STUDIES IN SCIENCE

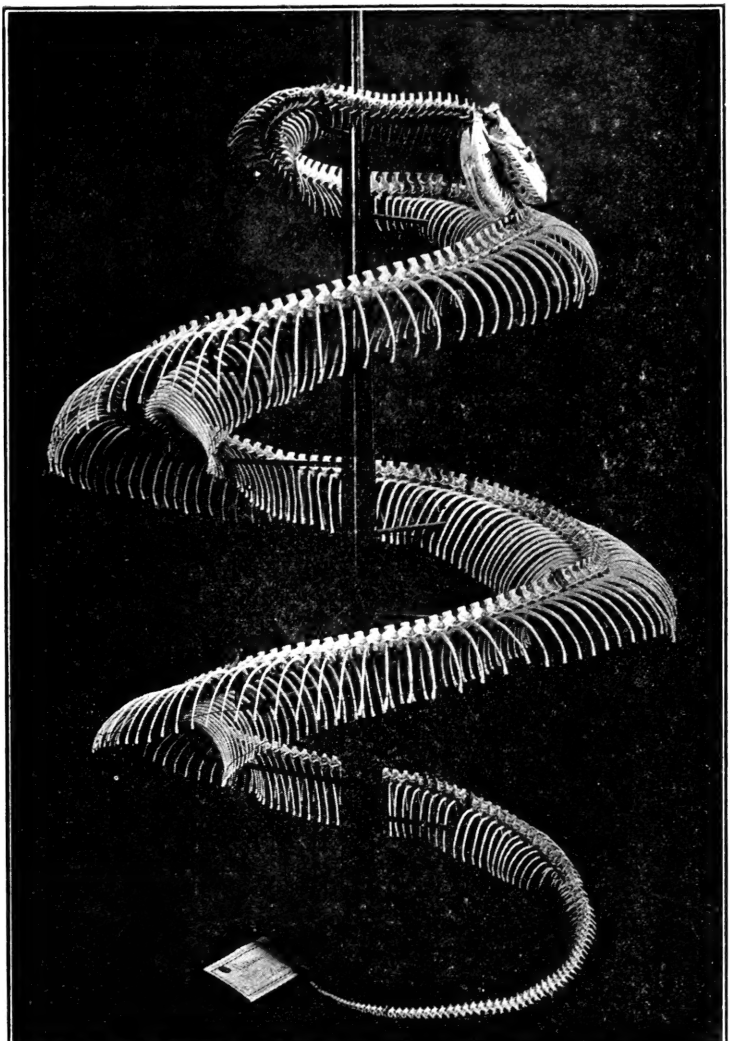

FIGURE 56. SKELETON OF PYTHON.

THE UNITY OF THE ORGANISM

OR

THE ORGANISMAL CONCEPTION OF LIFE

BY

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CONTENTS

PART I

CRITIQUE OF THE ELEMENTALIST CONCEPTION OF THE ORGANISM

B. The Production of Individuals by Other Individuals (*Concluded*)

LIST OF ILLUSTRATIONS

PART I

CRITIQUE OF THE ELEMENTALIST CONCEPTION OF THE ORGANISM

B. The Production of Individuals by Other Individuals

THE UNITY OF THE ORGANISM

Chapter XIV

EVIDENCE FROM METAZOAN GERM-CELLS THAT SUBSTANCES OTHER THAN CHROMATIN ARE THE PHYSICAL BASES OF HEREDITY

Evidence from Spermatozoa

IN our discussion we will be guided by the principle laid down earlier and followed throughout our treatment of heredity in the Protozoa, namely, that descriptive ontogenesis brought to bear on the actual transformations which result in the production of specific organs and parts, is the final tribunal for the determination of what substances are hereditarily formative. The first inquiry will be whether there exist among the metazoa single-cell organs or parts which when fully formed exhibit species characters in the sense of taxonomic biology. If such elements do exist, observation on the constituents of the undifferentiated cells which take part in the transformations, obviously may be expected to give us the information sought as to what substances are formative.

Spermatozoa Subject to Heredity as Well as "Bearers of Heredity"

The comparative anatomy and comparative ontogenesis of the male germ-cells among animals, which have been pursued with great assiduity and skill during recent decades, furnish perhaps the largest mass of relevant facts we possess from any one field.

Innumerable researches on fully formed spermatozoa, the greatest single research being that of Retzius, give us extensive knowledge of the variety of structure of the spermatozoa in the larger and smaller taxonomic subdivisions of the animal world. It would be going beyond the evidence to say that every well-characterized animal species may be identified by its spermatozoa; but unquestionably the trend of investigation has been toward such a conclusion. I believe, for example, it would be impossible to assert on the basis of evidence that any two species of animals belonging to different genera, no matter how much alike if their distinctiveness is not questioned, have indistinguishable spermatozoa. "One may say," writes Ballowitz, "that each animal species has its own sperm-form of definite size."[1] An attempt to illustrate fully this variety of form and size by specific examples is out of the question here. We will refer only to the specificity of the sperm of man himself. Retzius was able to compare in detail sperms of the Chimpanzee, Orang-Utan, Gibbon, and Homo, and found that while their resemblance is rather close, each possesses clear differential marks. For example, the spiral structure of the envelop of the central piece is considerably more distinct in the Chimpanzee than in Homo. Worthy of mention is the fact that, according to Retzius, the sperm of the Chimpanzee resembles that of Homo more closely than does that of the Orang, thus falling in with the fact that in several particulars of adult structure the resemblance of the Chimpanzee to man is closer than that of the Orang.

The spermatozoa of a given animal group having a closer resemblance to one another than to those of other groups; in other words, having a resemblance due to descent, are *themselves* subject to heredity and are not alone concerned in the transmission of hereditary attributes from parent to offspring.

Especially important for us is it to notice that to a great extent the diversity of structure among spermatozoa is in the locomotor organ, the tail; that is, the organ chiefly concerned with the unique life of the sperm as such, and very little if at all concerned directly with fertilization and hence with hereditary marks of offspring. This fact deserves attentive consideration. "In its more usual form the animal spermatozoon resembles a minute, elongated tadpole, which swims very actively about by the vibration of a long, slender tail."[2] In some respects comparison of the spermatozoon of the type here indicated with an Appendicularian, a minute Tunicate which possesses a tail throughout its life, is more instructive. Any one who has had opportunity to observe both sperm cell and appendicularian when alive and active will not have failed to remark the general resemblance, not only as to form but as to kind of movement in the two cases. Is the development of the Appendicularian's tail a manifestation of heredity? Surely no one would think of giving any but an affirmative answer. How, then, deny that the development of the spermatozoon's tail is also a manifestation of heredity? I cannot see that it would be less inconsistent to affirm that the wriggling appendicularian is alive but that the wriggling spermatozoon is not, than to affirm that the ontogeny of the first is guided by heredity while that of the second is not. To bring the point onto somewhat more familiar ground, let us revert to Wilson's comparison of the spermatozoön to the tadpole stage in the life of the frog. Our contention is that the tail of the frog's spermatozoon is as indisputably modeled by heredity as is the tail of the frog's tadpole, and consequently that we are bound to search for the physical basis of heredity in the former as well as in the latter. Our inquiry is, then, What observations have we as to the substances concerned in producing the spermatozoon tail?

(*a*) *Illustrated by the Ontogeny of Mammalian Sperm*

Without exception, so far as I know, positive description of spermatogenesis affirms that the tail is produced from

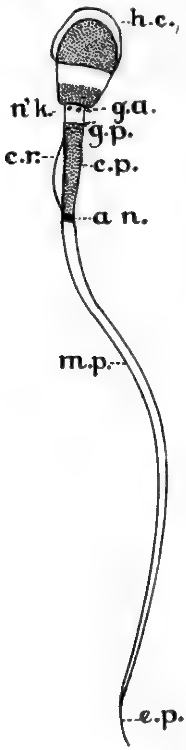

FIGURE 36. SPERM OF FUR SEAL (AFTER OLIVER).
h.c., head cap. n'k., neck. c.p., connecting piece. c.r., cytoplasmic remnant. g.a., anterior granules. g.p., posterior granules. an., annulus. m.p., main piece. e.p., end piece.

other parts of the spermatid than the nucleus. For the little we can do in the way of giving objectivity to this general statement we will first examine the nearly mature, typical mammalian spermatozoon (figure 36) of the Fur Seal.

For the origin of the various parts we will make use of the summary given by Ballowitz (figure 37). Concerning

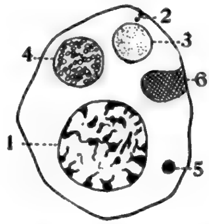

FIGURE 37. "YOUNG" SPERMATID (AFTER BALLOWITZ).
1, nucleus. 2, centrioles. 3, idiosome. 4, mitochondrial body. 5, chromatoid body. 6, spindle remnant.

the nucleus, *1*, "it is certain that in all animals the chromatin-containing part of the sperm proceeds from the nucleus." [3] On this point there is agreement among observers; so whatever may be the truth about the sperm-chromatin

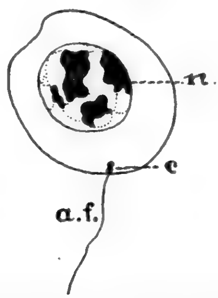

FIGURE 38. SPERMATID OF FUR SEAL (AFTER OLIVER).
n., nucleus. c., centrioles. a.f., axial filament.

as the physical basis of heredity for the adult animal, there is no question about its being such for the head of the spermatozoon itself.

The centrioles (*2*, figure 37) are "almost always double and occupy a place in the spermatid close beneath the surface of the cell."[3] The pair is typically so placed that the axis joining them is perpendicular to the surface of the spermatid, the member

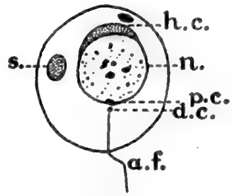

FIGURE 39. SPERMATID OF FUR SEAL (AFTER OLIVER).
h.c., head cap. n., nucleus. p.c., proximate centriole. d.c., distal centriole. a.f., axial filament. s., remnant of sphere.

toward the surface being known as the *distal centriole* and the one toward the center of the spermatid the *proximal centriole* (figure 39 *d.c.* and *p.c.*).

As to the part played by each of these in the development of

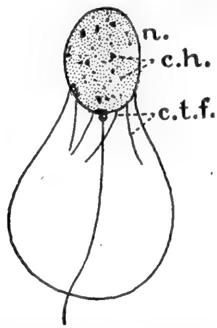

FIGURE 40. SPERMATID OF FUR SEAL (AFTER OLIVER).
n., nucleus. c.h., chromatin granules. c.t.f., caudal tube filaments.

the sperm we learn that after migrating inward until they come to lie very near the nucleus, if not in actual contact with it, the centrioles begin their development with the following outcome: "The proximal centriole . . . divides into two portions, closely adherent to the nuclear wall, each connected by a filament to one distal group. The distal centriole divides into an anterior and a posterior portion. The posterior portion becomes the annulus (*an.*, figure 36) while the anterior one divides again, forming the Noduli posteriores" [4] (*g.p.*, figure 36). Throughout their career these bodies or granules are highly stainable with certain dye-stuffs.

Out of the idiosome (*3*, figure 37) which as a rule is a body "*für sich*"—in itself alone—develops the perforatorium, or head cap of the adult sperm (*h.c.*, figure 36). It is agreed that the mitochondria (*4*, figure 37) in the spermatids of many animals, particularly of many vertebrates "furnish the material" [3] for the spiral found in the connecting piece (*c.p.*, figure 36) of the sperm. Although no spiral is present in the Seal sperm it may be represented, according to Oliver, by numerous granules surrounding the axial filament in the connecting piece. But while the connecting piece of the Seal sperm seems not to be typical as regards the spiral, it presents another structure, the caudal tube, or "manchette" of some authors, in a form which is specially instructive from our standpoint. In the adult sperm this structure is a thin sheath enveloping the cytoplasmic part of the connecting piece and lying in close contact with the persisting cell membrane. The point of special interest about it is that its persistence in the completed sperm of the Seal appears to be exceptional, for it is known to disappear entirely in the course of development of the sperm of several other mammals. It is a transitory or embryonic organ in some species of sperm, but a permanent one in other species, just as gills, for example, are transitory organs in the ontogeny of some species, as a frog, but are permanent in others, as fish.

The development of the tube in the Seal sperm is especially favorable for observation. "It may be readily followed," writes Oliver, "from its first appearance up to its final incorporation in the connecting piece as a peripheral layer, or sheath." [4] Here then is a structure having all the essential marks of development due to heredity and likewise one the "physical basis" of which has been carefully observed. Mentioning a long list of investigators who believe in the "derivation of the caudal tube by a process of cytoplasmic differentiation alone" Miss Oliver tells us that her study of the development of the fur seal sperm is a complete confirmation of this view. As to the very beginning of the tube we read: "Shortly after the centrosomes and their tail filament have reached the nuclear membrane there appears in the cytoplasm surrounding the axial thread a series of delicate filaments attached to the nuclear membrane. The proximal ends of these arise in a circle around the basal end of the nucleus with the centrosomes as a center, while their distal ends project freely into the cytoplasm." [5] (figure 40 *c.t.f.*) These filaments "are at first very short and thin, but they increase in length and thickness rapidly. By the progressive differentiation of the cytoplasm between them they soon fuse into a hyaline tube, surrounding the axial thread and open at its lower extremity." (figure 41, *c.t.*) The capital point is that we have here a well-defined structure the development of which is in-

FIGURE 41. SPERMATID OF FUR SEAL (AFTER OLIVER).
h.c., head cap. c.t., caudal tube. an., annulus.

dubitably proved to depend primarily on parts of the cell other than the chromatin. Indeed no one, apparently, has pretended that the chromatin takes a part in its production, for even those investigators who have not believed that it arises from the cytoplasm alone have held that it originates from the membrane of the nucleus.

About the chromatoid body (*5*) and the "spindle remnant" (*6*) (figure 37), little need be said in this connection as they seem to be inconstant structures the significance of which is in much doubt. Finally, mention should be made of the fact that a considerable portion of the cytoplasm is cast off entirely in the ontogeny of the sperm of many animals, as for example the seal, the body (*c. r.*, figure 36) being all that is left of the substance at the late stage represented. So much by way of illustration of the portions of the spermatid, or germ of the spermatozoon, in the vertebrated animals, for despite the great variety in structural details presented by the sperm of this part of the animal kingdom I think all will agree that so far as concerns the chief point being made in this discussion, what we have presented is true of the whole phylum.

(*b*) *Illustrated by the Ontogeny of an Insect Sperm*

We will now examine the ontogeny of a very different type of sperm, from another portion of the animal kingdom, the insecta. The particular species chosen is the fowl-tick (*Argas miniatus*). The investigation made use of is by Doctor D. B. Casteel.

