongly the appearance of *seeking* an end (in fact later it transformatively becomes this), it is important to remember that in its primary mechanism such conduct is always *avoidance*. Perhaps all learning by trial-and-error is at bottom learning to avoid.

In his chapter on *Instinct* Prof. Holmes (1923, pp. 108-9) remarks: "Several modern writers have over-emphasized the element of responsiveness in instinct, as if an animal were like an instrument played upon by outer forces and had its actions fatally determined by the action of those forces on its own inner mechanism. Other writers have treated instinct as determined by a sort of internal impulsion. . . . It is without question that internal states form the promptings of many instinctive acts. . . . It is of course difficult in many cases to ascertain whether activity results, perhaps indirectly, from outer stimulations or from internal changes." And very often, of course, the sensory pattern is made up of impulses from both these sources. The fact that appetitive stimuli, when present, are persistent and of increasing intensity until the appetite is 'gratified,' after which they are for a while probably completely absent, fully justifies Bain's use of a separate term, 'appetite,' for internally initiated activities.

RESPONSE TO MILD ANNOYERS: ABIENCE WITH ADIENCE

THERE are several external annoyers of a mild yet persistent character which present a slightly different problem from that of appetitive learning. Two of these are atmospheric warmth and cold. Within a moderate range of outside temperatures, the internal temperature of the body is kept nearly constant by an automatic regulatory process. But temperatures that are above or below this range produce a physiological restlessness (nervous overflow) that resembles appetitive restlessness, and in general becomes more pronounced the longer it lasts. An animal in this situation wanders restlessly about, and at random if this is its first such experience, until it reaches some spot where a temperature more nearly approximating its normal ceases to stimulate it and where its restlessness, therefore, subsides. Such natural inclemencies as wind, rain and sleet, or widely disseminated odours such as the powerful stench emitted by a skunk or the fumes that hang about large smelters and other industrial plants, or again the monotonous din in the near vicinity of factories, or the irritating texture of some fabrics that are used for underwear, the scratchy quality of a foot-path overgrown by brambles, and even the joltiness of uneven ground beneath one's feet, all are mild physiological annoyers that lead, sooner or later, to some degree of avoidance.

In regard to the avoidance of overstrong stimuli, I suggested that the random movement which chances to carry the animal out of range of the stimulus, the successful avoidance movement, is stamped in and fixed as a response engram because the successful movement is the one last

made, and the overstrong stimulation has done enough in-
jury to tissue to leave a milder after-stimulation that con-
tinues to flow in on the same sensory paths and that will
flow out over the last-used motor paths, to the muscles that
brought avoidance. Hence if this sensory pattern is re-
peated, even at low intensity, it will go directly over into the
avoidance response without production of random move-
ments. It is well known that a *single* experience in which
there is tissue injury suffices to stamp in an avoidance
movement in young organisms. The animal frequently re-
mains for some time in its avoidance posture, and a slight
manipulation of its limbs reveals that the posture is main-
tained under considerable muscular tension. A successful
appetitive avoidance (*i.e.* 'gratification') is stamped in sim-
ilarly, for here although the appetitive stimulus, save in ex-
treme cases, is not overstrong, it continues *while* the appe-
tite is being gratified, and so gets its motor outlet in the
direction of these successful activities.

In the case of the milder forms of external annoyer
there is commonly no tissue injury, and so no after-stimula-
tion to stamp in the last, the 'successful' movement. There
is sometimes but by no means always a mild overstimulation
which is strong enough to 'overflow' into antagonistic mus-
cles and cause immediate reflex reversal. Furthermore,
there can hardly be, in the case of the mild external annoy-
ers, any specific invariably 'successful' movement. Atmos-
pheric warmth and cold, for instance, prevail in a wide area,
and cannot be avoided, as can a definitely located object, by
any single response act (of direct withdrawal). Often-
times one can observe, even in adult human beings, that a
mild annoyer will keep the individual restlessly on the move
until quite by accident he finds himself out of range of the
stimulus, and so no longer annoyed. This is exactly analo-
gous to some cases of negative phototropism in the minutest
organisms. Perhaps nothing is here learned: certainly no
'successful' movement is stamped in. But again, diffuse
and undirected energy in the nervous system will touch off

whatever engrams are there, and so a restless animal will do the things that it has learned to do; and these are largely acts of adience. If then an object for which it has a pronounced adience happens to stimulate the senses of a restless animal, this adience will be re-enforced by the (random) annoyer: its restlessness will turn into very vigorous adience. And thus abience from the mild annoyer and adience to some other stimulus will become one act. I think that I have observed this in animals often enough: and one can observe it introspectively. Trudging homeward through the snow on a wintry night one is focussed adiently on one's house as soon as it comes into view: but (if introspection is worth anything here) the frosty air is fairly propelling one in that direction.

There is here, however, no salutary principle of selection. And when one remembers that very often these mild annoyers are not avoided at all (since many individuals become inured to them), one will hardly expect to find here any sharply patterned modes of avoidance, or any simple principle which should develop such patterns. It seems to me that mild annoyers are specifically avoided only when they become *obstacles*, directly hindering an individual in something that he wishes to do. Thus frosty air is an obstacle to manual action if it benumbs and stiffens one's hands. The surmounting of obstacles by trial-and-error learning we shall consider in the next chapter. Except when they act as obstacles, mild annoyers merely continue to play on the organism until, with habituation, their afferent energy becomes diffusely canalized into the general life activities of the creatures. Herewith they cease to annoy, and serve instead as a mild and diffuse stimulant to action ('dynamogenesis'). The motion of a ship's deck which at first annoys the land-lubber, soon becomes a sort of prop to the life of the sailor which the latter misses, sometimes sorely, when he is on shore. In general those who are inured to mild annoyers are annoyed by their removal. The same habituation would be possible to the mild appetitive annoy-

ers, and would presumably be the Freudian 'sublimation,' were it not that the appetitive annoyers if thus ignored soon cease to be mild; and become overstrong.

There is a popular notion in regard to some mild annoyers, that if only they arouse 'fear,' this latter will produce direct and violent avoidance responses: and 'fear' is of course 'hereditary.' It is regrettable that even physiologists will drop into this confusion instead of stating in *physiological* terms what they mean by 'fear.' Of course if a stimulus is once so overstrong that it produces a widespread and deep canalization of nerve paths (and they will be paths of avoidant response), then a mild repetition of the same stimulus will produce a widespread and apparently disproportionately intense motor response of avoidance; as one spark if it lighted many trains of gunpowder could produce a violent explosion. But 'hereditary fear' has cloaked so much loose observation, and even looser generalization, that it may be worth while to consider one or two cases in some detail.

An instance of the early avoidance of a mild stimulus is described by W. Preyer (1884, p. 120) as follows: "A hen with her first brood, which was about a week old, was (by Spalding-Douglas) frightened by releasing a young hawk. In a moment all the chicks were hidden in the grass and bushes." Again, Preyer himself tried a similar experiment with thirty-three chicks, hatched in an incubator and kept from any contact (*Umgang*) with older fowl, '*im geschlossenen Raum.*' Holding by the wings a young and very lively hawk (*F. tinnunculus*), about the size of a common rooster, he brought the hawk near the chicks; "they seemed at first not to notice him. But as soon as they heard his cry [*Stimme*], they all took notice, became quiet and moved hardly at all. Then I let the hawk loose: at once the chicks scattered in all directions and hid themselves. How, except by inheritance, did the chicks come to hide on seeing and hearing the [*beim Anblick und Hören des*] hawk?" (p. 120). I would point out that by Preyer's own account

the chicks *did not* hide on either seeing or hearing the hawk: on the contrary, they "became quiet and moved hardly at all." They 'hid' only when the hawk was allowed to fly. And one is justified, after such an inexactitude, in doubting whether they "scattered in all directions and hid themselves," or merely scattered in all directions and *came to rest,* perhaps, in darkish corners: for darkness makes chicks quiescent, while true *hiding* is a more complicated action, to be imputed to chicks only on clear evidence. Great care is required to determine precisely what an organism is really doing. Yet Preyer is one of the most scrupulous and reliable observers whom we can consult, so careful indeed that (unlike many another) he himself reveals that he was not quite careful enough.[1]

The two cases quoted require more observation. If one will watch incubator chicks from the instant when they emerge from the shell, one will find that at the very outset they do not avoid one's hand, they will run against it and peck at it, as at everything else. If one feeds them by scattering food in small handfuls, from the hand, and over a space of about one foot radius out from the hand, they will after four or five feedings run towards and on to one's hand, from all directions, when the hand is brought near the floor of the coop or brooder (as it is in scattering food), so that it is difficult to feed them without hitting some of them. After five or so more feedings those chicks that are near at hand, as this approaches the floor, will run away from the hand. This looks like avoidance. But at the same time the chicks that are farther away run towards the hand. Another glance reveals that *all* the chicks are running *towards* that circle round the hand (of two-foot diameter) where the scattered food is wont to fall. It is an adient response to food; but it involves that the chicks which are near one's hand must run away from it in order to reach

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[1] One need not, I think, worry oneself for an instant over "the moorhen's first dive" and many other remarkable 'first performances' and exhibitions of 'inherited fear.'

the food. What they 'are doing' is not avoiding the hand, but going to food. The stimulus is sight of the food or hearing the livelier peeping of the other chicks that have already found the food; and there may be other such conditioning stimuli. (Avoidance is here our topic, and I assume that the chicks have otherwise learned adient responses to sight, sound, etc.) But now for a chick that happens to be near the hand when food is scattered, some sight of the quick motion of the hand that scatters must immediately precede the sight of food and the sound of other chicks peeping, which call the chick away from the hand and towards the food. This response now becomes partly conditioned to the former stimulus (motion of the hand), so that soon the sight of a quick jerk of the hand contributes (with the peeping of the other chicks) to make the chick turn about and run away from the hand.[2] The response is comparable to that of a dog who leaps away from his master's hand as the latter throws a stick for the dog to retrieve; but it can easily be mistaken for a 'shying away' from the hand. Thus many an avoidance response to mild stimuli is produced by these stimuli being conditioned to, or we may say superimposed on, an adient response to some other object. In such cases the elimination of any random or superfluous movements is merely the obverse aspect of the slow conditioning of these stimuli to some adient response. There is here no distinct 'principle of elimination.'

Since Preyer and many others use 'fear' to explain many avoidance responses, the alleged manifestation of it in the two cases cited invites still further consideration. In the Spalding-Douglas case, week-old chicks hatched by a mother 'hid' in grass and bushes when a hawk was released near them, and Preyer cites the case as an instance of 'inherited fear.' First, as to the 'hiding.' Chicks very early acquire

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[2] Either stimulus will presently cause the chick to 'expect' food, *i.e.* to do everything that contributes to the motor posture of complete readiness to eat—a very modest instance of 'ideation.'

an adient response to the clucking of the mother-hen. It is also often a part of their sensory pattern when they are snuggled quietly under her feathers, in the nest: darkness is another part of this sensory pattern. In the yard when the hen clucks they run to her, and if she then covers them with her feathers they nestle down under her (in more or less complete darkness) and become quiet: but this is not hiding and it is not fear. The sound of clucking makes them run; clucking plus darkness puts them motionless (so-called Pavlovian inhibition). If a hawk appears, the hen clucks very loudly (and with a qualitative difference of sound, that counts with the chicks after a while) the chicks run to her, she ruffles out her wings to cover them, and walks with them huddling under her into the coop or other protected spot. Whatever sensory stimuli reach the chicks from the hawk will soon, as explained in the preceding paragraph, condition their running response (*to* the mother *if* she is there clucking), and the hawk stimulus *plus darkness* will condition their quiescence.

This last item is important, and I am stating it not as a deduction but from repeated observation. Many times when I have tried to catch a chick, a small bird fallen from the nest and able to run but too young to fly, or a young individual of any of the smaller wild quadrupeds, if the animal has gained cover in some dark little nook and I have come up to it, it has merely looked at me; and has often without the slightest protest let me handle it *until* I brought it out to the *light*, whereupon it would struggle with all its might again to get away. So long as it was in the dark it would act quite unconcerned, looking tranquilly at me and I looking at, or gently stroking it. Thirty seconds before it had been running from me in the 'wildest terror,' as I had supposed, and now when overtaken in its 'hiding' place I had expected it to show 'fear.' This phenomenon has often mystified me, and I never got the slightest clue to it until I began to look at these things with the conditioned reflex in mind. (I am speaking only of very young

animals, of course; an adult animal when 'cornered,' even in the darkest hole, is savage.)

The Spalding-Douglas case, at least as described, seems hardly to justify an interpretation in terms either of 'hiding' or of 'fear.' The hawk stimuli made the chicks run, and they stopped running and became quiet when running had brought them to dark spots under grass or other cover: there, hawk stimuli plus darkness (or darkness alone) made them remain quiet. To call this 'hiding' is to read something into the facts; but it is of course a good beginning towards that which will, later on, be true hiding. Not long ago I was walking with a friend along a woodland path. He was walking ahead when suddenly, at about a yard from his feet, a partridge flew up with a great noise and came to the ground about twenty yards away; then with loud cries, partly limping and partly by short flights, she took a curving course, the circumference of a circle, roughly speaking, of which we were the centre. Her first rise uncovered some ten or twelve little partridges (more than I had supposed a brood would contain) and these, too young to fly, ran in various directions. We can imagine how Preyer and others would describe the scene:—mother partridge terror-stricken by the 'ancestral enemy,' feigning disability in order to deflect our attention from her helpless young; these frightened, running wildly in all directions, and hiding! Howsoever, if the mother's 'inherited fear' had not started her up, we should have passed, ignorant of her existence: for her protective colouration had made her practically invisible. I cannot say certainly what the 'feigning disability' was, for I have had too little chance to study it: but I *suspect* that her movement in a circle was a compromise between two opposed responses; one, to avoid two men, the other, to keep near her brood; and twenty yards was her compromise distance. Such motor antagonism always makes for impeded, ungainly progress: and as she did not wish to progress, yet dared not (or was too excited to) remain still, she took the circumferential course. But

all this is conjecture. I think it certain that her loud out-cries were to the brood. And if indeed the mother was 'feigning disability' she was very silly, for she had just exhibited an unimpaired power of flight. The little ones scattered, and were soon lost to view in little shaded nooks among the leaves. But so little was this *avoidance* that one of them ran straight into the outstretched hand of my friend who had stooped, hoping to catch one. And I could easily have caught a couple of others if I had been willing to risk injuring them in the act of seizing. The one that ran into my friend's hand we examined at leisure (and then released undamaged) ; except for its dark greyish-brown colour I could not have distinguished it from a domestic chick three or four days old.

The second case of avoidance quoted from Preyer is very similar. On seeing the hawk when it was held by the wings, the chicks seemed 'not to notice him': on hearing his cry "they all took notice, became quiet and moved hardly at all." I should have to reproduce this situation and study it, in order to be able to say why they did this; but certainly 'inherited fear' is hardly in evidence. These incubator chicks were "three and a half weeks old," and at that age the whole brood (if not busy finding food that is already on the ground) will regularly approach to a 'respectful' dis-tance and stand quietly round as if waiting for food, when-ever the distributor of food makes his appearance. If not hungry, they do not peep. When the hawk flew, "the chicks scattered in all directions and hid themselves." We have seen how incubator or other chicks learn a response that looks like 'shying away' from the quick motion of the hand that scatters food. And this is often turned into a true avoidance response if the feeder carelessly strikes some of the chicks when he scatters the food (an overstrong stim-ulus). There are probably other, similar circumstances that teach the chicks to run away when they see any quick motion, for some sudden motion is apt to go with any over-strong stimulation. The young partridges scattered when

the mother flew up, but they were not running away from the two human intruders for several of them ran toward the intruders. There was every indication that they did not see my friend or me, and that it was the noisy flight of the mother that set them running in various random directions. Nor did the young partridges 'hide'; they ran until they found themselves in *darkish* nooks, where they stopped, and so were lost to view. But on looking round carefully I could see several, and they had more the air of placidly waiting than of hiding; showing no agitation or concern. The scattering of the young partridges when the mother flew up, which seems parallel with the scattering of Preyer's chicks when the hawk was allowed to fly, is simply, I think, the running response away from quick motion (the mother's) ; accelerated perhaps by the simultaneous sound stimulation from the old bird's flight and cries. Preyer adds, "When now after a long pause I let a pigeon fly over them instead of the hawk, they were just as frightened, scurried apart and hid themselves" (pp. 120-1). An 'inherited fear' of the 'ancestral enemy,' the pigeon? Surely in all these cases the imputation of fear is as unjustified as it is unnecessary; and further, if the 'emotion' were present, its power to produce an avoidance response would in turn need to be explained. Emotion and its relation to motion will be considered later.

The cases of avoidance response to mild annoyers so far discussed show, when extricated from erroneous interpretations, merely random restlessness conditioned to and so facilitating a previously established adient response. Since an organism can never go toward one thing without going away from other things, these other, and at the start *nearer*, objects are bound to figure in the organism's sensory pattern as it starts away towards its objective; such stimuli find *motor outlet then* (Pavlov) in the *motion of departure*, and particularly in the earliest phase thereof, *i.e.* the act of *turning about and starting* away. If the objects so left behind include any mild physiological annoyers, these pro-

vide an additional nervous drive (energy but undirected) to the act of turning about and starting away. It is clear at once that the principle involved in this situation, *i.e.* abient conditioned to adient response, unites in a rather unexpected way the two types of response that until now have seemed to be so distinct.

Leaving now the supposed manifestations of 'fear,' let us consider some of the aspects of learning connected with mild appetitive annoyers, taking first any young individual (say, one of the higher mammals) at that moment after birth when it becomes for the first time mildly hungry. The deficit stimuli provoke a random restlessness and, save for maternal intervention, nothing more: though there are cases that seem to argue otherwise. The chick, for instance, as the adherents of inherited instincts always point out, will peck corn or meal the instant it emerges from the egg: "a clear case of heredity." In fact, the chick will peck at pebbles, old matches, cigarettes, or even corn and meal as soon as it comes out of the egg; for the excellent reason that if it does not learn first to peck its way through the egg-shell, it never does come out of the egg. The chick learns the pecking reflex inside the egg even more inevitably than primates acquire before birth the grasping reflex or extensor thrust. Fortunately, too, the chick's neck is so placed that terrestrial gravitation will pull a few small objects down its throat, and so help to develop a swallowing reflex (which is never very adequate, if one may judge by the way in which adult fowl swallow water). But so little is the chick instructed by its 'heredity' that, unless there are more kernels of grain lying in its immediate vicinity than there are pebbles and old matches, it will die of pebbles and old matches. As Turró (1911, p. 47) has shown, it does not peck with any idea of obtaining 'food'; and the notion that any such instinct is 'inborn' is an illusion. Every chick can peck, just as every helpless little infant "inherits a loving mother, close at hand, to minister to its wants."

The young primate, restless with its first hunger, has lips that have acquired prenatally a grasping (closing) reflex, and the mother (as even the feline mother) sees to it that one of her breasts finds its way to the little lips. At first the human mother has usually to squeeze her breast and squirt milk into the little mouth. And here she will do well to hold the child so that gravitation carries the milk down the gullet; for the passage of food down the œsophagus by gravity is of very great assistance, if it is not indeed an indispensable factor, in establishing the downward peristalsis of the gullet. Here we have as *afferent* impulses the mild hunger contractions (physiological annoyer), such dermal, utricular and saccular, and joint stimulations as are produced by the infant's position in its mother's arms, stimulation of the infant's lips grasping the breast, and tactual and thermal stimulation of the membranes of the mouth, tongue, and gullet as little spurts of milk strike the tongue and mouth and flow down the gullet; and as *efferent* outlets contraction of the lips upon the nipple, some movement of the tongue and cheeks, and a downward peristalsis in the gullet.[3] This efferent or motor pattern constitutes the act of eating so far as the infant is as yet capable of that act. We know from Pavlov that every part of this sensory pattern is finding outlet into every part of the motor pattern. And the differences of permeability in this developing network of paths are such as can be deduced from the situation. Thus the pressure of nipple on lips has its old, prenatal outlet to the muscles for lip-closure well established; and is now beginning to acquire further outlet to the rest of the now-prevailing motor pattern. Since if the mouth chances (by some random intercurrent impulse) to press a little harder adiently against the breast a gush of milk into the mouth results, the stimulations (gustatory, thermal, tactile) which this produces on the tongue and mucous membrane of the mouth, will find the path for

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[3] There are, of course, minor processes going on such as salivary secretion, heightened inhalations, etc. *Cf.* W. Preyer (1885, p. 453 *et seq.*)

forward pressure of the mouth (face) especially open at that moment: hence this becomes a slightly more permeable outlet for such stimuli. This, then, starts a rhythm, of which the pacemaker is chiefly the time elapsing between forward movement of the mouth and gush of milk into the mouth (which varies with the fulness of the mammæ, etc.). Similarly, the reader can trace out for himself very many, greater or lesser differences of permeability that will be produced in the paths leading from any element of the sensory pattern to the various elements of the motor pattern.

Any form of the ingestion of food is a concatenation of reflex-circles, each of which has an additional motor outlet of just lesser permeability into the paths for the next following act in the concatenation. The needed connections and the correct permeabilities are first acquired (learned) under the guidance of the food substances themselves; they are, that is, an adaptation to the food. Such guidance, of course, continues; that is, the concatenation continues to be carried to some extent, and *timed*, by the food itself.

In the case of a tubular tissue (such as the œsophagus or intestine) provided with ring musculature and tactile sense-organs on its inner surface, each ring muscle has a proprioceptive circle that re-enforces its own contraction; and the random contraction of any ring-muscle will bring the inner sensitive surfaces together at that point, setting up a tactile afferent impulse that will further re-enforce the same contraction (very much as in lip closure). If now a lump of soft substance, such as masticated food, be introduced at the middle of such a tube its pressure will cause the ring muscle at the point to contract. This will squeeze the soft mass in two, and push the halves in opposite directions through the tube. If the half is pushed far enough to stimulate the inner surface of the tube under the next ring muscle, that muscle will next contract. Thus a peristaltic movement is started along the tube in both directions from the point where substance was introduced. In the œsopha-

gus the food is introduced at one end, the upper, and gravitation to some extent at least assists the food downwards, and helps in establishing a downward peristalsis. But as may be deduced, the time relations are very nice, and it takes but little to reverse the direction of the peristalsis. So far as I know, all actual peristalses are rather readily reversible.

But processes so intricate as ingestion and peristalsis cannot be altogether deduced in this easy way. The actual time relations and permeabilities are factors that must be empirically studied. The processes are also complicated, doubtless, by various reflex inhibitions.

It is probable that many well habituated chain reflexes can go on with proprioceptive arcs acting as the sole connecting links; as was described in the section on chain reflexes. But certainly in many cases, if not in most, the object dealt with continues to assist in the chain co-ordination (*cf.* Mangold, 1923, p. 395). One will notice a difference if one tries masticating and swallowing with nothing in the mouth, and again, with food in the mouth. I suspect that the difficulty which some persons find in swallowing a pill (an unusual shape and texture) is a similar phenomenon. And it is more difficult to swallow against the force of gravity than with it. It is possible from printed notes to make the correct finger movements on a silent piano; I doubt that any one could do this in the air, with the same accuracy. If one plays on a piano and then the same composition on an organ, one will see that the longer time elapsing between pressing a key and the production of sound, on the organ, considerably alters the concatenated process. Almost every game, outdoor or indoor, would become a chain reflex were it not that at frequent intervals the chain is interrupted by some object dealt with (ball, quoit, playing-card, chessman) calling for an act of calculation.

At all events, each unit of the feeding chain is an adient response, and the whole concatenation is adient in its effect

—which is the ingestion of food. But, as we have seen, it is when mild hunger makes the infant restless that the mother takes it up and herself starts the chain reflex of nursing; and the afferent impulses from the deficit stimuli find outlet into the *entire* motor process of nursing. One can observe that the infant's restlessness suddenly ceases when it commences to nurse, but that the nursing movements are now carried on most energetically. The unwonted efferent energy here is that which had previously gone into random restlessness: it is from the appetitive afferent impulses. It appears, too, that these latter find motor outlet into every part of the chain, re-enforcing each motion of the nursing reflex: for as soon as the nutriment deficit is allayed it is more difficult, and sometimes almost impossible, to excite any one of the component activities of the chain (*cf.* Preyer, 1884, pp. 111-115). This is an illustration of 'physiological state' modifying response. When the hunger contractions start up again they will raise the tonus of (subinnervate) all the muscles involved in nursing, and it will be easy to set in motion the whole concatenated response. Such preparation for a general course of action by preliminary augmentation of tonus in the muscles to be employed, seems to be illustrated in a large way by the alternate or even opposed relation between the sympathetic and the parasympathetic nervous systems. A heightened tonus of the parasympathetic system prepares the organism for ingestion, digestion, and the whole *vegetative* sequence; and tonus of the sympathetic prepares for activity of the skeletal musculature, thought, etc. (Hess, 1924, 1925). But preparation for action by antecedent heightened tonicity (Bahnung or undertone) is of great importance in psychology: it explains, among other things, what the psychologist calls 'mood.' We shall consider it later.

The process of nursing or eating has two results, deficit stimulations are stopped (avoided) and food is obtained. As in the case of hawk and chicks, a more or less mild physiological annoyer finds outlet into the motor paths of

some adient response, and so is eventually avoided. Here as in the previous case abience is superimposed on adience. I should be far from asserting that this is the only way in which mild annoyers are avoided, but it is certainly a rather characteristic way. All through life one meets objects, persons, conditions and situations that are more or less mildly annoying, and yet one does not 'know how' to avoid them, or does not 'care' to avoid them, until some *positive* (adient) course of action accidentally suggests itself as a way out, and then one adopts this course of action promptly and energetically. The mild annoyance augments one's energy for the project; and action is fed from two sources.

OBSTACLES: TRIAL-AND-ERROR LEARNING

THERE is an adage to the effect that a child will become strong and intelligent only through suffering privations and hindrances. And indeed it appears true of both animals and men that only those develop any great resourcefulness or practical intelligence who have met and surmounted a goodly number of obstacles. The process involved in this must be of significance as a case of learning, and will perhaps be instructive as to what intelligence itself is.

We are accustomed to consider an obstacle as a hindrance to the fulfilment of some want or desire of a living creature; and of course it is true that no creature meets an obstacle unless it is active about something. But if we were to put the psychological term 'want' or 'desire' in place of a description of the concrete process which suffers the hindrance, we should incur two penalties: first, we should lose track of the connection between the creature's stored (food) energy and its life career; and second, we could not expect to understand what happens, and why, when the creature's activity is obstructed.

A creature encounters an obstacle only when it is active. We know, therefore, that some of its sense-organs are being stimulated by external, perhaps also by internal (appetitive), stimuli; that these stimulated receptors are sending impulses along many nerve paths, canalized by previous learning and leading on to various muscles; and that the muscles so actuated are now impelling the creature to the actions which we are able to watch. Knowing as we now know a little something of the mechanism whose movements we are watching, we know that it is fundamentally capable of only two types of action, adient or abient, with regard

to the stimuli that are activating it; or perhaps simultaneously adient toward some and abient from other stimulations. If, for instance, the creature moves toward food and eats it, we know the essential features of the process which we are watching. By observing carefully *what the animal is doing* we can ascertain many, and usually the more important, of the external stimuli that are exciting it to action. For in general its responses are toward or away from objects which are at the time stimulating it.

The care and labour required for this are sometimes great, but consist, as in any other field of science, in getting definite concomitant variations; that is, in ascertaining what stimuli by being present or absent make a difference in the creature's actions, and what the difference is. With advancing age of the animal, owing to cross-conditioning and 'ideation,' two physiological processes which we have not yet discussed, it becomes less and less feasible to ascertain the actual exciting stimuli in any given case of conduct. It also becomes in many respects less and less important. It will be advantageous, I think, to consider the surmounting of obstacles by trial-and-error in young organisms, where these complications play as yet no decisive rôle.

Some obstacles are so simple as to necessitate merely a swerving to one side on a direct line of progress towards a steadily visible goal. The obstacle seen, excites an avoidance reflex when the animal comes near enough, which merely qualifies momentarily the adient response that is maintained by the visible goal. Such avoidance of *too close* proximity to the obstacle is, of course, that which the animal has learned from early experiences of bumping, falling, knocking, etc., against hard and rigid objects; *i.e.* from previous overstimulation. The course which the animal now takes is a resultant of the two motor tendencies: and such a geometrical resultant seems to be a natural consequence of the algebraic summation of nerve impulses.

But other obstacles are sufficiently positive to bring the animal's adient progress more or less to a standstill. Here

learning will begin. And, so far as I am aware, all observers agree that this learning is by trial-and-error.

If we take as a simple illustration a moderately hungry cat in a good-sized box with bars on the front through which the cat can see a plate of food, and set in the centre of this side a door held to by a simple latch (which the cat could easily raise if it knew enough to try), we can observe at once that the cat, even though it avoid all contact with the bars, remains adiently set toward the food: and if it sits or lies down it will still face the food. So continuous is this adient motor adjustment that a hungry animal will often tire itself out in efforts to get through the bars without once seeing an open exit from the box on the side that is farthest from the food. We must note exactly what the cat's situation or 'problem' is in physiological terms. The food, seen or smelt through the bars, stimulates such adient reflexes toward itself (progression, prehension, mastication) as the cat may have previously acquired. The bars and other appurtenances of the cage, through the senses of sight and touch mainly, stimulate such reflexes (adient or avoidant) as the cat has previously acquired in its explorations of similar objects (to seize the bars, to climb the walls of the cage, to sniff, to push, etc.). In other words, each object in the cat's environment, including the food, so far as it stimulates the cat's sense-organs, excites in the cat reflexes peculiar to itself.

But now the cat is also 'hungry.' Its having on previous occasions sought food and eaten while hungry, that is while the deficit stimuli of hunger were producing afferent nerve impulses, has brought it about that these afferent impulses have found motor outlet into all motor channels that it has so far learned in getting and consuming food. A hungry cat when no food is in sight will look about, lick its chops, and make movements of mastication with an empty mouth. Its hunger, then, stimulates those same reflexes of adience toward food, which are stimulated also by the food now seen through the bars. So the cat's adience toward food,

sustained from two sources, becomes prepotent; hence the cat's persistent orientation toward the food. This holds the cat to the situation; keeps it 'seeking food.' Further, if the deficit stimuli of hunger become fairly intense, they will overflow into general motor restlessness, the cat's random 'trials' and 'errors.'