The series of figures (42 *a, b, c, d, e, f, g,*) will help to an understanding of the remarkable, almost unique sperm and spermogenesis in this animal. Figure 42*a* shows the nearly mature primary spermatocyte. Especially to be noted are the mitochondria, *mi.*, scattered uniformly through the cytoplasm, and the striated layer, *s. l.*, on the outer surface of the cell. This layer is sharply demarked from the underlying cytoplasm. The striæ, disposed perpendicular to the surface of the cell, are excessively fine, and when looked at *in situ* end on "suggest the appearance of a faceted compound eye or of honey-comb." [6] Concerning the genesis of this layer Casteel says, in a personal letter, that the layer begins to appear at the surface and gradually increases in depth until the completed state shown in figure 42a is reached. "The striæ," he says, "appear to be forming from the undifferentiated cytoplasm sheath."

All the figures from 42 b to g have to do with the transformation of the spermatid (figure 42b) into the spermatozoon. From figure 42b one sees that the striated layer has disappeared on one side of the cell and thinned out greatly in a smaller area on the opposite side; that the nucleus, *n*, has moved to the surface of the cell in the middle of the area of disappearance of the striated layer; that the large plasmosomes, *pl.* 42a have almost entirely disappeared from the nucleus; that the vesicular bodies *v.b.*, assembled for the most part in the vicinity of the nucleus, are

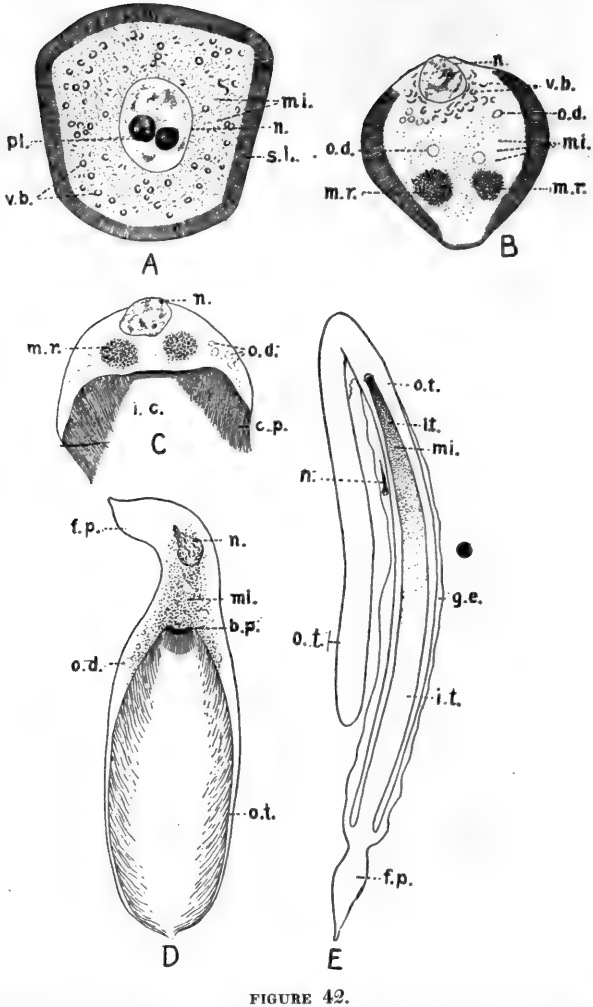

FIGURE 42.

FIGURE 42. DEVELOPMENT OF SPERM OF ARGAS MINIATUS (AFTER CASTEEL). a., rupture point of outer tube. c.p., cilia-like processes. fl., flagellum. f.p., finger-form process. g.e., gelatinous envelop. i.c., in-

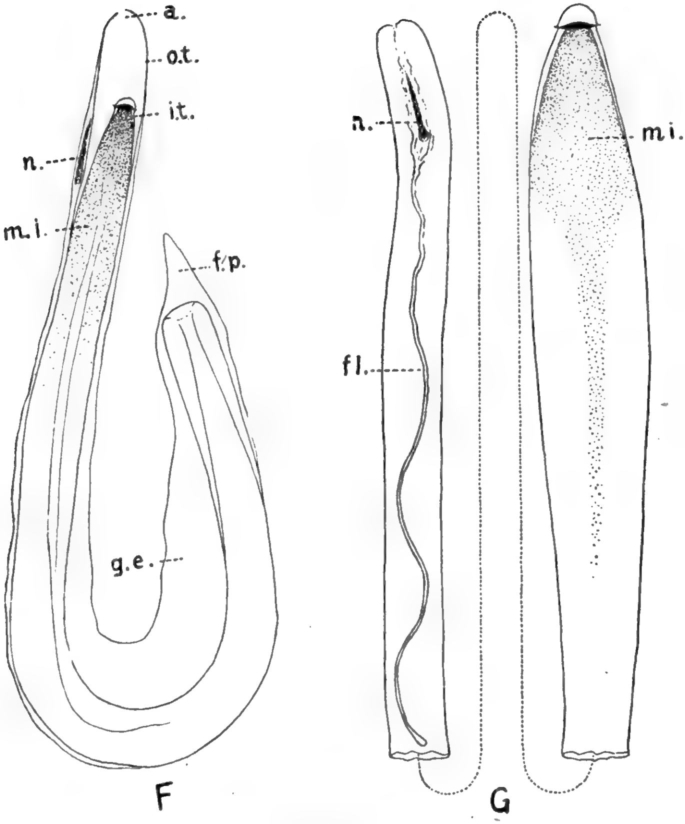

FIGURE 42.

vagination cavity. i.t., inner tube. mi., mitochondria. m.r., mitochondrial ring. n., nucleus. o.d., oil droplet. o.t., outer tube. pl., plasmosome. s.l., striated layer. v.b., vesicular bodies. b.p., beginning of inner tube.

in processes of degeneration and disappearance; and that the mitochondria, *mi*, look as though they had collected into two well-defined spherical masses *m.r.*, on the side of the cell opposite the nucleus and near the small area of thinned-out striated layer. As a matter of fact these two apparent mitochondrial masses are the opposite sides of a ring seen in optical section.

Transformation of the general form of the spermatid now begins by the indentation of the side opposite the nucleus, this going on to produce first the quarter-moon shape shown at *i.c.*, figure 42c. By the still further growth and narrowing the edges of the cup finally come together to produce the elongated cavity shown in figure 42d, *o.t.* This is the beginning of the outer tube which becomes long and relatively narrow as development continues, (figure 42e, *o.t.*). At an early period in the growth of this tube the striated layer which naturally becomes shut into the tube breaks up over most of the circumference of the tube into what resembles a dense layer of long cilia. However, since the processes are not motile and later dissolve and produce a gelatinous mass within the tube, their resemblance to cilia is only superficial. This account of the fate of the striated layer applies, as previously intimated, to most of the circumference of the outer tube. But the small thin area of the layer opposite the nucleus, shown in figures 42d and 42c, retains the cilia-like processes. This persistent basal patch (*b.p.*, figure 42d) is the starting point for an important part of the future spermatozoon, the "inner tube" so called by Doctor Casteel (*i.t.*, figure 42e).

While these profound changes are going on in the portion of the spermatid opposite the nucleus, a stout, somewhat finger-like process, (*f.p.*, figure 42d) is formed on the nuclear side of the spermatid, into the base of which the nucleus migrates. In the meantime the vesicular bodies have entirely disappeared, and the mitochondria, no longer disposed in the ring of earlier stages, have assembled into an irregular, rather diffuse mass toward the basal patch (*mi.*, figure 42d).

At the time when the outer tube has reached its maximum length and is somewhat coiled, the inner tube, starting at the basal patch previously described, begins to grow into the cavity of the outer tube. This growth continues until the inner tube is approximately as long as the outer tube. Figure 42e presents an advanced but not completed stage of growth of the inner tube *i.t.* In reality, according to Doctor Casteel's interpretation, the inner tube grows at the expense of the outer tube, for when the two are of nearly equal length the entire spermatozoon is only about half as long as it was before the inner tube developed. All of the mitochondria of the cell are drawn into the inner tube as it grows, and finally form a deeply staining mass at the distal end of the tube (*mi.*, figures 42e and 42g).

During these transformatory operations the nucleus, greatly reduced in size proportionally to the spermatozoon as a whole, has left its former place at the base of the finger-like process, and been making its way along the wall of the outer tube, burrowing through the gelatinous layer on the outer part of this tube (*n*, figure 42e). This migration continues until, when the distal end of the inner tube reaches nearly the end of the outer tube, the nucleus lies in the wall of the outer tube opposite the end of the inner tube (*n*, figure 42f.)

The final act of transformation takes place after the sperm has left the male tick and lies in a spermatophore sac within the genital ducts of the female. This act begins with the perforation of the end (*a.* figure 42f) of the outer tube by the inner tube. Through the opening thus made the whole inner tube finally passes, really by a slipping back of the outer tube, so that, the eversion of this latter being completed, the two tubes constitute one continuous tube. By this act of turning inside out, the finger-like process in which the nucleus formerly lay (figure 42d, *f.p.*) is brought into close proximity again with the nucleus.

Some of the details of the final steps in the transformation Doctor Casteel has not yet been able to make out; but these are of little consequence for our discussion. Nor has the act of fertilization been observed. Going on the usual criteria, the end of the sperm containing the nucleus would be regarded as the head. But surprisingly enough, in moving, the opposite end, the end containing the mitochondria, goes foremost.

This sperm and its development are so unique as contrasted with those occurring in most animal groups, that one might be almost inclined to question whether there may not be something wrong here—whether the case may not be one of diseased growth, or the result of manipulative malformation, or something else. Any such suspicion is, however, completely done away with by the fact that much the same type of spermogenesis is known to occur in other ticks. Casteel cites particularly the observations of Katharine Samson on the development of the sperm of *Ixodes ricinus* and *Ornithodes moubata* as furnishing cases to which that of *Argas* is "in many respects parallel."

After all that has been said in the previous pages, it is almost needless to point out the significance for our general contention of this remarkable case of spermogenesis. Were we living in that comfortable era of life-philosophy wherein theologians studied nature for the purpose of proving that everything in it was made to meet some human need, we could easily recognize this case as one designed expressly to assist man in refuting the false dogma of Chromatinic Omnipotence in heredity.

It is hard to imagine a developmental process in which denial of form-determining power to non-chromatinic, even non-nuclear parts of the cell would be a greater folly than would be the denial of cytoplasmic "form-determination" in the production of the striated layer of the spermatid, or of the growth of the outer tube, or of the inner tube, or of the turning inside-out of the outer tube. In fact, by far the greater part of the astonishing transformations here gone through are cytoplasmic, the nuclear changes being relatively slight.

It may be worth while to remind the reader again, so boldly does the real truth about hereditary substance stand out in this case, that the cardinal evil in the chromatin dogma is that it implies the denial of great masses of the most direct observational evidence we have as to what the physical bases of heredity may be, and so tends to detract attention from them. We may predict that the important research which has made known this unique case of organic genesis will pass almost unnoticed by the geneticists of our day. Were their attention called to it they would probably frankly say that they have little interest in genesis in this sense. That the sperm here described is not peculiar in every respect to the species *Argas miniatus* is certain from the meager comparative information we possess, as Casteel has shown. Nevertheless not merely general analogy, but strong indications contained in even the little comparative knowledge we have in this particular case, warrant the supposition that in some respects the sperm of the species would be peculiar to the species, to say nothing of the genus, family and so forth. The development is, consequently, due to heredity, and the cytoplasm is "inheritance material" as ascertained by direct observation.

Evidence from the Ovum

We now turn to the ovum to see what can be learned concerning hereditary substance in the development of the ovum iself. Attention should be called at the outset to the important difference between the sperm and the ovum in the kind of specialization in each. The sperm, it will be noticed, is far more specialized for its own particular life than is the ovum, this "particular life" of the sperm consisting in its great power of locomotion. As a consequence of this difference, the ovum as an entity has no such sharp distinction from the ovum as a germinal element as has the spermatozoon. This difference is expressed in one way by the assertion that the fertilized ovum *is* the individual organism in the one-celled stage of its life. No such statement is ever heard about the spermatozoon for the obvious reason that the sperm does not transform directly into the embryo as does the egg. From the absence of so distinctive a character of the ovum as such, it happens that the heredity of the ovum is not so distinguishable from the heredity of the organism of whose life it is a stage, as is the case with the sperm. Nevertheless we are bound to recognize that the egg no less than the sperm has hereditary attributes of its own, and that other substances than chromatin play a demonstrable part in the production of these. Indeed the main discoveries concerning what in an earlier chapter was called the promorphology of the egg are of this sort. There is one kind of promorphology that is of special importance to the present stage of this discussion. I refer to the kind known sometimes as "germinal localization" and sometimes as "organ forming substances" in the ovum. The idea, expressed in a sentence, is that in the eggs of some animals, portions of the egg destined to give rise to particular parts of the future embryo are visibly different from other portions before cell division begins, in some cases even before maturation and fertilization occur. According to our understanding of heredity, these distinguishable portions of such eggs are themselves hereditary attributes not only of the animal species to which the eggs belong, but of the eggs, no less than are distinctive morphological features of the adult animals or of any developmental stages. The study of these attributes of eggs is peculiarly interesting since, belonging to germ-cells par excellence, if we can get observational evidence on both their origin and destination, we shall have direct evidence that one and the same substance is determined on the one hand by heredity, and on the other is a determiner in the strict genetic sense of hereditary attributes yet to be developed.

(*a*) *Eggs of Ascidians—The Facts*

Because of the great importance of the observations of E. G. Conklin in this field, and of his general views concerning the bearing of his observations on heredity, we shall make his work the center of our examination. One of the most important of Conklin's investigations was on the eggs of several species of Ascidians. Among the great merits of this investigation are the facts that the normal living eggs were studied with great care, and that the comparative method was employed to a considerable extent.

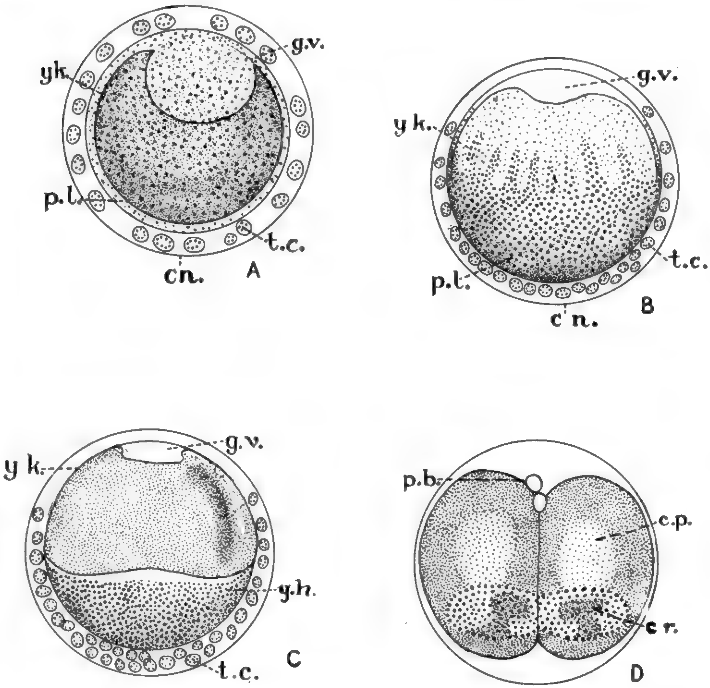

FIGURE 43. DEVELOPMENT OF AN ASCIDIAN EGG (AFTER CONKLIN).
cn., chorion. c.p., clear protoplasm. c.r., crescent of mesodermal substance. g.v., germinal vesicle. p.b., polar body. p.l., peripheral layer of protoplasm. t.c., test cells. y.h., yellow hemisphere of egg. yk., yolk.

Three species of simple Ascidians, *Ciona intestinalis, Molgula manhattensis* and *Styela partita* furnished the eggs for the investigations.

We will begin the examination by quoting from the summary of results, and then use these as the basis of a closer scrutiny of the conclusions. Under the head "Organization of the Egg" we find: "14. In the ovocyte of *Cynthia partita* there is a peripheral layer of yellow protoplasm, (*p. l.*) a central mass of gray yolk, (*yk.*) and a large clear germinal vesicle (*g. v.*) which is eccentric toward the animal pole (figure 43a). These same parts are present in the eggs of other ascidians, but are differently colored.

"15. When the wall of the germinal vesicle dissolves at the beginning of maturation divisions a large amount of clear protoplasm, containing dissolved oxychromatin, is liberated into the cell body. This clear protoplasm is eccentric toward the animal pole and is distinct from the yolk and peripheral layer.

"16. Immediately after the entrance of the spermatozoon the yellow and clear protoplasm flow rapidly to the lower pole, where the yellow protoplasm collects around the point of entrance; the clear protoplasm lies at a deeper level. The yellow protoplasm then spreads out until it covers the surface of the lower hemisphere. This flowing of protoplasm to the point of entrance of the sperm is comparable with what takes place in many animals, though much more extensive and rapid here than elsewhere (figure 43b).

"18. The sperm nucleus moves from the point of entrance toward the equator in a path which is apparently predetermined. This path lies in the plane of first cleavage and the point, just below the equator, at which the sperm nucleus stops in its upward movement, becomes the posterior pole of the embryo. The median plane and the posterior pole are probably not determined by the path of the spermatozoon but by the structure of the egg. All the axes of the future animal are now clearly established antero-posterior, right-left, dorso-ventral.

19. The yellow protoplasm "collects into a yellow crescent with its middle at the posterior pole and its horns ex-

tending about half way around the egg just below the equator (figure 43c. *y.h.*) This position it retains throughout the whole development, giving rise to the muscle and mesenchyme cells (figure 43 d. *cr.*)

"20. . . . At the close of the first cleavage (figure 43d) the nuclei and clear protoplasm move (*c.p.*) into the upper hemisphere and thereafter, throughout development, this hemisphere contains most of the clear protoplasm and gives rise to the ectoderm.

"21. . . . When the clear protoplasm moves into the upper hemisphere the yolk is largely collected in the lower hemisphere. This yolk-rich area gives rise to the endoderm.

"24. The chief factor of localization is protoplasmic flowing; cell division is a factor of subordinate value." [7]

From the numbers given in this quotation it will be recognized that only a portion of the summing up of results is here presented; and it will be understood that each item in the summary represents a lengthy, detailed description in the body of the memoir. This summary we may again summarize as follows: By the time the first division of the egg-cell is completed the portions of the egg which are to give rise to three great groups of tissues of the future animal are distinguishable from one another by definitely visible attributes. The clear protoplasm situated at the upper pole of the cell will give rise to the external epithelium or skin; the yolk-laden protoplasm in the lower hemisphere will produce the epithelial lining of the digestive tract; and from the yellow protoplasm gathered on the surface at the lower pole will come most of the muscle and connective tissue of the animal.

(*b*) *Conklin's Interpretation*

Passing now to a consideration of these facts in relation to heredity we must not neglect to notice the two-fold aspect of the question, namely that we have to do on the one hand with attributes of the egg itself which are *results* or *termini* of the kind that gave rise to the conception of heredity; and on the other hand with these same attributes not as *results* but as *causes* or *forerunners* of later-appearing attributes, also by the same criteria due to heredity. That the egg attributes with which we are dealing are due to heredity is obvious from the fortunate circumstance that, as already indicated, the observations under review were comparative to considerable extent. Thus we have this general comparison of the unripe eggs of the three species: "In the living eggs of *Cynthia* this peripheral layer is clear and transparent and contains uniformly but sparsely distributed yellow pigment, which seems to be associated with these small refractive spherules. . . . In *Ciona* and *Molgula* also these three areas are distinguishable in the living egg before maturation, but not so clearly as in *Cynthia*. In *Ciona* the peripheral layer is nearly transparent, the yolk is a brownish red. . . . In *Molgula* both the peripheral layer and the germinal vesicle are transparent, while the yolk is gray, with a faint lilac tinge." [8] As an example of a very definite statement of specific difference between the eggs we read, "In *Ciona* the same type of protoplasmic movement occurs as in *Cynthia*, but with certain minor differences." [9]

The evidence brought forward by Conklin of species differences in eggs is not restricted by any means to Ascidians. In some of the gastropod mollusca, for example, similar results were reached by a very notable research on the quantitative relations of various egg organs of several species of the genus *Crepidula*. To be explicit in a single case only, the relative volume of macromeres 3A-3D to 3a-3d was found to be 12.1:1 in *Crepidula plana* and 59.3:1 in *C. convex*. From a long series of determinations of difference, Conklin remarks: "One cannot study the eggs of different animals without being much impressed with the fact that the distribution of yolk to the four macromeres is highly characteristic of different species and orders."[8] Yet, as we shall have occasion to point out later, it seems certain that facts of this kind have deeper meanings than Conklin has appreciated.

After all that has been said in previous pages about specific differences, the point aimed at here will be obvious. The "decidedly thicker" layer of peripheral protoplasm at the lower pole in *Ciona* than in *Styela*, and the brownish red yolk in *Ciona* as contrasted with the white yolk tinged with lilac in *Molgula*, are indeed "minor differences." But species differences in adults and in all other stages are minor as a rule. The difference between the transparent-white color of the full-grown *Ciona intestinalis* and the pale, greenish yellow of the adult *Molgula manhattensis* is also minor as contrasted with the "same type" of organization of the two animals. But it is just this being true to type as regards minor differences in living beings that has given rise to the conception not only of organic species, but to a large extent, of heredity as well; and how can any one recognize the differential color of the grown-up *Ciona* as compared with the grown-up *Molgula* as due to heredity, but refuse to recognize the differential color of the eggs of the two animals as due to the same cause?

We are, then, bound to accept the attributes of the eggs as the *results* of heredity, and so inquire about the "physical basis" or "bearers" of these attributes. And this brings us again to the main issue of this section: Have we or have we not observational evidence that any other substance or substances than chromatin contribute to the production of the egg-phenomena under consideration?

Conklin's conclusions and mode of reasoning as to the bearings of his observations on heredity are so crucial for the point now engaging us that we must examine them in considerable detail. Returning to the summary of results upon which we have already drawn we read: "26. The organization of the ovocyte is not the initial organization. The yellow protoplasm (mesoplasm) of the *Cynthia* egg is probably derived, at least in part, from sphere material (archoplasm) which arose from the nucleus at the last ovogenic division. The yolk (endoplasm) is formed by the activity of the 'yolk matrix' (Crampton) which also is probably sphere material. The clear protoplasm (ectoplasm) is derived from the germinal vesicle at the first maturation division. Thus many important regions of the egg come, at least in part, from the nucleus, and a method is thereby suggested of harmonizing the facts of cytoplasmic localization with the nuclear inheritance theory." [10]

Again, we find: "This truly remarkable condition in which considerable portions of the cytoplasm are traceable to the nucleus is of the utmost theoretical importance. From all sides the evidence has been accumulating that the chromosomes are the seat of the inheritance material, until now this theory practically amounts to a demonstration. On the other hand, all students of the early history of the egg have observed that the earliest visible differentiations occur in the cytoplasm, and that the position, size and quality of the cleavage cells and of various organ bases are controlled by the cytoplasm. However, in the *escape of large quantities of nuclear material into the cell body, and the formation there of specific protoplasmic substances we have a possible mechanism for the nuclear control of the cytoplasm, and when, as in the case of the ascidians and fresh water gastropods, these substances are definitely localized in the egg, and can be traced throughout the development until they enter into the formation of the particular portions of the embryo, a specific mechanism for the nuclear control of development is at hand, and the manner of harmonizing the facts of cytoplasmic organization with the nuclear inheritance theory is clearly indicated.*" [11]

(*c*) *Critical Examination of Conklin's Interpretation*

One is struck at the outset of a critical examination of these sentences by the fact that what is claimed is that observations have been made whereby a method is "suggested of harmonizing the facts of cytoplasmic localization with the nuclear inheritance theory"; and that this suggested harmonization of such localization with the "nuclear inheritance theory" (not "chromatinic inheritance theory" be it noticed) is the final count in a mass of evidence which "practically amounts to a demonstration" that the "chromosomes are the seat of the inheritance material."

In other words objective facts which are only suggestive of a conclusion as touching the inheritance rôle of the *whole* nucleus rise to the demonstrational level as touching the same rôle of very small *parts* of the nucleus.

The method by which this particular piece of logical sleight-of-hand is performed is easy to see, for though varied to meet the exigencies of the special case, it follows the general scheme of elementalistic interpretation with which we have become familiar. In the first place, the fact that the observations furnish very little if any direct evidence that the chromosomes cause the cytoplasmic flowing and localization is made innocuous as evidence against the chromosome dogma by assuming that the flowing and localization do not themselves come under heredity. That such an assumption is implied in the argument seems certain from the fact that Conklin did not consider that the general theory of chromosomes as the bearers of heredity which he had espoused made it incumbent upon him to take cognizance of the fact that he himself had in reality testified that portions of the nucleus other than chromosomes are the seat of inheritance material. We have here another and a very notable case of shielding a prevalent theory by definition; that is, of shutting relevant but inimical facts away from it by definition. That Conklin should have been unwittingly led into this is the more surprising in that his own studies, particularly those on comparative morphology and physiology of the eggs of several groups of animals, have added largely to the proof that many of the gross attributes of the eggs themselves are subject to heredity. Indeed no biologist has expressed more positively than he the conception that the egg is the individual animal in one of its stages of development ". . . from its earliest to its latest stage an individual is one and the same organism; the egg of a frog is a frog in an early stage of development and the characteristics of the adult frog develop out of the egg, but are not transmitted through it by some 'bearer of heredity.' "[12]

One wonders if Conklin really would maintain that only the attributes of an animal in the adult stage of its life are subject to heredity; or even that the attributes of any of its developmental stages are not so subject, were he confronted with the question in this form. Although I have been unable to find statements in his writings from which one may positively infer what his answer would be, yet several passages can be brought together which can not, I believe, be harmonized with one another, but reveal real contradiction. Thus in The Mechanism of Heredity, already cited, we find: "Differentiation, and hence heredity, consists in the main in the appearance of unlike substances in protoplasm and their localization in definite regions or cells. Such a definition is as applicable to the latest stages of differentiation, such as the formation of muscle fibers, as it is to the earliest differentiations of the germ cells, and the one is as truly a case of inheritance as is the other. In short, different substances appear at an earlier or later stage in the development of all animals, and these substances are then sorted out and localized; this is differentiation [= heredity: see above]. Physiological division of labor involves morphological division of substance; sorting out of functions implies sorting out of the material substratum of functions."[13]

If cytoplasmic sorting out and localization as well in the earliest as the latest stages of development constitute differentiation "and hence heredity," how, one must ask, can the hypothesis that "the chromosomes are the seat of the inheritance material" ever "practically amount to a demonstration"? The only way, so far as I can see, to reconcile these two statements is to say that in so far as the expression "seat of inheritance material" means anything, both chromosomes and cytoplasm are such seats, and hence that neither is *the* seat of it.

But it is not enough to point out that there is contradiction here. We must try to discover just how so careful a reasoner as Conklin should fail to detect it, for we may feel certain that the failure is due not to mere oversight or carelessness but to some defectiveness in standpoint or general mode of procedure. Conklin fully realizes, as our quotations show, that movements of the cytoplasm go far toward determining the attributes of the eggs of Ascidians and many other animals. But the following makes the recognition still more definite: "Undoubtedly the most important of all the localizing factors so far recognized are cytoplasmic movements."[14]

Assuming that our contention is valid that these localizing factors of the cytoplasm are inheritance factors (and the virtual admission of this by Conklin in one of the two statements which we hold to be contradictory should be noted) we have still to see by what facts and reasoning Conklin reaches the view that his observations support the theory that "chromosomes are the seat of inheritance material." The observations in this case which support the chromosome theory are that the three kinds of cytoplasm of the egg: the yellow protoplasm (mesoplasm), the sphere material (archoplasm), and the clear protoplasm (ectoplasm) come, at least in part, and at one time or another, from the nucleus. It is not claimed that the chromosomes or even chromatin can be observed to produce the cytoplasmic localization or any of the other distinctive features of the egg, as for example the different colors characteristic of different animal species; or the different kinds of protoplasm in the same species. Just what can be observed as to the relation between the chromosomes and the several kinds of material which pass out of the nucleus into the cytoplasm is not much dwelt upon by Conklin in this investigation. The clear protoplasm (ectoplasm) he describes and figures very definitely as being the major clear mass of the germinal vesicle set free in the cytoplasm as the nuclear membrane dissolves at the beginning of maturation. The chromosomes are said to be distinguishable at this time as numerous small deeply staining bodies. They can be observed to collect together in the "center of the nuclear area" and certain things can be made out about the shapes, and arrangement relative to one another of the individual chromosomes; but nothing is recorded to indicate that they take any part in the movements of the ectoplasm, much less in the production of it.

Concerning the relation of the other two kinds of cytoplasm, the yellow or mesoplasm and the sphere material or archoplasm, to the chromosomes, Conklin's description is still more meager. An investigation by Crampton, however, has given particular attention to the relation of the sphere material, or as he calls it the yolk-matrix, to the chromatin. Crampton's results are the more significant for this discussion in that Conklin was undoubtedly acquainted with them. As the result of several searching tests of the chemical character of the yolk-matrix, Crampton says: "In *Molgula*, certainly, these granular masses are not *chromatin* in the proper sense." [15] Although Crampton believes the substance called by him yolk-matrix and considered by Conklin to be the same as what he calls sphere material, arises from the nucleus, this belief rests not on direct evidence of the passage of the substance from the nucleus into the cytoplasm, but on the facts that when the substance is first seen in the oocyte it is a small mass situated in the cell-body or cytoplasm close in contact with the nuclear wall, and that it reacts to stains and digestive fluids in the same way that certain granular contents of the nucleus react. But Crampton is very explicit in pointing out that this substance in the nucleus is not chromatin, at least of the ordinary kind. Upon treating the cells with digestive fluids it disappears from both inside and outside the nucleus, the true chromatin being then left in clear view as fine granules in the nuclear reticulum.