This prepotent adience, however, is *inhibited at the motor level* because at every turn some object (stimulus) intervenes which stimulates the cat to other reflexes, and which if pressed too hard will excite direct avoidance by overstimulation. In this inhibited phase of the learning process even if the cat appears to do nothing, its inactivity is a dynamic equilibrium; and labile. Frequently the adient impulses break through (especially as its hunger intensifies), and the cat tries to push out between the bars; at other times, avoidance of the bars or dalliance with some feature of the cage predominates. Its labile equilibrium is not a state of muscular rigidity because, as we have seen, the antagonistic motor impulses extinguish each other by interference at motor half-centres before they reach the muscles. According to that oversimplified scheme of the nervous system which we have so far relied on, and which assumes merely that any sensory cell is capable of acquiring functional connection with any motor cell, it should seem that this interference could take place only at the last synaptic region preceding the final common path (motor neurone) to any muscle.[1] But in this case such a muscle would be, at moments when the antagonistic impulses are exactly balanced, completely inhibited; and would lack even that degree of tonus which it has in the normal state of repose. It could not be set in action by impulses from any part of the nervous system. In fact, however, it appears that normally

[1] It seems not improbable that the interferences between the proprioceptive afferent impulses from immediately antagonistic muscles (which, as we saw in Chapter VIII., return to the same muscles, and in combination produce alternating movements of progression) do take place at this last synaptic region. Most antagonistic muscle pairs are anatomically and functionally very closely related.

in these cases of conflicting motor impulses (such as the one we have been considering) the muscles which would have been used by either the one or the other of these conflicting impulses, are quite at the disposal of reflex impulses from any independent source (*cf.* Sherrington, 1905 [b], pp. 805, 839). There is scarcely an active moment in the life of any organism when it is not subject to several such pairs of conflicting impulses, yet this does not give rise to any symptoms of muscular paralysis. It is probable that these interferences normally take place at synaptic regions 'higher up' in the nervous system, *i.e.* not immediately preceding the final common path. Probably, to, this is a point at which our oversimplified scheme of the nervous system (*cf.* Chap. VI.), by neglecting the more detailed topography of nerve paths, fails to account for the facts. (One is led to query whether motor half-centres, or their equivalent, for any given pair of antagonistic muscles do not exist at several segments of the spinal cord.) At any rate, observation of behaviour shows, and I think beyond any possibility of error, that when an organism is inactive by reason of motor conflict, many of its other reflexes, even insignificant and trivial ones, and even though they employ the same muscles, can be readily stimulated. And they are stimulated, in profusion, since the sense-organs of every animal or man are at every moment being bombarded by external stimuli of all sorts and from every direction. Any organism in so far as it is doing nothing is peculiarly susceptible to being started doing anything: a fact of wider scope than our present discussion.

It is not remarkable, then, that practically all observers agree that when any creature meets an obstacle, so that its major activities are suspended in this way, it goes at once to indirectly related, more random activities. These are now called 'trials and errors' because, in our example, it is said that the cat wants, desires, wishes, aims, intends, aspires, or wills to get to its food; and if it gets there, it gets

there—'success.' And now the crucial question is: how in such a process is anything learned?

A considerable amount of ingenuity has been expended by various investigators, in trying to puzzle out the answer to this question in the terms of mathematics. Perhaps as plausible and as clearly stated a solution as any, of this numerical sort, has been presented by Prof. Watson (1914, pp. 262-8; 1919, pp. 293-4). It is understood that an animal has been placed in a situation of obstructed activity (as the cat in the box, which we have been considering), and has had to try and try until it succeeded in surmounting the obstructions. This is repeated until the animal is able to surmount the obstacles without making any random trials, *i.e.* errors. In such experiments it has been found in fact that as, time after time, an animal makes its way past the obstructions and escapes, it does so with less fumbling, fewer and fewer false starts: and at last, when put in the same situation, it escapes directly. Random trials and errors are somehow eliminated, and the animal has learned the right movements to make, and in their right sequence, in order to escape. Now Professor Watson adduces a principle of frequency of occurrence (of the right or wrong movements), showing that with each repetition of the experiment every correct (successful) movement is bound to be repeated (and in the correct sequence) while any other, random movement (error) may or may not be repeated. This would give the successful movement (and in the successful sequence) a slight advantage in point of frequency. Now frequency of repetition, of course, confirms association paths; and even a slight advantage will be, in the long run, sufficient.

Dialectically, it seems to me, such a solution leaves little to be desired. But I have the conviction that to reduce such an interesting physiological process to a mere scheme of frequencies (numbers) is in all probability to overabstract, that is, to ignore significant aspects of the process. And after watching an animal, in a trial-and-error experiment,

repeat the same wrong movement over and over and over again, I begin to despair of mathematical 'frequency' ever coming to the assistance of the right movement. Certainly, frequency is all on the side of the error, of many errors, in the first repetitions of the experiment; and how shall the right movements gain even a foothold, to say nothing of the ascendency? Not by 'chance,' for if chance arrives on the scene, she will favour errors and successes impartially; and the errors, already in the lead, will even "in the long run" remain so. Certainly 'chance' is not going to hand 'frequency' to the 'successes.' I bring up this solution of the trial-error-and-success problem not merely because it appears to belong here, but also because it is a pretty illustration of number magic (common in the biological sciences) and will be useful to us later. It helps us not a bit toward understanding the process of trial-and-error learning. And I cite Prof. Watson's account, rather than one of many others, because it is more lucidly and convincingly presented than most of these others.

What helps us toward the solution of our problem is a significant observation of Prof. M. F. Washburn (1926, pp. 277, 334; 1928) : "Experiments indicate that in maze learning it is the movements nearest the 'success' that are earliest learned" (1928, p. 27). This means that if, in learning to surmount an obstacle or series of obstacles, a number of 'correct' movements must be concatenated in a certain correct sequence, this chain of reflexes will be built up (learned) from the later end backward; that is, in a time order the reverse of that in which these movements will be performed after the concatenation has been learned. From the point of view of number magic this observation means but little; and seems indeed to present an additional problem. From the point of view of physiology, however, instead of adding another problem, it goes a very long way toward solving the original one.

Let us return to our cat in a good-sized box with a latched door and bars on the front, through which the cat can see

and smell a plate of food. The cat will remain so persistently oriented toward the food that even if it should see an open exit on the side of the cage away from the food, the impulse to go out, that would be thereby stimulated, would probably be inhibited by the antagonistic motor set to face the food and to press ever adiently toward it. But in its random restlessness the cat may execute any reflexes which objects in the cage or parts of the cage stimulate it to, provided that such reflexes are not antagonized by its persistent adient set toward the food. Many such reflexes will be neither antagonistic to this adience nor synergic with it, but indifferent. It is to be noticed that *at this juncture no conceivable movement* of the cat *would be* for it *'successful' except the 'last,'* that is, the movement that actually releases its adience and lets it eat.

The significant moment, and the new step in learning as pointed out by Prof. Washburn, comes at the moment of actual *release* ('success') when for the first time the cat chances to raise the latch and push the door ajar, and its food-adience, stimulated all along from two sources (external and appetitive) but until now largely antagonized (inhibited) by obstacle-avoidance, is at last unimpeded and becomes overt movement, actual advance toward the food.[2] Here it must be that the factor which is responsible for the learning, is that which *physiologically differentiates* a 'successful' movement; and this, I think, is the sudden *release from inhibition* of the postures and reflexes which pertain to food adience, and it follows at once after the successful random movement (*i.e.* as soon as the animal can see the food *without also seeing* some impediment to action in between). This means a burst of proprioceptive afferent impulses from the muscles just released from inhibition, and this afferent burst must inevitably precede any overt movement toward the food. If we take a 'close up' of this

[2] The case which we have here is not the same as the case of learning a chain reflex, which was described in Chapter XI. It will be recalled that in that case trial-and-error was not in question.

moment, the last used and therefore 'open' motor paths are those used in the successful movement, and the incoming burst of afferent impulses (from the muscles hitherto engaged in postural adience, and now commencing actual progression movements, towards the food) will tend to acquire motor outlet (Pavlov) into the now 'open' paths of the successful movement (raising the latch and pressing against the door).

These re-enforced proprioceptive afferent impulses *follow*, it is true, the motor outflow which produced the successful movement. But we have already seen (Chapter XI.) that the investigations of Beritoff (1926) and Switzer (1930) show (*contra* Krestovnikov) that in reflex conditioning the to-be-conditioned stimulus (which in our present instance are the proprioceptive afferent impulses from nascent progression movements) *can* follow the 'unconditioned' reflex, *i.e.* the motor outflow to which the stimulus is to be conditioned. It often happens, moreover, that the cat or other animal retains for a brief moment the posture of the successful movement as if only 'slowly realizing' that the road ahead is clear. In this case the proprioceptive afferent impulses from the nascent movements of progression (toward the food), while gaining strength, are *simultaneous* with the continued innervation of the posture of the successful movement.[3]

It may happen that the cat makes the 'success' movement, but not 'seeing' that the door is unlatched and slightly ajar, passes on to some other random effort. 'Not seeing' means that the cat's food-adience *is not released from inhibition.* In this case nothing is learned. If at this juncture we 'help' by pushing the door wide open, the cat will of course go to the food. But it has learned *nothing:* as will appear

[3] It sometimes looks as if the impulse to advance toward the food had to overcome an inhibition left over from the preceding experiences of frustration. One is reminded of the 'too good to be really true' attitude in children and men. It would be interesting to learn experimentally how much the brief retention of the 'success' posture assists in confirming this so-called 'backward' conditioning.

when the cat is put back in the cage for another lesson. Or rather, it has learned something: for at the next trial it will in all probability go to the door and there *wait for 'help';* as we should expect from the very principle shown in the preceding paragraph. In a case like the above an onlooker is tempted to say, "The cat made the right movement, but didn't know it."

If the cat does 'know' it and moves on to its food, so that a proprioceptive afferent volley from released food-adience movements is returned at the right moment to its central nervous system, what is *learned* is this: the proprioceptive impulses from the movements and postures (for movements are only more strongly innervated postures) of food-adience will now (to the extent of the learning) directly innervate the movement that brought 'success,' *i.e.* that raised the latch and pushed open the door. Since the food-adience motions and postures are innervated by the sight and odour of food and by the deficit stimuli of hunger, these too will innervate, although indirectly, the movement that raises the latch. It is interesting to see how after a few successes of this sort the hungry cat becomes so markedly adient to the *latch* that its adience toward the food (although certainly more deeply canalized) is hardly to be detected in its overt behaviour until after it has lifted the latch and opened the door. When placed in the cage the cat makes immediately for the latch. It looks as if such components of its food-adience as are *not also* latch-adience were held in abeyance (partially inhibited) by the repellent bars and other features of the cage. Human behaviour is similar: a man wishing to enter a house appears to be interested *only* in the handle and lock on the door; though if balked here his ulterior interest comes to expression, as we shall see in Chapter XXI.

It is clear from the nature of the case that, as I pointed out before, only the last step can be 'successful,' and only the 'successful' step *can* be learned (on the principle here described). For it is 'success,' precisely, that differentiates

physiologically some certain movement from all other random efforts, and makes a special conditioning possible. In our case of the cat and box there is nothing further to learn. But when there is, that is, when there is a *sequence* of steps to be learned, the same principle will hold, and the successive steps will be *learned* in the backward order; the last first, then the next-to-the-last, and so down to the first.

The element of inhibition, previously mentioned, which is introduced by the bars and walls of the cage has been well described by Smith and Guthrie (1928, pp. 124-5; see also Perrin and Klein, 1926, pp. 241-3). By as much as many parts of the cage have resisted the cat's advance, they have become mild (or even overstrong) annoyers, and have become to this extent avoidance stimulators. The cat will no longer 'try' them. To some extent, then, and independently of Washburn's type of learning, the cat is steered away from various parts of the cage which it would be futile to attack. This factor obviously makes for the positive elimination of 'wrong' movements, and Smith and Guthrie can hold very plausibly that this factor alone (together with food-adience) will account for successful maze learning. I do not see, however, that it would account for the backward order in which a sequence of 'right' steps is learned; if one believes, as I do, that this backward order of learning is an actually observable phenomenon. The account which I have given of it implies, of course, that Pavlovian association is always to some extent at least a reversible association. I believe that it is such, but that the readiness of reversibility depends on the adiences and avoidances which are in force while associations are being learned.

The successive steps, although learned in the backward order, are in performance, even during the learning, always performed forwards—as could not be otherwise. So that a forward concatenation of the successive steps is learned, as in any ordinary chain-reflex (*cf.* Chap XI.). And here as in piano-playing, after sufficient practice proprioceptive

impulses alone, without the co-operation of vision, will suffice to carry the concatenation; so that the animal could in the dark retrace (or *re-create* by its movement) the correct course to the food. But this sort of learning differs from learning to play a piano-piece from the printed score, in that the score stimulates the correct movements in correct sequence from the very outset; the score cuts out 'trial-and-error.' In our present case there is a set goal to be reached only by trials and errors. And it is only in trial-and-error learning, I believe, that a chain reflex, which (like all chains) associates forward, is built up or learned in exactly the reverse order. In such case, truly, *c'est le premier pas qui coûte.*

In this manner, what was once a fact of the objective situation (the pattern of the maze) becomes a fact of the animal's internal organization, its sensori-motor patterns. It *is* objective situations (manifolds or 'configurations') of course, and nothing else, that animals and men *have to learn.* And precisely this transfer of such configurations to the internal sensori-motor organization, enabling the animal to *re-create* them, *is learning.*

There is one further point. The deficit stimuli of hunger, we have supposed, are active throughout the experiment. They keep the animal restless, and they assist the learning more directly by maintaining its food adience. They assist the *learning* process, I believe, in no other way. *But* they form a steady component of *all* afferent patterns throughout the experiment; therefore both while a maze, say, is being learned and thereafter they will, when active, be *among* the stimulators that innervate the whole chain reflex which carries the animal successfully through the maze. In other words, the hunger drive becomes finally conditioned to the whole process of finding and appropriating food; a phenomenon which we have already met in the chapter on mild annoyers (Chap. XV.), where we saw that food adience is also at the same time hunger avoidance. It is because the deficit stimuli thus participate in stimulating the whole

chain reflex, that their presence or absence is often enough to determine whether an animal, placed in a familiar maze, will make his way through it at once or will merely lie down and go to sleep. In the terms of Prof. H. S. Jennings reflexes are often 'reversed' by a change in an organism's 'physiological state.'

In the foregoing paragraphs I have merely tried to give Professor Washburn's theory, supplemented by that of Professors Smith and Guthrie, of this type of learning, in physiological and Pavlovian terms. Washburn's view has been experimentally confirmed by Borovski (1927); and I believe that I have observed this sort of learning not merely in animals but in myself. Yet this theory has not met with universal acceptance (cf. e.g. Warden, 1924); and the reader is now in a position to see that various circumstances can in fact mask or even preclude this type of learning. Factors making for the positive elimination of wrong movements (Smith and Guthrie) partly mask it: an experimental setting which permits the animal from the start to see the goal (as in our example of the cat in the cage) helps to mask it; and in general any circumstance which tends to abridge or short-circuit the process of blind trial-and-error will, *ipso facto*, short-circuit the learning backwards of a chain reflex. And very notably the presence of 'ideation' largely or even wholly precludes this type of learning; ideation being, in my opinion, a sort of preliminary excursion, an exploration of the field in advance, made by means of the motile distance-receptors (especially the eyes), which the whole organism afterwards re-executes as if with 'insight' (cf. Mach, 1920, pp. 183-200). Such factors account for at least some of the objections which have been made to Washburn's observation. One might add that experimenters in this field all too often fail to see what sort of problem their apparatus of mazes, etc., is really setting *for the animal;* as when, for instance, an experimenter arranges a cage with a door on one side, through which the cat can plainly see the spot where the food is always placed,

but further devises that food shall never be placed there *unless* the cat *approaches* the spot *roundabout* through a different exit, in the *top* of the cage. Of course the cat that solves this puzzle knows as much about the psychology of his tormentor as the latter knows about the psychology of his cat; and I incline to think, more. Again, although Prof. Peterson (1920, p. 279) finds that "backward elimination of blind-alley entrances is the rule in maze learning," I cannot imagine that this rule would not be completely masked in his 'mental maze' (Peterson, 1922) in which "frequency and recency factors are negative"; for here, I think, an elaborate ideation is involved.

Of course, too, any one who distrusts the idea that a chain reflex is *learned* in the reverse order of its performance, is free to emphasize the elimination-by-avoidance aspect of this type of learning. An animal will often, early in the learning, explore a cul-de-sac or an electric shock-mat to its so complete satisfaction that it will never even look in that direction again. In short, the local possibilities presented by any particular experimental arrangement are so various, that only a careful analysis of the stimulus-response processes going on in the animal will reveal the fact that the actual concatenation of *successful trials* is being learned from the end nearest the goal, backwards. And nevertheless, would not any animal experimenter find it unexpected and excruciatingly funny if he were to see an animal run through the *first* half of a maze with speed and assurance, and then halt—as if to say, "But where do I go from here?"

The question as to the correctness of Professor Washburn's solution of this problem is of wider significance than so far appears: it comes up again in connection with the general question of motor-block. And there, in my opinion, Washburn's view is all too overwhelmingly confirmed. We learn only in *proximity* to goals; and not, notoriously and regrettably not, far in advance of our immediate apprehension of that which we desire.

It is to be noted, lastly, that the learning to surmount obstacles will yield a considerable by-product in the way of general motor training. Under the stress of inclination and appetite obstructed, the learner is constrained to explore many objects for which intrinsically it has but feeble adience. And exploration develops, even under these circumstances, some motor facility with regard to such objects.

REFLEX CONDUCTION AND INTEGRATION: THE OBJECTIVE REFERENCE OF REFLEXES

AN organism that has learned adient and avoidant responses and the other types of response to its environment which we have so far studied, is already tolerably adapted to that environment. These are the mechanisms that underlie 'instinctive' behaviour and even some of the simpler forms of 'conduct.' This emergence of conduct, by virtue of the learning process, in an organism which was at the outset capable only of random movements, is a clear case of the phenomenon known as 'emergent evolution' (*cf.* J. S. Mill, 1887, p. 269; C. L. Morgan, 1923; W. M. Wheeler, 1926, 1928), that is, of novelty arising through synthesis. The novelty in this case is conduct, and the elements synthesized are sense-organs, nerves, and muscles *in* an organism which is living *in* an environment.

But another phenomenon has been emerging by virtue of the learning process, and this is awareness, consciousness, or mind. And we cannot follow the learning process much further without recognizing these products of synthesis as such, or in their own right; exactly as, if we were watching the construction of a house from bricks, cement, iron, and wood, we should soon be observing walls, floors, rooms, roof, and—house. Therefore it is time now to consider some remaining, still physiological phenomena connected more specially with the *interrelations* of simultaneous functions and the interactions of simultaneously excited responses. The accepted caption here is, I suppose, 'integration.'

In previous chapters, while entirely neglecting the chemistry of the subject, we have seen that if 'animal drive' is interpreted strictly, that is, as the energy which does the work, this is always the chemical energy which was con-

tained in food, and has now been stored in sense-organs, nerves and, most of all, in muscles. The food which an animal consumes maintains its sense-organs in a state of readiness to be irritated by stimulation, its nerves in readiness to transmit this excitation, and its muscles in readiness to contract when the excitation reaches them. But if the term 'drive' is taken in the other sense, that is, as the agencies which *release* this stored energy, these are all stimuli which impinge on irritable sense-organs (external or internal), and so release those trains of energy that lead to the contraction of muscles.

This simple scheme of things would, I believe, be perfectly self-evident were it not for the extraordinary intricacy of those trains of energy, the nervous pathways, which intervene between the sense-organs and the muscles. The *motor outcome* of this intricacy, so far as it is *overt* behaviour, is open to direct observation; yet this behaviour, and the non-overt play of muscle tonus, present complications which are the work of this intricate apparatus of neurones and synapses that lies between the organs of stimulation and the muscles of response. We have so far treated the several types of response which we have studied, much as if they were so many separate 'tropisms,' quite isolated from one another in the organism. And indeed some of the adient and avoidant responses *are* tropisms, pure and simple, just as some other responses which we have studied *are* instincts. But we must not make that great mistake which was made in the tropism theory (*cf.* Hachet-Souplet, 1912, pp. 52-74), of neglecting the dynamic interrelations between reflex responses.[1] It is owing to this omission that tropistic behaviour has never seemed to be a true picture of animal conduct. Sensori-motor responses are 'integrated.'

[1] A blunder which is not repaired by any merely general references to 'associative memory' or to the 'organism as a whole.' It is strange that competent biologists should ever have made such an error, when they have every reason to believe that not a single, isolated reflex path exists in the entire animal kingdom.

The concept of integration, it seems to me, remains even in physiological literature (*e.g.* Sherrington, 1906) somewhat indefinite. It implies, doubtless, a unification of function, and beyond that a vague notion of satisfactory and harmonious functioning. The term is encomiastic; but for any definite objective meaning we must return to the concrete phenomenon, the developing organism. The question is, what is really taking place as the random movements of the fœtus, as described, say, by Minkowski, slowly develop into the 'integrated' behaviour of the adult individual?

The early 'random' movements are, of course, not undetermined causally: they are not even entirely chaotic or patternless. The developing nerve tracts, the very architecture of the peripheral and central nervous system, are, as we have seen, the engrammatized 'behaviour pattern' (Child) of the embryonic and early fœtal stages. In them the fœtal growth-behaviour is irreversibly recorded as neural structure.[2] The 'random' quality of fœtal and embryonic movements lies merely in the fact that an onlooker cannot predict what movement is coming next, nor what movement (if any) will follow stimulation applied to a definite sensory area. And this is merely the functional expression of the fact that at this stage any and every afferent impulse spreads very widely in the central nervous system, there being as yet no paths from sense-organs to muscles of definitely lowered (synaptic) resistance. All movements tend to be massive; the trunk bends and unbends, limbs flex and extend, but seldom if ever is there isolated action of a single muscle or even of a single joint.[3]

One is tempted to imagine that the effect of 'integration'

[2] It seems to me that many points in neuro-muscular physiology and in behaviour will remain obscure until we understand in greater detail that growth-behaviour which comes to be irreversibly recorded as the *gross* anatomy of nerve tracts. The studies of R. G. Harrison, S. R. Detwiler, G. E. Coghill and others are contributing toward this.

[3] A pathological, partial reversion (predominantly of a flexor type) to such massive movements has been studied by Head and Riddoch (1917, pp. 217-231) in cases of gross injury to the spinal cord. These general flexor contractions are called, by Head and Riddoch, 'mass-reflexes.'

will be somehow to sharpen down these massive movements into a vast number of exquisitely differentiated single reflexes; each one corresponding to a single reflex arc. But in this we should have misled by that 'convenient working hypothesis,' the reflex-arc theory. In Chapter X. I described in some detail the 'education' of sensory surfaces, and we saw there that the "motor education of sensory points is *limb-wise,* and not in terms of this or that muscle; it is in terms, that is, of any physiologically possible *adience* of the motile member," or of several members acting simultaneously. It is not even limb-wise but, rather, organism-wise; that is, if stimulation on a finger causes not merely the finger but also the hand, arm, and shoulder to respond adiently, *all* so that the finger presses back against the stimulus; just so, sooner or later, and by the same principle (reflex-circle) of motor education, muscles of the trunk and legs will come to respond so as *further* to contribute to the adient pressure of the finger against the stimulus. In short, after a due amount of 'random' experience such as any healthy young organism gets, every muscle in the body which is so placed anatomically that it can, will respond to a touch on any point of the skin so as to contribute to the return pressure of this part of the skin against the stimulus. This is motor *synergy;* here working adiently. Now all motor education, whether adient or avoidant, progresses toward this same complete synergy of muscle response; and by reason of principles which we have studied. It needs hardly to be pointed out, further, that the same progress toward synergy will accompany every type of learning, and will continue with the repetition of habits after they have been learned.

Clearly, it would be somewhat strained to speak of this motor education as a 'sharpening down' of massive responses. The original sheaf-like spread, or irradiation, of the afferent impulse through the central nervous system, although now somewhat delimited, remains very much a sheaf-like spread. As Minkowski (1924, pp. 240, 257) has

said, "at least in a latent manner the whole organism participates in every one of its reactions, so that what we think of as a reflex is only the specially manifest and visible component thereof. . . . These conditions suggest that in the adult organism, along with differentiated paths and connections which provide for specialized and delimited reflexes, we shall also find connections which are more like those in the fœtus and which thus give rise to a diffuse and mostly hidden spreading of impulses and reflexes." (*Cf.* Beritoff, 1922, pp. 412, 425.)

Yet something has happened, and something that makes for precision and 'integration.' This something cannot be found if we look only at the nerve paths which the motor education has canalized, for the essence of the matter lies *entirely* in the negotiations that have gone on between the sensitive exterior of a blindly motile creature and objects of the environment which have impinged on that sensitive surface. The movements that have been organized within the matrix of purely random motility, *all refer* to that factor (whether an object or an energy) *outside* the organism, which has stimulated the movement: and it is a strictly geometrical reference. The neural processes are not significantly simpler than they were at the first: nor do the movements themselves differ save in this, that *now* out of all possible movements of the organism those and only those are made at a given moment which refer spatially (adiently, abiently, etc.) to the stimulus which is acting at that moment. It is a matter of selection, by neural canalization. The reader will readily recall that every activity which we have so far studied has inevitably just this quality of external reference. And these movements cannot be described except with reference to some factor or factors outside of the organism. They are now not merely 'reflexes,' they are responses: they are aimed or oriented, as the early random movements were not. And *external reference* is the great significant feature of this first step in integration.

It is fair to say that the tropism theory, so far from neglecting this fact of external reference, was mainly founded on it. But also the idea more or less prevailed that a tropism was built up through the co-operation of more primitive, single reflex arcs,[4] whereas in fact it is merely delimited by canalization in an undifferentiated matrix in which any afferent impulse can spread to every part of the musculature.[5] And again, the tropism theory, as I previously remarked, did not take due account of any functional relations between tropisms. And this, if I am not mistaken, is the *next* step in integration.

The early responses are acquired (learned) in such a way that their muscular synergy is, as it were, guaranteed. But as soon as such responses are formed, that is, irreversibly canalized as neural engrams, there arises the possibility that two or more external stimuli acting simultaneously on the organism will set off responses which *cannot* be simultaneously executed: as when a child is adiently stimulated by the light of a candle but repelled by its heat. And experience in an inconsiderate world soon provides such motor conflicts in profusion. The result, in the simpler cases, is precisely what would be expected—the stronger impulse prevails, though weakened by the antagonistic impulse (*cf.* however, Goldstein, 1925, pp. 381, 393) : and we have seen some cases of this in the preceding chapters. Then, too, there is always the possibility that simultaneously excited responses will re-enforce one another. This interaction of previously learned responses produces phenomena of behaviour which it may be not misleading to distinguish in two groups, a simpler and more immediate effect, and a more complicated, remoter one. The first is

[4] There is an instructive analogy between this notion and the theory (also now abandoned) that in the formation of languages words are built up from primitive roots, or elementary units of meaning.

[5] "Behaviour develops by individuation of elements within a primary unity, and not by integration of primarily independent elements into a total pattern" (G. E. Coghill, 1926, p. 53).

the immediate dynamic interplay, resulting in behaviour which can be understood causally, if the several contributory responses can be disentangled.   The second, more complicated effect we shall consider later, as the last step in integration, under the caption of 'motor block.'

REFLEX CONDUCTION AND INTEGRATION: MOTOR HALF-CENTRES, RE-ENFORCEMENT AND INHIBITION

THE general topic of the immediate mutual inhibition and re-enforcement of responses is distinct from that learning of responses which we have previously considered; and of course this functional or dynamic interplay of responses cannot begin until at least some response neurograms have been to some extent permanently canalized. In very young organisms the learning of responses is the more prominent phenomenon; function is steadily recorded by dendritic growth. But as an organism grows older, the functional interplay of responses already learned becomes increasingly important and prominent. Here, too, since dendritic growth merely slackens but never ceases, this functional interplay continues to be recorded by further dendritic growth. That is, there is always further learning.

The afferent nerve current, we recall, that initiates a general response synergic [1] to the given stimulus, spreads at every synaptic region which it passes, to more and ever more neurones, and finally reaches a great number of muscles, all of which contribute to a movement that is definitely directed with respect to the stimulus. Schematically the neurogram is like a pyramid resting on a base of many muscles, to which the nervous impulse spreads downward from a single sensory cell or group of sensory cells at the apex. Since innumerable stimuli are impinging on the

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[1] Synergy, as can be readily seen, is not a relationship between muscles, but a relation between the bodily movements produced by muscular contraction and some external stimulus. The muscles which are synergic for one responsive action are not necessarily so for another.

organism at all times, a vast number of such pyramidal neurograms are simultaneously active, and their bases more or less overlap, in so far as they involve the same muscles. Or, from the point of view of any muscle, a great number of sensory impulses *converge* on it, or rather, its motor neurone, the "final common path" of these impulses.

But for every muscle there is another that is its functional antagonist; such that if one muscle flexes, say, a joint, its antagonist extends the same joint. And the two motor neurones leading to two such muscles are, in certain cases at least, while still within the central nervous system, functionally related in such a way that usually whatever the impulses, strong or weak, many or few, that are converging on each neurone, only one of the two will contract its muscle. The other will, in general, be so far thrown out of function for the moment that *its* muscle actually relaxes. The central (functionally related) ends of two such motor neurones, leading to a pair of antagonistic muscles, are the 'motor half-centres' which we have previously met (Chapter VIII.). This arrangement for reciprocal innervation (and reciprocal inhibition) is often thought of as a provision against the waste of physiological energy, against, that is, the ineffectual tugging of antagonistic muscles against each other. It obviously makes for economy and speed in organisms whose movements, particularly in progression, can be only the swinging to and fro of limbs (Wachholder, 1928, pp. 698, 725-731). This reciprocal arrangement, which is far from being absolute, since the simultaneous reflex or voluntary contraction of both members of a pair of antagonists is also common in everyday life (Sherrington, 1913[a], p. 269; R. Wagner, 1925[b], pp. 124, 143), complicates the study of the interrelations between reflexes. But it remains true that an organism in which several responses are simultaneously stimulated, executes them all so far as they can be simultaneously executed; in so far as they cannot, it is ruled by the stronger impulse which is, however, weakened by the amount of the opposing impulse (Magnus, 1925, p.