So far as concerns the endoplasm, then, even though it be derived from sphere material and this in turn from the nucleus, the most trustworthy evidence we possess is to the effect that its primal nuclear source is not chromatin. In view of such facts as these, made known by Crampton and others, through what reasoning would Conklin, to whom the facts are familiar, still hold that taken all-in-all they support the chromatin dogma of heredity? Readers whose minds have become sensitized to the general type of reasoning which pervades nearly all elementalistic theorizing and makes it to some extent fallacious, will readily anticipate about how the argument will run in this case. But it will be profitable to see it in actuality. After saying that "some of the important cytoplasmic substances can be actually seen to come from the nucleus" but that "this does not indicate that these substances exist from the beginning in the nucleus; on the contrary there is direct and visible evidence that they arise epigenetically," Conklin continues: "Such epigenesis, however, does not signify lack of primary organization; on the other hand all the evidence favors the view that *back* of the organization of the cytoplasm is the *organization of the chromosomes, which is definite, determinate and primary*."[16] (Italics mine).

There you have it again! Although it is freely granted that you can see the cytoplasm in the very act of arising epigenetically and moving about to become definitely located, that is, to become organized, still what you see is no part of the real essence of the business. "Back" of this, in the chromosomes, which, be it specially noticed, can *not* be seen to take any active part in the operations, we must conceive is the "organization" which is "definite, determinate and *primary*"—in other words which is The Ultimate Cause, so far as heredity is concerned. Again I repeat, wearisome as the iteration has become, that the fallacy in this sort of reasoning is not in holding that there is *some* causal power "back" of the phenomena to be explained, but that *all* such power is located there. That is, stating the general point in its application to the special case, the fallacy lies not in holding that the chromosomes contribute *something* to the hereditary attributes of the ascidians and other animal groups whose development Conklin investigated, but in the implied denial that the cytoplasm contributes *anything* to it.

Conklin probably would not admit that there is real contradiction between the observations by himself and others, on the part played by cytoplasm in the early stages of development, and his contention that the evidence now "practically amounts to a demonstration" of the correctness of the theory that the "chromosomes are the seat of the inheritance material." What he would probably contend is that the observations are opposed merely to the extreme form of the chromosome theory. Thus, speaking from an angle of the general subject a little different from that of cytoplasmic localization, he writes: "This conclusion is not a refutation of the nuclear inheritance theory, but it is a profound modification of it."[17] And still more recently he has said, "Many biologists maintain that the nucleus and more particularly the chromosomes are the exclusive seat of the 'inheritance material' and that all the 'determiners' of adult characters are located in them. Against the extreme form of this theory many general and specific objections may be urged. General objections are based upon the consideration that the entire cell, cytoplasm as well as nucleus, is concerned in differentiation and that neither is capable of embryonic development in the absence of the other. Differentiation is indeed the result of the interaction of nucleus and cytoplasm, and how then can it be said that the nucleus is the only seat of the inheritance material?"[18]

An elaborate discussion of whether the language here used can be harmonized with the statement quoted above about the demonstration of the correctness of the chromosome theory, by saying that the views expressed in the last quotation merely involve a "profound modification of the nuclear inheritance theory" would smack too much of pure dialectics to deserve a place in this volume. Our sole concern is with the truth about the thing itself.

Conklin's position would be so far satisfactory if he would permit us to understand his statements "the entire cell, cytoplasm as well as nucleus, is concerned in differentiation," and the one about modification of the nuclear inheriance theory, to mean that cytoplasm is "inheritance material" and contains "determiners." Evidence that cytoplasm contains "determiners" is even more positive than is that for the theory that chromosomes are the seat of such things, for the simple reason that we can observe abundantly cytoplasm in the act of producing hereditary structures, whereas we rarely observe chromosomes operating directly in this way. But such permission would not, I fear, be forthcoming. If it would be, one is at a loss to understand why the terms "hereditary substance," "physical basis of heredity," "determiners," "factors" and the like, constantly used in connection with the chromosomes are never used in connection with cytoplasm. Indeed, so well does Conklin present the *general* argument for the participation of cytoplasm in the development of hereditary structures that it is surprising, not to say disappointing, to find him neglect to present the most *specific* argument we have to the same effect, namely that the genesis of a vast range of such structures can be directly observed to be largely due to cytoplasmic transformations.

One other passage in Conklin's general argument is so significant that we must reproduce it. "Differentiation is indeed the result of the interaction of nucleus and cytoplasm, and how then can it be said that the nucleus is the only seat of the inheritance material? If held rigidly, this theory involves the assumption that the cytoplasm and all other parts of the cell are the products of the chromosomes, and that therefore the chromosome and not the cell is the ultimate independent unit of structure and function; an assumption which is contrary to fact. Furthermore, since heredity includes a series of fundamental vital processes such as assimilation, growth, division and differentiation, there is something primitive and naïve in the view that this most general process can be localized in one specific part of the cell, something which recalls the long-past doctrine that the life was located in the heart or in the blood, or the ancient attempts to find the seat of the soul in the pineal gland or in the ventricles of the brain." [19]

This passage contains several well-sent shafts not only against chromosomal elementalism, but against the elementalist standpoint generally. And I must recur again in connection with it, while the facts of egg organization as presented by Conklin are fresh in mind, to the perception, indicated in previous chapters, that the physical-chemistry conception of the *cell* must be extended to the *organism*. If

Conklin once sees the full force of this contention, he will, we may hope, be ready to let go entirely of the idea that the facts of cytoplasmic organization must be "harmonized with the nuclear inheritance theory."[11] He will then see that there is no more necessity for harmonizing the facts of cytoplasmic organization with the nuclear inheritance theory than there is for harmonizing the facts of nuclear organization with the theory of cytoplasmic inheritance.

REFERENCE INDEX

1. Ballowitz 255
2. Wilson, E. B. ('00)..... 135
3. Ballowitz 277
4. Oliver 489
5. Oliver 479
6. Casteel 646
7. Conklin ('05) 111
8. Conklin ('05) 11
9. Conklin ('05) 21
10. Conklin ('05) 114
11. Conklin ('05) 101
12. Conklin ('08) 90
13. Conklin ('08) 92
14. Conklin ('05) 102
15. Crampton ('99) 48
16. Conklin ('05) 101
17. Conklin ('08) 98
18. Conklin ('15) 162
19. Conklin ('15) 163

Chapter XV

EVIDENCE FROM SOMATIC HISTOGENESIS IN MULTICELLULAR ORGANISMS

WE must now give the greatest possible concreteness to the general truths stated by Conklin that growth is the result of the interaction between nucleus and cytoplasm and that heredity includes such fundamental vital processes as assimilation, growth, division, and differentiation. But the one and only way, I again insist, to attain concreteness and certainty in the matter is through a maximum of observation coupled with a minimum of inference. That is, the goal must be reached mainly by direct study of the anatomical, histological and physiological transformations through which hereditary attributes are produced. The issue can be met squarely only by a still further consideration of what we actually know about the participation of all sorts of elements of relatively undifferentiated cells in the production of obviously hereditary parts. The study of interactions between nucleus and cytoplasm and of assimilation growth, etc., in germ-cells is not enough. What we have seen in preceding pages about the development of organs in the protozoa and in spermatozoa is that much toward the end sought. Our task now is to consider the local transformations by which structures are produced in multicellular organisms, especially in those which develop from eggs.

The Mitochondrial Theory of Heredity

This task may well begin by examining the recent efforts to locate the "hereditary units" in the mitochondria of the cytoplasm instead of in the chromosomes. This effort is just as misdirected as is the effort to make chromatin the sole hereditary substance. According to the organismal conception, all life phenomena, including those of inheritance, consist in the activities and interactivities of an enormous number of substances and units and forces, all of which, in exhaustive analysis, are dependent upon the organism as a living whole. It is, therefore, as futile to hunt in one corner as another for the physical basis of heredity in an exclusive and more or less metaphysical sense. Any one who has grasped this idea will know beforehand that the proposal to make mitochondria fully explain heredity is doomed to failure no less certainly than was the proposal to make chromosomes or any one kind of cell element play such a rôle. But hypotheses, the falsity of which might have been seen before they were tested, may still be useful. If those who propose them can be convinced of their fallacy in no other way than by testing them then it is better that they should be tested even though much time and labor be given to the task. Again, the evidence brought out which disproves an hypothesis may be highly useful in establishing some alternative general view. This result has been especially striking in the case of the mitochondrial hypothesis of hereditary substance. By bringing the cell-body back into the field of interest, from which it had been largely excluded so far as heredity was concerned, by the nuclear inheritance theory, the mitochondrial hypothesis has resulted not merely in proving that mitochondria are not bearers of heredity in the elementalist sense, but that in a rational sense they are sometimes rather closely concerned in the production of hereditary structures; and, of even greater importance, that still other cytoplasmic material is likewise so concerned.

The only reason why the mitochondrial theory of heredity is less interesting than the chromatin theory is that there is so much less observational evidence in support of it.

In fundamental principle the one is no more and no less acceptable than the other.

Any half plausible suggestion that a particular minute, obscure part of the germ-cell may be a "bearer of heredity" seems to endow that part with a peculiar fascination to biologists who have the elementalist habit of thinking, and this secures to it an inordinate attention until the untenability of the hypothesis is so overwhelmingly proved that even the most credulous are forced to abandon it.

Several biologists have recognized something of the state of mind here indicated without, however, perceiving its real meaning. Thus in a review of the work of Meves, which will be examined presently, we read: "the new interpretation which Meves gives at this time indicates that many are still dissatisfied with the all-sufficiency of the [nuclear] theory, and are eagerly seeking and grasping, as it were, the first visible sign of any other substance which may serve to carry the hereditary qualities."[1] The remark to be made about any statement of this kind is that the real though usually unperceived ground of dissatisfaction is not with the all-sufficiency of the nuclear theory of heredity, but with the all-sufficiency of any theory that attempts to localize the function of carrying heredity in some small, specific fraction of the germ-cells; and that the attitude which Doctor Payne well characterizes as "eagerly seeking and grasping" which has marked so much of recent search after the physical basis of heredity, has a large measure of genuine illusion in it. Inspired by the ages-old, alluring belief that an imperceptible final cause and explanation is hidden somewhere within or behind whatever is grossly sensed, the pursuit becomes "eager and grasping," which is another way of saying that it becomes more or less irrational and fitful.

The truth of these remarks is rather strikingly exemplified in the short, somewhat feverish history of the mitochondrial theory of heredity. The meager observational support for the hypothesis that these particular cell members are generally bearers of heredity relieves us from the necessity of examining it in any such detail as we examined the chromosome theory. But there are several things about it of so much importance that we must look into it somewhat. The name mitochondria was first used by Benda for a "new cell organ, perhaps serving a specific function." Benda's original view was that the function served is that of the motility of the cell. But in a later publication he presented observations which seemed to him conclusive proof that the mitochondria of the sperm are situated in parts of it which enter the egg at fertilization. This last suggestion gave an added impulse to the study of the bodies occurring in the cytoplasm and soon many new names were applied to them, for they were soon found to present differences in size, shape, and reaction to chemicals. It seems, however, that the present state of knowledge justifies us in applying to them all the one term, mitochondria, though without implying that they are all exactly alike. They may be held to be generically alike but specifically different.

The first investigator to set up definitely the hypothesis that cytoplasmic elements of this class are bearers of hereditary qualities seems to have been Meves.

Only a few of the very many investigations since devoted to the subject can be noticed, these being selected for their bearing on particular aspects of the problem. In the first place, the position of Meves himself is important. Accepting the assumptions formulated by O. Hertwig in 1875 of a "substance which carries over the beginnings (Anlagen) of the parents to the child," [2] and that "this substance exists (in the germ-cells) in an original, histologically undifferentiated condition"; [3] and adding his own reflection that not all the cytoplasmic parts of the spermatozoön (for example, the axial fiber of the tail) possess inheritance potencies, he advanced the hypothesis that the mitochondria answer the conditions of Hertwig's theory for the cytoplasmic part of the male, though probably not of the female germ-cells. Thus Meves reached the rather attractive conception that "heredity is accomplished through protoplasm and nucleus together." [4] Were we to know no more than this about his theory we might suppose his position to be that of a genuine integrationist. However, his very next sentence does not permit us to question any further the orthodoxy of his elementalism. "The qualities of the nucleus," he says, "are carried over by the chromosomes, those of the plasma by the chondriosomes." [5] In other words the working together of nucleus and cytoplasm which he conceives is not of the sort which makes the part played by each contribute to all the results, but that one set of elements produces its particular part of the total effects, while another set produces another part of the effects.

The Mitochondrial Theory Tested by the Ontogeny of the Spermatozoa

The utter inadequacy of this hypothesis is shown by some of the same evidence which revealed to us the inadequacy of the chromatin theory, namely that when we come to study the histogenetic processes by which innumerable hereditary attributes are produced, we find portions of the cytoplasm other than the mitochondria taking the leading part in the production. Perhaps no single one of the many instances examined by us of cytoplasmic participation in the production of attributes refutes Meves' hypothesis more completely and instructively than that of the developing sperm of the fowl tick already described.

We have already examined this case as one of special weight in proving that the cell-body, in contradistinction to the nucleus, is hereditary substance. Now we must see the conclusiveness with which this particular ontogeny disposes of the mitochondrial hypothesis of heredity so far as this case is concerned.

Recurrence to the description and figures (42 a, b, c, d, e, f) will recall that the mitochondria, widely distributed through the cytoplasm in the early spermatocyte (*mi.*) assemble into a rather sharply defined ring-shaped mass (*m.r.*, figure 42b) as the cell transformation proceeds, and finally take a position in the developing sperm about as remote as they can get from some of the most actively and fundamentally changing parts of the organism. (*mi.*, figures 42 e, f, g).

There is no more possibility of explaining the development of many of the parts of this sperm, the outer sheath, for example (*o.t.*, figures 36e and f) as due to the influence of the mitochondria than as due to the influence of the nucleus. And I point out again for the hundredth time what the real issue is here. The observations certainly do not exclude the possibility that the mitochondria exercise some invisible influence on the development of, say, the outer sheath. There may be or there may not be such an influence. But the observations do show conclusively that cytoplasmic portions of the cell other than mitochondria are operative in producing the outer sheath.

It should be said in concluding this reference to the sperm of the chicken tick that Casteel finds the mitochondria located finally in the end of the sperm opposite that which contains the nucleus; and that this end goes ahead in locomotion, the motion being produced by a circlet of mobile processes at this end. From this he believes that the contractile elements are mitochondrial in origin. The conjecture that the mitochondria of the spermatocyte take part in producing the motor elements of the sperm tail is perhaps strengthened by the observations of other students, notably Lewis and Robertson. These investigators were able to follow the mitochondria in the ontogeny of the living sperm directly into the tail, where they transform into two equal threads situated alongside the axial filament.