344: *cf.* however, Goldstein, 1925, pp. 381-9; 1926, p. 104). Or the two impulses may balance each other, with complete inhibition as a result. This is, it should seem, the phenomenon to which Sherrington refers (1925, p. 542) when he speaks of the 'algebraical resultant' of opposed innervations; although this means, as he conceives the phenomenon of inhibition, the algebraic summation of the impulses on 'excitatory' and 'inhibitory' efferent nerves (*cf.* Sherrington, 1908 [b]; 1913 [a], pp. 269, 272).

Motor half-centres and the detailed neural processes underlying the re-enforcement, inhibition and algebraic summation of reflexes are still being debated by physiologists, and I can of course offer merely a tentative and manifestly incomplete picture of the subject. It is important, however, that we should have on this point as much light as the present state of physiology affords. I have already tried to give a somewhat detailed, though provisional, picture of motor half-centres in connection with alternating movements of progression (Chapter VIII.). It there appeared that the (proprioceptive) afferent nerve from any muscle acquires as its most deeply canalized path of discharge, a connection with the motor nerve of that same muscle, and that its second most deeply canalized path (*i.e.* its first path of 'overflow') is a (collateral) connection with the motor nerve to the antagonistic muscle. Either of the motor nerves, then, leading to a pair of antagonistic muscles, will receive impulses from the proprioceptive sense-organs of its own muscle and impulses, by a collateral path of somewhat higher resistance (Brücke, 1922, p. 52), from the sense-organs of the antagonistic muscle. These symmetrical connections between the (proprioceptive) afferent nerves from two antagonistic muscles with the motor nerves of those same muscles, would constitute a 'pair of motor half-centres.' And a diagram of connections more or less similar to those which I have described, will be found in most of the discussions, after the earliest two (Sherrington, 1900, p. 842; H. E. Hering, 1902, pp. 532-3), of motor

half-centres (Sherrington, 1905 ᵃ, p. 286; 1906, p. 201; Fröhlich, 1909, p. 98; Lucas, 1917, p. 94; Forbes, 1921, p. 304; 1922, pp. 397, 411; Brücke, 1922, p. 52; Adrian, 1924, p. 413; Cooper and Adrian, 1924, p. 77; Fulton, 1926, pp. 339-341). I believe that these 'half-centres' are purely functional units and not, as the term might be taken to imply, distinct anatomical entities; that they are developed on the reflex-circle principle, as I described in Chapter VIII., and as a direct consequence of the existence of antagonistic pairs of muscles which are provided with proprioceptive organs; and that they develop only where the contraction of one of the muscles distinctly *stretches* its antagonist. If these suppositions are correct, we can form some idea as to the position and distribution of such paired half-centres. (It is worth noting, by the way, that the path by which any proprioceptive afferent nerve sends impulses to the motor nerve of its antagonistic muscle, *is only its first* path of 'overflow'; and that it will in general have other paths of overflow, of various and somewhat higher resistances, to various other muscles.)

Nerves entering the spinal cord by the posterior (sensory) *roots generally divide at the level of entrance, and send a (longer) branch up the cord and a (shorter) branch extending through several segments down the cord: both branches give off short collateral branches at each segment which they come to. If now this is true of those sensory nerves which bring impulses from the proprioceptive organs of muscles, it seems to follow directly that a pair of half-centres can, and in general will, develop at each spinal segment which receives a collateral branch from the (proprioceptive) afferent nerves of any pair of antagonistic muscles. The paired centres of such a series, superimposed on one another at different spinal levels, will all play upon one and the same pair of antagonistic muscles. It would thus be possible for afferent impulses to meet at the half-centres of one spinal level so as to produce inhibition of one, or even of both, of the antagonistic muscles, while at the same time

these same muscles could be set in function by other afferent impulses through a different pair of half-centres: and this is a well-established (and otherwise somewhat puzzling) phenomenon.[2] Only if the inhibition existed at the paired centres immediately adjacent to the final common path, the actual motor roots (and possibly not even then), would the two muscles be inaccessible to any stimulation whatsoever. So far as I can see, further pairs of virtual half-centres might possibly develop, even in the cortex, provided that the proprioceptive afferent neurones acquired canalized connection with neurones that extended into the 'higher' centres.

The foregoing considerations regarding motor half-centres are frankly conjectural: and the reader will judge whether they are plausible and apposite by their bearing on the problem which we now come to—that of re-enforcement and inhibition. Re-enforcement, or facilitation, presents no great difficulties; it is obviously the co-operation or summation of processes which make for the same result. If two or more nerve impulses converge, at a synaptic region, on some one neurone, it seems natural that they should produce a summed impulse of higher frequency, and a muscle contraction that is correspondingly more vigorous. Inhibition on the other hand, does present difficulties. In preceding chapters I have presented the view that the inhibition of nerve impulses (and thereby of muscular action) is due to the 'overcrowding,' that is the interference, of the minute, all-or-none pulsations, either at a region of decrement (synapse, myoneural junction) or on the nerve fibre. Not only is this theory supported by a considerable range of observations, but also it is the only theory with which I am acquainted which does not, directly or indirectly, fall back on the convenient verbalism that one nerve inhibits another because it is an 'inhibitory nerve,' or one impulse inhibits another impulse because it is an 'inhibitory impulse.' Apart from the weakness of such verbiage, there is, I be-

[2] It was first demonstrated, I believe, by Verworn in 1900; and the observation has been confirmed (cf. Sherrington, 1905 b, pp. 805, 839).

lieve, the most conclusive evidence that inhibition (outside of the autonomic nervous system at any rate) is a mutual and strictly symmetrical process. *Either* of two mutually opposed nervous processes can be called the 'inhibiting' one. Of course, if one process starts later than the other, the later one will always be said to 'inhibit' the earlier; and if a process, as say with a contralateral spinal reflex, is observed only under conditions such that it is always and necessarily the later, supervening process, it is soon felt to be 'intrinsically inhibitory.'

Nevertheless, the theory of inhibition by overcrowding has not been, at the present time, universally accepted. Such eminent physiologists as Sherrington (1925) and Howell (1925) oppose it; Beritoff (1922, p. 415; 1924 [a]) seems hardly to accept it. And Adrian, who has contributed as much as any investigator to the development of this theory, has recently expressed serious misgivings (Adrian and Bronk, 1929, pp. 145-7). Apart from this circumstance, we need now to consider inhibition again and in a somewhat broader way than heretofore, that is, in connection with the general integration of the organism.

Looking at the theories, for a moment, we find that Howell (1925, p. 178) states that, "Inhibition is produced in all cases through specific inhibitory nerves": but again (p. 179), "Inhibition is determined by the nature of the [nerve] ending," *e.g.* by the nature of the myoneural junction. Sherrington (1913 [b], p. 225) speaks of 'excitation of the motoneurones' and 'inhibition of the motoneurones' as different and opposite processes; again (p. 228) of 'excitation potency' and 'inhibition potency.' In a later paper (1925, p. 529) he describes inhibition as a sort of 'chemical' process at synapses, consisting of "the production of an agent I which neutralizes less or more of the exciting agent E and so lessens the amount or concentration of it." Beritoff (1922, p. 415) declares that both excitation and inhibition are 'active' processes, but 'of different sort.' Aside from the hypostatization of the word, which is involved in

such statements, they quite ignore the urgent question as to how, then, 'excitatory' and 'inhibitory' processes come to be appropriately distributed in the neural mechanism. I make no doubt that all three writers, if asked, would refer to 'heredity' or some other variant of preformist doctrine. Now this way lies not knowledge, but verbiage.

That pulsations on a nerve fibre, if they are in close enough succession, *do* extinguish one another has been observed so repeatedly that it is practically as well established as the existence of the negative deflection (nerve impulse) itself. The next pulsation must fall within the relative refractory period of the preceding; and it will then have no other effect than to prolong the refractory state. In this way, if a nerve fibre is stimulated at about the rate of 1000 per sec. (Cooper and Adrian, 1924, p. 77), or somewhat less, for any length of time, *no* propagated disturbance or impulse (after a first, *single* pulsation) is produced.[3] If pulsations succeed one another at a frequency (variously stated as) between 225 and 350 per sec. they are propagated along the nerve fibre, but at a reduced intensity, so that they are apt to be extinguished in a region of decrement (synapse, myoneural junction). That this actually happens in reflex conduction seems definitely established by the familiar phenomenon of Wedensky inhibition (Fröhlich, 1904, p. 479; 1908 [a], p. 427; 1909, p. 85; Lucas, 1911; Adrian, 1913; Forbes, 1922, p. 381). Further, Verworn (1913, p. 221) who, like Fröhlich, believed interference (overcrowding) to be the explanation of all inhibition, predicted that 'beats' (analogous to those produced by sound waves) would some day be observed between two nerve impulses of different frequencies: and this prediction has been realized by Brücke (1922) in a remarkably ingenious and clean-cut experiment.[4] As early as 1914 Hoffmann ob-

[3] Of many papers on this subject, perhaps Adrian's of 1913 is the clearest exposition.

[4] Nevertheless, Sherrington (1925, p. 532) puts a different interpretation on Brücke's observation. It seems to me that an experiment of Sherrington's (1929, pp. 359-361) which he adduces as evidence for a specifi-

served the mutual extinction of two series of nerve pulsations. Fulton and Liddell (1925) have observed, also, that the rhythm of an 'inhibitory' stimulus can often be seen in the 'inhibitory' response; which goes to show that the 'inhibitory' nerve impulse is, like the 'excitatory,' a series of periodic pulsations. Experiments of Adrian (1924; summarized by Fulton, 1926, pp. 342-4) and of Beritoff (1924 c) have given what seem to me closely analogous results. But the observations which, in one way or another, support the theory of inhibition by interference, or overcrowding, are far too numerous to cite. And it seems hardly to be doubted that inhibition by overcrowding must at least occur in the normal vertebrate nervous system. Whether this is the only type of inhibition, or whether it is of significance for the integration of the organism, is a further question.

The reader will recall that I have spoken of the *external reference* of all learned responses, as being the first step in the integration of the organism; and of the establishment of *functional interrelations* between the various learned responses, as being the second step in this integration. These interrelations consist simply, I believe, in the mutual re-enforcement or mutual abatement of responses that are stimulated into action simultaneously. This is, of course, a purely automatic process; and it is observed that, as between the *responses*, there is the simple rule of algebraic summation. The hand that grasps a ball, may grasp it vigorously; but if the ball is hot or prickly the grasp is less vigorous because a degree of avoidance is also stimulated; and if the ball is very hot, it is dropped (not grasped). It is to be observed that the antagonism or principle of negation (which alone justifies the expression '*algebraic* summation') enters here solely from the circumstance that the limb *motion* produced by the contraction of one muscle *opposes* the *motion* produced by the contraction

cally 'inhibitory' process, could quite as well be interpreted in Brücke's terms of 'interference beats.' Brücke's observation of 'beats' has been experimentally confirmed by Cooper and Denny-Brown (1929).

of another muscle, and that this is a mere matter of the *location* of two muscles on opposite sides of a joint. There is nothing in the facts before us, to suggest that any *intrinsic* antagonism whatsoever exists between nerve impulses as such, or between muscular contractions as such. In short, I believe that the tendency among physiologists to look for some peculiar principle of inhibition (inhibitory nerves, inhibitory type of nerve impulse, inhibition potency, or inhibition substance at synapses) rests solely on an insufficient analysis of the phenomena. The inhibition of responses has been observed, and the interference (overcrowding) of nerve pulsations has been observed: but the inhibition of a muscular contraction or of a nerve impulse has never been observed, so far as I can ascertain, *except* under such conditions that there is every reason to suspect, and more often to *know*, that a motor half-centre or some other seat of overcrowding has been involved in the process. In short I know of no reason for suspecting that nerve impulses act on one another otherwise than by plain summation until, when the frequency is high enough, they are extinguished by overcrowding (as in a motor half-centre).

The rule of algebraic summation between simultaneously stimulated responses ('inducements,' 'motives,' 'intentions,' etc.) is almost as absolute a law of human and animal nature as is the law that all healthy organisms are fundamentally adient. And I can discern no factor in evolution that should complicate or tend to obscure this principle save the general fact that all organisms are limited in their movements to a to-and-fro motion of limbs, wings, fins, vibrissæ, tentacles, flagellæ, ciliæ, etc. In the case of limbs, in the higher animals, the mechanism for this to-and-fro motion is thought to be comprised in the so-called 'motor half-centres.' We are led, as it seems to me, from considerations both detailed and general, to enquire seriously whether *all* the phenomena of re-enforcement and inhibition are not referable to motor half-centres and the overcrowding there of nerve impulses. If so, these two would con-

stitute the whole *internal* mechanism of this second phase of integration.

As I have already suggested, the motor half-centre is not a distinct anatomical unit, to be readily identified, say, under the microscope; it is, I believe, purely a matter of canalization on the reflex-circle principle, as I have described in detail in Chapter VIII. A motor half-centre is in principle merely a synaptic region on the route of a circular reflex arc (proprioceptors of a muscle to the central nervous system and back to the motor end-plates in the same muscle) *at which also* a collateral branch from the afferent nerve of the corresponding arc of the antagonistic muscle delivers proprioceptive impulses from that antagonist. In other words any synaptic region at which proprioceptive impulses from the first muscle and, by a path of slightly higher resistance, similar impulses from the antagonist muscle *converge* on a motor neurone leading to the first muscle, is a motor half-centre. And it seems not improbable that a pair of such half-centres, innervating one and the same pair of antagonistic muscles, develops (*i.e.* becomes canalized) at several spinal levels: and possibly even in the mid-brain, cerebellum and cerebrum. In any actual half-centre this arrangement exists, as we may say, *multiplied,* since the afferent neurones come from several different *types* of proprioceptive organs (in both muscles) and the efferent neurones go to two different types of muscle fibre; the red fibres which contract slowly, and white fibres which contract rapidly (*cf.* Denny-Brown, 1929, pp. 289-293). I venture to add that in all probability there is in both half-centres of any pair an afferent connection from the sense-organs in the *joint* which the two antagonistic muscles actuate. It may be noted in passing that the half-centre as here described, is entirely consistent with the fact, which I have all along emphasized, that every afferent path has many motor outlets. In this case a proprioceptive afferent brings impulses which are conducted over several different paths (of different lengths) both back to its own

muscle and to the antagonist. Nor does this preclude the same proprioceptive afferent from having canalized outlets to many other muscles.

It is clear that a structure as loose as this will allow great latitude of muscular action, depending on the afferent impulses that are brought to it. And certainly no very rigid structure exists on the central side of the final common path, for 'reciprocal innervation' is only one of many common forms of muscular innervation—such as tonus, postural tonus, clonus (very interestingly discussed by Denny-Brown, 1929, p. 275 *et seq.*), simultaneous inhibition of antagonists, simultaneous (graded) innervation of antagonists (Sherrington, 1913 [a], p. 272; Wagner, 1925 [a] and [b]), etc. Our first question now is, will such a pair of motor half-centres explain reciprocal innervation? It will do so if one takes into consideration the fact that nerve impulses are extinguished by overcrowding.[5] Let us consider a pair of half-centres, and impulses coming to each centre from both muscles (as previously described) and from two sorts of proprioceptors in each muscle. And let us suppose the two kinds of proprioceptors to be, one that is stimulated by transverse compression (squeeze) as the muscle is contracted, and another which is stimulated by longitudinal tension of the muscle (whether actively contracted or passively stretched).[6] If at first the two muscles M and M' are receiving merely the ordinary tonic innervation (which is perhaps round 5 pulsations per sec.: Denny-Brown, 1929), then if an additional innervation arrives at the half-centre

[5] The following argument is a slightly more detailed version of that which was given in Chapter VIII., in connection with circular reflexes and progression.

[6] That there *are* two such types of proprioceptors is commonly accepted: the former has been thought to be the muscle spindle, which lies in the contractile portion of the muscle; the latter, to be the tendon organ of Golgi, lying in the muscle tendons. That the organs stimulated by stretch lie within the muscle tendons has, however, been disputed (Asayama, 1916; Liddell and Sherrington, 1924, p. 219). Denny-Brown (1929, p. 286) states that the tendon organs of Golgi "seem well adapted to appreciate tension, passive or active," and assigns this function to them.

of either muscle, as say that of M, this muscle begins to contract, the 'squeeze' receptors begin to be stimulated and later the 'stretch' receptors. The often repeated observation that alternating movements of progression, if once started from outside, do, under some circumstances at any rate, maintain themselves autonomously and with vigour (Beritoff, 1923, p. 268), indicates that these proprioceptive sources (including the proprioceptors of joints) suffice not only to maintain but to increase the contraction of M: that is, that circular reflexes tend to intensify themselves. But soon M′ begins to be stretched, and its stretch receptors are stimulated, sending impulses both to M′ and collaterally to M. The half-centre of M is now receiving impulses from the sq. and st. receptors of M and collaterally from the st. receptors of M′: the half-centre of M′ is receiving impulses from its own st. receptors and collaterally from both receptors of M. If, now, the collateral paths are less deeply canalized than the direct proprioceptive arcs, as is almost certainly the case (*cf.* Brücke, 1922, p. 52), the half-centre M is receiving more innervation than half-centre M′. And if overcrowding occurs, as apparently it must, it will occur first in half-centre M. With overcrowding, all pulsations (even those at 5 per sec.) are cut off from muscle M. Now the contraction of M′ has been already started by its own stretch receptors (the familiar stretch or myotatic reflex),[7] which at first were, but no longer are, re-enforced collaterally from M: and M′ is now mechanically unopposed, since M is for the moment toneless. M′ now contracts with increasing force, exactly as M has previously done; and M begins to be passively stretched, as M′ was previously. Exactly as before, overcrowding occurs, but this time in the motor half-centre of M′. M′ becomes toneless and M

[7] Denny-Brown (1929, p. 286) has reported that in some reflexes, *e.g.* muscle clonus, "the muscle spindle exerts an inhibition, the tendon-organ an excitation, upon the centre." So here, it may be that the stretch-receptor is mainly responsible for starting each new contraction (of its own muscle), and that the squeeze-receptors mainly contribute the last, the overcrowding ('inhibiting') pulsations.

begins to contract again.   And so on in continued alternation.[8]

Such a conception of reciprocal inhibition at motor half-centres can hardly be accounted purely hypothetical, for it merely explains reciprocal inhibition in terms of a well-known phenomenon, the so-called Wedensky inhibition, in which (Forbes, 1922, p. 381) "there is a critical frequency of stimulation above which the Wedensky 'inhibition' occurs, and below which it is replaced by contraction."   This phenomenon was first explained, on the basis of overcrowding, by Fröhlich (1904, pp. 479-484; 1908[a], p. 427; 1909, p. 85), who was convinced that overcrowding explains *all* inhibition (1908[b]; 1909).   Fröhlich's explanation was revised, still on the basis of overcrowding, by Lucas (1911). And the best short exposition of this phenomenon, and its explanation, is Adrian's paper of 1913.

We have now to enquire whether from such a start we can go on to a fairly consistent picture of the general rôle of re-enforcement and inhibition in the life (and integration) of the organism.

From the general fact of the wide spreading of every afferent impulse, and its consequence, that impulses from many sensory sources converge on every muscle, we might

[8] The above account would seem to be quite invalidated by the observation reported by T. Graham Brown (1914), that rhythmical progression movements persist after the severance of *all afferent fibres* from the limb under observation.   As an experimental datum this seems to conflict with data of Sherrington and many other observers: and in fact the essential rôle of proprioceptive impulses in all normal movements of progression can hardly be so easily called in question.   When Brown concludes (p. 36) that "progression is not fundamentally a 'reflex' act," but (pp. 37-8) a co-ordination that is timed mainly by fatigue, he seems to be adopting a theory of inhibition by fatigue to which few physiologists would assent. (I believe, too, that any 'fatigued' synaptic region, if duly examined, would be found to be overcrowded.)   Forbes (1921, pp. 303-7) after a critical survey of Brown's experiment, admits that the latter's results are possible under the very unusual experimental conditions in question; but concludes (p. 311) "that so far as we can see at present there are no insuperable obstacles in the way of explaining reflex phenomena on the basis of nerve impulse frequency proposed by Lucas."   And this means, *inter alia,* inhibition by overcrowding.

expect that the entire musculature would be in a more or less perpetual state of inhibition from overcrowding. If this is not the case, the reason can only be that the synapses, at which nerve impulses converge, are as they have always been called, regions of *decrement*. The converging impulses must regularly suffer a *reduction* at each region of decrement. Adrian and Bronk (1928; 1929) have recently reported surprisingly low impulse frequencies even on motor neurones. And it looks very much as if this impulse reduction at synapses, which itself may be due to *partial* overcrowding, must forestall and preclude, under normal circumstances, all complete inhibition by overcrowding *except* where motor impulses are *simultaneously re-enforced from immediate proprioceptive sources, i.e.* at motor half-centres. But in states of extreme fear there is often a more or less complete and sudden paralysis, at times interrupted by local motor spasms, which decidedly suggests a general, abnormal overcrowding at synapses. The mysterious 'death feigning' in animals may be precisely such a phenomenon.

Even with this allowance the theory of inhibition by overcrowding implies a consequence which at first glance seems not to be in line with the facts. The summation of impulses all contributing to a certain movement always, except in the extreme cases just cited, *increases* the force of that movement. Why should such summation not often lead to overcrowding and *arrest* of movement? And if so, how would 'algebraic summation' be possible? In fact while he was still an exponent of the theory of overcrowding Adrian (1918, p. 45) remarked that the interference theory meets a difficulty in 'algebraic summation.' The difficulty, as formulated by Adrian and Bronk (1929, p. 145, footnote), is that, "on any scheme based on the interference of trains of impulses we have to face the difficulty that in an intense excitation, where the central paths are discharging at fairly high frequencies, inhibition should be brought about much more easily than in a feeble excitation where the frequency is lower. In fact if intensity of excitatory effect depends

on frequency, the greater the excitation the smaller will be the degree of inhibition needed to extinguish it."

If we correct the last clause, which should of course read "the smaller will be the degree of (additional) *excitation* needed to extinguish it," this implication, whether a 'difficulty' or not, is at least a fact. It is the phenomenon of Wedensky inhibition; and probably also the inhibition that is observed in cases of extreme fear and some other forms of over-excitement. But the implication is far from being a difficulty if we bear in mind that algebraic summation is effected *through* motor half-centres. If the sum of impulses to a certain movement is high, they will effect that movement vigorously and rapidly, and quickly reach the full swing of the limb and the moment of extinction by overcrowding; whereupon the reciprocating mechanism, together with such extraneous impulses, if any, as make for the opposite phase of movement, will swing the limb in the opposite direction until overcrowding again occurs; whereupon the first group of impulses will effect a second vigorous and rapid swing in the same direction as at first. By as much as extraneous impulses playing on one of a pair of motor half-centres are able to produce extinction by overcrowding they are able to produce a vigorous muscular contraction, and the half-centre reciprocating mechanism will swing the limb back, ready for another vigorous contraction.[9] 'Algebraic summation' of opposed impulses is effected by the energy and speed which these opposed impulses impart to the to and fro phases, respectively, of a limb's motion. I believe that it is not only an unnecessary but even an unworkable and meaningless hypothesis which assumes that an algebraic summation (an excitatory versus

[9] This action reminds one of the previously quoted observation made by Magnus (1925, p. 349) at a 'higher' neural level: "The cortex cerebri evokes during ordinary life a succession of phasic movements, which tend over and over again to *disturb* the normal resting posture. The brain-stem centres will in the meantime *restore* the disturbance and bring the body back into the normal posture, so that the next cortical impulse will find the body prepared to start again."

a specifically inhibitory process) occurs in *each* of two motor half-centres.

The persistent tendency of physiologists to consider Wedensky inhibition (overcrowding) a merely 'apparent inhibition,' as Lucas has called it, or a sort of curiosity of the laboratory, and to argue that 'real inhibition' must be some sort of subtractive factor which, combining 'algebraically' with positive (!) nerve impulses, directly obliterates them, has its source, I believe, in a false interpretation of the *phenomenon* of inhibition. A stimulus which excites extension of the contralateral hind-limb *inhibits* extension of the homolateral hind-limb. This inhibition is then instantly and unthinkingly hypostatized as an 'inhibitory principle' *sui generis* that is supposed to be working in the *extensor* motor nerve of the homolateral hind-limb. And 'algebraic summation' is found to hold. But we know that a stimulus which excites extension of the contralateral hind-limb also excites postural *flexion* of the homolateral; and we have good reason to believe that this last is a 'reflex reversal' (by overcrowding) of the normal postural homolateral extension (previously in force), *i.e.* that the homolateral extensor inhibition is due to reciprocating action between the two homolateral half-centres. And all reciprocation between paired half-centres is, I believe, more naturally to be explained in terms of degrees of overcrowding than in terms of any 'inhibitory principle.' [10]

The error of hypostatization is one of the most inveterate

[10] I do not see that 'postural reciprocation' presents any new difficulty for the theory of overcrowding, since it is obviously merely the functioning of reciprocating half-centres at relatively low tension. If our supposition is correct, that those impulses which produce *overcrowding* are always the direct contribution of proprioceptor organs in the joint and muscles immediately involved, we should expect the phenomenon of reciprocation to be observable through a wide range of intensities and rates: as in fact it is. J. v. Uexküll's 'law of stretch,' as confirmed by Magnus and de Kleijn (1913), that a stretched muscle has through its proprioceptors a lowered reflex threshold, is specially suggestive in this connection. Magnus (1924, pp. 26-49) has described at some length the influence of body postures on the distribution of reflex thresholds (*cf.* v. Uexküll, 1929).

tricks of the human mind. And if the reader should care to see it operating fast and furiously with this very phenomenon of 'inhibition,' he has only to read a certain paper of Pavlov's (1911). I believe that Pavlov's later theory of 'central inhibition' is likewise a case in point. Yet one may still ask whether there are really *no* phenomena which reveal some other type of inhibition than that of overcrowding at motor half-centres.[11] It is a large question, and I can only say that I know of no such phenomena. I am certain that the so-called 'inhibition of ideas' will offer no problem when we have really discovered the mechanism of motor inhibition. Brücke (1922, p. 49; 1929), who altogether favours inhibition by interference, has admirably summed up the present physiological evidence bearing on this question.[12]

A specific difficulty which grows out of any theory of a distinct 'inhibitory' process, such even as Sherrington's (1925), is that no reason appears (nor, as I believe, ever would appear) *why certain* nerves are 'inhibitory,' or why *they* produce at certain synapses an 'agent I' which neutralizes the 'exciting agent E' that is produced there by *certain other* nerves. This difficulty becomes acute when it is admitted, as it is, that the same nerve acts at one time as 'inhibitory' and at another time as excitatory; and again, that a nerve can transmit excitation to one muscle and inhibition to another at the same time.

The only reason for any inhibition, its sole significance, lies in the fact that the contraction of a muscle opposes or negatives the contraction of its antagonist. If we lose sight of this fact, we lose all clue to the problem. I am convinced that the 'inhibition' of the text-books is the survival merely

[11] The relations existing between sympathetic and parasympathetic efferent fibres are not included in our present inquiry; though I believe that here too a case could be made against any 'specific inhibitory' process.

[12] I cannot so far see that the 'chronaxie' or the 'resonance' (P. Weiss) theory of inhibition and co-ordination (*cf.* Wachholder, 1928, p. 751 *et seq.*) contributes towards a coherent picture of the neuro-muscular process.

of an old superstition. The Soul poured out inhibitions from the pineal gland: or, less flagrantly, the 'higher centres' delight to inhibit the 'lower,' and do so most of the time. But a physiology which treats afferent and efferent as *one continuous* process (a profoundly different point of view) discovers no such phenomenon.

The view of Wedensky inhibition effective in motor half-centres, which I have outlined, is confessedly incomplete; it by no means finishes the picture. Rather generally, it should seem, innervation to the muscles must (as it were) filter through the reciprocating mechanism of motor half-centres; and such innervation undoubtedly *qualifies* the to-and-fro motion of the limb by modifying the speed, force, and amplitude of every stroke. This is one of the externally visible aspects of 'algebraic summation.' A greater innervation flowing to the flexor (than to the extensor) side of a pair of half-centres would render the flexor stroke quicker, briefer, and more forcible.

But since the proprioceptor-half-centre mechanism is, so far as present evidence goes, able to perpetuate its own motion, it is not easy to see why any reciprocating motion ever *ceases*. Here, of course, is exactly where the convenient 'higher centres' are supposed to come in with a merciful dose of 'inhibition.' But it is only tradition, I believe, that makes such a view plausible;[13] reciprocating movements are in abeyance even in the spinal animal, though they can be stimulated. Then too, the possibility of rigid innervation of a limb (Sherrington, 1913 [a], p. 269; Wagner, 1925 [a]; 1925 [b]; Wachholder, 1928) and of several other varieties of innervation which are not reciprocating or are only partially reciprocating, is puzzling. The fact is that, quite apart from any theories of inhibition, the relations between

[13] From his observations in the psychiatric clinic and more especially on victims of the late war, Dr. K. Goldstein (1925; 1926) has been led to oppose utterly the notion of inhibition by 'higher' centres. His testimony is the more independent of this present connection, since he is decidedly indifferent (1925, p. 371; 1926, pp. 84-5) to the views of Magnus and v. Uexküll on *Schaltungen*, stretch reflexes, etc.

reciprocal and non-reciprocal innervation are not as yet made out. The functional, and indeed the anatomical, possibilities here are almost endless, and 'hypotheses' would be as easy to make as they would be worthless. But these obscurities will presently be cleared up by the admirable experimental studies which are constantly appearing; such, for instance, as Denny-Brown's recent study (1929) on clonus.

The general picture of inhibition which I have tried to sketch, has been very broadly outlined by v. Uexküll (1929, p. 762) as follows: "A fully developed apparatus of locomotion (suited either to the earth's surface, or to locomotion in water, or in the air) is almost without exception the foundation of all animal organisms. It is built up on the principle of the antagonism of muscles, and its rhythm is determined by the law of stretch or in the case of reciprocal innervation by the antagonism of motor centres (mechanism of reciprocal inhibition). If there is enough tonus, this automatically working machine is set in motion. Only the question as to whither it shall move, is determined by stimuli impinging on the external receptors."

"It is only in this connection that we can understand the arrangement for reflex-reversal, with its weak and strong reflexes that depend on the intensity of the stimulus, and set the organism in locomotion now toward its prey or, again, away from its foe."

We have next to look at the actual process of conduction in the synapse, and especially Sherrington's recent theory of synaptic 'charge.'