The Mitochondrial Theory Tested by Histogenesis

If this hypothesis that mitochondria in developing sperm cells give rise to the motion-producing structures of the sperm tail, then the mitochondria would be genuine "inheritance material" for these particular elements, the hereditariness of the motile elements being especially striking in the case of the chicken tick sperm from the fact that the mode of locomotion in this spermatozoon is almost unique. But while the case of mitochondria and other non-nuclear parts of the cell in the development of the spermatozoa, ought to be conclusive that although mitochondria can not be "hereditary units" in any general sense, they, as well as other cytoplasmic parts, may contribute to the production of hereditary structures, yet it would not be so accepted, probably, by the most exacting theorists because such biologists would not allow that a spermatozoon, being unicellular, can have organs and parts which are subject to heredity "in the strict sense" (i.e. in the sense of the definition of heredity set up by these persons). We must, consequently, proceed to the specific task of this section; namely that of considering what is known about the part played by mitochondria in the histogenesis of hereditary structures in multi-cellular organisms.

Nearly all the studies centered upon the question of whether the mitochondria are bearers of heredity have gone on the assumption, quite inadequate according to my view, that the problem is to be solved by ascertaining whether or not the bodies are persistent cell organs, take a definite part in fertilization, and are contributed in equal quantity by the female and male germ-cells. In other words the assumption has been that the same criteria which have been relied upon to prove that chromosomes are bearers of heredity, must also be applied for deciding whether or not mitochondria have the same office. But numerous studies have also aimed to follow the mitochondria in the genesis of tissues, and herein lies the chief importance of investigations in this domain. Not only have they greatly increased our knowledge about the rôle played by various parts of the cell in histogenesis, notably of the cytoplasmic parts, but they have put us in possession of much precise information as to what "hereditary substance" is.

The results and conclusions thus far reached are on the whole so diverse, often so conflicting, that any attempt at a general review of them would be useless for this discussion. However, certain of the results, actual or claimed, are important. For example, at one extreme it is contended that the mitochondria are the immediate precursors of the most distinctive elements of all classes of adult tissues. Thus Meves: "All these differentiations (of embryonic cells into tissue cells) however heterogeneous they may be, arise through the metamorphosis of one and the same elementary constituent of the plasma, the chondriosomes. The chondriosomes are the material substratum lying at the basis of the processes of differentiation, which become the specific substances of the different tissues."[6] As an extension of this view we learn from Lewis and Lewis and other reviewers that the following tissues are reported on the authority of a long list of workers to be produced by the mitochondria: fibrillæ, myofibrillæ, fibrillæ of epithelial cells, corneous substance, secretory granules, fat, leuco-, chloro- and chromoplasts, the test substance in foraminifera, and various other tissue elements.

But several investigators, notably E. V. Cowdry, have shown the inconclusiveness of the evidence on which the contention is based that neurofibrils originate from mitochondria. "There is no evidence," Cowdry says, "that mitochondria are transformed into neurofibrils. . . . The mitochondria do not show, either by a variation in their morphology, staining reactions, or in any other fashion, . . . indications of being transformed into material of different chemical composition."[7] Furthermore, he shows that the mitochondria do not diminish in quantity in any way commensurate with the increase of neurofibrils, as the neuroblasts transform into ganglionic cells. Eminently worthy of note, as bearing on our contention made some pages back, that cytoplasm itself is hereditary substance, is Cowdry's detailed description of neurofibrils as a "differentiation of the ground substance" of the neuroblasts. And in a later paper the same author makes a strong case of the view that while mitochondria are "associated in some way with the formation of many substances," [8] it is highly improbable that they transform into them.

On the whole the tendency of the latest investigations appears to be to deny that the bodies produce, in a strict sense, any tissue elements. Thus as a result of their quite remarkable studies on mitochondria of living cells Lewis and Lewis say, following the enumeration given above: "The above theories seem impossible to correlate. It seems evident that the mitochondria are too universal in all kinds of cells to have the function of forming any one of the above structures of differentiated tissue, and in the light of what cytological chemistry is known, it appears practically impossible for the mitochondria to form all the cell structures mentioned above. In view of the fact that the mitochondria are found not only in almost all animal cells but in plant cells as well it seems more probable that they play a rôle in the more general physiology of the cell." [9] The idea that the mitochondria are primarily concerned with the metabolism of the cell appears to be gaining ground under the present comprehensive and critical methods of investigation that are being applied to them.

The Untenable Hypothesis that the Cytoplasm of the Ovum is Inheritance Material for General but not for Special Characters

A number of biologists have recently put forward the hypothesis that while the cytoplasm of the egg-cell may be "hereditary material" for certain of the general attributes of the organisms, chromosomes "carry" the hereditary, of the more specific attributes. This conception has arisen from a considerable range of observations to the effect that for quite a time in the early ontogeny of many animals some of the attributes of the embryo can be seen to come directly from the cytoplasm of the egg. Thus both Driesch and Loeb have taken special notice of the fact that, as expressed by Loeb, "when the protoplasm of the egg possesses a striking pigment the larva will possess the same for some time at least"; and that "if such an egg is hybridized with the sperm of a form whose egg is unpigmented, the larva will, of course, possess a 'maternal' quality which is due solely to the protoplasm (Driesch)".[10] And in the same connection, Loeb continues: "It is obvious, then, that during the first stages of development an influence of the protoplasm upon heredity may make itself felt, which will disappear as soon as the protoplasm of the egg has been transformed into the tissues of the embryo." One of the cardinal questions we have to consider may be formulated in connection with this last quotation: Have we a right to assume that because an obvious influence of the protoplasm upon heredity disappears on the transformation of the protoplasm into tissue, therefore all such influence of the protoplasm ceases? To answer this question through observations upon the protoplasm of the cells concerned just before, during, and just after the transformations is exactly the central aim of this section. I can not refrain from making use of another sentence from Loeb to aid in defining the problem more clearly. "It does not seem to me," he writes, "that a discussion as to the relative influence of protoplasm and nucleus upon heredity will prove very fertile, but that it is necessary to transfer this problem as soon as possible from the field of histology to that of chemistry or physical chemistry."[10] I quite agree that "discussion as to the relative influence of protoplasm and nucleus upon heredity" can not be very fruitful. But the grounds of my skepticism are widely different from those of Loeb. According to my view, the question is not one to be settled by *discussion* at all, but by observation coupled with a measure of consistent reasoning. Assuming that I am right, to "transfer" the problem "from the field of histology" if this really means, as it seems to, that the problem should be taken away from histology, no matter whether to the field of chemistry or any other, would be to remove it all the further from observation and plunge it so much the deeper into discussion.

I have not the slightest doubt that chemistry, especially biochemistry pursued on the principles of physical chemistry, will have to be made large use of before the fullest possible understanding of the mechanism of heredity is reached. But this use will have to go hand in hand not only with morphological studies on germ-cells, but as well on hosts of cells during the whole ontogeny. Chemical investigation will have to supplement, it cannot supplant, it cannot even lead, histogenic investigation. If there is one thing made more positive than any other about heredity by modern study of the subject, it is that heredity is something which pertains to the smaller taxonomic grades of organisms, races, varieties, species and so forth. It would seem, accordingly, that hardly any suggestion for the study of heredity could be wider of the mark than one to transfer it from the only field which makes any pretense of investigating the details of development, and taking it into a field like that of physico-chemical activity, which is notoriously devoid of the very attributes without which there would be no such thing as heredity. Having once ascertained by observation as much as possible about how hereditary attributes are actually produced, it will then be in order to learn as much as possible about the chemistry of the processes. Chemistry can do its share in solving the problems of heredity *after* and not *before* histogenesis has done its share.

Conklin has expressed more definitely than any other biologist with whose writings I am acquainted, the idea mentioned above, that cytoplasm "influences" heredity in early ontogenetic stages, and also influences adult attributes of the major taxonomic groups, but becomes inoperative in the later stages of development, the heredity of these being transferred to the nucleus. He says, "*In short, the egg cytoplasm fixes the general type of development and the sperm and egg nuclei supply only the details.*" And further: "We are vertebrates because our mothers were vertebrates and produced eggs of the vertebrate pattern; but the color of our skin and hair and eyes, our sex, stature, and mental peculiarities were determined by the sperm as well as by the egg from which we came. There is evidence that the chromosomes of the egg and sperm are the seat of the differential factors or determiners for Mendelian characters, while the general polarity, symmetry, and pattern of the embryo are determined by the cytoplasm of the egg." [11] If two points in this last quotation be viewed in the light of a large mass of relevant evidence not usually taken into account in recent discussions on heredity, and if strict consistency in the use of terms be maintained, the general conclusion will be quite different from that stated by Conklin. These two points, the conception of "differential" and of "determiner," must now receive attention; but first I will illustrate my position by a case presenting the kind of evidence to which I have referred.

Species Attributes in Single Cells of Adult Organisms

In general, this evidence comes from the field of histology, or more strictly histogenesis. The most convincing, because the most direct, evidence from this source is that pertaining to the development of hereditary structures in adult metazoa and metaphyta. The structures in such organisms which are the most indubitably hereditary are those which distinguish the smaller but yet definite taxonomic groups. A little consideration will convince one that about the most crucial cases would be those in which the development of differential attributes could be traced directly to cell structure and development. It so happens that vast as is our knowledge of histogenesis, the part of it which answers directly to the requirements here laid down is by no means large.

The Spinules of the Ascidian Genus Styela

The best instances I have been able to find are superficial appendages in some animals and plants, so small that they consist of a few cells or even of a single cell. One striking instance of this sort has come to light in my own studies.

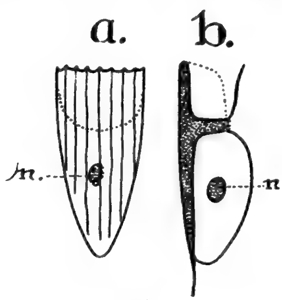

FIGURE 44. SPINULE CELL OF STYELA YAKUTATENSIS (AFTER HUNTSMAN). n., nucleus.

It is that of the minute spines which cover the inner surface of the branchial siphon in some species of the ascidian group of Styalids. Huntsman first called attention to the fact that each spinule is a single cell, and that at least in some cases the structures furnish differentiating attributes for species. Miss Forsyth and I have reëxamined the point for *Styela montereyensis* and *S. yakutatensis*, and are able to confirm Huntsman's results. As Huntsman had opportunity to study the matter in a larger number of species than Miss Forsyth and I have had, the following description is taken largely from his paper. Figures 44a, b, 45, 46 are from Huntsman and figure 47 is from Ritter and Forsyth. By comparing figures of what may be called the dorsal view (figures 44a, 45, 46) with the side view (figure 44b) it is seen that the distinctive feature about the cell which constitutes the spinule is a shield-like plate on one side of the somewhat elongated cell, the distal end of which projects more or less beyond the cell body, the whole resembling to

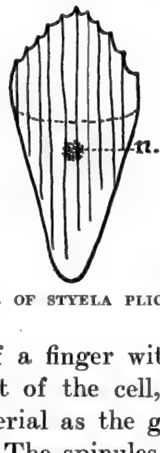

FIGURE 45. SPINULE CELL OF STYELA PLICATA (AFTER HUNTSMAN).

some extent the end of a finger with its nail. The shield is harder than the rest of the cell, and is probably composed of the same material as the general "test" of Ascidians, animal cellulose. The spinules are so placed that the basal portion is embedded in the surface layer of the test on the inside of the siphon, the shield being on the free side of the cell with its free edge pointed toward the lumen of the siphon. The specific attributes furnished by the spinules depend upon the shape and structure of the shields. The free edges may be truncate (figure 44, *S. yakutatensis*), or long-pointed (figure 46, *S. greeleyi*), or low-conical (figures 45, 47, *S. plicata, S. montereyensis*). Again the edge may be smooth (figures 46, 47, *S. greeleyi* and *S. montereyensis*), or it may be serrate (figures 44, 45 *S. yakutatensis, S. plicata*).

Viewing this case in the light of considerations put forward on the preceding pages, the pertinent queries about the heredity of the shields almost ask themselves: What is

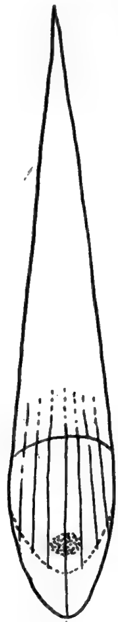

FIGURE 46. SPINULE CELL OF STYELA GREELEYI (AFTER HUNTSMAN).

the "inheritance material" that causes the shield to be short and truncate in *S. yakutatensis* and long-pointed in *S. greeleyi;* or that explains the serrated edge in *S. yakutatensis* and *S. plicata* as contrasted with the smooth edge in *S. montereyensis* and *S. greeleyi?* Is the "seat" of that material in the cytoplasm or the nucleus of the shield-producing cell? Unfortunately we have no direct observational information about the genesis of the spinules. But the indirect evidence which bears on the point favors overwhelmingly the view that the cytoplasm is chiefly responsible for the shield. Huntsman supposes the spinules to be derived from the cells of the cellulose tunic characteristic of ascidians. He may be right in this; but it may be, too, that they are derived from the epithelial lining of the siphon. The matrix of the cellulose tunic is undoubtedly largely if not wholly produced by the ectodermal cells. It is usually held to be secreted by these cells; but in some cases, in

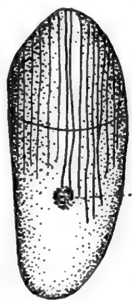

FIGURE 47. SPINULE CELL OF STYELA MONTEREYENSIS (AFTER RITTER AND FORSYTH).

Perophora for example, a portion of the cytoplasm of the cells seems to become transformed into the cellulose. The process of transformation can be particularly well seen in the cells which line the branchial siphon of developing blastozooids as shown in figure 32, plate III of my memoir.[12] The parts of the cell-bodies turned toward the cellulose are here long drawn out and the protoplasm gradually becomes indistinguishable from the surrounding cellulose substance in which it is imbedded. If now we imagine this protoplasm to transform not into the characteristic cartilage-like cellulose mass spread over nearly the whole surface of the body, that from innumerable cells fusing into a common mass, but each cell to retain its individuality, its protoplasm becoming the shield of a spinule, we should have what these styelids actually present.

But whatever be the cells which transform into the spinules, all the available evidence indicates that the cytoplasm is the chief source of the shield part of the spinule. The possibility is not excluded that the nuclear chromatin also plays some part in the development. Indeed investigations, notably those by Duesberg on the ascidian egg made since "cytoplasmic inclusions" have come into prominence are distinctly favorable to such an hypothesis. But this is very far from proving that such chromatinic influence, assuming it to exist, reduces the cytoplasm to pure passivity.

In the light of what is here set forth let us examine the view expressed by Conklin and quoted above that the egg cytoplasm fixes the general type of development, while the nuclei of egg and sperm together "supply only the details." The examination should be the more cogent from the fact that Conklin's idea was based largely on his investigation of ascidian eggs, some of which pertained to the very same genus, *Styela,* as do the spinules just described. The reader should not fail to notice that both Conklin and myself are dealing with single cells, he having to do with egg-cells, while I am concerned with spinule cells. Nor should the fact be lost sight of that these two categories of cells stand at about opposite extremes in the life career of an individual *Styela,* the egg being at the very beginning, while the spinule is produced at or near the completion of adulthood. Since, however, both cells possess attributes distinctive of the species, there can be no more "filling in of details" at one end of the series than at the other, so far as concerns the differential attributes under consideration.

Now comes the main point. The differential attributes of the egg-cell and early embryo, their "polarity, symmetry, and pattern," are "determined by the cytoplasm," to use Conklin's own words. What is the evidence that these attributes are thus determined? That of direct observation, as the examination of Conklin's work has shown. But if on the basis of such observation it can be asserted that attributes of egg and early embryo are determined by the cytoplasm, how escape asserting that the same sort of evidence touching the production of the adult attributes, those pertaining to spinule cells, are likewise determined by the cytoplasm?

It was remarked above that if all known relevant facts were taken into account and consistency in terminology be maintained, Conklin's statement to the effect that we are vertebrates because our mothers were vertebrates and produced eggs of the vertebrate type, but that our species and racial characters, color of skin and hair, and so forth, are determined by the chromatin of both parental germ-cells, would have to be greatly modified. We are now in position to see what modification is necessary. Although the statement is undoubtedly true that we are vertebrates because we develop from vertebrate eggs, the implication that the attributes which identify us with the human species and the Caucasian race are explained, so far as heredity is concerned, by the chromatin of the germ-cells, whether male or female or both, is not in accordance with all the observed facts bearing on the problem. The same kind of evidence on which the assertion is based that the embryonic characters are determined by the egg cytoplasm, requires the assertion that skin and other adult characters are determined by the same means.

This leads to the remarks we have to make about consistency in the use of terms. The critical reader will hardly have failed to notice the difference in application of the word "determined" as used by Conklin in the quotation we are examining. When it is said that the color of skin, hair, stature and so on are determined by the germ-cells, the determining act or condition is far removed from that which is determined, and no direct causal connection between the two is established. On the contrary in the statement that the polarity, symmetry and pattern of the egg are determined by the cytoplasm, the determination is immediate and observed. Manifestly in a literal sense "determined" is properly used in the second connection but not in the first. The cytoplasm is operative on the spot, so to speak, in the second case. It is concerned with an immediate result. The germ-cells on the other hand are not really determiners. They are not concerned with an end result, but are if anything instigators of a long developmental series at the far end of which appear the attributes in question: skin, color and the rest.

If this case of spinule production stood alone as an instance of specific characters in adult animals traceable to cytoplasmic activity of individual cells, it would be a rather small base on which to erect a general argument in favor of cytoplasm as inheritance material. But it does not stand alone. Indeed, the company to which it belongs will almost certainly be found to be legion when systematic investigation of the subject shall have been made.

The Spicules of Sponges and Other Invertebrates

I will cite a few more cases. In several widely separated groups of animals, spicules, usually either calcareous or silicious, are present in some of the tissues. These are often produced by one or a very few cells, and often, too, their shape, size, and probably other attributes differ from species to species even of the same genus.

The spicular system reaches its greatest development and has been most studied in sponges. "The spicules of sponges," writes Sedgwick, "in the diversity, symmetry, and intricacy of their form, in the perfection and finish of their architecture, constitute some of the most astonishing objects in natural history." [13] Figure 48 gives a hint of the facts on which this statement is based. Although the problem of what all these spicules are for does not directly concern us, indirectly it does, as the following further remarks of Sedgwick will indicate. "While it is pretty clear," he says, "that the main function of the skeletal structures is the support and protection of the sponge body, it is by no means easy to give explanations of the diversity and com-

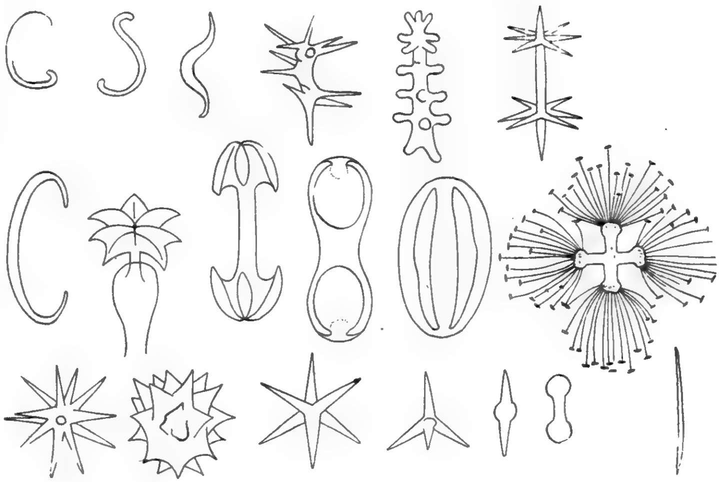

FIGURE 48. SPICULES OF SPONGES (AFTER LANKESTER).

plexity of form which they present. The form of the megascleres is probably connected with the form of the canal system with which they are in relation (F. E. Schulze); but the form and even the existence of the microscleres defies any reasonable explanation." And then comes this statement, highly significant for almost any discussion of heredity: "By some spongologists the small spicules are regarded as functionless, and as having on that account a greater value for classificatory purposes."[13]

If any one wishes to be convinced of the extent to which the spicule forms differ with different species, he should consult such systematic monographs as those of Schulze and Sollas in the reports of the Challenger Expedition. A picture of such a group of spicules as that shown in figure 48 reminds one of pictures of ice crystals he has seen; and the question may well be raised, Are not these spicules in reality crystallization forms, and hence as devoid of hereditary significance as are snow flakes? The fact that the form they have depends on the particular group of sponges to which they belong, i.e., that they follow the rules of biological taxonomy, is very strong evidence that they are

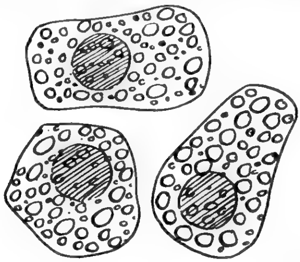

FIGURE 49. DEVELOPMENT OF A SPICULE (AFTER LANKESTER).

genuine organic productions, and not mere crystallizations. And the further fact that they follow this rule even though many of them appear to have no functional significance indicates that their particular forms are due to heredity and not to modeling by extraneous influences in each individual sponge. But we are not left to such general evidence for support of the supposition that they are true organic products and subject to heredity. Their development has been studied by several zoologists and the results leave no doubt about their nature so far as this point is concerned.

A single instance will be enough for our purpose, but it should be remarked that many others could be given. This is taken from the excellent summary of what is known about sponges written by Minchin for Lankester's *Treatise on Zoology*. "To form a triradiate spicule three cells migrate into the parenchyma from the dermal epithelium and become arranged in a trefoil-like figure (Figure 49). The nucleus of each cell then divides into two, in such a way that one nucleus is placed more deeply and one more superficially. Between each pair of sister nuclei a minute spicule ray appears, the three rays being at first distinct from each other but

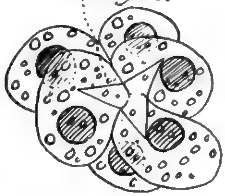

FIGURE 50—SEE 49.

soon becoming united at the center of the system (Figure 50 *tr. syst.*). As the rays grow in length the protoplasm of each actinoblast becomes aggregated around each of the two contained nuclei and finally more or less completely segmented off to form two *formative cells,* of which the one placed more internally travels to the tip of the spicule ray, while the other remains at the base

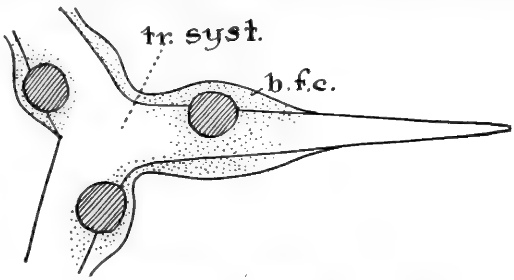

FIGURE 51—SEE 49.
b.f.c., basal formative cell. tr.syst., triradiate system.

(Figure 51, *b.f.c.*). The apical formative cell sooner or later disappears, returning, apparently, to the epithelium. The basal formative cell remains at the base of the ray (Figure 51), until this portion is secreted to its full thickness. It then migrates slowly outwards along the ray, and in the fully formed spicule is found adherent to the extreme tip."

That such a mode of development is entirely foreign to crystal production hardly needs to be remarked. But if further proof to the same effect were demanded, one other strong piece of evidence is the fact that most of the spicules are not composed of inorganic substance alone but have a core of organic matter.

Although the technique for the examination of cytoplasm developed during these last years has not, so far as I know, been applied to the spicule-producing cells of sponges, we can be reasonably sure from the study of other secretory cells what the general results will be when such application shall have been made. They will bring out numerous details not now known of how both the nucleus and the cytoplasm act during spicule production.

Surely it is not necessary for me to dwell again on the main point of the evidence here presented. The nuclei of the spicule-forming cells may take an active part in producing the spicules. Indeed from our general knowledge of nuclear activities, illustrations of which were given in an earlier chapter, it is probable that such will some day be demonstrated to be the case. But the proof of nuclear activity in spicule production *will not be disproof of the already observed cytoplasmic activity in spicule production.*

Other animals that may be mentioned in which spicules are produced in much the same way and have the same taxonomic diversity and constancy are the alcyonaria among coelenterates, the holothurians among echinoderms, and some of the compound ascidians, particularly of the family didemnidæ. Relative to the specificity of the structures in holothurians, we have this piece of significant information: "These calcareous bodies are of great value to the systematist in classifying the smaller groups, such as genera and species. Although their general characteristics are fairly similar within the several families, the different shapes of spicules are not sufficiently constant to be used as diagnostic characters of such large divisions."[15] In other words, so far as these animals are concerned, should it be found, as it almost certainly will be, that the cytoplasm is "hereditary substance" for the production of spicules, the reverse of Conklin's generalization that the cytoplasm determines the larger taxonomic features of animals while chromatin is the seat of the inheritance factors for "filling in details" turns out to be true. Species-marking details are just what we are able to see the cytoplasm fill in.

The "Hairs" of Higher Plants

For a few more instances of species characters in multicellular organisms brought down to single cells, we turn to the plant world.

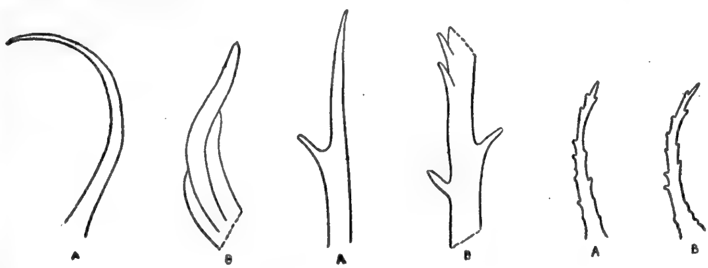

FIGURE 52. FIGURE 53. FIGURE 54.

FIGURE 52. TRICHOMES OF PAPAVER ORIENTALE (AFTER CANNON).
FIGURE 53. TRICHOMES OF P. PILOSUM (AFTER CANNON).
FIGURE 54. TRICHOMES OF P. SOMNIFERUM (AFTER CANNON).

The "hairs" or trichomes borne on the leaves, flowers and smaller stems of innumerable flowering plants are usually composed of only a few cells, so that the characters they have are often referable to the individual cells. Cannon has lately investigated these structures in several groups of plants, and while he was not aiming at the particular question now occupying us, some of his results are quite to the point for this discussion. An example of particularly distinct specificity of the hairs is presented in the following: "The trichomes of the three species [of poppy] are similar in form and size, but they are unlike in quality of roughness. In *Papaver orientale* (52 a, b) and *Papaver pilosum* (53 a, b,) the distal ends of the superficial cells project beyond the general surface of the trichome and turn out at a rather acute angle. In *Papaver somniferum,* however, these cells did not extend beyond the general surface, with the effect that the trichomes of this species are smooth (figure 54 a, b)." [16]

This is especially instructive because the attribute in question pertains to the shape and position of cells, and not to differentiation within the cells. There, consequently, is no room for even a reasonable surmise that the attribute is explained by the chromatin instead of the cytoplasm. Even though the account gives no information about the position and behavior of the nuclei of the cells, it is hardly conceivable that any one would maintain that the substance itself of the cell-tips is not the main factor in the outturning of these tips characteristic of *Papaver orientale* (figure 52 a, b).

Dealing with quite another type of trichomes, those of the walnut, Cannon writes: "A character which easily distinguishes the short secreting trichomes of *Juglans californica* from those of *Juglans regia* or *Juglans nigra* is the length of the head-cells." [17] Both drawings and tables of measurements of the heads showing lengths and diameters are given to bring out the positiveness of the distinction. Furthermore, details of cell division and cell structure during the development of the trichomes are furnished; so the *visible* evidence that the cytoplasm is "inheritance material" in this case is beyond question. What the invisible evidence may be remains for further investigation to discover, but of one thing we may be sure: no matter how many facts of development now invisible may later become visible they will not destroy the validity of the present visible evidence.

How far it would be possible to go on pointing out specific characters of plants that are referable to individual cells I do not know; but judging from the instances that come to view even in my limited knowledge of the subject it might be carried to an almost indefinite extent. Another instance which I recall from the experience of my student days is the case of mosses. The serration of the leaves, I remember, was one of the features relied upon for generic and specific characteristics, and I also remember that the individual teeth often if not always consisted of one or a very few cells.

Cell-wall Structures in Higher Plants

That the cell-wall is a structure of great importance in plants is known to everybody; and the veriest tyro in plant histology has learned something of the enormous variety and definiteness of character in different tissues and different kinds of plants presented by this part of the cell. A very brief reference to two plant structures, pollen-grains and wood tissue, will be, perhaps, a sufficient reminder of what there is for us in this domain. A typically formed pollen-grain is a minute spheroidal body containing two cells, one known as the antheridial or germinative cell and the other as the sterile or vegetative cell. The wall of the grain consists of an outer coat, the *exine*, and an inner, the *intine*. The elaborateness of structure which these coats may reach is astonishing if regarded in the crepuscular light of the theory that cells are "simple" things. The most distinctive thing about the pollen-grain is the pollen tube which is produced on one side of the grain and through which the antheridial cell reaches the ovule in fertilization. The taxonomic variety which is our main interest just here pertains largely to the sculpturing of the surface of the exine and to the structure of the exine at the point where the pollen tube will break through. It is well known to botanists that the "spikes, warts, ridges, combs, etc." of the surface of the grains are in general definitive of taxonomic groups of plants. And concerning the places of emergence of the pollen tube we read: "The number of these peculiarly organized points of exit is a fixed one in each species, and often in whole genera and families." [18] As to the way these various structures are produced we have this very definite statement: "The sculpturing upon the outer surfaces of the spores of mosses and ferns and the corresponding pollen grains of the Phanerogams can in most cases be attributed to the activity of the protoplasm surrounding the developing spores." [19]

That the main tissues of plants present taxonomic characters is amply illustrated in the wood-tissues. The facts concerning these tissues have been almost forced into prominence by the needs of fossil botany, though they have also been much studied as a part of ordinary plant morphology. The section on fossil wood in Zittel's Handbook of Palaeontology is a considerable resumé of knowledge in this field, and contains numerous statements and figures which bring out impressively the general truth of the specificity of the tissues of trees. After necessary allowance is made for the strictly botanical unsatisfactoriness of many of the species and genera recognized by palaeobotanists, it is not doubted, so far as I know, that on the whole the kinds of tissue they describe do in reality represent different kinds of trees. A single illustrative quotation will serve to make concrete what is here dealt with in general terms: "The phloem segments, like those of the xylem, are divided by few-seriate pith rays into rather regular two- to four-seriate rows of cells made up of thin-walled, small-celled elements in the trunk of *Zamia floridana*, etc., and *Stangeria*. But . . . in the trunks of *Cycas*, *Dion*, *Encephalartos*, *Macrozamia* and doubtless most cycads, as likewise in the Cycadeoideae, sclerenchymatous elements are more or less numerously and regularly interspersed among the row cells, thus giving much more strength to the stem."[20] And the author goes into considerable detail in discussing the presence and the absence of "wood tracheids," "scalariform pittings," "border pits," "spirally thickened walls" and "sieve tubes" in the genera *Zamia, Cordaites, Stangeria* and *Cycas*.