REFLEX CONDUCTION AND INTEGRATION: SYNAPTIC
CHARGE, PARTIAL AND COMPLETE OVERCROWDING

IN general a synaptic region appears to consist of connec-
tive tissue in which are embedded, in greater or less prox-
imity, terminal fibres of various afferent neurones, and
on the other hand dendrites of various efferent neurones
('afferent' and 'efferent' relative to this synapse). A nerve
current of sufficiently high frequency arriving on any
terminal fibre could pass across to all the dendrites that lie
fairly near, and could continue along the corresponding
neurones. This would be like the original undifferentiated
condition, which produces random movements. But Pav-
lovian learning consists precisely in the neurobiotactic
growth of certain of the dendrites (*i.e.* those that are simul-
taneously stimulated) toward an *active* terminal fibre: and
this *reduces* the amount of resistance (connective tissue)
between *these* dendrites and the terminal fibre in question.
Hence a moderately intense nerve current will pass *only*
to the dendrites which have made this growth; and this
produces definite responses such as we have studied, in
place of random movements. As the learning of various
new responses continues, it will come about that between
any terminal ending and the near-lying dendrites there will
be various degrees of resistance, according to the amount
of growth that learning has brought about in the several
dendrites.[1] These various resistances are the various de-
grees of *canalization*, previously mentioned. And in gen-

[1] Beritoff (1924 b, p. 112), who seems to favour the Pavlovian type of
learning, states that it appears to be inexplicable in terms of any known
properties of reflex conduction; but here Beritoff has failed to notice that
Kappers' observations on neurobiotaxis provide exactly the neurological
basis that is required.

eral at a synapse there are terminal endings of several different neurones lying more or less within range of the dendrites of several other ('motor') neurones.

It is an accepted fact that the energy of the nervous impulse is not conducted through the synaptic tissue in the same way as it is propagated along the nerve fibre. This latter, true nerve transmission, resembles in some respects the passage of a spark along a thread, save that the thread is almost instantly restored; *i.e.* after an 'absolute refractory period.' The chemistry of this process is thought to be explained by the Nernst theory of nerve action. But at the synapse this stops. The minute amount of energy which is probably delivered by each nerve pulsation is conducted into and sometimes through the synaptic tissue by a process which is not a process of *release* (the combustion of a thread) but is some simpler sort of energy conduction. The electro-chemistry of this process need not concern us, but its physiological properties do. Firstly, there seems to be no refractory period (absolute or relative) in the synapse, and hence no overcrowding or interference in the true sense; but there is a *resistance* which seems to be analogous to electrical ohmage. A nerve impulse of not too high frequency is often conducted across a synapse and onward on the next neurone without any alteration of its frequency. Higher frequencies are apt to be altered, in fact reduced, by going through a synapse. Impulses of very low frequency are apt to be extinguished. Further, such phenomena as that of reflex 'after-discharge' seem to prove that there is at the synapse not only a summation but a somewhat brief *retention* of energy. That is, if nerve pulsations (whether from one or several neurones) reach the synapse in rapid enough succession, it will acquire a 'charge,' somewhat resembling, perhaps, an electrical potential. This is, in substance, Sherrington's theory of synaptic charge (Sherrington, 1921; 1925, pp. 520-527). The energy which is conducted across, and which in sufficient quantity may accumulate in, the synapse serves to excite

the dendrites of the next following neurones and thus initiates the nerve impulses that travel on in a motor direction from the synapse. Hence the motor nerve cell with its dendrites is stimulated at the synapse *very much as a sense-organ* is stimulated at the surface of the body (Adrian and Bronk, 1929). In so far as the energy of the 'charge' is not expended in stimulating dendrites, it is slowly dissipated by "diffusion and adsorption" (Sherrington, 1921). It seems to me that these views are scarcely open to question.

But a question arises, as we have already seen, when we come to consider the process of inhibition. In order to explain this, Sherrington (1925, p. 529) hypothecates "the production of an agent I which neutralizes less or more of the exciting agent E and so lessens the amount or concentration of it." So far as I can discover, this is an hypothesis purely *ad hoc*. There seems to be no direct evidence for the liberation in synapses of two mutually neutralizing substances (nor for the liberation of any substance at all), and the hypothesis merely defers the actual problem of inhibition. It leaves us enquiring why some nerves at some synapses and at some times, declare themselves as 'inhibitory' by releasing the 'inhibitory potency.' To such a question there will never be an answer if, as I believe, the phenomenon or the 'problem' of inhibition has its very source and its sole significance in the fact that the contraction of one muscle antagonizes the contraction of another. The alternative theory of inhibition is overcrowding, and overcrowding, more specially, at motor half-centres.

The theory of synaptic charge, or as it seems more accurate to say, the fact of synaptic charge, just about completes our picture. We are now in a position to understand both why converging nerve impulses are regularly reduced at synapses, and why actual inhibition normally occurs only at motor half-centres, where re-enforcing proprioceptive impulses flood in.

Energy conduction in synaptic tissue exhibits not over-

crowding but *resistance* (analogous to electrical ohmage or, in mechanics, to friction); it also exhibits retardation (lag), diffusion (spread and leakage) and, above all, summation (analogous to the summation of electrical potentials). Observation has shown that a very few nerve pulsations arriving at such a region of decrement may be unable to cross it or stimulate any of the dendrites at the other side; they have succumbed to ohmage, and been unable to establish a sufficient synaptic charge. An impulse of moderate frequency (say 50 per sec., as in an experiment of Brücke's), *provided that* the individual pulsations are of the normal 'all-or-none' intensity, may cross a synapse and stimulate dendrites at the *same* frequency. Impulses of higher frequencies (*e.g.* 300 per sec.) will get across and stimulate dendrites (and more of them), but not, generally, at the original frequency. And here it looks as if the individual pulsations delivered to the synapse were merged in one general 'charge,' a mounting potential, by which latter the dendrites are in turn stimulated. Very appositely Adrian and Bronk (1929, p. 145) compare the motor nerve cell, or its dendrites, to a sense-organ; an electroceptor, we might perhaps add. Although this general synaptic charge does not stimulate the motor nerve dendrites ('motor' with respect to this synapse) at the same frequency as that of the arriving pulsations, it is generally true that as these last increase and the synaptic charge mounts, the impulses on the motor neurones also increase in frequency—up to the point of inhibition. This is substantially the same picture of the synapse as that given by Adrian and Bronk (1929, pp. 146-7).

They anticipate the following objection. It is a commonplace of the laboratory that a constant current applied directly on a nerve stimulates it when the current is made and when it is broken, but at no other time. Is not this situation sufficiently like the continuous synaptic charge acting on the motor nerve dendrite, to make it improbable that the latter would ever in this way be stimulated to a

multiple discharge of periodic pulsations?   The answer is that the familiar laboratory notion is merely a hasty generalization.   S. Garten (1909), as Adrian (1926 [b], p. 48) points out, has recorded multiple discharges in a nerve trunk by stimulating it with a constant current.   Verzár (1928, p. 23) has reported the same phenomenon in the nerves of molluscs.   Further, as Adrian (1926 [b]) says, the dendrite at the synapse is comparable to the free nerve terminations in the outer epithelium: these are the receptor organs of 'pain,' and they respond with *multiple* discharges when stimulated by a constant electric current.   There is, then, no difficulty on this score.

Observations indicate that as the synaptic charge becomes more intense, the frequency of impulses leaving the synapse increases.   The phenomenon of 'after-discharge' shows that after incoming pulsations have ceased, the synaptic charge continues to stimulate the dendritic endings and at a rate which diminishes as the synaptic charge is dissipated.   At the dendrite, as at the intraepithelial 'pain' receptor, there may be a 'local excitatory process' (Lucas, Adrian) that initiates the nerve pulsation.   Whether this is so or not, the phenomena strongly suggest that the *frequency* of the multiple discharge that is excited in the dendrite depends on how *early* in the recovery (from one excitation) the synaptic potential is able to start *another* excitation.   We know that pulsations initiated during the relative refractory period (after a previous excitation) are of *less* than the normal, all-or-none intensity; and such weakened pulsations are at a disadvantage when they come to the *next* region of decrement (synapse).   Adrian (1924, p. 414) has stated that if impulses on a nerve succeed one another at a rate of less than .004 sec. (*i.e.* 250 per sec.) they will be so reduced in intensity as to be unable to pass a region of decrement.   We may call this partial overcrowding.   Such impulses, though they can not (unless re-enforced by others) pass the next synapse, can still contribute their enfeebled quota toward charging that synapse.

The spread of afferent impulses, from one neurone to many, at each synapse, is so general and extensive that it can hardly be doubted that if the frequencies of all pulsations converging on a synapse were merely added numerically, they would more often than not yield a total frequency far higher than the next neurones could carry. But they are not so added; they are merged in one mounting total of synaptic charge. And surely the simplest assumption that can here be made is that the synaptic charge will stimulate the next dendrites at a frequency that varies *as* the synaptic potential varies, but at a pulsation intensity that (owing to partial overcrowding) varies *inversely* as the frequency (or the potential). For the higher the synaptic charge, the earlier in the relative refractory (recovery) period it can probably stimulate the dendrite again, and if so, the less intense will be the individual pulsation. In this way, that is by partial overcrowding, a reduction of impulse energy would be effected at every synapse. The summation (as 'charge') of energy arriving at the synapse would probably be arithmetical, but the reduction would probably be logarithmic. I think it can hardly be doubted that such a reduction takes place.

It is worth while to enquire what, approximately, the frequencies here involved are. If "the smallest interval between successive impulses set up in a nerve by direct stimulation lies between .001 and .002 second" (Cooper and Adrian, 1924, p. 77. Spinal cat.) then the absolute refractory period must lie between, or perhaps fluctuate between, .001 and .002 second. Field and Brücke (1926) give it as .001 sec. or more, up to .00225 sec. on some nerves, and still higher when the nerve is fatigued. They state that the lengthening of the refractory period seems to be the most sensitive test for fatigue of myelinated nerves. These determinations indicate a maximal nerve impulse frequency ranging from 1000 per sec. down to 444.44 per second. Forbes, Ray and Griffith (1923, pp. 613-614) give maximal observed frequencies as 600 per sec. up to possibly 1000 per

sec.; Brücke (1922) as round 600 per second. These figures roughly indicate the frequency just *barely* below that at which total extinction by overcrowding will occur *on the nerve fibre*. But pulsations even at 600 per sec. are enfeebled almost to the point of extinction; they could not pass a region of decrement.

The lower frequency, at which nerve pulsations *begin* to be enfeebled, can be determined only in a similarly approximate way. Let us recall that the recovery process after the passage of a nerve pulsation presents three periods or phases: 1, an absolute refractory period, during which renewed stimulation has *no* effect on the nerve (lasting about .001 sec. or perhaps .0012 sec.); 2, a relative refractory period, during the *very* earliest part of which renewed stimulation excites no new pulsation but *does renew* the refractory phase (this is the moment for absolute inhibition by overcrowding, according to Lucas and Adrian; *cf.* Lucas, 1911; Adrian, 1913), and during the later parts of which renewed stimulation excites pulsations of more or less reduced intensity (as described above); 3, a supernormal recovery phase, during which renewed stimulation excites pulsations of *more* than the normal all-or-none intensity. I have no direct figures for the duration of the relative refractory phase. But Lucas (1917, p. 77) gives as the *most favourable* rate of succession, at which a succeeding impulse will fall in the supernormal recovery phase of a preceding, .01 to .015 second. (It is about .03 sec. in the frog; Adrian, 1918, p. 34.) This corresponds to a frequency of 66.66 to 100 per second. The frequency, then, at which pulsations *begin* to be reduced in intensity, by partial overcrowding, must be *above* 100 per sec.; possibly, say, 150 or 200 per second. From this frequency on, the pulsations will be progressively reduced in intensity, until at a frequency which, as we have seen, is variously given as 444, 600 or 1000 per sec. the pulsations will be totally extinguished *even on a nerve fibre*.

If partial overcrowding begins at a frequency of 150 to

200 per sec., most of the measurements of frequency in 'reflex conduction' (averaging perhaps round 300 per sec.) would indicate that partial overcrowding is a common and normal occurrence in the central nervous system. And this, in view of the convergence at synapses of impulses from many sensory sources, as we have seen, must effect a reduction of energy, and preclude what would otherwise be, according to all appearances, a total paralysis from overcrowding.

We can now see very easily why a more serious degree of overcrowding can, and indeed must, occur at motor half-centres. Whether the pulsations reaching a half-centre from other parts of the nervous system are of supernormal intensity (66 to 100 per sec.) or of subnormal intensity (150-200 per sec. or higher), the synaptic charge mounts and the muscle begins to contract. And *then* re-enforcement begins to arrive, pulsations from several types of proprioceptors which are now stimulated by the contraction. And as the contraction becomes more intense, this re-enforcement is steadily augmented. It is even possible that the proprioceptive pulsations reach the half-centre at full intensity, not having crossed any synapse on the way. The synaptic charge (for the half-centre is a synapse, as I have shown) will steadily increase; and continue to increase until the synaptic potential is strong enough to stimulate the motor nerve dendrite during the *earliest* part of its relative refractory period, and this would, as we just saw, prolong the refractory period but excite *no* further pulsations. This would be complete inhibition by overcrowding.

Probably, however, this seldom, perhaps indeed never, happens. The myoneural junction has usually, and so far as I know always, been considered to be itself a region of decrement. If so, the more frequent and more enfeebled pulsations on the motor nerve will be extinguished at the myoneural junction, allowing the muscle very gradually to relax, and its proprioceptive afferent impulses to lapse, long

before any actual extinction by overcrowding occurs where the synaptic charge is stimulating the motor nerve dendrites. The frequency of the action current on the muscle itself becomes of interest here. Beritoff (1924 c, p. 187) who, like many physiologists, believes that muscle is an 'all-or-none' tissue, states that the absolute and relative refractory phases together are not less than .01 second: so that (p. 188) overcrowding begins on the muscle (cat) at 100 pulsations per second. However that may be, Beritoff (p. 176) gives as the highest observed frequencies during reflex contraction (action current of flexor muscle of leg: cat) 250-300 per sec.; and this only for short intervals of time. It is known that the maximum action current frequency varies in different muscles. But 300 per sec. would be something like a general average of the measurements of this so-called *Eigenfrequenz* (in warm-blooded animals, *including man*) as obtained by different observers. I am not aware that any one has measured the action current on a motor nerve or on a muscle, at the exact moment when, by reciprocal inhibition, the contraction of a muscle stops and relaxation begins. This would be an interesting datum, but determinations so far obtained seem to suggest that the frequency of 300 per sec. on the motor nerve is not very far below that frequency at which a muscle will cease to contract and begin to relax. This rate, of course, would not involve complete overcrowding on the motor nerve.[2] The main point of the whole matter is that, owing to direct re-enforcement from proprioceptive sources, a degree of overcrowding is ensured at motor half-centres as it is ensured *in no other parts of the central nervous system.*

In two recent papers Adrian and Bronk (1928; 1929)

[2] Forbes (1922, p. 408 *et seq.*), in an important paper on reflex conduction, seems to ascribe the impeded conduction in the synapse largely to the attenuated size (capacity) of the finer dendritic branches. Such a factor might affect the rate of the local excitatory process. Verworn (1919, pp. 62-5) held that the passage of impulses not only stimulates the growth of dendrites (neurobiotaxis), but also enlarges (by 'trophic' stimulation) the 'capacity' of the entire neurone.

have reported frequencies for action currents on motor neurones, in both reflex and voluntary contractions, which are too low to suggest that inhibition by overcrowding is *ever* likely to occur. The evidence here has seemed so conclusive to these investigators that, although Adrian has been for years a leading exponent of the theory of inhibition by overcrowding, they take the following very definite position (1929, pp. 145-6) : "We must therefore revert to the view that two qualitatively different processes can occur in the synapses to account for the inhibitory and the excitatory effect. A scheme on these lines has been worked out in detail by Sherrington [1925], and it is there suggested that excitation of the motor neurones by the afferent impulses is due to the liberation of a specific exciting substance in the synaptic regions." And that inhibition is due to a specific inhibiting substance.[3]

From a series of studies in the Journal of Physiology, running from 1923 to 1929, Adrian (sometimes in collaboration with Cooper, Zotterman, Matthews, or Bronk) has been led to the view (A. and Z., 1926 [a], p. 164) that, "both on the motor and the sensory side . . . the nerve fibres are never pushed to the limit of their power of response." Four of these studies (A., 1926 [a]; 1926 [b]: A. and Z., 1926 [a]; 1926 [b]) and one by Bronk (1929) are of peculiar importance, both for their extraordinary nicety of technique and because they contain, if I am not in error, the first direct observations of action currents on a *single* nerve fibre. It was found that the maximal rate of discharge from a single sense-organ of pressure (A. and Z., 1926 [b]) is about 150 per sec., and it falls to 20-30 per sec. when the pressure has been constant for 5 sec. The range of impulse frequency is much the same for pain as for touch receptors (A., 1926 [b]), but

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[3] The argument that precedes (p. 145) is somewhat elliptical. But I believe that, apart from considerations (such as 'algebraic summation') which we have already taken up, the present objection of Adrian and Bronk to inhibition by overcrowding rests altogether on the low frequencies which they have found for motor nerve impulses.

the duration of the discharge is longer for pain if the stim-
ulus is fairly intense.

On the motor side (A. and B., 1929, p. 137), "We con-
clude that the voluntary contraction in man is maintained,
like the reflex contractions in the cat, by a series of nerve
impulses which range from 5 to 50 or more a sec. in each
nerve fibre, and that the gradation in force is brought about
by changes in the discharge frequency in each fibre and
also by changes in the number of fibres in action." The
(p. 145) "remarkable point" is "that the discharge of a
motor nerve cell can scarcely be distinguished from the dis-
charge of a sense organ. . . . in both the frequency varies
over much the same range under average conditions of ex-
citation. . . . there is little doubt that a range of 5 to 100
a sec. would cover the frequencies found in a large number
of sense organs subjected to a stimulus of normal intensity,
just as it covers the normal frequencies of the motor dis-
charge." In the spinal flexion reflex the average frequency
of the motor discharge (p. 148) "with a fairly strong con-
traction is round about 50 a sec. in our nerve fibre records."

The general picture would seem to be that most (perhaps
all) afferent impulses are initiated in the sense-organs at
frequencies ranging from 5 to 100, or 150 at the most, pul-
sations per sec., and falling rapidly with fatigue of the
sense-organ; that however many such impulses converge at
a synaptic region, the dendrites of the next succeeding
neurone, including those of the 'motor nerve cell,' will act
very much as a sense-organ would act and will *take up* and
transmit only some very moderate frequency, as say 50 to
100, or 150, pulsations per second. This analogy between
the dendrite of the nerve cell (or the nerve cell itself?) and
a sense-organ (against which I see nothing to urge if we
exclude from the analogy the specific irritability of sense-
organs to different forms of energy; light, heat, etc.) does
not, of course, instruct us as to the frequencies with which
such an organ will respond when stimulated by the synaptic
charge. But in view of the motor nerve frequencies which

they have recently observed, Adrian and Bronk argue that overcrowding could hardly occur, and that we must look elsewhere for the source of inhibition.

The figures 5 to 50 per sec. as the ordinary motor nerve frequency in reflex and voluntary contractions, are certainly interesting. Five per sec. has been sometimes given as the rate which maintains ordinary muscular tonus (Denny-Brown, 1929). The frequency which maintains postural tonus (non-fatiguing) has been supposed to be somewhat higher; round 10-25 per sec. if I remember correctly. And I believe that 50 per sec. is the figure originally given by Piper in his early paper on *voluntary* contraction. If at 66.6 to 100 per sec. the pulsations are most intensified by falling in the supernormal recovery phase (Lucas), 50 per sec. is not far below the maximal rate at which the individual pulsations are of normal, all-or-none intensity. If the contraction is more than 'fairly strong,' Adrian and Bronk (1929, p. 148) find that the impulse frequency may increase to "well above 100 a sec.": here the frequency would range through that region in which the pulsations are of supernormal intensity.

Yet I am unable to make out from several careful readings why these authors arrive at so definite a conclusion against inhibition by overcrowding. They admit (1929, p. 148) that "We have made no attempt to deal with the arguments which have appeared in support of the view that the motor nerve fibre may discharge at frequencies so high that the muscle is unable to respond to each nerve impulse. The records of single nerve fibre discharges show that in fact the frequency is often so low that the muscle fibres must fail to give a smooth contraction, but we cannot say that maximal sensory stimulation may not produce reflex discharges of higher frequency than those we have recorded"; and (p. 140) "On such questions as the maximum frequency of discharge, . . . we must reserve judgment until we have more perfect electrodes which can be trusted to pick out the action currents of single fibre groups at the

height of a contraction." Why, then, did they not reserve judgment?

To some extent Adrian and Bronk ignore the many earlier determinations of nerve impulse frequency (which have, of course, yielded much higher values), and to some extent tend to dismiss these as erroneous. Thus Cooper and Adrian often found in the same electromyogram two simultaneous series of waves, 'large' (of low frequency) and 'small' (of higher frequency); Adrian and Bronk (1929, p. 144) have found them again; Richter (1928) has similarly found two types during voluntary contractions, large waves of 45-55 per sec. and small waves of 180-250 per sec. (during reflex contractions he obtained only the larger waves, at 60-75 per sec.); and Wachholder (1928, pp. 706-718) has reported two similar series, 'types A and B.' Adrian and Bronk (1928, pp. 92, 100-1; 1929, pp. 136-7) ascribe the 'large' waves to a more or less synchronous discharge in a number of fibres, and they somewhat lightly dismiss the more frequent 'small' waves as being due to a superposition of several series of pulsations working out of phase with one another but each at much the same frequency as that of the large waves.

Adrian's new technique for investigating the pulsations on single nerve fibres is a great advance, and yet it should seem hardly feasible to discredit all the measurements of impulse frequency that were made previously. The earlier investigators, including Prof. Adrian himself, were quite alive to the danger of mistaking low frequencies, superimposed and out of phase, for a single current of much higher frequency when their electrodes led off from nerve trunks containing many simultaneously active fibres in which 'volley fire' (*i.e.* synchronism of pulsation in the several fibres) could by no means be assumed. The likelihood of, say, 3 or 4 independent series of pulsations out of phase superimposing into a series of *evenly distributed* pulsations (of higher frequency) is rather slight; the likelihood of their doing this on several successive records should seem to be

negligible. And not 3 or 4 but a much larger number of independent fibres have usually been in question, and their superimposed irregularity is mostly illegible. The assumption has always been, I believe, that in such an electroneurogram if even a fairly short series of outstanding and *evenly spaced* pulsations appeared, it represented the actual rhythm either of some predominant group of synchronized fibres or of some one fibre which the electrodes, as placed, most successfully tapped. Competent investigators have been, it seems to me, cautious in the interpretation of electroneurograms.

But if their precautions have been useless, and this difficulty of interpretation is insurmountable, I cannot see that Adrian's new technique escapes the same criticism. Very seldom has Adrian had independent histological evidence that his electroneurograms are in fact taken from a single nerve fibre—only, I believe, in certain studies of *afferent* action currents from a single sense-organ; and even here, as he himself pointed out, the possibility of 'antidromic' action introduced a complication. In general, he has *concluded* that only one fibre was involved *from the appearance of the electroneurogram.* For instance (Adrian and Bronk, 1928, p. 87), "Of the two fibres which appeared to be undamaged it is unlikely that more than one is concerned in the discharges shown in Fig. 4 C and D, for the impulses appear to form a more or less regular series and never follow one another at less than .02 sec." [4] Or again (p. 90), "It will be seen that all five experiments agree very closely with one another and this is perhaps the best proof that they show the normal discharge of a single nerve fibre. In every case we are dealing with a nerve containing several undivided fibres (the actual number varying from three to six or more), but a further reduction in the number of

[4] A singular slip! If "never . . . less than .02 sec." (*i.e.* never more than 50 pulsations per sec.) was their *criterion* of a truly single impulse series, they had then assumed in advance that all higher frequencies are *ipso facto* artifacts due to out-of-phase superpositioning. But surely this is a mere slip of the pen.

fibres has always left us with one or two apparently intact but with no impulse discharge of any kind." In short, we see that all physiologists alike are still forced to judge as to what are true impulse frequencies (and not out-of-phase artifacts) from the appearance of the electroneurogram; judging, of course, not from the frequency (!) but from the *regularity* of the recorded waves. And if, furthermore, those histologists are correct who see the single nerve fibre as itself a cable containing many independently conducting units, the fibrillæ, the human hand will perhaps never tease out an isolated fibrilla. I think, then, that the records of Adrian and Bronk and the earlier records of other investigators *as well* should stand unless invalidated on more specific grounds.

Let us agree, then, that in all probability voluntary contractions in man (A. and B., 1929, p. 137) are commonly "maintained, like the reflex contractions in the cat, by a series of nerve impulses which range from 5 to 50 or more a sec. in each nerve fibre," and (p. 145) "that a range of 5 to 100 a sec. would cover . . . the normal frequencies of the motor discharge," just *so far as* these observations do not presume to invalidate other equally competent and more generally accepted determinations of common and normal frequencies. Forbes, Barbeau and Rice (1927, p. 477) find the motor nerve frequency in the "sustained flexion reflex" to be "usually above 300 per second and often above 400 per second." Beritoff (1924 [a], p. 41) states "that the highest excitation rhythms on nerves of warm-blooded animals, and of frogs in summer, do not exceed 500 per sec.," and he implies that they do sometimes approximate that figure. Adrian and Olmsted (1922) once reported 150-200 per sec. as the highest flexor frequency in the spinal cat; 160 per sec. being a fair average figure (m. tibialis anticus). To this Beritoff (1924 [c], p. 176) replied that the figures seemed to him too low; he finding a maximum action current of 300 per sec. on the semitendinosus muscle (spinal cat). Beritoff added (p. 190) that on stimulation above 300 per

sec. the muscle becomes *inhibited.* Whereupon Cooper and Adrian (1924) experimented anew, and found that the maximum frequency of action currents on the tibialis anticus muscle (spinal cat) range from 240 to 320 per sec. They admitted that there had been an error of technique in the previous work of Adrian and Olmsted.   (They added, p. 81, "We have, therefore, no evidence that the rate of discharge from each motor neurone can exceed 240-320 a second"; though I could not see that there was any evidence as to whether the discharge on the *neurone* did or did not exceed these figures.)   Since this work was done in order to settle a controversy, I make no doubt that it was done with exceptional care, and that it is exceptionally reliable.

The value 300 per sec. is, very roughly speaking, a fair general average of the meaurements by various observers both for the very *maximal* action currents (*Eigenfrequenz*) of muscles and, as I have said before, for the higher frequencies on motor neurones.   And, as I have already tried to show, this rate is certainly not far below the frequency at which the individual pulsations on the motor nerve must be so attenuated, by partial overcrowding, that they will hardly pass the decrement at the junction between the nerve and its muscle (myoneural junction).   This is apparently what Beritoff (1924 ^{c}, p. 190) was dealing with when he found that stimulation above 300 per sec. produced inhibition of the cat's semitendinosus.   I do not think, however, that any observations which I have so far found, enable us to decide definitely, in the case of reciprocal inhibition at motor half-centres, at precisely what frequency on the motor nerve inhibition takes place in the muscle (inhibition by partial overcrowding); nor to decide, even, whether or not inhibition by *complete* overcrowding regularly occurs on the motor nerve.   I do think that in our present state of knowledge, reciprocal inhibition of muscle by *partial* overcrowding on the motor nerve and extinction at the myoneural junction seems the more probable; and that the nerve impulse frequency at which this occurs is probably

not far from 300 per second. Yet any numerical value here is quite provisional.

Inasmuch as Adrian and Bronk deem that their recent low-frequency determinations bear adversely on the theory of overcrowding, one is astonished to find, on re-reading, that not a little but *most* of this work was done with the proprioceptive impulses *deliberately excluded* by section of the proprioceptive afferent nerves. In some cases, indeed, the motor nerve was severed from the muscle as well. In the 1928 paper the authors discuss this point (pp. 96-8), and conclude (p. 97) : "Though we cannot deny the importance of the afferent fibres we do not think that the general character of the discharges has been much affected by their destruction. . . . It may be that the frequency in each nerve fibre is slightly higher when the afferent fibres are intact, but our records give no indication of this for the large waves occur at much the same rates with the nerve cut or uncut." [5]   The authors have just mentioned (p. 96) that "Dusser de Barenne and others have shown that the frequency of the large waves in the electromyogram may be reduced by 30 p.c. or more when the muscle is deafferented" (*cf.* Cooper and Adrian, 1923, p. 210).

The statements quoted of course *do* "deny the importance of the afferent fibres"; and if one is to accept them it can be only on the ground that this paper of 1928 deals exclusively with the *phrenic* nerve, a nerve which leads to the diaphragm and partly regulates breathing, and that the functional connections of this nerve are very possibly so unlike those of the nerves that lead to skeletal muscles as to render the phrenic nerve entirely worthless for any study that is designed to throw light on questions of skeletal innervation or inhibition.

The paper of 1929 is in two Sections. In Sect. I. we find that proprioceptive impulses were again excluded either

[5] The 'large waves' are, of course, the *slow* ones (ranging up to 50 or 100 per sec.). It is rather stupefying if one must understand the above to mean that the authors considered any experimental arrangement satisfactory and typical so long as it left the *large slow* waves intact.

wholly or partly, the muscle being in most cases completely severed from the (mixed, motor and sensory) nerve of which the pulsations were being studied.[6]   Here (1929, p. 132) "we have to reckon with the fact that some or all of the sensory impulses from the muscle are prevented from reaching the cord.   But it is unlikely that they will produce a radical change in the frequency of the discharge."   So far as I am able to discover, this statement is based on the authors' experiences with the phrenic nerve.   And whatever it may be based on, I submit that unless one is prepared for no adequate reason whatsoever to set aside a large number of perfectly concordant investigations, by many of the ablest physiologists of the last quarter-century, including of course Sherrington, it is an assured fact that the *exclusion of proprioceptive impulses does produce a most radical change* in the frequency of the motor discharge that goes to any skeletal (if not indeed to any visceral) muscle.