That this well-nigh endless variety of character of the cell-wall in adult plant tissues is due to the activity of the cell protoplasm appears never to be questioned by botanists so long as they are dealing with the actual structure and development of the wall. "The cell-membrane is produced by the protoplasm," we read in the section on morphology in the *Lehrbuch der Botanik* by Strasburger, Jost, Schenck and Karsten (11th German edition) this section being from the pen of Strasburger himself. This simple, unqualified statement of fact by Strasburger is the more noteworthy because, as we saw in another connection, he has been one of the extremists on the chromosome dogma of heredity. Reading his statement that the cell-membrane is produced by the protoplasm, with the indubitable fact in mind that this membrane presents innumerable characters which are taxonomically definitive, and hence are hereditary according to the best criteria we have of hereditary characters, it seems impossible to avoid seeing that it implies an irreconcilable contradiction of Strasburger's often-repeated view that the chromosomes are the sole bearers of heredity.

To round out the primarily factual part of this discussion two questions remain to be considered: first, that of heredity in the main classes of tissues of multicellular organisms; and second, that of the results being reached by the latest methods of cytoplasmic study on the behavior of different portions of the cells in the histogenesis of these tissues. What is implied in these two questions can be made clear by a special case. Is the minute structure of striated muscle tissue, for example, subject to heredity? If so, are the hereditary attributes determined by the chromatin or by the cytoplasm of the myogenic cells? Applying our usual test for hereditary structures, we ask whether or not this tissue presents attributes characteristic of the taxonomic groups, species, genera, families and so on. Here again, while we have a vast store of knowledge about the structure and genesis of muscle tissue in many kinds of animals, only incidentally, as a rule, have the studies been made from the taxonomic standpoint.

The Morphology of Striated Muscle Fibers

For one series of very recent studies that comes near this standpoint we are indebted to H. E. Jordan. On the taxonomic aspect of the matter Jordan writes: "A quite general consensus of opinion considers them more or less closely related, and ranks them both between Crustaceans and Arachnoids. *Limulus* muscle, however, is in appearance very much more like vertebrate than like insect muscle; while the muscle of the marine arthropod *Anoplodactylus* is of the typical insect type." [21] Of the numerous differences between the fibers of the two animals compared, reference to two will suffice for our purpose. They concern the so-called *M* and *Z* lines found in the light bands of most striated muscle tissue. What Jordan regards as one of the important results of his work on *Limulus* muscle was the evidence secured that the *Z* line represents a membrane, as some observers have believed, the specially convincing evidence being the fact that the "line" is attached to the sarcolemma peripherally and to the nuclear wall centrally. These relations are lacking, he says, in the sea-spider's muscle. The *M* line, he tells us, is especially well developed and hence easily demonstrated in the sea-spider muscle in some states of contraction, while he failed, as have other students, to detect it at all in the *Limulus* muscle.[22]

So we come again to the real issue. Assuming the *Z* membrane to be a cytoplasmic structure, as it has practically always been held to be, are we going to deny that the cytoplasm itself causes the peculiarity of the *Z* membrane in the sea-spider as compared with that in *Limulus*, that denial being necessitated by the dogma that the real "seat" of the difference is the chromatin of the nucleus operating by some invisible "factor" perhaps of the nature of an enzyme?

The extent of variety in striated muscle tissue is brought impressively to view in such a comparative study as that by Marceau. His main object is not to find differences but to discover whether in spite of structural differences they have similar traits, as if they might all be derived from a single primitive form which has undergone more or less profound modification.

Of the many differences which the investigation sought to reduce to orderliness on the basis indicated, only two will be mentioned. From an elaborate table of measurements of the diameter of fibers, we find the following results for the sheep and pig:[23]

	Maximum	Minimum	Average
Sheep	25μ	5μ	15μ
Pig	45	5	20

The other point selected concerns "striated scleriform transverse bands" characteristic of the muscle fibers of the vertebrate heart. This time the animals we choose are the horse and cow. The thickness of the band is given as exactly the same in these two, but the distance between the bands is 140μ for the horse and 120μ for the cow.[24]

The Physiology of Muscle Fibers

We might go on almost endlessly, pointing out slight but constant specific differences that involve differences in muscle structure, questioning in each case whether the hereditary cause of this difference lies in the cytoplasm or chromatin of the muscle cells. But such repetition would be useless for the present discussion. Striated muscle tissue is specially favorable for testing hypotheses about inheritance material from the functional side as well as from the structural side. For example, there are innumerable differences, larger and smaller, in the limb movements of animals belonging to different species and genera. I know of no observations which precisely connect activities of this sort with muscle tissue; but information concerning the electromotor force in various animals is available. The following on the authority of Englemann, taken from Winterstein will serve our purpose. The values are those of the "demarcation current" of galvanic electricity of heart muscle, this current being generated by making a cut surface at the base of the heart and the natural surface at the apex act as a galvanic pile: [25]

Animal species	*Electromotor force in D.*
Anguilla fluviatilis	0.0265
Rana esculenta	0.0311
Triton cristatus	0.0124
Tropidonotus natrix	0.036
Testudo graeca	0.022
Columba livia	0.0458
Cygnus oler	0.0168
Mus musculus v. albino	0.040
Mus rattus v. albino	0.0446
Lepus cuniculus	0.0363

These investigations appear to have been made from the standpoint of general physiology, and therefore not to have been carried out with the systematic exactness and exhaustiveness demanded for taxonomic discrimination. We may consequently presume that more searching examination of the same series would considerably modify these results, but we have no reason to suppose that they would eliminate altogether the differences due to the animal species.

After due allowance is made for the purely physiological and environic causes which undoubtedly explain a great many of these differences, probably no biologist would hesitate to grant that many of them have an hereditary basis. Turning again to the question of the seat of the hereditary factors, we are now especially attracted by the functional aspect of the subject. To an unsophisticated physiologist studying the phenomena involved in this question, it would probably never occur that more than one answer is possible. Well-informed as such a physiologist may be supposed to be on the important part known to be played by the nucleus in the life of the cell, he would undoubtedly take it for granted that the whole nucleus, its chromatin with the rest, contributes in some fundamental way to the result. But unless well indoctrinated beforehand with the chromosome dogma of heredity, he would almost certainly be amazed were some one to contend seriously that the cytoplasm is not the material basis of the hereditary peculiarities exhibited. He would reply, "Why, you are virtually denying that the substance of the muscle fiber is the real seat of muscular activity, thus implying a contradiction of the 'universally accepted principle that the *potential chemical energy of the muscle substance is the primary source of muscular energy in all its manifestations*' [26] for surely muscular energy 'in all its manifestations' would include those elements of muscular activity which are hereditarily distinctive of different kinds of animals."

That the cytoplasm is at least the main source of the muscle substance furnishing this energy would not be questioned, probably, by any histologist, but the definiteness of view held at the present time on this subject is worth recalling and is indicated by such statements as the following: "The energy of contraction is the transformed surface-energy of the ultimate structural elements or colloidal particles (submicrons) composing the fibrils." [27]

Presumably there would be much difference of view among physiologists as to the validity of the chemico-physical part of Lillie's theory of muscular contraction; but apparently there would be very little dissent from that part of his view which locates the processes, whatever their exact nature, in the muscle fibers. So it would be merely a matter of sufficient patience to go over all the tissue systems, epithelial, glandular, bony, nervous, and the rest, and point out numerous certain, and innumerable probable instances of differences for different taxonomic groups of animals, and to show that these hereditary differences are expressed primarily in the cytoplasm of the cells.

Summary of Positive Information about the Physical Basis of Heredity

We have explored a vast region of fact and theory concerning propagation and development in organisms, for the purpose of ascertaining what is actually known about the organs and substances by which hereditary attributes are produced. Expressing the matter in terminology familiar to current discussion on heredity, we have been trying to find what is actually known about the physical basis of heredity. If clear-cut, unequivocal information of the kind sought is contained in all we have seen, it ought to be statable in a few simple sentences.

What has been accomplished may be epitomized in two such sentences:

First. *Overwhelming observational evidence has been secured that the cytoplasm of cells participates directly in the formation of organic parts which have hereditary attributes.*

Second. *A great mass of evidence, partly of observation and partly of legitimate inference from the principles of organic integration, has been secured, that the chromosomes of the germ-cells in plants and animals which propagate by means of such cells, participate in the production of organic parts having hereditary attributes.*

Any substance which plays such parts in development may be named a physical basis of heredity; and these two groups, or categories of knowledge must, it seems, serve as the foundation of all legitimate reasoning about such "basis of heredity," or "inheritance material," or "hereditary factors," or "bearers of hereditary qualities," or whatever expression for the idea be employed.

REFERENCE INDEX

1. Payne 190
2. Meves 816
3. Meves 817
4. Meves 820
5. Meves 850
6. Meves 845
7. Cowdry ('14) 416
8. Cowdry ('16) 436
9. Lewis and Lewis 393
10. Loeb ('06) 181
11. Conklin, ('15) 176
12. Ritter ('93)
13. Sedgwick 82
14. Minchin ('00) 107
15. Goodrich 224
16. Cannon 13
17. Cannon 23
18. Goebel 367
19. Campbell 51
20. Wieland 197
21. Jordan ('16) 493
22. Jordan ('16) ......fig. 7 496
23. Marceau 273
24. Marceau 280
25. Winterstein 111
26. Luciani ....III, 85
27. Lillie, R. S. ('16)...... 255

Chapter XVI

THE INHERITANCE MATERIALS OF GERM-CELLS INITIATORS RATHER THAN DETERMINERS

Antecedents of the Cytoplasmic and Nuclear Theories of Inheritance Material

FOR the purpose of calling vividly to mind the character of the evidence on which the two propositions rest with which the last chapter ended, it will be profitable to cast a glance back on the course along which biology has come down to us, with a view to finding a sharply outstanding spot in the early growth of knowledge which led to each of them. On the botanical side such a spot in the knowledge of cytoplasm as hereditary substance, is the work of Schleiden on the microscopic structure of adult and developing plant tissues. The publication of his *Ueber Phytogenesis*, 1838, may be taken as the starting point of our knowledge of cellular transformation in the production of tissues. It should be remembered that the observers of that period had very hazy notions about the distinction between nucleus and cell-body, or cytoplasm. On the zoological side the publication by Ehrenberg, in 1836, of *Die Infusionsthierchen als volkommene Organismen* may, I think, be looked upon as the first milestone in the progress of knowledge of cytoplasmic transformation into tissue substance.

The ever-broadening stream of knowledge of the chromosomes in relation to heredity is usually held to have originated in the discovery forty years ago, by O. Hertwig, that the most essential fact in fertilization is the union of the nuclei of the male and female germ-cells.

That cytoplasm is a physical basis of heredity is proved by a great body of direct observational knowledge. That the chromatin of chromosomes is a physical basis of heredity is proved by much observational knowledge when this knowledge is supplemented by reasoning involving the principles of biological comparison and correlation. These two masses of knowledge constitute, as already indicated, the foundation of all legitimate reasoning about inheritance material. Whether chromatin and cytoplasm are the only substances which participate in the formation of hereditary structures can not now be stated with certainty, though there are both observational and general grounds for believing that they are not. But into this question we need not enter in this discussion. Nor is it necessary for our purpose to inquire very particularly whether the conceptions of chromatin and cytoplasm really imply just two substances or two great classes of substances, though it is best to have in mind the undoubted fact that the latter alternative is the true one. Beyond a doubt "chromatin" and "cytoplasm" ought always to be understood to mean "kinds of chromatin" and "kinds of cytoplasm."

Function of Chromosomes in Heredity Acquired and Secondary

The question which specially concerns us here is that of what the relation is between chromatin and cytoplasm in virtue of which they play the particular rôles they are found to play in producing hereditary structures. Perhaps the most important aspect of this general question is that which the theory of phyletic evolution naturally brings up: does the evidence in hand suggest any answer to the question whether chromatin or cytoplasm is the more primitive and fundamental as hereditary substance? Surveying as we have the whole field of organic propagation, including the process in unicellular organisms as well as in multicellular, and sexless as well as sexual methods of reproduction, and taking the facts as they actually present themselves, it seems as though but one answer to this inquiry is possible: *The substances included under the generic term cytoplasm are the more fundamental and primitive.*

The only possible way of escaping this conclusion is by excluding from the conception of heredity the vast majority of developmental phenomena presented by unicellular organisms and by monogenic propagation in multicellular organisms. As our examination of these provinces revealed, such exclusion is exactly what the chromatin dogma of heredity has undertaken, implicitly or explicitly, to fix upon biology. The utter unwarrantableness of this undertaking was made sufficiently obvious, we may assume, by the examination; so we need spend no time on that now. All that is necessary is to remind ourselves vividly of the main positive outcome of the examination: Heredity is a universal phenomenon of the living world. It is coextensive with organic propagation and development, while "carrying heredity" by chromosomes is, according to the evidence, very far from a universal phenomenon. It is a long way from being coextensive with organic propagation and development. We may, consequently, proceed in our quest of a more rational, more consistent, more satisfactory conception of the purely operative side of producing hereditary structures. Pursuing the quest, we remind ourselves of having found that only the most meager observational evidence is afforded by the protophyta and protozoa, and by monogenic metaphyta and metazoa, that chromatin is hereditary substance, while these organisms afford an overwhelming mass of such evidence that cytoplasm is hereditary substance.

But if in the lower, more primitive moiety of the organic realm, chromatin is a physical basis of heredity to only a limited extent and in a partial way but has this office widely and positively fixed in the higher moiety, the moiety, that is, in which bisexual propagation is fully established, what other conclusion can be drawn consistently with the modes of reasoning universally sanctioned by evolutionists, than that the function of "carrying hereditary qualities" by the chromosomes in higher organisms is a secondary or acquired, or better a delegated or assigned function? Heredity is far *older*, phylogenetically, and far *broader* taxonomically than is the chromatin mechanism by which it now in part manifests itself. Under this interpretation the acquisition by chromosomes of the function of carrying heredity would belong to the same evolutional type as for example the acquisition by certain cells of the function of muscular contraction, or by certain other cells of conducting nervous stimuli. The advantage and satisfaction of a conception of the rôle of chromosomes in heredity which ranges them naturally and easily with all other organs and tissues of the plant and animal body will be quickly seen by every one to whom the seemingly endless chance of discovering new interrelationships and consistencies in living nature is one of the most rewarding things about biological investigation.