Section II. of the 1929 paper was done without the exclusion of proprioceptive impulses, but here, incredibly enough, the authors decided no longer to measure the pulsations in the motor nerve but in the muscle; the single muscle fibre, so far as might be.   To return to the sentence (1929, p. 132) which I have just quoted in part: "But it is unlikely that they will produce a radical change in the frequency of the discharge, and this is so low that the muscle fibres should certainly respond to every impulse reaching them from the nerve.   Consequently the electric responses in the individual muscle fibres should give just as accurate a measure of the nerve fibre frequency as the record made

[6] Here, as usual, the appearance of the electroneurogram, or sometimes the action current as *heard* on a loud speaker, was used to indicate the successful resection of all but a *single* active fibre.   In one case (1929, p. 122) "although the impulses evidently form a single series the regularity of the discharge is by no means absolute—in fact the intervals between successive impulses may vary by as much as 30 p.c. from the average value."   A rather irregular regularity!   Is it conceivable, after all, that "impulses evidently form a single series" regardless of their irregularity so long as their frequency is not too high, as say, never more than 50 per sec. (*cf.* the second footnote preceding)?

from the nerve itself." As the authors remark, "the muscle fibres should certainly respond to every impulse reaching them from the nerve" *if* the frequency *is low;* but equally, as every physiologist knows, if the frequency is *not* low (if it ever exceeds the *Eigenfrequenz* of the muscle) the muscles *will not* respond to every impulse on the motor nerve.[7] Beritoff (1924 c) and others have shown that even with rapid galvanic stimulation of the motor nerve, direct, the action current in the muscle cannot be made to exceed a certain frequency (the *Eigenfrequenz* of the muscle). In other words, when at last the authors do not exclude proprioceptive impulses by surgical means, they select for recording a phenomenon (the muscle action current) which will even more effectually cut out *all higher* frequencies, proprioceptive or other. Proprioceptive impulses at 5-25 per sec. which maintain the tonic contraction of decerebrate rigidity, were recorded (p. 137).

It is true that even under these conditions one would rather expect to find higher frequencies on the muscle than Adrian and Bronk recorded; for these frequencies ranged for the most part only between 5 and 100 per second. One would expect to find higher, that is, during *vigorous* muscular contractions; but alas (p. 141), "As before we were unable to analyze the rhythms of individual fibre groups at the height of the discharge": the "records throughout weak contractions and at the beginning and end of powerful contractions" showed, it is gratifying to know, "one or more rhythms ranging between 10 and 20 a sec."

And lastly, in spite of all, the authors ran directly on to the perfect picture of incipient overcrowding so far as a process taking place in motor half-centre, motor neurone, and myoneural junction *can* pass down and be pictured in the muscle (pp. 138-9) : "The increased contraction produced by manipulating the leg, etc., leads to an immediate increase in the frequency of the persistent impulse dis-

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[7] It begs the question to say "to every impulse *reaching* them," since attenuated motor impulses may be extinguished at the myoneural junction.

charge and to the appearance of other independent rhythms beginning at a low frequency and increasing rapidly until the record may become completely confused. Records made during the stretch reflex in the quadriceps or gastrocnemius show the same features and are often quite indistinguishable from those made from the human triceps during a progressive voluntary contraction."

Eheu! quot homines, tot sententiae.

It is unnecessary, I think, to dwell further on the papers of Adrian and Bronk; although more could be said. Any work that proposes to throw light on the theory of overcrowding, will need to recognize that the proprioceptive return impulses are of the very first importance; to study the action currents on the last motor neurone even more carefully than those in the muscle, and the processes in the motor half-centre and myoneural junction as well; to study these processes *just when* inhibition (specially reciprocal inhibition) is taking place: and lastly to study these processes, if it be in any wise possible, in the intact animal. Even a glance at the decapitated, decerebrate, or deafferented animal shows that its whole neuro-muscular tone is seriously deranged. In my opinion the two papers by Adrian and Bronk (1928; 1929), which we have just reviewed, present not the remotest vestige of evidence against the theory of inhibition by overcrowding, not even against the more extreme form of this theory, that is, the complete extinction of pulsations on the motor neurone.

In arguing against the conclusion of Adrian and Bronk so far as this relates to overcrowding, I am not arguing against any other aspect of their investigation. The generally low frequencies which they find for afferent and motor pulsations help very much to show why inhibition by overcrowding does not occur regularly and disastrously at pre-motor synaptic regions. It seems to me established by observation that (Wedensky) inhibition by overcrowding does actually, on occasion, occur. But until we better understand the relation between the *reciprocal* innervation

of antagonistic muscles and other forms of innervation, particularly the simultaneous 'rigid' innervation of antagonists (which seems on any theory incompatible with reciprocal innervation), I do not think it infallibly established that reciprocal innervation is due to overcrowding, whether at motor half-centres or at myoneural junctions. I have presented such a view because it seems to me the only one which, in the present state of physiological knowledge, has a really considerable empirical support. The theories of inhibition by nerves, impulses, or substances that are 'specifically inhibitory' seem to me purely verbal.

Inhibition by overcrowding presents serious problems; which is to say that inhibition itself is a serious problem. An argument *against* overcrowding and stronger, I think, than that of Adrian and Bronk, is to be found in the merely casual observation that many to-and-fro movements, apparently 'reciprocal' (such as the swing of a conductor's baton) occur, in which the excursion is so short and the muscular tensions so moderate that it is hard to see how any proprioceptor organs can be enough stimulated to produce overcrowding; specially if two antagonists can be vigorously and simultaneously contracted without producing it, as seems to be the case. There are many points connected with inhibition which await further investigation.

One is sometimes inclined to question whether the rôle of inhibition has not been to some extent exaggerated. The hand which under 'guidance' of the eye traces a zig-zag line is possibly, it seems to me, under no inhibition whatsoever. The to-and-fro swing of a conductor's baton is certainly in part 'guided,' *i.e.* innervated, by distance receptors; though here the reciprocating mechanism doubtless plays the leading part. In general, where a motion is innervated from other receptors (in addition to the always active proprioceptive innervation) the pattern is determined, I think, so far as the *other* receptors are concerned, not by inhibitions but by the ever varying amount of innervation supplied to

this or that muscle. But it must be admitted that almost everything in this field remains to be learned, so powerful and so mischievous have been the old theory of the cerebrum as the dark tower of the 'Soul' and the specious hypostatization of a 'specific inhibitory process.'

FURTHER FACTORS IN INTEGRATION: SUSTAINED
RESPONSES AND THE LOCUS OF FREEDOM,
CROSS-CONDITIONING

WE have considered, as the first two steps in the process of integration, the external reference of all acquired responses and the immediate functional interrelations of these responses. We now come to further phenomena that make for integration. As with all classifications, this attempt to divide the integration process into steps or stages is misleading even though unavoidable. The crude succession of these phenomena *in our attention* corresponds only to a faint hint of *such sequence between* the phenomena. These for the most part are simultaneous, and they occur largely as the environment prescribes. In the living organism they are not listed.

Through the learning of responses, adient and avoidant, an organism comes to move and act with definite reference to objects in its environment. But indefinitely many stimuli are at every moment soliciting the organism, and yet in general the organism responds only to some of *these* appeals; so that instead of merely falling a prey to confusion it normally responds with some single and more or less definite action.

One is inclined at first to ascribe this tendency toward selection and unification to the fact that certain pathways, because of more frequent use, are more deeply canalized. Such pathways will present at synapses somewhat less resistance to the nerve impulse, so that deep canalization probably favours reflex response much as intensity[1] of sensory stimulation favours it. Both factors of course

[1] *I.e.* higher frequency of all-or-none pulsations (Forbes and Gregg, 1916: Adrian, 1926 a; 1926 b: Adrian and Zotterman, 1926 a; 1926 b).

count in the final functional competition between reflexes which takes place at motor half-centres, as we have seen in the two preceding chapters.

The main clue, however, to this tendency toward selection and unification, the third step in integration, lies in the fact that by the principle of reflex-circle every adience *tends to maintain itself*, to get *more* of the stimulus. As we proceed on this clue we shall see a phenomenon emerging which resembles what is called 'concentration of attention': but it is not precisely like 'concentration' as described in text-books of psychology. The concentration of attention is traditionally supposed to involve some sort of selection among afferent ('sensory') impulses, and this implies a rejection, exclusion or inhibition of many of the afferent impulses. In fact, however, except in the case of the eye with its semiopaque lid, contractile diaphragm (and retinal pigment), afferent impulses cannot be excluded: and I am not aware that exclusion or 'inhibition' of afferent impulses has any physiological meaning. The selective aspect of 'attention' is to be found altogether on the motor side; as we shall see. And this fact gives us a significant hint as to where, in connection with the physiological mechanism, 'consciousness' is to be found.

If one observes a little child of two years, say, with a ball in its hand, one will often notice the little fingers bending and unbending while *retaining the contact* between the tip of each finger (of the palm also perhaps) and the ball; thus the fingers *slide* to some extent over the surface of the ball. If both hands are in contact with the ball, one can more easily observe the finger-tips and palms sliding over its surface while retaining their contact with it. The same is more evident still if the little child's hand has touched some larger, more stable object—the edge of its crib, or the leg of a chair; the hand retains its contact and slides along the surface as far as the arm can reach, and then back again still touching. Such feeling along objects, gropingly, is characteristic of the very young.

The principle of this is simply *random movement with adience sustained.* The contact between hand and object, once made, is maintained on the reflex-circle principle, and it defines a *locus* within which random movement still has freedom to play. This locus or dimension of freedom is the *surface* of the object touched. And within this locus the random movements are constrained to 'explore' the surface of the object; thus chain reflexes are developed (and fixed as neurograms) which, even when the object is absent, will *re-create* its surface contour. We shall meet this process again in connection with 'empathy.'

Random movements within the locus set by one or more sustained adiences, give us the very pattern (indeed the definition) of all 'exploration.' A wheelbarrow, or a toy similarly constructed with a wheel and handles, sets such a locus of freedom for any little child, and it will push the wheel about for a considerable time contentedly. It learns much in the course of these 'explorations.' Such a toy 'fascinates' a child; that is, the adience in question easily maintains itself against competing sensori-motor responses. It can be observed, too, that this adience is not merely that of the grasping reflex; rather, the two handles serve as extensions of the child's arms, and the adience maintained is the steady contact between the wheel and the floor or ground. This means physiologically that not merely the grasping muscles but all muscles which can contribute to keeping (the child walking and) the wheel in contact with the floor are acting synergically to this end. It is this contact which sets, in this case, the locus of freedom. And not different fundamentally is the behaviour pattern of an explorer to the South Pole; the adience sustained being a motor orientation toward one pole of the mariner's compass. Within this limitation the explorer fares through thick and thin.

It may be noted that the general principle of random movement with one or more adiences sustained presents no

particular difficulties when considered in terms of reflex conduction. Certain reflex-circles remain prepotent, and any other reflexes *not directly antagonistic* to these remain free to function. It is to be noted too that the locus of freedom is at the same time a limitation of freedom; the *limitation*, precisely, being that which makes for unity and integration in the behaviour of the organism. Or in other words, the object for which adience is maintained serves in the all-salutary capacity of *obstacle*. And *obstacles are guides*. It is not to be wondered at that integration cannot be found by looking merely within the organism, for it is always a progressive negotiation between the organism and its environment.

Now if one adience (reflex-circle) can maintain itself, so can others; and the number of simultaneously sustained adiences is bound to be fairly large. In the human infant since the hands easily touch each other, they acquire a considerable adience each to the other; and since the eyes inevitably see the hands, they soon learn adiently to follow the hands, and the hands to follow the eyes. This means that the proprioceptive afferent impulses arising from a movement of any one of these become conditioned to (canalized into) motor paths controlling the others, in such a way that the two hands and the two visual lines of regard tend to come together at one point in space. Therefore the eyes and hands as sense-organs will tend to receive impulses from one and the same object, and as motile members their responses will be responses to that same object. For instance, if a child's two hands are holding a ball, his eyes will almost certainly be looking at the ball; and if the surface of the ball is the locus of freedom for further movements of the fingers and hands so it is the locus of freedom for further, simultaneous movements of the eyes. Clearly the child's activities ('attention') will tend to be held down to the ball.

It may be somewhat in this direction that Sherrington (1906, p. 347) is looking when he says, especially of the

higher forms of animal life, that "the juxtaposition of groups of specially refined receptors in one set of segments, the leading or heaa segments, conduces toward their simultaneous stimulation by several agencies emanating from one and the same environmental object. Thus, the property of brightness and the property of odour belonging to an object of prey may then better excite in unison a reaction in the distant reagent [responding muscles]. . . . Thus a reaction is synthesized which deals with the environmental object not merely as a stimulus possessing one property but as a 'thing' built up of properties. A reflex is attained which has its psychological analogue in a sense percept." Afferent impulses will tend to be sent in simultaneously from all the senses, and emanating all from the same object. Thus when a source of sound stimulates, as usual, both ears, not the sound as 'consciously heard,' but the different intensities of the sound waves as they stimulate the two ears (or, in the lower range of pitches, the difference of phase in which the waves reach the two ears) causes the head and eyes to turn toward the point in space from which the sound emanates. If the source of sound is near enough, the hands will tend to follow the eyes, and to grasp the sounding object.

In general, every sustained adient reflex (reflex-circle) defines a limited locus of freedom for random movement. If then, several adient reflexes are simultaneously (and continuously) operative, the actual locus of freedom is reduced to that more limited range that may be *common* to the several loci defined by the several sustained reflexes. The freedom of the organism is more sharply defined and limited by every additional response that is actively maintained. Thus if the child trundling its wheel is also adient to certain bright spots in the pattern of the carpet, it will roll the wheel from one bright spot to another: and if it is called to dinner it will push the wheel toward the door but only *via* the bright spots. For brevity I have mentioned only adient reflexes, but clearly any sustained reflex, adient

or avoidant, will contribute equally well toward defining a limited and definite locus of freedom. Thus the child just mentioned may have its locus of freedom further circumscribed by avoidant reflexes that are stimulated by a hot radiator on one side of the room and on the other a petulant aunt asleep in her chair. It is 'alive to,' that is, responding to many stimuli *at once,* yet its activity instead of being thereby confused or scattered is made just so much the *more definite and precise.*

We have been considering a strictly physiological (sensori-motor) phenomenon, of which we understand the mechanism, and yet its connection with the 'concentration of attention' is unmistakable. If the phenomenon of attention is once understood from the strictly physiological point of view, many of the 'faculty' presentations which one finds in text-books are so far astray as to seem well-nigh idiotic. The principle of locus of freedom narrowed and made precise through the simultaneous action of many responses throws more light on the mental life, and the so-called 'field of consciousness,' than any other *single* principle that I know of.[2] The number of simultaneous responses which the outside world stimulates and sustains is very great: they are sustained at varying degrees of intensity, the larger part of them at the intensity of the merest *tonus* or *posture* since many stimuli are weak or spatially remote (though if they grow more intense or advance nearer, the organism is ready and 'alive to' them) ; the component responses, in their varying degrees of stimulation, interact most variously on one another, inhibiting and re-enforcing, and their 'algebraic' total in each and every pair of antagonistic muscles becomes evident, moment by moment, as *conduct*—both physical conduct and mental conduct.

I have previously spoken of a *total afferent* (or sensory) *pattern.* Each least afferent impulse, as we know, spreads

[2] It is an aspect of the phenomenon which I have elsewhere (1915, p. 75) somewhat loosely called "the recession of the stimulus." The reader will perhaps perceive that we have here, *in part,* the physiological basis of 'abstract' thinking.

widely to many of the organism's muscles, so that the total pattern of afferent impulses, here re-enforcing and there inhibiting one another, will involve in contraction or tonus or relaxation more or less of the entire musculature of the organism—a *total motor* (or efferent) *pattern.* At every moment of life, corresponding to the total sensory pattern there is a definite and total motor pattern. From moment to moment the sensory pattern shifts, as some stimuli wax or wane, others cease altogether, and some new stimuli commence to play on the organism; and therewith shifts at every moment the total motor pattern. Each new stimulus has its own pattern of motor outlets, but its various efferent currents play into the *already functioning motor* pattern, and to a greater or less extent alter it, in some places re-enforcing and in others antagonizing the muscle states which they find functioning at the moment. So that never can the motor result of a given stimulus be observed integrally, but only as it *modifies* some already functioning total efferent pattern; hence the large variation found, in physiological and psychological experimentation, between the readings obtained even from absolutely invariant and successive stimuli. As Beritoff has remarked, 'a stimulus' is merely *one additional* stimulus (with a spreading motor pattern of its own).

In this superimposition, so to say, of new motor patterns on an already prevailing *total* motor pattern, Magnus (1924, p. 26) remarks, "the motor centres of stretched muscles are, as Uexküll expresses it, 'set,' and any sensory stimulus therefore will cause, in the animals in question, only the stretched muscles to contract" (*cf. ibid.,* p. 43). This is oftentimes true, and can be readily demonstrated while you are caressing your pet dog or cat, provided that the animal is in such a posture that some muscles *are* actually *stretched.* The observation, if I am not mistaken, was made on (and is more specially applicable to) those muscles which with their motor half-centres are most habitually employed in reciprocating movements of progression; it is not so

readily demonstrated on other pairs of antagonists as, say, those which move the finger joints. But in a reciprocating movement, when a limb has almost completed a swing and its motor half-centres are almost ready to reverse, nearly any stimulus will hasten the reverse swing, *i.e.* will cause the stretched muscles to contract. I believe that this observation is only seemingly at variance (although, as we have seen, delicate problems are here involved) with the fact, implied in 'algebraic summation,' that in general a muscle is all the more ready to contract in response to an additional stimulus when its tonus is already high or if it is already partly contracted *provided that* its antagonist is not yet very much stretched. In order to think dynamically in these matters we must quite discard the notion of single reflex arcs, and realize that any the least stimulation of a living organism involves the superimposition of an additional motor pattern on an already functioning total motor pattern, with the result that inhibitions, tonuses, and active movements may be increased, reduced, or reversed in widely distributed portions of the musculature.[3]

The factor of integration which we have been considering is, briefly, as follows. Every response, whether it is tonic, postural, or actively phasic, is *action*, and it defines a residual locus of freedom; every additional response that is simultaneously in force makes the action more definite and precise, and by just so much narrows down the residual locus of freedom. Within this ever narrowing locus, random activity can still play.

We now turn to another and fourth factor making for integration, of which the mechanism, dynamogenic or cross-conditioning, is quite different. Any young animal or a child that is sufficiently left to its own devices, almost as soon as it can creep about at all independently, will begin

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[3] Prof. Köhler (1929, p. 130) has well remarked that "at some point functional problems must be treated as truly functional." Within the frame of 'machine theory,' as the *Gestalttheoretiker* likes to call it, we can see that there is still room for the most copious display of 'dynamical interrelations.'

to show that it prefers certain lines of activity to others; it will have its favourite nooks for repose, its favourite haunts, favourite playthings, etc.   If the environment is fairly stable and such nascent habits are not disturbed by outside interference, the preferences grow stronger and very soon the young organism will pursue these preferences even in spite of obvious hindrances and discomforts.   It has begun to "have a character and will of its own."   Something, clearly, has already begun to operate toward not only selection but also fixation of the organism's activities, that is, toward further integration.   We are so addicted to word magic that we dismiss this phenomenon as being merely a case of 'heredity' or 'habit,' as if such a word explained the matter.   And as a penalty we forfeit an important clue to the mechanism both of character and of mind.

At the outset it can be seen clearly that the young organism moves about aimlessly; its little repertoire of adiences and avoidances is merely played upon by the objects, of one sort or another, that happen to impinge on its sense-organs. One action follows another, as the environment dictates and elicits.   If a mild annoyer is present, the organism is restless.   If there is any activity that is not clearly dictated by the environment, it will be of the strictly appetitive sort (hunger, etc.) which is touched off by an immediate internal stimulus.   But there is no trace of that mysterious internal 'will,' that apparent self-determination which will presently assert itself seemingly in defiance of the environment, and to some extent even in defiance of appetite.

The phenomenon in question does depend, at the outset, on 'habit,' that is, on the existence of fairly well canalized pathways, or neurograms, leading to definite responses toward definite objects that are stimulating the organism; those adient and avoidant responses which we have studied. Such habits are bound to develop, as we have seen.   Further, they will become well canalized if the organism is not incessantly disturbed by outside interferences (as, say, by an over-solicitous mother), because the same organism liv-

ing in the same environment will be stimulated day by day in much the same way.

But a neurogram howsoever deeply canalized will not induce a posture or other response unless it is fed by impulses from some source of stimulation. Why then should a little boy carry to his play the stooping posture that he got from leaning forward to study at his school desk, or carry to his home the blinking expression of his eyes and the scowl that he got from playing all afternoon in the glare of the sun? This can be explained in part from the fact that after exercise, if it has not been too excessive, any muscle will maintain itself at a heightened tonus for several hours; and this is rather certainly due to heightened activity of the proprioceptors. The benefits of gymnastic exercise are doubtless due in good part to the reawakening of such circular (proprioceptive) reflexes which then maintain themselves at a healthy tonic, or indeed at *postural* intensity. I believe, too, that such *self-maintained circular reflexes,* which may escape any serious inhibitory check for considerable intervals of time, are of great significance in the mental life.

But the little boy's stooping posture and left-over facial expression will soon be fed from a much more durable source. While stooping over his books the boy's total afferent pattern comprises many impulses besides those which are stimulated by the desk and books to which he is immediately responding. There are the characteristic sights, sounds, and odours of the school-room, and also various more or less continuous afferent nerve impulses from the boy's own internal organs. All of these concomitant afferent impulses, according to Pavlov's law of the conditioned reflex, are to some extent acquiring *collateral motor outlet* into the lad's posture of sitting and leaning forward over his books. As this continues day by day, and if it is not counteracted by distinct change of posture and diversity of motor activity, the stooping posture besides being well canalized will come to be so *steadily innervated* by such

extraneous and ubiquitous afferent impulses as to be beyond correction. And this is dynamogenic or cross-conditioning. Of these concomitant stimulations, the sights and odours of the school-room will be active only when the boy is there, but all the afferent impulses from organic sources he carries about within him. And when any posture or motor habit has been so persistently maintained as to become, more deeply than any other postures and habits, cross-conditioned to afferent impulses from the internal organs (as, say, heartbeat and breathing rhythm) it has become in very truth 'second nature.' It is established for life.

Cross-conditioning manifests itself in a variety of ways. It is clear that ubiquitous and perennial external sources of stimulation, such as gravitation and daylight, apart from the responses they may elicit which are directed toward themselves, are bound to feed collaterally into the very channels that activate the incessant bodily processes such as heart and lung rhythms, perhaps even all autonomic channels; sunlight, for instance, has more than an actinic effect on our somatic (and mental) well-being. The rocking motion of ships does not annoy sailors, but instead contributes tone to even their vital processes; they suffer, sometimes acutely, if obliged to remain ashore. The rumble of a waterfall, the din and odour of factories and cities, and other 'mild annoyers' contribute unsuspectedly yet demonstrably to the organic tone of persons who have long lived in their vicinity. Such stimuli as these, unless encountered in babyhood, are at first 'mild annoyers,' but if circumstances force a person to remain within their range they slowly gain outlet into all of his habitual motor channels, vegetative and animal. Annoyed at first by the presence of these stimuli, he comes later to suffer from their absence. If encountered in babyhood they become almost indispensable to bodily well-being and normal behaviour. Hence the feeling of being 'let-down' physically and mentally when one is obliged to leave an environment to which one is utterly habituated, and the sense of exhilaration on return-

ing after an absence; that is, if one has enjoyed health and contentment in that region. The prevailing local stimuli will tend, of course, to reinstate in the one who returns, any motor pattern that has been most characteristically his during the previous residence.

Cross-conditioning throws considerable light on the phe-nomenon which is commonly called 'mood.' A yachtsman, for whatsoever reason he has become one, is by and large out on the sea only on bright days when the wind is fresh. Sunshine and wind, then, are steady concomitants of his yachting activities, and through this circumstance they are cross-conditioned to innervate mildly the postures and mo-tions of yachting. So presently, though inland far he be, and tied to his office desk, the appearance of sunshine and a fresh breeze outside his window may stimulate the man's whole yachting complex, and plague him with an unaccount-able 'mood' or 'desire' to go yachting which may demoralize his whole working day.

The following illustration is taken from Preyer (1884, pp. 165-6) : it is clearly a case of cross-conditioning, and I omit Preyer's interpretation in terms of 'pleasure and pain.' Two children suffered during their first six months of infancy, from eruptions of the skin. At first, their arm movements were too unco-ordinated to enable them to scratch the affected parts. But they soon learned to reach up to their faces, which were accessible because uncovered, and then, "At every moment when they were not watched the hands went up to the head, and the skin, even where healthy, was rubbed and scratched. These scratching move-ments cannot be inborn and must be acquired. An acci-dental contact of hand and head resulting in a decrease of the itching sensation must necessarily induce a preference for the hand-to-head motion, over all other movements." That is, the restlessness produced by a mild annoyer led to trial-and-error learning and alleviation of the annoyance. "Now this reflex reaching toward the head led, as a further consequence, to a peculiar association in one of the two cases.

(Observations on the other child are lacking.) As, namely, the eczema healed and finally disappeared, the habit of lifting the arms and carrying the hands to the head persisted, and reappeared whenever the child met anything disagreeable or whenever it manifested opposition, as when it did not wish to play or did wish to stop playing. . . . In this way peculiar expressive movements originate from acquired reflexes." [4]   In physiological terms this means that the hand-to-head motion was so widely cross-conditioned to irrelevant afferent impulses that not only it frequently reappeared after the original source of stimulation (eczema) ceased to exist, but also it served as a ready and characteristic path of nervous 'overflow' in every situation of partially obstructed activity; for, as we shall see, the 'disagreeable' means motor conflict, and this in turn produces more or less (random) nervous overflow.

This motor re-enforcement which responses receive through being cross-conditioned to irrelevant and more or less ubiquitous stimuli has received the name of 'dynamogeny,' [5] and it is to be found at all physiological levels. It is bound to puzzle any student of behaviour, since it impels the animal (or man) to lines of action for which any immediate and visibly operating stimuli seem inappropriate, inadequate, or indeed absent. Without a knowledge of cross-conditioning, the observer would hardly surmise that present yet *totally irrelevant* stimuli are providing in-

[4] Preyer astutely adds: "Obviously we have here a primitive process of induction or generalization." That is, the hand-to-head motion became, in the adult individual, the 'expression' or 'general sign' of *any* state of displeasure.

[5] Many of the phenomena here in question have been described by Charles Féré (1887), William James (1890, II., pp. 372-382), and others. There is some confusion in psychological literature as to the application of the term *dynamogeny* ($=dynamogenesis$). It was introduced, so far as I can learn, by Féré, before the conditioned reflex was at all generally known (*cf.* James, 1890, II., pp. 583-5), and he used it to designate two phenomena which are now seen to be quite distinct: first, the re-enforcing effect due to cross-conditioning, which most authors call *dynamogeny*, following James; and second, the phenomenon which James and many others have called *ideo-motor action*.

nervation for the animal's present course of action. Nevertheless, a closely related phenomenon was observed long ago and partially explained: it was named *'Bahnung,'* I believe by Exner (1894). It is the phenomenon in which a reflex motor path, because it is *already* (and often from unknown sources) *subinnervated,* tonically or posturally, produces an unexpectedly vigorous movement on receiving a very slight additional stimulation. Such a path is said to be *gebahnt,* or subinnervated.[6] A very notable source of Bahnung, or undertone, is of course cross-conditioning.

Not only is the fact of *Bahnung* through cross-conditioning hidden from the observer of animal behaviour, but also it is no direct fact of consciousness for the organism itself. A person whose heart-action is quickened by listening to music, hears the music and may or may not be aware of his heightened pulse; but the afferent impulses which go from the ears and speed up the pulse, figure not at all in his consciousness. This shows that at least some sensory impulses help to innervate movement without presenting themselves to consciousness. The monotonous din of waterfall or factory ceases after a time to be consciously heard at all, though it continues to exert its dynamogenic influence, as is shown plainly enough whenever such a sound suddenly ceases.[7] This is sometimes called 'sensory adaptation,' but called so merely because if 'consciousness' sits in the cerebral cortex, as many persons seem to believe, any interruption between sensory stimulation and 'consciousness' must lie in the sense-organ or somewhere along the afferent path. There are genuine cases of sense-organ adaptation (as

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[6] Here again the nomenclature is confused (Fröhlich, 1909, pp. 72-3). Bahnung really involves summation of nerve impulses by *successive* increments; for this pre-paration I shall use the term *'Bahnung'* itself, 'subinnervation,' or 'undertone.' The *simultaneous* convergence of nerve impulses (at a synaptic region) will be called 'summation.' The terms 're-enforcement' and 'facilitation' seem bound, in view of current usage, to refer to either or both phenomena: although 'facilitation' rather *tends* to mean the same as *'Bahnung.'*

[7] It is at least worth looking in this direction for the explanation of most cases of 'stimulation' through *discontinuance* of the stimulus.

light-adaptation of the eye), but in the cases which we are considering the sense-organ is *not* too far 'adapted' (or fatigued), as is proved by its continued dynamogenic action.[8] The monotonous sound ceases to figure in consciousness because the organism cannot continue indefinitely long to respond specifically to it, but is sooner or later forced to other activities which inhibit this specific response. Yet the organism can still at any time 'hear' the sound merely by responding ('attending') to it. It is important to bear in mind that *sensory impulses can pass through the central nervous system and accelerate, or retard, motor processes without 'entering consciousness' so long as they do not arouse a response specific to themselves.*

I incline to believe that the importance of cross-conditioning as a factor in integration, that is, in the permanent setting of the organism's motor predilections and general character, can scarcely be overestimated. The organism's earliest motor habits which are not only well canalized but are also cross-conditioned to those ubiquitous external stimuli which will always play upon the organism (gravitation, sunshine, oxygen, etc.) and also to the deep internal rhythms and other somatic afferent impulses (which seldom if ever figure directly in 'consciousness'), are bound to remain *prepotent* through the organism's entire life. They are, as Le Dantec would say, the great, early 'deformations'; the preferred action systems. They give to the organism, and very early in its life, an appearance of 'self-determination,' an *apparent* independence of immediate external stimuli, which baffles the outside observer and prepares him to accept without further question the doctrine of 'inherited tendencies.'

One of the most indisputable of Freud's observations is that a child's character is in almost all essentials *set* very early in its life, say by the end of its fifth year; and Freud-

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[8] The terms 'sensory adaptation,' 'sensory fatigue,' and 'sensory training,' as used in psychology, are in too large part 'logical constructions,' the legacies of Cartesian dualism, and are subject to revision from the side of physiology.

ian literature is full of examples which illustrate and con-
firm this view.[9]   And in the second volume of this book we
shall meet further confirmation of it, coming from an un-
expected quarter.   The child who becomes very early cross-
conditioned, firstly, to a correct carriage and a generally
normal deportment, and secondly, to some *definite* type of
'play,' is already 'deformed,' and vastly to his advantage.
The line of play will be a true focus of *interest*, the nucleus
of his outer and of his inner life; it will determine the di-
rection of his trials and errors, and will be the root and
stem to which practically all that he subsequently learns will
(*if* he is not too much interfered with) be organically at-
tached.   This 'spontaneous interest' is like the sap which
from the roots and stem pushes out buds in all directions,
which later bear leaves and flowers.   This, however, only
on condition that the very little child is left largely alone
to its own devices in some safe little world of simple and
inarticulate objects, with as little molestation as may be;
apart, of course, from the normal ministrations.   And if
any one supposes that the normal unfolding and organic
development of an infant's personality is due to 'hereditary
factors' rather than to the cross-conditioning of spontane-
ous activities, he can find a plenty of cases to show how
easily 'heredity' may be circumvented.   Many children are
so continuously distracted by neurotically solicitous elders,
so bewildered by a profusion of amusements, 'advantages,'
and 'opportunities,' or so persistently harassed by a grim
and nagging meddlesomeness (frequently downright sadis-
tic) of parents, attendants, or teachers, that no spontaneous
activity of the child *can* acquire re-enforcement from cross-
conditioning; no *nucleus* toward an independent personality
can get even a start.   The result is that 'heredity' takes a
quite different turn, producing a colourless, desultory, and
listless creature, whose 'fated destiny' is disaster.   And if

[9] An interesting picture of this cross-conditioning, occurring between
the fourth or fifth and tenth or eleventh years of a boy's life, is to be
found in Mr. H. G. Wells' "The New Machiavelli." The description reads
like a bit of autobiography.

we had less 'heredity,' 'fate' and other forms of superstition, we should have vastly less disaster.

The four factors which I have described as making for integration, are: first, the external reference of all learned responses; second, the mutual summation and inhibition of responses; third, simultaneously sustained responses with a residual locus of freedom (for random movement); and fourth, cross-conditioning. Of these I suspect that, as a nicer point of logic, the second is not an independent integrating factor, but rather an aspect of the mechanism in which and through which the other three integrating factors operate. I must confess to the same misgiving with regard to a fifth phenomenon, *motor block*. Like summation and inhibition, it is an important phenomenon in both physical and mental life, and is closely related to inhibition: but whether, exactly, it should be classed as an independent factor making for integration I do not quite know. Nor do I greatly care; lists are artifacts, and I do not in any case presume to have made out all the factors of integration. Motor block is at least a very important phenomenon, not unrelated to integration, and it deserves separate consideration. Moreover, it leads directly to what I suppose is the ultimate phase of trial-and-error learning.

MOTOR BLOCK

THUS far, our growing organism has learned the basic out-going instincts, and the concatenated and other habits therefrom derived of going out to explore anything that it comes upon, of echoing, of imitating, and so on. It has also, we will suppose, through random trial and error, ac-quired the habits that will enable it to escape ordinary harm and those which will appease its appetites, as these, at their more or less regular periods, make themselves felt. Once these habits are learned, the appetite stimuli act not other-wise than the other, the external, stimuli to touch off their respective reflexes; not otherwise, that is, save that the appetite stimuli are so located that they admit of no evasion, and not safely of much delay. In short, we will assume that the organism is equipped, quite mechanically as we have seen, to procure for itself the means of subsistence and of sexual satisfaction, and to avoid the ordinary injuries and discomforts. And let us also suppose that through cross-conditioning and the other factors of integration it has acquired a tolerably unified, preferred action system of its own, the foundation of character and personality. The adi-ences, here, predominate over the avoidances if the early environment has been normal.

Although now many of the organism's actions, by reason of cross-conditioning, seem to be 'self-determined' and in-dependent of present sense stimuli,[1] they are actually no more so than they were in the beginning. The fundamental scheme of animal drive remains unmodified. The energy derived from food is stored in the sensori-motor tissues and

[1] An appearance which has led one physiologist to the semi-playful hypothesis of 'so-to-say stored stimuli.'

there released by the stimulation of sense-organs, external and internal; so that, as a man drives a horse, so the man himself is driven to action by the moment-to-moment irritation of sense-organs, without and within.

It may repay us to glance for a moment at a few familiar schemes of animal drive. These all err, in my opinion, through a failure to analyze the psychological phenomena into concrete physiological terms. The most familiar, and historically the most ancient, 'drives' are 'pleasure and pain': and these are, in more precise modern terms, the two 'affects' or 'feelings,' namely 'pleasantness and unpleasantness.'[2] These terms are of course purely psychological, and tell us nothing if we are seeking for the actual mechanism of bodily and mental processes. The same is true of all other psychological categories, such as 'emotion,' 'desire,' 'impulse,' etc., when they are presented in the guise of drives to action.[3] It is a step toward physiology when Prof. Thorndike divides the releasing stimuli into 'satisfiers' and 'annoyers'; as was indicated by Ribot (1889, p. 12), "Inasmuch as pleasure and pain are only the signs that certain of our tendencies are satisfied or opposed," etc. Yet a mere stimulus neither satisfies nor annoys, it stimulates; so that a larger setting has to be defined before we have the physiological processes which are implied by 'satisfaction' and 'annoyance.' Satisfaction ensues if the new activity stimulated re-enforces or harmonizes with what the organism was already doing; annoyance ensues if the new antagonizes the preceding activity or if the new stimulus produces random restlessness (as in the case of

[2] The traditional 'pain,' of course, never meant merely that particular sensory *quality* which is now designated as 'pain.'

[3] Psychologists, however, may well be pardoned for such shortcomings as long as prominent contemporary physiologists continue to speak of 'fear,' 'anger,' 'emotional and affective states,' and the like as dynamic factors, in monographs on reflex conduction and other strictly physiological topics. We shall have occasion, in the second volume, to consider 'pleasantness and unpleasantness,' the 'emotions,' etc., and to discover *why* psychologists have from the earliest times associated these phenomena with physiological drive.

mild annoyers). Ettore Regalia (1919, pp. 45, 110) has
insisted that 'pain' (*dolore*) is the sole spur to action; ad-
mitting, oddly enough, 'desire or unsatisfied need' as the
'negative form of pain.' The system of Richard Avenarius,
uncouth and in ways fantastic, has an air of coming closer
to physiology. He uses the notions of stimulus (*Reiz*), the
nervous system (*System C*), and utterance-value (Aussage-
werth). The last is a peculiarly happy invention for a
philosopher, since it involves a triple equivocation: it com-
prehends, without any petty or inconvenient hair-splitting,
physiological responses which are merely responsive muscle-
twitch and then, firstly, responses (vocal and other 'utter-
ances') which involve meaning; secondly, those which in-
volve the conative element of 'value'; thirdly, with these
two illicit props, the term is to signify any mental state!
In this way epistemology is greatly simplified. It presently
appears, furthermore, that the stimulus general (*Reiz*) is
for Avenarius an *annoyer,* and that the System C, con-
stantly annoyed, is always *seeking* to recover its 'equilib-
rium' (so that teleology is again illicitly slipped in). So
far as physiology goes, the system of Avenarius is merely
a heavy caricature. And yet I believe that the reader will
find nothing that approaches any more closely to a respon-
sible reckoning with the facts of physiology in either the
Objective Psychology of W. Bechterew (1913) or W. Köh-
ler's *Gestalt Psychology* (1929). I regret even more to say
that the Freudian factor of drive, the 'libido,' is, in my
opinion, one of the weakest points in the whole Freudian
edifice: I know of nothing either in physiology, or in the
phenomena of behaviour, or yet in the introspective field,
to which this 'libido' can be said to correspond (*cf.* Prince,
1929, pp. 127-8). It will be recalled that in regard to drive
I have spoken of stimuli, external and internal; of adiences
learned by reflex circle when the stimuli are weak or mod-
erate; and of avoidances (indirectly, of further adiences [4])

[4] I believe that, if logically analyzed, all reactions that are acquired
by trial-and-error are initially of the avoidance type. But when, as in

learned by trial-and-error when the stimuli are overstrong, or when for any other reason there is nervous overflow. I believe that the concepts here involved are strictly physiological, and that they involve no surreptitious element of teleology.

Such, in barest outline, is the mechanism of 'drive' and of the learning process in the higher organisms. We have studied it in some detail. In our second volume we shall see whether in the action of this mechanism we find involved those phenomena which by social and academic tradition we have come to believe constitute consciousness and mind. I say 'academic' rather than scientific, because not a little of this tradition, not a little of the 'science' of psychology, consists of pedantry and myth. Civilization is addicted to verbal magic. And I may say at once that the mental phenomena with which I am acquainted, even the myth, magic, and pedantry, do find their place and, I believe, their explanation within our biological scheme.

Our immediate question, still looking at the mechanism, is—What more? There are humbler organisms in which, to our alien and therefore rather purblind human vision, these elementary activities appear to comprise the whole round of life—though I doubt the justice of such an observation. The haughty-minded of humankind judge precisely thus of their humbler brethren who labour in factories and mines, till the soil, or go down to the sea in ships. At any rate these elementary instincts and appetites are obviously too meagre an equipment to explain the life activities of

the case of appetites, the source (as the deficit stimuli of hunger) of the nervous overflow which produces restlessness is internal, while the stimulus (food) or action (eating) which causes the internal stimuli and restlessness to cease is external and conspicuous, it seems far-fetched to call the response 'avoidance'; though in truth it is that. Moreover, *after* such avoidances have been learned the internal appetitive stimuli are in fact drafted into the motor paths of adience (the seeking and consuming of food, the seeking of a mate, etc.) ; the internal stimuli *do* produce adience (*cf.* Chapter XIV., end). Entirely analogous is the case of that trial-and-error learning which, as we shall soon see, results from motor block. The learning is avoidance of motor conflict, the result may be and often is adience.

any human being whom we observe at close range. We know of hopes and ambitions, unrealized dreams, tenacious loyalties, grim determinations and, perhaps pervading all else, that surely potent thing—self-interest. There is so much more, in fact, that the fundamental acquirements so far discussed seem hardly more than the setting of the stage on which the drama of life is to be played.

It is here that psychologies, even the best, are strongly tempted to float off into myth; to talk of man's pursuing pleasure and fleeing pain (as if these were forces), to adduce the emotions as mainsprings of the higher life, or to discourse quite solemnly of an exalted *vis a fronte* superseding at this point the mundane *vis a tergo* with its vulgar utilitarian drives.

Just as it is possible to observe at the behaviour level, and regardless of the concealed physiological mechanism, that an organism obeys the call of hunger or of sex, so it should be possible here to observe what keeps an animal, and specially man, when his appetites are appeased, *still* plodding on and ever planning something further. Yet opinions differ widely as to what men are wanting, what they are *really doing,* in the drama of life.

To me it seems clear and certain that, looking as impartially as may be at the spectacle of human society, one finds that *self-interest,* whatever that may be found to mean or imply, sums up all the higher reaches both of animal and of human activity. It is true that an observer so acute in many ways as M. Gustave Le Bon (1914, p. 178) believes that "it can be said, to the honour of humanity, that utility, that is to say egoism, has never been its principal motive of conduct." We must remember that M. Le Bon lives in a Europe where, after centuries of Christianity triumphant, brotherly love and a tender consideration for the weaker fellow are carried almost to a vice; as was so plainly evidenced in the years of our Lord 1914-1918. Never having dwelt in a milieu so idyllic, I am quite as firmly convinced

not only that never has any principal motive of conduct *other than* utility or egoism existed, but also that it is a piece of sentimental misapprehension to imagine that any other motive of conduct would more redound to the 'honour' of humanity.[5]

It is true also that Thomas Huxley (1905, pp. 81-2) believed that ethical conduct is slowly evolving to a reality, notwithstanding that "the practice of that which is ethically best—what we call goodness or virtue—involves a course of conduct which, in all respects, is opposed to that which leads to success in the cosmic struggle for existence. In place of ruthless self-assertion it demands self-restraint; in place of"—etc., etc. I suppose it is because the 'struggle for existence' loomed so large on Huxley's horizon that, so far as he was concerned, John Stuart Mill had lived in vain. Was Huxley unacquainted, one wonders, with the Socratic doctrine that virtue is wisdom, and unaware that Mill had demonstrated, convincingly and at length, that even the most admirable human conduct is 'enlightened self-interest'? The 'struggle for existence' never did justice to the rôle of intelligence even among animals, never recognized that even in the 'cosmic struggle' (*sic*) it does *not pay* to bite the kindly hand that proffers food, or to disembowel the indispensable friend. Jackals and hyænas know this, so even do the pismires; and so did John Stuart Mill, but not the gentle Darwin or the valiant Huxley. Nor, for that matter, do most other *homines sapientes* know it. Wrong doing is deplorable not because it overdoes self-interest, but because it fails to achieve precisely that desideratum; it is the conduct that later or sooner *defeats* self-interest. And *for no other reason* is it 'wrong'! The doctrine of self-restraint in place of self-assertion (Huxley's 'ruthless' is logically quite out of order) is in theory nonsense, and

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[5] It is only fair to add that M. Le Bon sees, at times, a very different side of the matter (1911, pp. 19, 25, 33): "Hunger is the most dreaded of pains; love [*l'amour*] is the pleasure that is most sought after, and one can repeat, with the great poet Schiller, that the world machine is driven by hunger and love" (p. 19) . . . "pleasure and pain. Our entire activity is derived from them" (p. 25).

wherever publicly professed it is humbug. One does well
to beware of the man who pretends to be acting *not* in his
own interest; without exception he is meditating some
coup.

In the words of the late Samuel Clemens (1917, pp. 15,
20, 34),[6] *"From his cradle to his grave a man never does
a single thing which has any* FIRST AND FOREMOST *object
but one—to secure peace of mind, spiritual comfort, for*
HIMSELF." Man works to avoid one or another sort of
annoyer. "Men pretend to self-sacrifices, but this is a thing
which, in the ordinary value of the phrase, *does not exist
and has not existed.* A man often honestly *thinks* he is
sacrificing himself merely and solely for some one else, but
he is deceived; his bottom impulse is to content a require-
ment of his nature and training, and thus acquire peace
for his soul. . . . But I pray you, do not accept this law
upon my say-so; but diligently examine for yourself.
Whenever you read of a self-sacrificing act or hear of one,
or of a duty done for *duty's sake,* take it to pieces and look
for the *real* motive. It is always there."

Helvetius, Adam Smith, and Bentham were not in error,
I believe, in declaring egoism to be a fundamental trait of
human nature. Indeed I deem it a truism that every ani-
mal motion, without exception, is self-interested. But on
the other hand the proposition is too summary and general
to carry us far into the *details* of human nature. There are
factors of training and habit, of physical and mental mor-
bidity, and the inexhaustible diversity of (self-) interests,
which have to be taken into account. Moreover with the
growth of intelligence the self, so far as conduct goes, to
some extent expands; and men act *as if* their chattels, their
rank, their parents, wives, children, and friends in varying
degree, were included in their very selves. The boundary
of the 'self' is thus not fixed. The self expands as a func-
tion of intelligence, of intelligent or 'enlightened self-
interest' (Mill).

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[6] This important little work, "What Is Man?", published posthumously,
is said to represent Mark Twain's definitive views on human nature.

Enlightened self-interest is merely foresight, and I am only astonished that altruism (by which I mean an honest interest in the welfare of others) makes such slow progress, since it rests on the simple truism, that any fellow creature is more useful to oneself (or at the worst, is less harmful) as a friend and ally, and a *prosperous* one, than as an enemy. It is easy enough to see that a selfish, predatory carriage engenders for any man a hostile and difficult environment. Kaffirs understand this; why do not Christians? It is not that men are too self-interested, but that they are too shortsighted. They sadly miscalculate their own best interests; and are so stupid as to muddle into every seductive pitfall that they come to, ever eager for the trifling advantage if only it be immediate, and oblivious of the disadvantages which it holds for the future; oblivious also of the advantages which an immediate inconvenience or renunciation would procure. As I remarked in connection with Washburn's theory of learning (Chapter XVI.), man learns only in proximity to goals, and not far in advance of his immediate apprehension of that which he desires. Still, it is unmistakable that even the silliest creature *supposes* that he is using the best means at his command for getting what *he wants*. And that I call acting in *self-interest* (*cf.* Holt, 1915, pp. 130-3). But it remains to analyze self-interest and want.

A difficulty is that 'interest' seems to be altogether forward-looking, while 'want' signifies a lack, and implies dissatisfaction with the present whatever it may imply for the future. This ambiguity, however, clears up if we look once more at the random trial-and-error situation as we have already met it. In each case of this, whether it was learning to escape from harm or from the milder physiological annoyers which we call discomforts, or learning to obtain food or sexual mate (*i.e.* escaping from internal appetitive annoyers), we have as *cause* a persistent and more or less intense flow of afferent impulses which, having no already established motor outlets, overflow into various motor paths

producing a more or less acute restlessness, *until* some random movement, perchance a movement adient to some other stimulus, is made which causes the 'annoying' impulses to cease. Since, in each case, only a rather definite movement will do this, an appearance of 'end sought' always characterizes, as I previously pointed out, the process of learning by random trial. This appearance has given rise to much misleading speculation about a *vis a fronte*. In reality the *vis* is always strictly *a tergo*. If now the activities which we characterize as 'self-interested' are all cases of learning by trial-and-error, there is no difficulty in understanding why they show both dissatisfaction with the present (random restlessness) and an apparent 'forward-looking' or 'end-seeking' reference to the future. Beyond question they *are* all cases of learning by trial-and-error. In fact I believe it is safe to say that all the new activities (habits) which are acquired by our growing organism from this point on, are learned by random trial-and-error. 'Thought' itself is such a learning; and almost every empirical study of reasoning or other 'higher' process points definitely to this conclusion. If any reader dissents from this, I beg him to glance at the history of any of the very 'highest' forms of intellectual, or of volitional effort. The history of philosophy, psychology, biology, medicine, or any other science, like the history of a people, a religion or any other human institution, is indisputably a history of trial-and-error; and overwhelmingly of error.

Returning from this brief consideration of animal and human conduct 'as a whole,' we have to ask whether the picture of physiological learning as we have so far seen it, is sufficient to account for the so various courses of action which we observe in animals and men. I believe that it is not. Considered merely as behaviour, and all problems of the mind apart, the adult life of any creature exhibits almost continuously a restless dubiety, cogitation, and scheming; and this is too multifarious and intricate to be subsumed under the rather simple principles of adient and

avoidant learning which we have studied. That it should *all* be in the last analysis self-interested, follows inevitably from the physiological structure of every organism. Fundamentally adient, it is bound to go out toward and to get always *more* of the stimulus, and to grasp and to retain it; unless the stimulus is overstrong, whereupon the organism is equally bound to avoid it. All this is the very gist of self-interest. Yet this self-interest manifests itself in so many derived forms, which are not directly (though they are indirectly) traceable to adience and avoidance!

One further step in learning does intervene. And it can be studied objectively, in the behaviour of a fellow creature, or introspectively. It is the process which follows upon a *motor conflict* arising *between* the elementary instincts, appetites, and habits which we have already studied; for the world about us is so little prearranged for our convenience, that our naïve adiences and avoidances are perennially in conflict with one another. Impelling us to diametrically opposite movements, they land us from time to time at a motor block: one is attracted in opposite directions, or attracted and repelled from the same direction; introspectively, one halts between inclination and necessity, adventure and safety, gain and risk, desire and duty, imagination and reality. The possible dilemmas cannot be numbered.

The result of such a motor block is a more or less marked *arrest* of the individual's activity (but let us not too hastily assume that this is true 'inhibition'), random restlessness ('annoyance'), and trial-and-error until some way out of the dilemma is hit on ('learning'). The ultimate motor outlet, if I am not mistaken, is not necessarily one which brings cessation of any of the afferent impulses (save proprioceptive) that are in play, but one which lets their energy flow away from the point of conflict into other and nonconflicting motor channels. All this follows so inevitably from the situation that it may be felt that I am conjuring up an artifact in emphasizing 'motor block' as a special process. It should be recalled, however, that the observa-

tions of Freud, and of his coworkers, have demonstrated that conflict, which when analyzed is always *motor* conflict, is the most significant phenomenon in the mental life. After the simpler instinctive and appetitive modes of response are acquired, a large part of life consists in learning how to avoid the conflicts, or 'surmount the difficulties' as the phrase is, in which the performance, or attempted performance, of these simpler activities (as incited by external and internal stimuli) soon involves us. In these conflicts lies the whole vast drama of the mind: out of conflict arise the most intricate and baffling modes of behaviour, and out of conflict, when trials and errors fail to bring 'success,' arise those mental disorders that are known as 'functional.' Motor conflict is the one further phenomenon which we must examine, if we are to find in physiology a fairly complete explanation of mental phenomena.

Whenever action is blocked by reason of conflict between antagonistic motor tendencies, it is to be observed that nervous energy *overflows* into random motor channels. For instance: a man sits by a country roadside with a Scotch shepherd dog lying on the ground near him. Another man, to whom by chance the Collie owes about the same degree of allegiance, comes by on the road and undertakes to call the dog away. The Collie starts up, stands looking first at one man, then at the other, and back and forth; he begins to bark and spring about, yet without moving perceptibly nearer to either man, and does both more and more violently (random action) in an actual frenzy of indecision; finally, with a sudden spring he bounds towards the person whom he was originally accompanying, sits down snugly against the man and rests his head firmly on the man's thigh. The Collie is now outwardly motionless, but his eyes are fixed on the other man and he is still under considerable tension, which subsides gradually.[7]

The heightened innervation following on motor conflict,

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[7] An actual case. As the Collie bounded over to his chosen companion, he uttered the most articulate 'emotional cry' which I have ever heard from an animal. It sounded like a four- or five-syllable word.

apparently overflowing into random channels at first, is here conspicuous. It is so marked that one is led to suspect that there was a discharge of adrenaline into the blood, an 'emotional' discharge; but even so, this would be due to the same nervous 'overflow' which produced the agitated behaviour. In this case the upshot course of action, although delayed, was not very far from being an 'algebraic' resultant of the Collie's impulses, for he had reason to fear and hate as well as to love the second man; so that this man, as a stimulus, excited both adient and avoidant responses in the dog—a nascent canine schizophrenia. It was undoubtedly the Collie's fear and hatred for the second man, acting synergically with his unqualified adience for the first, which made him in the end rush so madly to the first man and lean against him in a posture which clearly said, "I take my refuge here."

The next example illustrates 'overflow' less clearly, but random movement more clearly. Once while a guest in a friend's house, I was to spend an evening elsewhere, returning alone and late. In this house two doors intervene between the porch and the front hall. A key to the outer of these doors was given to me, and a maid was instructed to leave the inner door (by exception) unlocked. I returned as planned, and after unlocking the outer door found the inner one locked. I could, of course, have forced the door, and it was my unwillingness to do this that made the situation one of internal conflict. I at first tried such keys as I had in my pocket, but none fitted. Then came a more distinctly random period: I recall that for some seconds I stared stupidly at the friendly light visible in the lower hall and up at the dark second-story windows of the house, looked about at the neighbouring houses, blinked at the glaring electric street-lamp, commented unfavourably on the thoughtlessness of the maid, and noted the rather vivid hue of the lawn as illumined by electricity and the moon. Then I spoke and whistled softly under the windows of such rooms as I knew to be occupied, tried the three other out-

side doors of the house, observed that all the first-story windows were closed, and bethought me that the nearest hotel was not too far away, also that night-clerks take an unfavourable view of strangers who appear without luggage at one o'clock in the morning. At length it occurred to me that the maid might have been stupid enough to leave a first-story window or two unlocked. Quite so: the first window I tried was unlocked, and I climbed in almost noiselessly. That something was *learned* is shown by the fact that for a long time afterwards that particular window, unlike the others, seemed to me peculiarly inviting as a means of entrance; which means that the sight of this window put me in a posture of readiness to clamber in.

The trial-and-error learning which follows motor conflict is doubtless similar to that which follows when a tangible obstacle is encountered (*cf.* Chapter 16). If one observes a case of motor conflict at the level of behaviour, one can observe the phases which I have already indicated: motor block, increased activity in the form of random movements and suggestive of nervous overflow, cessation of these random movements when at length some way out of the motor block is by chance found. At the deeper level of mechanism, this process raises questions, some of which we must leave unsolved for the present, relying for our security on the observation of behaviour.

Firstly, the *total* motor pattern is seldom, perhaps never, involved in motor conflict and block. The conflict is more or less focalized. The action of only certain pairs of antagonistic muscles is blocked. This need not be due to true physiological inhibition. An inhibited muscle is limp and toneless. The external aspect of motor conflict suggests rather that the blocked muscles are *tense,* and their opposed tensions balanced: though of course this external aspect is not a final criterion.

The uncertainty as to whether the observable arrest of action (motor block) is due to a true inhibition of muscles or to a balance of muscle tensions, leaves us in still greater

uncertainty on a second point, the phenomenon which I have ventured to call nervous *overflow* into random motor channels. It remains problematic, so far as I can see, whether or not the mechanism here is that of actual nervous 'overflow.' This, if we recall our diagrammatic picture of the pyramidal neurogram with a sensory cell at the apex and a widely spreading sheaf of neurones leading down to many muscles at the base, consists in the synaptic charge (at one or more synapses) *increasing*, thus stimulating more of the near-lying dendrites, and spreading the nerve impulses so that they innervate more and more muscles (at the base of the pyramid). The increased activity which accompanies motor conflict suggests such a nervous over-flow, and is often so striking that I have been tempted to think of motor block as a new (though of course derived) source of motor energy, a sort of secondary drive. It is *as if* the motor impulses that are now blocked, meeting some resistance at the point of conflict, backed up as it were, overflowed at pre-motor synapses, and spread in (relatively) random directions; thus innervating many previously idle muscles. But any such notion as that of the nervous impulses *backing up* (as water does in a pipe when the lower end is stopped) is unallowable; I can find no sort of support for it in any investigations on the nerve impulse or on reflex conduction. I mention it only because, the behaviouristic picture being so misleading, it needs to be definitely excluded. Until we can know with some cer-tainty whether motor block is due to balanced innervations or to actual inhibition, the source of the energy which pro-duces the (relatively) random activities, or restlessness, here in question remains problematic.

It is just possible, of course, that the random restlessness which is observable when there is motor conflict is not due to any new or indirect source of energy, but merely to the present stimuli which are and have been playing on the organism. The rock which I cannot succeed in pushing aside, soon stimulates me to the act of climbing over it.

This aspect of the situation becomes more evident when we consider the nature of all random trials which are executed at *this* (later) stage of learning. They are not purely random; but are common reactions, previously learned, to the objects which constitute the environment in which an individual is blocked. As I walked rapidly in at my friend's gate and up the front steps, all *en train* and adiently set to hurry upstairs and to bed, the windows of the house (as stimuli) could make no impression on me; I did not 'see' them. But when my prepotent course of action was blocked, *any* stimulus had a chance to stimulate me; and the stimuli could compete largely in proportion to their mere intensities (even the street-lamp became an object of 'interest'). Yet not wholly so. My adience toward the interior of the house, although blocked as overt action, was still in force as tonus and posture; and of the many stimuli presented by the façade of the house, the windows *taken as* holes, orifices of ingress, excited reflexes which were not blocked because they were in motor *harmony* (synergy) with my adient set to the interior of the house. Hence 'success'! And 'taken as' means only that of all the responses which a window can set in action, only one, the one which was *most congruent with the prevailing motor set*, was actually executed.

But I suspect that there may be more than *direct* and obvious sensory stimulation behind the restlessness that betrays a motor conflict; it is at any rate a matter for consideration. In the Freudian psychology, which explains so much of mental life, and explains it so conclusively, as a dynamic interplay of wishes, one finds an important case of motor conflict to be that in which one wish suppresses [8] another; and the observed phenomena indicate that every suppressed wish persists, as it were, a semi-latent energy. It exerts a pressure, and a counter pressure has to be exerted continuously if the suppression is to be maintained.

[8] Some psychoanalysts like to feel that Freud's term *'Verdrängung'* can be translated only as *'repression.'*

Freud's 'censor,' for instance, can be readily interpreted as an extensive system of sustained postures and tonuses (such as I previously described) which are continuously holding in check (direct motor antagonism) a great number of somewhat weaker action systems (suppressed wishes). And everything seems to indicate that both sides (the suppressing and the suppressed) maintain a steady, though slight, expenditure of energy. The Freudian literature provides innumerable illustrations of this. For an instance, Freud has shown that in any piece of wit some suppressed wish is at least partially released and achieves some measure of expression; in the mind that originates a bit of wit the suppressed wish works past the censor by *its own* (hitherto pent in) *energy*. But now, whether a piece of wit is invented or imparted to one by another person, the release of the suppressed wish is followed by a more or less violent 'outburst' (!) of laughter and other irrelevant and unco-ordinated contortions—apparently the very picture of nervous overflow. And *whence* comes the energy that innervates this outburst? [9] The Freudian wish-dynamics are somewhat figurative, and decidedly too reminiscent of the Herbartian method: Freud has taken but little pains to translate the conflicts between wishes (*i.e.* motor conflicts) into concrete physiological terms. But his incomparable observations of normal and abnormal mental conflicts are a great storehouse of data, to be considered and reckoned with in any more definitely physiological theory of motor conflict. It is certainly one of the most interesting of outstanding problems.[10] It seems to me that the prin-

[9] *Cf.* Holt, 1915, pp. 19-20.
[10] I confess to some vexation at being unable to say anything more definite on the finer dynamics of conflict. We could, of course, following illustrious precedent, fall back on a theory of *conflict-substance* 'liberated' at all blocked motor half-centres, and as a secondary drive energizing the trials and errors that follow a motor block! But let us, rather, fall back on empirical observations at the level of behaviour, amply confirmed as they are by Freudians and others, that motor conflict *does* lead to trial-and-error learning (often long protracted) and finally to some upshot course

ciple of many simultaneously sustained responses (tonic, postural, and actively phasic; including cross-conditioned sustained responses) and the locus of freedom is of prime importance in this connection.

Although a motor block is commonly restricted to a few elements, relatively, of the total motor pattern, yet its degree of focalization can on occasion vary widely. An instance of a very widespread motor block is to be found in J. M. Synge's moving little tragedy, 'Riders to the Sea.' A mother preparing supper in her cottage receives news that her sons, three I believe, have been penned between the cliffs and the waves of a rising tide, and drowned. She makes no outcry, but sinks down on a stool by the hearth, and after a spell of silence begins to babble wanly of a faggot that is smouldering in the fire (or some such trifle). Her world is shattered, her mind has collapsed, and the pretty firelight diverts her as it would a weary child. The effect on the spectator is profound: he has no need to know *why*. It is the 'recession of the stimulus' *reversed;* when all prepotent action systems are abrogated, all sustained tonuses and postures fallen limp, the simple responses to bright immediate stimuli may still remain. Only death completes that picture.

A circumstance which tends to obscure the importance of trial-and-error in adult life is the following. One's store of knowledge (habits) increases with experience, so that *with advancing years the various acts which are 'tried,' in random trial-and-error, are themselves by dint of previous learning more and more adapted.* Thus if one is brought to a standstill by some difficult problem, say in carpentry, one does not howl or vomit as an infant might do; nor yet throw one's tools round the room. But one tries in turn such habits as one has previously learned by working with the various tools that are now in sight. That is, these tools

of action which more or less successfully surmounts the conflict. The complete and exact physiological picture is not indispensable for our purposes.

and other physical objects appurtenant to the situation in which the 'perplexity,' *i.e.* the motor block, occurs are sensory cues which stimulate the organism to this or that line of *previously learned* activity. These are the 'trials.' And these trials are made, *i.e.* the sensory cues operate, more or less completely at random. Now the repetition of activities previously learned is not learning, so that the only element of learning to be found in this situation is the *new conditioning* of one of these activities (the 'successful trial') to the total sensory pattern stimulated by the 'perplexing' objective situation; and whatever is actually *learned* in such a case is learned strictly by the random trial method. Yet to an onlooker the process, owing to the tolerable adaptedness of each motion tried, may appear to be less a random one than it actually is.

The trial-and-error learning which follows the conflict of impulses and motor block is, I believe, the last and 'highest' type of learning (*cf.* E. Mach, 1920, pp. 194-200). The physiological principles which we have considered will carry us a long distance toward understanding the nature and mechanism of mental processes. There is, however, one authentic psychological problem, that of 'secondary' or sensory *quality,* to which physiology at the present time gives us not the slightest clue. It is, of course, a refractory problem from any point of view; [11] and the parallelistic conception that afferent impulses arriving in the cerebral cortex give rise to secondary qualities 'in the soul,' instead of making for clarification, merely propounds a repugnant and unnecessary mystery. We have no clue at present to those specific differences between nerve impulses which correspond to the different 'modes' of sensory stimulation. But there assuredly are such specific differences; some day (with improved methods of investigation) they

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[11] Any one to whom this problem is of special interest should consult Emile Meyerson's brilliant and scholarly discussions of 'the Irrational' (1926; 1927).

will be discovered, and the secondary qualities will be understood.

The relation of trial-and-error learning after motor block, to self-interest is not hard to see. If the simpler instinctive habits do not seem to call for that predicate, it is only because they are too rudimentary to involve any relation to other persons. But they are actually selfish enough, being the very foundation of general aggressiveness (adience) and of self-protection (avoidance). The appetites are certainly self-interested. But when· we come to the complex activities which result from internal conflict, self-interest is the most precise and the only comprehensive epithet that we can apply. I am aware that the term 'self-interest' excites a lively protest and gestures of abhorrence on the part of instructors of the young and all who presume to positions of leadership. But this gentry serves to re-enforce the point. For leaders (like specialists in any line) think, plan, and admonish, inevitably, from *their own* point of view; and any tyro should be able to see that by as much as young persons, 'common' people, or in short 'the *others'* can be persuaded to lose sight of their own best interests, can be incited to self-sacrifice, by so much more will money, power and what-not accrue to the insatiable old fellows 'higher up.' Thus after a lively harangue on how vastly more blessed it is to give than to receive, the contribution-box cannot be circulated too promptly. For this reason there has been at all times, in all places, and to some extent concertedly, a self-interested preachment designed in every possible way to misrepresent both the fact and the dignity of self-interest. And this preachment errs not by being self-interested, but by being mendacious, that is, short-sighted. Any one who sees his own interest clearly enough and far enough ahead honestly to wish his friends and neighbours well, will expect and *desire* them to pursue their own enlightened self-interest. But these considerations pertain to social psychology.

The reader may have felt that in the foregoing para-

graphs, and elsewhere in this volume, the phenomena of 'thought' and 'volition' have been at least implied in the argument, without being adequately defined as modes of response, or otherwise. We are entitled, so far, to speak only of an organism as responding or *doing*. But,the differences between *thinking, willing,* and *doing* are far less significant than the identities, for all are modes of response. The chief difference is, very briefly, that specific responses (doings) which when performed as overt movements are called 'volitions,' are when performed at low tension (that is, when the same neuro-muscular responses are reduced to a mere *play of tonus,* and any actual movement is hardly to be detected) called 'thoughts.' [12] Hence, whether at the high tension of volition or at the lower tension of thought, motor conflict (virtually an internal obstacle) serves in the same way to teach the organism, through trial-and-error, new modes of response and new lines of conduct. On the whole, perhaps, motor conflicts lead rather more characteristically to thought, trial-and-error thinking or 'thought experiment' (E. Mach), than to overt volition; and specially so in adult organisms, who have learned to "let the head save the heels." As Dewey (1922, p. 171) has said, "Thought is born as the twin of impulse in every moment of impeded habit": or again (1910, p. 11), "Demand for the solution of a perplexity is the steadying and guiding factor in the entire process of reflection."

As we fare forth on our little expeditions we soon meet, in the complicated environment, with hindrances; and it makes but little difference whether the hindrance is a direct physical barrier or a reflex which presents a muscular impediment to the activity that is already in progress. The physical barrier, such as the locked door in our illustration, which I did not *wish* to damage, is often translated into a

[12] In Prof. Watson's words (1919, p. 14), " *'Thinking,' by which we mean subvocal talking,* general body language [gesture ?] habits, bodily sets or attitudes which are not easily observable without instrumentation or experimental aid." I should not lay any special emphasis on voice or language, except in the case of *verbal* thinkers.

barrier within ourselves, a conflict between motions (and 'motives') ; although wishing to surmount, we also do not wish to demolish the barrier, or do not wish to hurt ourselves by attacking it, and so on.

If all the higher phases of learning are only more or less elaborate ways of surmounting obstacles (external or internal) by trial-and-error, the fact is of some practical importance. It becomes clear, for instance, that the only 'interest' which a teacher can successfully count on, and the only interest which he has any business to appeal to in his pupil, is that pupil's already active (self-) interest in discovering how to surmount the obstacle which happens at the time to be hindering *him*. The teacher must help to solve the student's own problem, if he in turn would have the student's interest and attention. Therefore the teacher must be interested in the student and his problems, if he hopes to secure and at all to guide the student's interest. It is a further instance of the 'echo' principle. A similar rule holds good of social intercourse at large. For it is true not merely in childhood and youth, but in mature life as well, that obstacles of one sort or another are the main incentive to interest and the sole incentive to new acquisitions of learning; often, even, those obstacles which are too difficult to surmount. An aged person, looking back on life, is apt to say, "I have learned most from my failures."

In short, at any age the only motive for thought, even the most theoretical and ambitious, is to override obstacles and in spite of them to attain and to secure freedom of *action*— an *open road* ahead. This open road leads to no definite or definable goal, it is merely a straightaway, a *Laufbahn*. Thus, as Prof. Brown (1920, p. 287) has remarked, "The comprehensive problem of philosophy is to find integrations of these many individual human impulses that shall be compatible with the facts of reality that are beyond our control." The same statement would also be true of science, religion, and indeed of all human activity.

THE ORGANISM AS A WHOLE

MAN is a machine. And his machinery in some way creates a mind. The truth of these two propositions is plain to any one who can look squarely at a living creature while it is in action. Yet they set a problem so difficult that the fact itself, although affirmed by many philosophers from the most ancient times, has been denied by as many others. One learns of the early 'materialists,' Leucippus, Democritus, and Lucretius: but one is not so likely to learn that even Parmenides recognized that "that which thinks" is "the substance of the limbs, in each and every man; for the highest degree of organization gives the highest degree of thought." [1] The college student is seldom told that Descartes wrote, "I conceive that the Body [of man] is nothing else than a statue or machine made of clay." [2] And if Descartes was prudent enough to invent a *res cogitans* to go with the *res extensa*, La Mettrie has told us why: "For after all, although he extols the distinctness of the two substances, this is plainly but a trick of skill, a ruse of style, to make theologians swallow a poison, hidden in the shade of an analogy which strikes everybody else and which they alone fail to notice." [3] Nor is it impressed on the student that Leibniz wrote: "All that goes on in the body of man and any animal, is as mechanical as that which goes on in

[1] *Cf.* G. H. Lewes, article 'Parmenides' in *A Biographical History of Philosophy* (pp. 51-2 in the 1905 edition).

[2] Article 'l'Homme' in Descartes' *Œuvres*, edition of Adam and Tannery, Paris, 1909, vol. XI., p. 120.

[3] J. O. de La Mettrie, *Man a Machine* (1748) translated by G. C. Bussey (Open Court Co., Chicago) 1912, p. 143. There are many turns of thought and expression in Descartes' writings which confirm La Mettrie's remark: but the little 'ruse' was too clever by half, since it has deceived editors and commentators quite as often as theologians.

a watch." [4]   It is true that Leibniz wrote much else, but that fact does not justify us in suppressing his more intelligible remarks. La Mettrie's "Man a Machine," cited above, is a little work on the general text that "the brain has its muscles for thinking, as the legs have muscles for walking." [5]   I have placed three short quotations from La Mettrie at the front of this present volume. Hobbes, Cabanis, Holbach, Büchner and many another could be cited to the same general purport. The following statement from Joseph Priestley well sums up the obvious fact: "I rather think that the whole man is of some uniform composition, and that the property of perception, as well as the other powers that are termed mental, is the result (whether necessary, or not) of such an organical structure as that of the brain. . . . I cannot imagine that a human body, completely organized, and having life, would want sensation and thought. This I suppose to follow, *of course*, as much as the circulation of the blood follows respiration." [6]

The problem thus raised by an empirical observation has in large part dominated the history not alone of philosophy, but of physiology as well. Further observations showed beyond doubt that the mind was somehow affected or impressed by the world outside the organism, and that it in turn reacted on this world and manipulated it. These two items, together with a tendency inherent in all human thinking,[7] led to the premature conclusion that 'the mind' is some sort of *entity* residing inside the organism, and between its

[4] Leibniz, *Opera*, edition of Erdmann, p. 777.   The quotations from Descartes and Leibniz are taken from É. Meyerson (1926, p. 55).

[5] La Mettrie, *op. citat.*, p. 132.   It would be profitable, in the present connection, to peruse this quaint essay entire; and also Mr. Joseph Needham's interesting little book bearing the same title, and written almost two hundred years later (1928).   I do not, personally, agree with all of the views expressed in Mr. Needham's final chapter.

[6] *Disquisitions relating to Matter and Spirit* (1777), second edition, 1782, vol. I., sec. XIII., pp. 150-1.

[7] Few writers on scientific and philosophic method have more keenly appreciated the inveterate tendency of the human mind to hypostatize every phenomenon, to conceive it as a thing or *substance*, than Émile Meyerson (1926, *passim*).

sense-organs and its muscles. Whether Descartes' absurd picture of the Soul sitting in the pineal gland was one of his little 'tricks of skill,' I do not know; it was at any rate in line with notions that were already in the air at that time. And the reader is sufficiently familiar with the discussions of 'psychophysical parallelism,' and kindred delusions, that have gone on ever since. The mind, as purely dynamic a phenomenon as any in the universe, was hopelessly reified (I should like to say 'thing-ized'), and philosophy (which is so largely epistemology) was lost in the darkest recesses of a tortuous cul-de-sac. But so also was physiology.

Not again, until very recently, could the neuro-muscular organization be conceived to function 'as a whole.' It was cut squarely into equal halves. All afferent impulses were conducted straight to the pineal gland, for the pleasure and instruction of the homunculus there residing. And there they turned into 'sensations,' and *stopped*. From there the homunculus at his pleasure initiated and despatched the nervous impulses which travelled down the motor nerves, and contracted the muscles. In the pineal gland went on the charivari of the 'mind,' a veritable 'tumbling ground for whimsies' (to borrow Wm. James' phrase).

In so far as the Associationists introduced some fact into this realm of faërie, for so they did, the homunculus was not merely dismembered, but even minced into the smallest pieces. For now each least sensation (conceived as a 'psychic atom') took place, or whatever it may be that 'sensations' do, in a separate 'brain cell.' With this, not a few philosophers fearing, and with reason, that the 'unity' of the mind or soul was being jeopardized, ceased to take interest in psychology or any of its works. Some faint hint of fact, too, began to creep into associationist views on volition; the 'ideo-motor theory' was based in part on observation. Nevertheless, the sensation-atoms and their compounds, the 'ideas,' were not one whit less reified than before; they were of a distinct 'psychic' *substance,* as is, for

instance, so explicitly set forth in David Hartley's great work on Association.

And in physiology the chasm between sensory processes and motor processes was hardly lessened. Here was still the lair of the 'psychic.' The theories of 'the single reflex arc' and of 'localization of function in the cerebral cortex' were framed in this tradition. For it was obligatory to find some place where the soul could receive 'sensations,' etc., and these must be presented to the soul neatly distinct from one another. So, of course, separate neurones from the retinal rods and cones, for an instance, must carry the points of the 'retinal picture' back to the brain, and reproduce it point for point somewhere in the occipital lobes; and this notwithstanding that a brief consideration of the relay station in the thalamus shows that here the 'picture' must become hopelessly blurred, and notwithstanding that even the older diagrams of the retinal layers (in cross-section) showed that optical impulses *spread* to two, three or more neurones even as they leave the retinal cones. The doctrine of 'cerebral localization' has given rise, in psychology, neurology, and physiology, to a vast mythology of its own. As a mere instance, consider the following sentence from a reputable and popular text-book of neurology: "The primitive emotion of parental feeling, probably arising first in the thalamus, is capable of great expansion in its psychic combinations within the cerebral cortex, where it develops into the sentiment of sympathy and fraternal feeling." (Tilney and Riley, 1921, p. 611.) Similar vagaries abound in most of the literature which deals in any way with the anterior portion of the central nervous system. The marked effort, still apparent in the three sciences just mentioned, to assign to the cerebrum some unique status of 'autonomy,' or other pre-eminence, is merely a survival of the old tradition that the cerebrum is where the ghost-soul lives and operates.[8]

[8] To those many persons who believe that science benefits from 'a convenient working hypothesis,' I would earnestly commend the history of the mind-in-the-brain hypothesis.

All this is to some extent passing. At least any physi-
ologist, if he cares to, can now learn from competent
sources that every afferent impulse passes directly through
the central nervous system and out to muscle, and that it
is nowhere waylaid by the 'psyche.' He can learn, too, that
every afferent impulse spreads very widely as it passes
through the nervous system, and emerges in many different
muscles. In the picture which I have endeavoured to pre-
sent of the living and responding organism, I believe that
every statement rests on plain biological foundations, and
has a meaning in terms of centimetres, grammes, and sec-
onds; and that I have nowhere surreptitiously introduced
any 'psychic' principle, to be conveniently discovered, later
on, and used as a *deus ex machina* in explaining mental
phenomena. It seems to me, too, that our picture gives due
prominence to the *dynamic* aspects of neuro-muscular phe-
nomena, and that we have sufficiently seen why the organ-
ism is bound, under all normal circumstances, to function
'*as a whole*' (in the popular sense of that term) in spite of
the great number and diversity of its interconnected parts.[9]
We are now ready to go on, in our second volume, to a study
of the mental life of the organism, and to follow out that
broad hint which was dropped so long ago by La Mettrie,
that "the brain has its muscles for thinking, as the legs have
muscles for walking." In this we shall leave the lower
organisms behind, and consider the more highly organized
animals, and especially man.

If from our strictly physiological start we do come upon
mental phenomena, or perchance the general mechanism of
the mind, it will be because we have reached a new 'emer-
gent level' (*cf*. C. L. Morgan, 1923; Wheeler, 1928), that
is, because nerves and muscles when organized in a more
complex unity necessarily give rise to a new phenomenon
which not at all resembles nerves and muscles; as happens

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[9] At least one eminent neurologist and psychiatrist, Dr. K. Goldstein
(1925; 1926), has arrived at a conception of the nervous system which
is surprisingly similar in many ways to the one which I have tried to
present in the foregoing chapters.

so often in chemical syntheses, where two or more substances combine into a new compound which bears no resemblance to the ingredients that were combined. And in fact, the mind *is* precisely such an emergent; it is a new transformation level.

The expression 'organism as a whole' has recently come into considerable prominence, and one often runs across a suggestion that in some way the *mental* life depends on the organism as a whole, and that the mystery of mind would be explained if we could only conceive the organism sufficiently *as a whole*. Now there is a hint of truth in this suggestion. But, and it is as if the Hydra-headed monster would never die, there are indications that with this suggestion the old hypostatization of mental substance is again knocking at the gate. A very able and very explicit discussion of this new 'organism as a whole,' or 'the organismal conception,' has been presented by Ritter and Bailey (1928). According to this view, it appears (p. 307) that in nature "wholes are so related to their parts" that "the whole exercises a measure of determinative control over its parts." (P. 308) "A natural whole stands in such relation to its parts as to make it and its parts mutually constitutive of each other. . . . Vigorous and persistent analytical research in many subdivisions of living nature has shown that this peculiar relation is so dominant in living bodies as to necessitate the conceptual subordination of elements to their wholes." (P. 309) "The elemental conception, in the sense that parts as we can know them as independent entities are wholly explanatory of their wholes, has proved its inadequacy in every subdivision of biology where basic problems are under investigation. This conception must be supplemented by an hypothesis which recognizes that living bodies are as real and potent in influencing the inorganic elements which they take into themselves, as these elements are in influencing the bodies which they enter."

These remarks (and the rest of the discussion) are a direct invitation to hypostatize every organic whole into a

thing which is different from and superior to the organized totality of its parts. In my opinion we have here superstition actively in the making. And if we have learned anything from the history of 'entelechies' in ancient and modern times (and of similar hypostatizations), we can be perfectly certain that, with the next step in the argument, this thing-ized and superordinated 'whole' will be discovered to be the 'mind,' the same old thing-ized, static, and mythical mental entity, the ghost-soul, once again. It may be that students "in every subdivision of biology where basic problems are under investigation" have indeed gone in for this hypostatization; if so, they have embraced a fallacy which is sometimes called the 'part-whole fallacy.'

Because a unified whole, such as 'organism,' 'man,' etc., receives a *name*, the human mind is prone to let all the concrete, interacting parts fade out, and then to carry on with the mere name-thing that remains, as with a chess pawn. Very little *objective content* is left in the thinking that is done with this name-thing; it is wish-thinking, and hence the enormous popularity of hypostatization. The process is closely related to that 'prelogical thinking' which M. Lévy-Bruhl has found to be characteristic of the most primitive mentality.

The part-whole fallacy is often urged with an effort at logic. The whole, it is said, is more than the sum of its parts. And this is easily demonstrated, thus: let all the bones of the human skeleton be laid out neatly on the top of a table; here now are *all* the parts, and yet they cannot walk. No, something more and other (an entelechy winging its way from afar) must be added to the mere parts, and organize them in marching order, before ever these bones shall walk. Hence such aphorisms as this, attributed to the botanist, de Bary, "The cell does not build plants; the plant builds cells"; or the more familiar one, *"Es ist der Geist der sich den Körper baut."* Utter gibberish! Nobody but an ultra-organicist or vitalist ever suggested that the parts of any organized whole are merely an arithmetical

sum, related only by the *and* or *plus* relation, which is the only relation subsisting between bones in a mere collection. And nobody but an ultra-organicist or vitalist makes a silly mystery about the accession of organizing relations, by thing-izing them into entelechies or *superordinated* 'wholes.' A whole is *not* more than the sum or totality of its parts *in* that arrangement or organization which *constitutes* the whole. To say that the 'whole' constitutes, generates, dominates, etc., etc., its parts is the most arrant nonsense. And this is as true of the component parts of the highest living organisms as it is of the straws in a thatched roof.

To say that a 'whole' regulates or in any wise operates on its parts makes of the 'whole' something other than the totality of its interrelated parts, a metaphysical other that lends itself to vicious thinking. Thus the statement that 'life perpetuates itself' makes of 'life' a metaphysical entity apart from that concrete process of living which in fact neither perpetuates nor maintains itself but depends abjectly at every instant on its environment. Such a reifying power of word is conspicuous in all vitalistic arguments. This may seem a small point of logic, but small lapses of logic often have grave consequences. Every piece of political corruption is perpetrated by politicians for their private ends, but they 'officially represent all the people' and in them the 'whole' is acting. Of course, no disguise or alibi could be more convenient. The same fallacy is involved in almost all discussions of 'the state,' 'society,' 'public' affairs, etc.

In celestial mechanics it is found feasible to consider planets and other stars *as if* each were merely a geometrical *point* having position in space, a certain mass, and a definite rate and direction of motion. Thus the astronomer deals summarily with the star 'as a whole,' and does so by making a rank abstraction from pretty nearly everything that the star actually is. In this he is greatly assisted, to be sure, by knowing next to nothing about that mighty whole from which he has abstracted so little. He finds the stellar point a great convenience.

We all deal in a similar way with the objects that are close at hand. The nouns and, even more, the less connotative pronouns tempt us to take *every* object, at least for the time being, as a mere stellar point. A man says of his motor-car, she skips, she skids, she purrs, she rides easily, she eats up gasoline, she is hard on tires, she is a great convenience to my wife. Here nevertheless the parts are severally so obvious, so troublesome, and so costly that the owner is in no danger of imagining that the parts are superseded by the 'whole,' or that the 'whole' keeps the parts in repair, etc.; nor is he likely to commit that hypostatization that would transform the pronoun 'she' into a metaphysical mystery.

But when we come to speak of the higher living organisms, precisely this absurd hypostatization *is* committed. If we say of a man that he talks, he thinks, he suffers, he propagates, he says his prayers and goes to heaven, it is the pronoun 'he,' according to what we have been systematically taught, that does all this, *and not* the physical man; although the pronoun has no meaning except to mean the physical man. In short, the actual living man has been so successfully suppressed and forgotten that nothing remains but an 'incorporeal yet substantial' (*i.e.* thing-ized) stellar point. Its name is 'he,' or for more general philosophical purposes 'his I,' that is, his '*Ego.*' And with this we are plunged fully in the 'mystery of the incorporeal but substantial' Ego, Mind, Self, Spirit, Soul, Transcendental Unity of Apperception, and so on: for all of these are the more or less capricious and interchangeable *names,* mere words, for that stellar point which has now been successfully substituted for the concrete living man. It is easy to conjure up a 'mystery,' and a personal pronoun, particularly in the first person singular, is a valuable aid. It is less easy to understand why any one should care to do it. The spectacle of Immanuel Kant, for instance, who is still the pampered darling of every philosophical faculty, is well-nigh incredible, when in his (modestly entitled) work, the "Pro-

legomena to Any Future Metaphysics," he endeavours to discuss sensation and perception ('intuition') and by dint of verbal subterfuges which are as ridiculous as they are pretentious, doggedly evades admitting that sense-organs or any other *bodily* members play even the slightest part in these processes.

Now we have had quite enough of the stellar point and other hypostatizations in psychology. And I feel no gratitude to the ultra 'organicists' for their efforts to get up a new mystery, a stellar-point 'whole' which shall preside over and 'regulate' the concrete, organized, and mutually interacting parts. Not only does their expression 'organism as a whole' lend itself to illicit inferences, but also it is at best so inexact as to be inadmissible at all, save in the most casual and popular way. For in fact the organism *never does* act as a whole. The many stimuli that environ us play on us the most varied tunes, very much as fingers play tunes on a piano: and to say that ever we are played on 'as a whole,' is precisely as if some infatuated musical critic were to declare that, "M. Paderewski never fingers the keys of his piano, he plays it only as one magnificent whole." Played 'as a whole,' *all* the strings would be simultaneously struck; and in the organism, *all* muscles would be simultaneously contracted (not to mention all else that must happen). The expression 'as a whole' is allowable only as a semantic abbreviation referring to that complicated machine, the organism; precisely as 'she' can refer to a whole automobile. In neither case need there be any silly hocuspocus unless the speaker courts it.

All vagaries of the stellar point or stellar whole order are designed to secure the *unity* now of the 'Ego,' now of the physical organism. In both cases they fail; for the 'unity' that is achieved by wiping out all internal configurations is the unity of nothingness. In both cases there is nothing interesting left—nothing to observe and nothing to explain.

To return to plain fact, neither the body nor the mind

ever arrives at any striking degree of unity. In Chapters XVII. to XXI. (inclusive) I have tried to describe the empirical factors, so far as I could ascertain them, which make toward the *integrated* conduct of the organism. It will be recalled that conduct was shown to be always an upshot course of action, a compromise by 'algebraic summation' between many simultaneously stimulated but more or less conflicting responses. Nor did any factor present itself which promised to lessen in any systematic way the occurrence of tensions and conflicts. Yet compatible with all this, we found for instance in the locus of freedom which is defined by many simultaneously sustained responses, a principle which makes conspicuously for integrated action.

It is precisely the same with the mind; and this is one of the reasons which justifies us in expecting to find a direct causal connection between the body and the mind. The life of the mind is a drama in which the several parts are played by impulses, habits, wishes, plans, and other traits of character, which are perpetually confronting one another and conflicting with one another. Nobody better knows than the psychiatrist what are the ingredients of personality; his motives for studying it are serious and practical, and he often sees it *en déshabillé*. The following observations of the late Dr. Morton Prince (1929) give us a picture which clearly corresponds with what we have learned in our physiological study. (P. 117) "It is commonly agreed, as I think must be admitted, that, from a descriptive point of view, personality is the sum-total of traits, and that differences of personality depend upon differences in traits, on the one hand, and on the varying combinations of them, on the other . . . by traits is meant the sentiments and ideals with their meanings for the individual; the more complex habits; the fixed acquired beliefs and prejudices and likes and dislikes; the accepted ethical and social codes of conduct; the aspirations and enduring desires; [p. 118] . . . impulsions and appetites . . . and many other analogous characteristics of human nature, all

of which, according to their varying combinations, distinguish one individual from another and determine behaviour. . . . [P. 121] Personality, in other words, is many-sided, one side being manifested in one situation, another in another, and so on. Thus, apparently, a person in actual life exhibits contradictory traits, but it is the situations that are responsible for the seeming paradox. [1925, p. 269] . . . a fact . . . which must be taken into consideration by any theory of personality, is that of the various and many organized dispositions, or neurograms, comprehended potentially in the whole personality and when functioning providing the traits, all are not assembled under all conditions and at all times into a functioning whole. The individual reacts at one moment with one set of traits and at another with another, perhaps of an opposite character." And he does this, of course, because different situations stimulate different responses, which *are* the differing 'traits.'

Whether one calls these elements of the personality 'traits,' with Prince, or 'wishes,' with Freud, makes little difference: they are, in either case, all *courses of action* which some neurogram (or other) in the neuro-muscular system is ready-organized to perform, either at high tension or at low, when it is appropriately stimulated. I have elsewhere (1915) treated of the aspects of this process which are more particularly related to character, and the field of ethics.

In the volume which is to follow this, we shall consider the bearing of the physiological learning process on the psychology of cognition, and more specially, I might add, on the psychology of those two eminently troublesome predicates 'reality' and 'unreality.' From the point where we now stand it is but one short step to a definition of awareness and consciousness in terms of physiological process; and it is a step which I have previously briefly outlined (1915, pp. 153-208). This definition or theory, if you prefer, as it now turns out, finds confirmation (and

gives it) in three series of observations which, it seems safe
to say, were made quite independently of one another.
These are, Freud's doctrine of 'pleasure thinking *vs.* reality
thinking,' V. Pareto's 'theory of residues,' and Hans
Vaihinger's 'philosophy of the as-if.' In this way one is
led on to a general picture of psychology, which is very
different to be sure from any reshuffling of the dear old
'faculties,' but which compares not uninterestingly with
mental life as it is revealed both in history and on the con-
temporary scene.

THIS MATERIAL WORLD [1]

By HAROLD CHAPMAN BROWN

EARLY Greek philosophy turned friendly eyes toward the world of matter, but Plato was quick to take alarm and exorcise this μὴ ὄν as unperceivable, without form, and void of self-respecting qualities. The Aristotelian restoration as δυνάμει ὄν damns it with faint praise, and the descent is rapid to the νεκρόν and κακόν of Philo, to which Plotinus added Plato's μὴ ὄν as a final quietus. Duns Scotus could find for matter no better characterization than '*subjectum generationis et corruptionis*' and even Galilei's revival of scientific atomism did not long soften the philosophic heart. Again matter was quickly dissolved, this time into a Berkeleian non-being or a Humean fiction, and it enters its latest phase in philosophic history humbly, a mere Kantian category of the phenomenal world.

It must be admitted, however, that the materialism of the eighteenth century was not outstandingly successful in its treatment of psychological fact, and perhaps philosophers are hardly to be blamed, in the light of that experience, for trying to play safe. Nevertheless, for us the situation is tantalizing to say the least. Philosophy must somehow adjust itself to science. Why must matter be a mere phenomenon just when science is finding it so extremely complex and interesting? What if the philosopher could link up this new knowledge with his own analyses and aspirations? Perhaps he has been misled by conceptions of matter now obsolete, and perhaps the new material universe may be a real source of illumination. To discover this would be joyous, hence I believe that a new adventure in materialism is timely and worthy of trial.

Professor A. N. Whitehead (*Concept of Nature*, p. 16) says that "the history of the doctrine of matter has as yet to be written." "Plato and Aristotle found Greek thought preoccupied with the quest for a simple substance in terms of which the course of events could be expressed. We may formulate this state of mind in the question, What is nature made of? The answers which their genius gave to this question, and more particularly the concepts which underlay the terms in which they framed their answers, have determined the unquestioned presuppositions as to time, space, and matter which have reigned in science" (p. 19).

"The succession of ideas starting from the crude guesses of the early Ionian thinkers and ending in the nineteenth-century ether reminds us that the scientific doctrine of matter is really a hybrid through which philosophy passed on its way to the refined Aristotelian concept of substance and to which science returned as it reacted against philosophic abstractions. . . . But substance represents the final philosophic concept of the substratum which underlies any attribute. Matter (in the scientific sense) is already in space and time. Thus matter represents the refusal to think away spacial and temporal characteristics and to arrive at the bare conception of an individual entity. It is this refusal which has caused the muddle of importing the mere procedure of thought into the fact of nature. The entity bared of all characteristics, except those of space and time, has acquired a physical status as the ultimate texture of nature; so that the course of nature is conceived as being merely the fortunes of matter in its adventure through space" (p. 20).

I quote at length from Mr. Whitehead's *Concept of Nature*, for I know no more succinct statement of the historical aspect of our problem. He has done us a great service in freeing the concept of matter from the entanglements indicated. I do not believe, however, that he is as emancipated from the influence of certain other no less pernicious traditions as he himself believes. As I find it impossible

to discuss matter without touching upon the concept of nature, perhaps you will pardon me if I digress for a moment to this concept and incidentally try to make clear my divergence from Mr. Whitehead, to whom, nevertheless, I am more indebted than to any other philosopher of nature.

Mr. Whitehead defines nature as "that which we observe in perception through the senses." If he really means this, all sciences, even the social sciences, become natural sciences and nature becomes co-extensive with the beginnings of all knowledge. We surely "observe in perception through the senses" not only mountains and streams, but also such things as beauty, a toothache, or a fine character, and if we accept the James-Lange theory of the emotions, even in its modified forms, emotions are also a part of this world of nature. The objection might be raised that such objects as beauty or a fine character require an evaluating activity of thought, but this would hardly defend Mr. Whitehead, for he admits that "it is a difficult psychological question whether sense perceptions involve thought." It is certainly true that "sense perception has in it an element which is not thought," but so have beauty, character, and emotion. In any case, although this residuum might be adequate to give us the bare datum of a world of nature, it does not give us the nature that science constantly leads us to assert as existent, or that Mr. Whitehead is interested in analyzing. We must seek further for a definition of the physical world.

I am as interested as Mr. Whitehead in avoiding a "Bifurcation of nature" into "the nature apprehended in awareness and the nature which is the cause of awareness" (p. 31). But this definition of nature prejudices escape. To loose this Gordian knot it must be maintained that there is nothing but a survival of theological metaphysics in justification of the degradation of nature to a mere *res extensa* as over against the spiritual or thinking man as a *res cogitans*. We must accept the extension of the concept of nature as implied in my comments on Mr. Whitehead's

definition. Beauty, character, and thought itself, together with its products, must be viewed as part of nature. It is as possible to study a process of thought naturalistically as it is to study a process of erosion, and as possible to give a naturalistic account of the development of a concept, as of the growth of a tree. But also, just as it is possible for the geologist, in his study of erosion, to abstract from the electronic and atomic properties that are constituents determining the character of his data, so it is possible to be interested in the character of any natural event in abstraction from the psychological processes involved in being interested in or in knowing it. This is in agreement with Mr. Whitehead's conception of nature as being self-contained for thought, that is, something that "can be thought of as a closed system whose mutual relations do not require the expression of the fact that they are thought about," but for me thought itself would lie in this system, for it is surely possible to think about thought in this same fashion.

In popular usage the concept of nature refers primarily to the subject-matter of the physical sciences and this is the way it is apparently understood by Mr. Whitehead. In scientific thinking the extension to the biological sciences has nearly won its way, although some few may still make reservations as to the nature of life. The extension to the psychological sciences is becoming accepted, but the social sciences are still without the pale. I think this is a mistake, but I do not care, at this point, to digress and give such an extension of the term the justification I believe it deserves. The present problem merely demands a few words as to the differentia of the various types of sciences.

Science begins whenever someone becomes interested in any facts and attempts to give a description of them in general terms. It becomes technical only by degrees. There is sound psychological insight in the remark of Professor W. S. Franklin, quoted in James' *Pragmatism* (p. 49) : "I think that the sickliest notion of physics, even if a student gets it, is that it is 'the science of masses, mole-

cules, and the ether.' And I think that the healthiest notion, even if a student does not wholly get it, is that 'physics is the science of the ways of taking hold of bodies and pushing them!'" At least this statement corresponds to the actual origin of physics. Similarly, we might say of the biological sciences that they appeared when somebody became interested in watching plants and animals grow and propagate; and chemistry, when human curiosity turned toward the consequence of mixing things together under different conditions.

Hence, the beginnings of scientific knowledge are spotty. In working out the problems that arise from any line of observation certain hypotheses have been suggested and verified, and certain concepts established. These constitute the practical differentia of the sciences. For example, space, time, matter, mass, and energy have come to be the dominating concepts of physics as the concepts of atoms, affinity, and valency dominate chemistry; the cell, biology; and sensation, perception, feeling, and the like dominate psychology.

When such concepts have been established they tend to be looked upon as the private property of the science within which they were generated. When they find new applications, the field of that science is extended. Thus the concept of the electron has enabled physicists to shed light on many problems, such as the operations of affinities and valencies, that had been previously looked upon as purely chemical. Hence physics invades the field of chemistry. Similarly, chemistry has entered biology through the recognition of the value of its concepts and methods in that field. If these sciences still hold aloof from psychology and sociology, it is because at present we see no useful application of their concepts in these fields, although with the study of the ductless glands, chemistry is threatening to invade the field of the psychologist, and the study of man's dependence on his environment is opening the door to the social sciences.

The physical world, then, is nothing but that aspect of reality which the concepts developed in the physical sciences interpret successfully. The concept of matter is most fundamental and there has been a somewhat premature attempt on the part of some scientists and philosophers to make this concept the fundamental concept of all science with the result of awakening the counter tendency to disparage the concept utterly. One interested in a restatement of materialism must ask (1) what is actually meant to-day by the concept of matter, (2) what conditions limit or qualify the extension of it to other sciences, and (3) whether these limitations and qualifications justify the popular disparagement of it by philosophers.

Scientific concepts undergo a modification through usage. For present purposes we must get rid of the accumulated tradition that obscures our vision of the significance of the idea of matter. Up to the beginning of the present century modern physicists have generally thought in terms of an empty vessel, space, a smooth flowing time, and unchanging, imperishable material atoms that move from place to place in definite units of time. The weakness of these concepts has gradually become manifest as science has recognized more clearly that its problem is that of forming general but empirically verifiable descriptions. Even geometrical space means nothing to physics except in so far as it can furnish diagrams illustrative of the shape, size, or motion of things. Geometrical theorems require verification before they, or the concepts involved in them, can be called true in the sense of throwing light on the world we live in—not, of course, as following by accepted principles of deduction from the assumption on which they are based.

Recent popular introductions to the relativity theory have made these and similar difficulties familiar. Euclidean geometry stands or falls with its concept of the straight line. This must be identified empirically with the path of a ray of light. But since light gravitates these paths are not the same in any two parts of empirical space, and that

space is not homologous. Of course, the Euclidean ideal can still be held, but if it is, empirical space must be thought of as coextensive with a dynamic field and every verifiable geometric measurement must be supplemented by a mechanical computation, for the geometry alone solves no empirical problem. To retain it is like retaining Ptolemaic astronomy and introducing epicycles *ad libitum*. Fortunately, we can neglect these complications for every-day tasks, just as we can look upon the sun as rising in the east and setting in the west, or as we can neglect the problems of the earth's curvature when surveying the potato patch or building a house. But such neglect is not justified when we are interested in the actual nature of physical reality.

Similarly, the smooth flowing of time means nothing unless there is some discoverable uniform motion by which it can be represented, but such motion nowhere appears. We can not even give an absolute meaning to the concept of simultaneity as referring to the occurrence of several events in different parts of space. Time, like space, becomes only an aspect concept, that is, a concept serving to describe certain characteristics of changing reality as space describes certain other features of them. Also no determination of either aspect is possible without reference to determinations of the other. Hence the new space-time concept, the four-dimensional world scheme of the relativists.

Is matter any more independent? Certainly there is no situation where the concept of matter is applicable in which the concepts of space and time are not also involved. I am fearful of getting beyond my depths in the intricacies of modern physics, but the situation seems to be this: in the business of living the classification of solids, liquids, and gasses early justified itself; through the labours of chemists the concept of the molecule and atom came to characterize elements manifested in the structure and behaviour of these; recently, the atom has been resolved into electrons and nuclei, and the joint labours of physicists and chemists are

making manifest its involved structure. But in this last stage of development something new has taken place. While the atom, as the unit of matter, had been conceived as a minute solid chunk of elemental substance, like a tiny pebble, the electron has no such character. It need not even be thought of as continuously existing. Kramers and Holst say: "During the transition from one stationary state to another we have no knowledge at all of the existence of the electron, indeed we do not know whether it exists at that time or whether it perhaps is dissolved in the ether to be reformed in a new stationary state" (*The Atom and the Bohr Theory of Its Structure,* pp. 133-4). Whatever the outcome of the evidence may be, even this speculation is revolutionary as to the concept of matter, for it deprives matter of its eternal existence, an essential attribute under the older conception.

Now in place of the solid atom, we have a nucleus composed of electrons and protons that may under certain circumstances be detached from it, and surrounding this, definite numbers of electrons also detachable under proper conditions. These are immersed in a sort of dynamic field of which the substantive aspect is called ether. This ether, for Einstein, is indistinguishable from physical space, and the elements may or may not be permanent existences in it. The next problem of the analysis of matter may lead to a disintegration of electrons and protons—or it may not. The problem of the analysis of ether has become the same as that of the empirical analysis of space.

The electron and proton are described as unit charges of positive and negative electricity respectively, but this gives them a somewhat unique status as physical entities for electricity is fundamentally an energy concept. Energy is technically defined as 'capacity to do work.' Its occurrence implies that the character of reality is such that change does not take place erratically. Under certain conditions specific changes occur and there is a certain equivalence between factors in the conditions such that a system of correspond-

ence units is possible. Thus a weight may balance another and keep it from falling, or it may compress gas and, since this means an increase in the movements of its molecules, give a heat equivalent, or it may turn a dynamo and give rise to a certain amount of electric current. Energy is merely a name for equivalence of such dynamic conditions in correlation with consequential changes. When we say the electron is a unit of negative electricity, we continue and explain: that it has mass, means that certain changes correlated with its presence are, under given conditions, equivalent to a definite fraction of those produced by a gram weight; that it has magnitude, means that the region of space with which the mass effect is associated is extended; that it moves, means that the region where the mass effect originates is not always in the same spacial relation to other definable regions.

We can neglect the detailed description of these characteristics. What is left of matter is this: if we examine the space-ether we find (1) a field of radiant energy, that is, a condition such that certain predictable, or at least theoretically predictable effects, like heat and light, occur in it at every point under proper conditions. But (2) certain minute regions of the field present an apparent discreteness. They are starting points with which radiant conditions can be correlated as moving outward from them. These regions have definite habits of behaviour such as attraction, the habit of moving closer to, or repulsion, the habit of moving farther from, each other. Or they may combine in definite and analyzable fashions.

The distinction between the ether and these regions seems to be that as we pass from one region to an adjacent region in ether there is an approximate continuity in the change of the occurrent dynamic state, whereas, if we pass from the ether to an electron or proton, there is a rapid, if not discrete, change in the dynamic condition of the region involved. Whereas there is no reason for taking any specific extent of ether to correlate with the dynamic state found in

other regions, the regions known as electrons and protons require such extent limitations. Also, since the extents these latter occupy can be said to move with respect to each other, it is further possible to correlate the rate of such motion with their dynamic effects, *viz.*, the mass of an electron varies with its velocity and approaches infinity as its velocity approaches the speed of light. The dynamic states of the rest of the ether, having no limited regional extensions, can not be so correlated, hence this portion of the ether constitutes a single continuous entity.

If the concept of space is made identical with that of the ether, space becomes equivalent to the manifestation of the energy conditions of the universe. The electron and proton are certain critical regions of space. It becomes an arbitrary choice whether we should apply the word 'matter' to the ether also, but it is more in accordance with general usage to limit it to electrons and protons together with the structures resulting from the integrations to which they give rise. From this point of view, nature, or the physical universe, can be defined as that aspect of reality that exhibits the energy character of the space-time continuum through the character of limited regions. The concept of matter merely notes the fact that there exist certain critical regions from the behaviour of which with respect to each other and the surrounding space-time in which they lie general ether conditions can be deduced. Matter is the name for a type of structure.[2]

Up to this point we have surely found nothing in the material world to make it *persona non grata* to philosophers. The crucial moment, of course, comes when it is a question of extending physical analyses and concepts to the biological and psychological sciences. We must now ask under what conditions such extension is possible, for the rudimentary processes of space-time give rise to the science of mechanics,

[2] *Cf.* F. J. E. Woodbridge; 'Structure,' *The Journal of Philosophy*, 1917, vol. 14, pp. 680-688.

and philosophers rightly hold aloof from a simple mathematical mechanics that engulfs all reality.

Perhaps we can best attack our problem by asking what we mean when we call an atom an entity and assign to it magnitude, motion, and qualities. The atom, you will recall, consists of a nucleus composed of protons and electrons, and certain more or less definitely ordered surrounding electrons. Any of these electrons and even some of the protons may get knocked out of it and may not be replaced, with no further consequences than possibly changing its kind as a chemical element. By the magnitude and shape of the atom we refer to the extent character of the space occupied by such a system. In ascribing motion to it, we point out that the characteristic space-time relations of its components may be retained while other relations existing between it and similar systems vary. When we speak of its qualities, we refer perhaps to colour and other radiant characteristics that are correlated with the movements of its constituents. Its affinities and valencies refer to its tendencies to unite with one or more such atom systems and are a function of the number and arrangement of the surrounding electrons. We call it self-identical in spite of losses or additions of electrons that do nothing more than, perhaps, vary its valencies, although if protons or nucleal electrons are lost, the character may be so radically changed that we count the result as a different element as in the case of radio-active transmutation.

The atom still falls under our definition of matter, for it is as a whole a critical structure of space-time. We can still deduce from it certain behaviour of other critical regions of space-time (other atoms) when sufficiently close to it. But we have also attained a first degree of real abstraction, for the character of the atom as a whole has no correlation with the character of the surrounding space-time that is neither electronic nor atomic in structure. These characteristics, light waves, for example, are correlated only with the behaviour of its electronic constituents,

but quite independently of the fact that they are its constituents. That is, the waves would be the same if the proper electron was moving as it is moving whether it were a part of the integrated system of an atom or not. But the integrated atom is nevertheless a whole in that it constitutes a region of space-time having properties that, although consequences of the properties of the elements from which it is integrated, are not identical with any of them. The first occurrence of any such whole in nature is the creation of a genuine novelty.

Since the sociable character of atoms, except in the case of a few inert gases, leads them to go about linked together as molecules, the molecule constitutes another space-time region next in point of complexity and magnitude. The analysis of a molecule may turn in either of two directions: we may investigate the correlation of the characteristics of its constituent atoms with those of the molecule as a whole, or we may study the behaviour of the whole molecule with respect to other molecules. This latter study leads us to aggregated states of matter, the gas, liquid, and solid, where the two points of view may be paralleled. Since the results are adequately describable in terms of positional change, that is, relative motion, rigidity, elasticity, flow, and the like, their analogy with the basic terms of our space-time description is so great that, apart from the historic fact that they constituted the original objects of the study which resulted in modern physics and chemistry, we have no hesitation in asserting that we are still studying the material universe.

Among the many integrations of molecules there appear certain ones of a peculiar character that tend to shift the direction of interest and seem to many to lead us away from the merely material world. Some among molecular systems involve peculiar molecular affinities and valencies, interactions of atomic groups within one molecule with atomic groups within other molecules, and this results in very peculiar dynamic states of matter. I refer to crystalloids

and colloids. There is a chemistry and physics of these substances, although as yet very inadequately developed, that tends to link their properties to the properties of their molecules and so to physical nature, but the complexities of their behaviour as wholes is such that there has also been a tendency to see in certain of them at least something alien to mere physical nature. As constituents of living organisms they seem to many to be set apart by a mysterious property called life. When complex material structures begin to grow, to assimilate, to excrete, to reproduce their kind, and to move without complete dependence upon the momentary character of their environment, the gross effect is too striking to be ignored, although a parallel to each of these processes may be found in what is clearly inorganic, non-living nature. Also the feeling of discreteness is strengthened by the fact that biology, as a science of living organisms, has already appropriated as its field the structures in which these colloids are chiefly occurrent.

The situation is further complicated by the fact that the living colloids usually occur only as parts of more complicated structures, as atoms are usually parts of molecules. The cell, unicellular organism, multicellular organism, and multi-organism—an integration of multi-cellular organisms —appear in succession. Each exhibits its own peculiar multiplicity of functions and energies. In the course of this integrative ascent the breach with the physicist's world seems to widen rapidly. In his world scientific description is possible in terms of a few sorts of energies and positional changes. For the concrete character of light, for example, ether vibrations of specified rate, length, and shape can be substituted. The properties of biological entities, however, become so complex and their variety so great that it is no longer possible to define simply the multiplicity of types of energy displayed, and the quantitative description of changes tends to fall in importance below that of their kinds. Of course, I do not mean that quantitative studies have no meaning for the biological, psychological, and social

sciences, but they get a different status in them, since even a comparatively familiar process like growth can not be so adequately described mathematically as the passage of a beam of light through media of varying densities, and the quantitative description of the disintegration of a radio-active atom is much more illuminating than any quantitative description now possible of the death of a rabbit.

To test out the significance of the materialistic hypothesis for philosophy, let us assume, as scientists are mostly coming to do, that the distinction between organic and inorganic matter is merely one of complexity and kind, that life is the name for a delicately organized dynamic balance in complex molecular structures. No property that life actually has is in any way modified by the assumption. Two problems are indicated: first, the careful description of the changes that living things actually exhibit and the classification of these changes as general laws of living substances; second, the analysis of these changes to discover the exact cellular, chemical, and physical processes with which they can be correlated as consequences. This second problem links life to the world of the physicist and chemist; the first exhibits the character of that world in a new state of complex organization. From the form of the analysis, the biological objects should still be called material.

The critical point for philosophers is the transition from the physical organism to the mind. If this problem is approached in a scientific and empirical attitude, freed from historical preconceptions, it exactly parallels the point of transition between inanimate and animate matter. The situation is confused because of a certain ambiguity that has crept into the current use of the word 'mind' in psychological discussion. Behaviouristic psychology, interested in the description of functioning, has tended to limit the term to *mere* functioning and to overlook the analysis of the entities through which the functioning takes place. Introspective psychology has, on the other hand, restricted itself too closely to describing these entities, neglecting both their

functional aspects and the conditions through which they are generated. Physiological psychology confines itself to these conditions and forgets the entities to which they give rise. All have their points of insight, but it requires a philosophic synthesis to put them in their proper perspective.

At some point in the development of organisms there appear certain integrations of special cellular structures within them that initiate a new order of entities in exactly the same sense that an integration of electrons and protons gives rise to an atom. These new entities have qualitative characteristics and typical modes of behaviour just as truly as the integrated aggregate of molecules that constitutes a rock or a cell. They are the entities the character and relations of which introspective psychology has undertaken to describe. As the atom rarely appears except as an integrated part of a molecule, so these entities appear in a more complex whole that we call mind. The mind can be described as a whole in relation to other minds, or its character can be analyzed as a consequence of the characters of its components. These components can be viewed in turn as the characters of integrated cellular structures as wholes. Note that in this analysis no verifiable aspect of mind is lost or belittled. The properties and qualities of mind are not explained away any more than the qualities of water, and the properties of it studied by hydraulics are belittled or explained away when water is conceived as an integrated molecular structure and its properties a consequence of this structure. It is asserted, however, that if the structure and organization of its components are changed its character as a whole will be changed. This is empirically verifiable. It is the justification of the stress laid on the learning process and, with respect to ideas, on the character of the nervous system or of the endocrine glands, as affecting states of mind.

This conception of the relation of the mind and the body is congruent to, although perhaps not identical with, that suggested by Professor Watson some years ago in a very

illuminating article on 'Behavior and the Concept of Mental Disease.' [3]   His point is briefly this: we assume disease where any bodily functions are disturbed so that they seriously hinder the normal actions of the integrated structure of which they are elements.   The notion is extended to mind when its conscious elements, such as memories, emotions, or ideas are changed to disturb the expected integrative functioning of the whole.   Such disturbances are of two types. Certain of them are due to the formation of what are popularly called bad mental habits and repressed impulses, that is, certain unfortunate synapses of the nervous system. The methods of re-education, psycho-analysis, and suggestion, may have more or less corrective efficiency in these cases.   We call such methods *mental* treatments for they ultilize only the ordinary means of *mental* communication. What actually happens is that certain stimuli to sense organs, the eye and ear, for example, excite nerve paths that result in a changed organization of synapses.   Other cases of mental disease are due to serious structural changes in the organism, such as nerve or glandular deterioration, that can only be corrected, if at all, by strictly physical treatment, the use of drugs or surgery.   Hence they constitute another category of *mental* disease.

In addition to the explanatory advantages of the extension of the forms of analysis characteristic of the physical world to the realm of mind and the concrete problems the hypothesis suggests, there are certain advantages that are of specific value to the special problems of the philosopher. In the first place, if his reflections are to be of any significance, it is necessary to assume that ideas are effective elements in the determination of conduct and are modified by the processes of experience.   Psychological dualisms have to meet this need by such perfectly unverifiable hypotheses as the parallelistic and interactionistic.   Interaction has the real merit of assuming that ideas are effective in determining conduct, although the manner of the determination is

[3] *The Journal of Philosophy*, 1916, vol. 13, pp. 589-597.

absolutely inexplicable. It has the disadvantage of producing that 'bifurcation of reality' that Mr. Whitehead so vigorously protests. In addition it raises the epistemological problem in such a form that it can receive only dialectic answers and is likely to result in such conclusions as appear in the subjective and solipsistic interpretation of Berkeley.

The parallelistic theory is also open to this objection, and in addition it shares the epiphenomenalistic difficulties of the double aspect theory. If mind is merely an aspect of nervous processes, or something flowing parallel to these processes, it is no more effective in determining the behaviour of an organism than is the paint on an automobile in directing the efficiencies of its engine. It is a pseudo-salvation of the dignity of mind; but a real salvation, that might justify the significance of ethical, esthetic, and religious attitudes, is sadly lacking. In contrast let me review briefly the main points of my restatement of the materialistic hypothesis as relevant to these points.

Materialism aims to interpret the processes of life and mind as manifestations of the fundamental processes of physical nature. While these processes were conceived as an interplay of solid, imperishable atoms, this meant an extension of the principles of mechanics to all reality. As justifying Laplace's assumption that the history of the universe could be reduced to a differential equation, it was foredoomed to failure for the sciences of life and mind, and especially for philosophy, inasmuch as philosophy is concerned with attitudes of evaluation. But in the light of modern science the fundamental processes of nature must be reconceived. The discovery of electrons and protons, with the study of their properties, is not a mere pushing back one step to new imperishable elements and new external forces. It is the substitution for such atoms and forces of a dynamic conception of certain critical regions in space-time reality. It introduces a new mechanics and makes clear that any mechanics is nothing but a description of the behaviour of types of entities that happen to be sim-

ple enough so that their motions can be adequately indicated by mathematical formulas. There can be many types of mechanics, all equally truthful and real.

The conception of the physical object has also a new meaning, for, unless it be the electron or proton, and of these it would be premature to express a decisive opinion, there are no more absolutely solid objects, in the sense that atoms have been conceived as such, but only structural regions functioning as wholes, but analyzable into interrelated lesser critical regions. The physical object has lost its attributes of spacial continuity and indestructibility. In this sense the living object, or even a mind or a society, is as much a physical object as an atom.

The thesis is becoming clarified that reality must be analyzed into levels of entities such that the properties of the entities on any level can be conceived as consequences of the properties on a lower level, and conditions of the occurrence of properties on a higher level, in the sense that if any entities change, their constituents must have changed, and their consequences be different. This is close to the Aristotelian world scheme of nature, but metaphysically it substitutes the empirically describable field of space-time for primary matter or pure potentiality. Also it omits the teleology of the pure form as a final cause and sees in the world processes an orderly and progressive manifestation of the implicit character of the space-time dynamic field. It is the unfolding of a character rather than the realization of an *a priori* plan.

It should be emphasized that in this system of levels each integrated entity acts as a novelty in its behaviour as a whole as compared with the behaviour of its constituents. It is this fact that removes the sting inherent in historical materialisms, and assures us that this new materialism may be philosophically acceptable. To explain: the laws describing the characteristic behaviour on each new level of integration are not merely novelties with respect to those of lower levels. Although consequences of them, they trans-

cend them in a very definite way. For example, while the path of an electron is determined by certain relations to other electrons and the space-time field in terms of its own level, if that electron happens to belong to an atom of a molecule of flowing water, its actual world line, *i.e.* its path in space-time, is dominated by the conditions of fluid behaviour expressing hydraulic laws. Or again, while gravitation may describe the behaviour of masses as such, the motion of the mass of a jack rabbit's body while fleeing from a dog is at least as much determined by chemical changes within the animal's body as by gravitation, and these in turn are greatly modified by the act of seeing and perhaps by ideas that sight has evoked.

In general, when any integrated whole reacts with another, the related processes in its constituents have to conform themselves to demands put upon them by the whole. This is particularly important for a materialistic philosophy, for it means that, even although man is conceived as an actual part of the cosmos, a physical object amongst other physical objects, nevertheless his behaviour is not reducible to mere chemical and physical laws, however it may be conditioned by such laws. Thus if an idea be conceived as an integrated set of organic structures with their consequent qualities, as an atom is such a set of electrons, the reaction of idea with idea or of idea with the rest of the organism to produce a certain action, is a transcendent process with respect to its constituent cellular and chemical processes, in just the same sense that the flowing of a liquid is transcendent with respect to the movements of the electrons that constitute its atoms. The character of the components sets certain limits to possible effects, as gravitation limits the leap of the jack rabbit. Nevertheless, when an action between wholes has been initiated the processes on lower levels have to adapt themselves to the new pressures that are put upon them in so far as their character permits. The word 'fire' as a stimulus to an organism that has acquired the requisite background of ideas sets in motion a

most complex group of chemical and physical processes within the organism, but the response itself is something that, as a whole, is neither physical nor chemical, although its character is modified by the limitations such processes impose. Also the full explanation of the occurrence of these processes will be unintelligible without reference to that structural whole which is the idea. This may be the basis of the hold that teleology has on the biological sciences.

If this position is accepted as a background for philosophy, it is easy to see that the philosophy of physical sciences can not begin with, or make use of, psychology and epistemology in solving its problems. It must hold that psychological entities and processes are simply occurrences in nature having highly complex structures. Psychology as a science must analyze and describe them, just as the biologist and the physicist must analyze and describe the entities and processes to which they direct their attention. Such a psychology will be interested in endocrine glands or neurones in just the same way that the biologist is interested in physics and chemistry, but it will be interested in conscious processes as the biologist is interested in the relations and behaviour of organisms. The philosophy of the physical sciences will only be interested in the meaning and significance of scientific ideas, or in the steps by which they have been attained historically. The psychological and physiological processes involved are hardly relevant.

The philosopher finds certain sets of ideas related to behaviour and its consequences in a special way that leads him to group them together as knowledge. They function by restraining impulsive tendencies to act, by anticipating consequences of action, and by redirecting impulse to attain or avoid the anticipated consequences in accordance with certain feeling states that appear with them. It is a psychological problem to record the history of the experiences that have led to these ideas becoming what they are, and their relations to other ideas, or the sort of behaviour to which they lead. It is another sort of problem when we

ask what we mean by calling them knowledge. Then we no longer think of them as terminal results of natural processes due partly to the character of the organism and partly to environments it has experienced. We concentrate wholly on their functioning as leading to or away from the realization of anticipated consequences. In the former case we call them true, in the latter false. The idea of the truth or falsity of an idea is the idea of the success or failure of that idea in producing results that agree with the expectations it has engendered. Thus the natural process of the quest for truth may be initiated by the idea of truth. The results of trying out tests of truth have given us logic, and objective reality becomes that with respect to which ideas can function as true or false.

Our materialism is also adequate as a basis for esthetics, ethics, and a philosophy of religion. If esthetics is concerned with emotional communication, there is in it a clear justification of psychological esthetics as concerned with the problems of how far elements like line, form, colour, tone, and rhythm can determine a precise emotional effect and, if desired, relate it to specific ideas. It is also suited to exhibit the grounds for asserting that esthetic experience has definite values and a claim to greater consideration in life, for it alone can clearly conceive the relations of emotion and conduct. Also it avoids the sentimental definitions of beauty that repel most clear thinkers, for it sees beauty clearly as an integration of those characteristics of objects that are capable of determining certain emotional effects in an observer.

For ethics it preserves the essential character of free will, in the sense of action directly controlled by ideas, and suggests such processes of rational valuation as constitute the essential theses of Westermarck's great book.

That a religious philosophy can be derived from materialism is perhaps a little less familiar. Whatever religion may have been in its beginnings, magic or spiritism, the religions of civilized peoples are outgrowing such conceptions, as

morals are outgrowing uncriticized custom and tradition. Developed religions all undertake to exhibit man's relation to some larger whole, whether humanity, the cosmos, or a Kingdom of Heaven, in such fashion that incentive may be given to aspiration and striving towards what are commonly called the higher things of life. This our materialism is admirably suited to do. In placing man definitely within the cosmos, but in recognizing the novel character of each integrated whole that reality has produced, it gives him a unique function to fulfil with reference to reality and consequently a unique significance. For since in man alone the capacity for the control of action by ideas has its fullest realization, in exercising that capacity to its uttermost, man is fulfilling his cosmic or divine mission. The virtue of holiness is the divine virtue of intelligence. "Each thing is at its best when it is most fully realizing its own nature." What is morality or even ideal aspiration but the fullest utilization of the capacity to control action by ideas? If we are mystically inclined, we may believe that it is not wholly a matter of human concern that man should submit to this destiny. If there is one feature of reality that scientific materialism throws into relief, it is that what *can be* produced *must be* whether it survive or not. It matters not if the life of humanity is merely episodic in the history of the cosmos, for it remains factual that only in humanity do certain aspects of reality find expression. In religious symbols men call this real nature God, hence it is only through man that the full will of God can be realized. Without man and without the episode of human life "God and the universe can not prosper."

I must ask forbearance for the extreme brevity of these last paragraphs. I am profoundly convinced that the reproaches that have been justly directed toward ethical and atheistic materialisms in the past have been due to a mistake in the formulation of the doctrine, a mistake fostered by scientific conceptions more immature than those accessible to us. I could not refrain from suggesting, although

merely suggesting, a possible line of reply to F. A. Lange's comment that "Materialism lacks relations to the highest functions of the free human spirit," that "it is, apart from its theoretical inadequacy, unstimulating, barren for science and art, indifferent or inclined to egoism in the relations of man to man. It can hardly close the circle of its system without borrowing from Idealism." None of these things I believe to be true. Yet the point of view advocated is a genuine materialism: It asserts the continuity of the processes of life and mind with those of physical matter and grants that the same method of analysis is everywhere valid, however its end products may differ through varied characters arising at different levels of cosmic integration. It is not a sublimated idealism for it does not read the distinctive character of mind back into lower levels of integration or make mind the model of the cosmos.

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