While we are duly impressed with the importance of perceiving that chromosomes fall into the great class of other organs and tissues when considered from the standpoint of phyletic differentiation of structure and function, we should not fail to notice that within the class they hold on a number of counts a very distinct place. Probably the most distinctive of these counts, at any rate the one most important for this discussion, is the fact that while the vast majority of tissues, taking the term in its usual meaning, stand at or near the termination of the ontogenic series—are, in other words, the final stage in the series—the chromosomes in their function as bearers of heredity stand at the beginning. They represent the initial stage in the series. Furthermore, their function in this respect is unique as contrasted with the function of other tissues, in that while other tissues have, typically, each a single function which they perform immediately, the chromatin of a given germ-cell has a great complex of functions, namely, that of initiating the development through which all the attributes of the individual during its whole life-career are developed. In a word, chromosomes of germ-cells are not *determiners* or carriers of determiners; they are *initiators* or carriers of initiators. They may, then, be called bearers, or carriers of heredity in a very literal sense, namely in the sense that they are made use of by one individual, the parent, to carry across or transfer from itself to another individual, the child, the hereditary attributes of the species in a latent or potential state. By virtue of their being thus used, they are members of a developmental series, in which series their place is at the beginning and not at the end, the nature of the series depending on the phylogenic history of the particular organism to which the particular chromosomes belong.

The Two-fold Character of the Problem of Hereditary Substance

At this point it becomes a matter of the greatest importance for theories about hereditary substance to distinguish between the problem of the operation of such substance in the developing individual, and that of how such substance ever came to be hereditary substance; stated otherwise, between the problems of how any substance participates either directly or as an agent in the building up of a structure having hereditary attributes, and that of how the substance itself became impressed with the attributes, in a latent state, of the progenitors of the developing individual.

Investigation of the first of these problems is to a large extent a matter of observation, as we have seen in the preceding pages. The sub-science of histogenesis consists largely in tracing out the processes by which completed tissues arise by the transformation of less differentiated or undifferentiated cells. And when such newly arisen tissues and structures are proved to be hereditary by such evidence as we have called attention to, then the study of histogenesis comes to be so far a study of hereditary substance.

When, however, we turn to the other problem, that of how hereditary substance comes to be such, we are in a different, a much more difficult case, for so far science has succeeded in getting almost no observational hold upon it; and despite the vast discussion it has received the darkness that envelops it is hardly an iota less black than it was the day of its original formulation. But stygian as the darkness is, here, especially as to details, we yet are able to see, probably, the quarter from which light will come if ever it does come. That quarter is the physical-chemistry conception of the organism as a system of phases the whole of which, as a species entity, is essential to its equilibrated activities. This nature of the organism, together with something akin to its internal secretory and enzymic productivity, enables it, we may conjecture, by some means now wholly unknown, to reflect its totality of transferable attributes upon the germinal cells and to transform them into a latent state. Ungrudging acknowledgment of the completeness of our ignorance of how any part of a cell or any other portion of an organism becomes endowed with the capacity to develop or causally to affect the development of an organism similar to that from which it came, should be an important item in the preparation to accept any and all indubitable facts connected with heredity, even though no causal explanation of them is forthcoming.

The childlikeness, as Conklin well characterized it, of the belief that chromosomes are a simple and complete explanation of inheritance would not be so bad in itself. If it stopped there, as genuine childlikeness would, no positive harm could be done. It is the making of this belief the starting point of a grand speculation which blinds the eyes and closes the mind to a vast number of facts and legitimate inferences about heredity, that plays havoc with thinking on genetics. It would be a great gain if genetic theory would recognize wholeheartedly that all organic development, as contrasted with mere enlargement, consists more fundamentally and obviously in transformation of substances than it does in unchanged continuity of substances. For under such recognition the futility of attempting to explain the transformation of one lot of substance by referring it to another lot which does not transform, or in other words to explain development by something that does not itself develop, would be manifest.

That the chromosome theory of heredity in reality deepens rather than illumines the darkness which surrounds the problem is seen when one reflects that not only does it throw no light on the question of how the chromosomes come to be bearers of heredity, but that it creates the new and equally difficult question of how the chromosomes (which according to the theory maintain unchanged their individuality not only from generation to generation but throughout each ontogeny) are yet able to be causally operative in the cell-bodies undergoing the transformations which they actually do undergo in the developing organism. That the germ-plasm-chromosome theory of heredity could have led its devotees to sidestep the details of ontogeny, especially those of histogenesis, to the extent which our review has shown it to have done, would be unbelievable but for what is actually before us in the recent history of biology.

Viewing heredity as being definitively a kind of organic transformation—transformation, that is, in accordance with a pre-existing or ancestral pattern more than it is a kind of continuity—it becomes obvious that even were the demonstration to become complete that the chromosomes are the only portions of the germ-cells * essential to fertilization, they still would not be proved bearers of heredity in such sense as the germ-plasm theory holds them to be. They would not because the problem of the transformations which constitute ontogeny would still be untouched. The theory would be established only when the demonstration should be produced that the chromosomes cause immediately all the particular ontogenic transformations known to be hereditary. All that would be proved about heredity by demonstration that the chromosomes alone participate in fertilization would be that the chromosomes alone constitute the first ontogenic stage of the hereditary parts of the particular organism to which the fertilized egg gives rise.

The Probability That Inheritance Material Becomes Such In Each Ontogeny

But because thus far failure has attended all efforts to get knowledge of how hereditary substance is produced, are we obliged to own that we know nothing at all, even inferentially, about its production? And is the search for such knowledge to be given up as hopeless? My answer is an energetic negative to both these questions. In the first place, there is much evidence to support the hypothesis, very general to be sure but yet by no means devoid of usefulness, that hereditary substance becomes such in some

* The utter unwarrantableness of the common assumption that as regards the male germ-cell such a demonstration is "practically complete" will be noticed presently.

way through being subject to the metabolic processes common to the whole organism. Undoubtedly the germ-plasm dogma itself has tended strongly to divert attention from this aspect of the problem of germinal material—indeed, has tended to minimize the importance of the metabolism of such material even if it has not tended to deny that the material is subject to this process.

So important is it from the organismal standpoint to conceive the material basis of heredity as part and parcel of the organism generally, especially as regards the basal growth and sustentative processes, that we must examine in some fullness the evidence favorable to such a conception. In its most brazenly evidence-ignoring form, the germ-plasm dogma asserts that the female parent does not really produce the eggs or the male parent the sperm, as they seem to, but that these are produced by previous germs *ad infinitum.* There are, to be sure, quite a number of observable facts, as those of the early formation of germ-cells in several animals, that can be forced into a seeming support of such a conception. But the familiar and all but universal fact that multicellular organisms, plants and animals alike, are sexually immature for a shorter or longer part of their lives, the very essence of the immaturity being the undeveloped state of the reproductive system, would be a sufficient refutation of the view for any mind not made impervious to facts by long and faithful sophistication.

Germ-Cells Subject to Metabolism Like All Other Cells

The biological commonplace that all germ-cells, like all other cells, undergo a process of growing and maturing before they can perform their distinctive office, and that this process depends upon the retention of the germs by the parent organism, ought, as already indicated, to be a sufficient antidote against the germinal continuity fallacy, even though nothing were known as to exactly what goes on in the germ while it is growing and ripening. But we are by no means without positive knowledge under this head. In fact the last few cell divisions immediately preceding the ripening of both ova and spermatozoa, and the ripening processes themselves, have received searching examination during the last few decades, with the result that hardly any cytological phenomena are better known than are the profound morphological changes which accompany these processes. That these changes are particularly manifest in the chromosomes, the assumed seat of the determiners of heredity, is one of the very things that has aroused so much interest in the processes. Nor are we wholly uninformed about the chemical changes taking place in the growing germ-cells. Unfortunately knowledge in this field has hardly passed the stage of early infancy, but at least enough is known to warrant the assertion that the young germ-cells are subject, as are all the other cells, to the general metabolism of the organism.

Chemical Changes in Germ-Cells During Parent's Ontogeny

About the most striking information we have in this field is what has come from such investigations as those on the chemical changes which occur in the sex glands and other body parts during reproduction in some fishes.* Miescher's work was ground-breaking in this domain for it was the first to show that the "sexual organs in the salmon develop at the expense of the muscular system, and that the salmine deposited in the testis during the breeding season must be derived from the proteins of the muscle, since the

* Notable among these studies are: *Histochemische und physiologische Arbeiten, gesammelt und herausgegeben von seinen Freunden,* by Miecher; and *Changes in the Chemical Composition of the Herring during the Reproduction Period,* by Milroy. Biochemical Journ., v. III, 1908, p. 366.

fish does not take any food during the period."[1] The work of Riddle and his collaborators is producing evidence to the same effect.

Such researches do not, to be sure, prove that the chemico-physiological changes extend to the chromosomes of the germ-cells, much less to the imaginary determiners of heredity in the chromosomes. But viewing the results in the light of the well-grounded general belief that the most fundamental test of living substance is metabolic change, it is seen that any hypothesis which assumes the existence in the germ-cells of something virtually not subject to the general metabolism of the cells, assumes at the same time the burden of furnishing objective evidence that such a something does exist.

As a matter of fact the prime offense of the germ-plasm-determiner hypothesis is that its very essence places it beyond the reach of scientific observation. Such truth as it may contain cannot be made really effective because it can not be proved, and such error as it may contain can not be robbed of its power for evil because it cannot be disproved. In a word, the hypothesis is one that belongs to the realm of dialectics primarily, and has no just claim to a place in inductive science.

The Possibility of Changing Sex By Influences on the Germ

But perhaps the most conclusive evidence of the fundamental dependence of true germinal material upon the organism, should somewhat fuller verification of the observations be obtained, are results like those reached by Whitman, King, Whitney, Riddle, and by R. Hertwig and his students, according to which sex may be reversed in several animal species by various conditions extraneous to the germ itself, acting on the germ-cells from which the animal is to develop. The instance of this usually regarded as best established is afforded by certain species of frogs and toads.

The widely known result reached by Hertwig and verified and extended by Kuschakewitsch expressed in a single sentence, is that the number of males and of females produced by the eggs of a given female depends upon whether the eggs are fertilized when newly ripe or when over-ripe, a great predominance of males coming from the latter class. In a tabulation of the results of four sets of experiments by Hertwig [2] almost every case shows the number of males increased when the time elapsing between deposition of the eggs and fertilization was increased, the highest percentage of males in any one lot being 759, fertilization in this instance having been twenty-two hours after the fertilization of the last preceding lot.

In a species of toad, *Bufo lentiginosus*, results have been obtained just the reverse of those on the frog, that is, the proportional number of females has been experimentally increased. This was accomplished by fertilizing the eggs in water made slightly alkaline. Since frog eggs are known to absorb water when they remain long in it, as in the case of those which gave rise to a preponderance of males in the Hertwig method of experimenting; and since alkaline solutions extract water from eggs, and likewise cause them to produce a preponderance of females, Miss King drew the obvious conclusion that the quantity of water contained in eggs of these animals may be a factor in determining the sex of the animals developed from the eggs. Although Miss King recognizes that her experiments do not furnish final proof of the conclusion she draws, she believes, rightly it would seem, that they weigh heavily in that direction. "As they stand," she writes, "the results strongly suggest that sex in *Bufo* is determined at or near the time of fertilization, and that external factors acting during this period may influence the sex-determining mechanism in such a way as to cause it to produce one sex or the other. The results also seem to indicate that in *Bufo* sex is determined in the egg, and that it may depend in some way on the relative amount of water in the egg at the time of fertilization." [3]

Riddle, perhaps the most outspoken opponent of sex predestination now writing, strongly espouses the hypothesis that the sex to which a particular egg will give rise is dependent partly on the quantity of water which that egg contains. But whether water is a factor in determining sex or not, the evidence presented by Riddle, coming partly from researches by C. O. Whitman and partly from his own, constitutes, when taken with the evidence to the same effect presented by other investigators, almost if not quite complete proof that sex is not the hard-and-fast thing which most present-day genetic speculation would make it.

Furthermore the evidence produced by these two investigators seems to connect the decision as to which sex a particular egg shall give rise, with some condition of the parents. It is well known to all zoologists, in the United States at least, that at the time of his death Professor Whitman had accumulated a vast store of data on the habits, particularly the breeding habits, of pigeons. To Doctor Riddle, who had worked with Whitman considerably, fell the task of carrying on to some extent Whitman's experiments and of preparing for publication the results which Whitman left in the rough. The following quotation from Riddle's paper referred to above, summarizes Whitman's results that are especially important for us now: "Whitman found that if certain very distantly related pigeons [*i.e.*, two individuals from different families] are mated that only male offspring resulted. If the matings were made of individuals not quite so distantly related—different genera usually—and if to this situation be added the element of *overwork at reproduction* [*i.e.*, the birds not being permitted to nest their own eggs, but forced to keep laying eggs in rapid succession] then the first several pairs of eggs produced in the *spring* will produce all or nearly all males. The last several pairs of eggs laid in autumn will produce all, or nearly all, females. At the transition period in the summer he found that some pairs, or clutches, of eggs produced both a male and a female. In these cases it was usually the first egg that produced the male; and the second egg—laid forty hours after the first—that gave rise to a female." [4]

Into Riddle's interesting discussion of Whitman's results and his own chemical studies on the eggs of pigeons and hens we need not go. Suffice it to say that it seems to me Riddle is justified by the evidence now in our possession, in his contention that "sex rests upon a quantitative and reversible basis" and that in this sense it has been controlled by conditions extraneous to the germ-cells themselves. This does not imply, as I understand, that such control would necessarily be practicable or even possible in all organisms, nor does it preclude the possibility that in some species there may be dimorphic or partially dimorphic spermatozoa or ova as regards sex production. Neither does it preclude the possibility that in some cases where a preponderance of one sex has been observed, this is due to selective mortality or some process other than the actual shifting of the sex tendency in the particular eggs.

These several concordant bodies of testimony must, it would appear, open the eyes of biologists sooner or later to the ludicrousness of a theory that would make the parent organism hardly more than a combined culture medium and incubating oven for its germ-cells.

The Determiner Conception Contrary to Ordinary Chemical Principles

If, on the basis of such facts as we have, we try to come still closer to the questions of how the assimilative and morphogenic processes of the organism occur, whether in the production of hereditary substance or in the transformation of such substance into actual hereditary structures and activities, we find ourselves hedged about on every side by partial knowledge, by dubious knowledge, and by complete ignorance on many fundamental points.

However, chemical considerations seem to point the way to future discovery. In the first place, it seems necessary to recognize that the whole germ-plasm conception as originally promulgated, with its interminable system of "bearers" and consummators, was contrary to what is well known about chemical processes generally. Thus the continuity presented by a complex chemical operation does not consist in an unchanged series of individual entities of some sort, such as determinants and determiners are, or originally were conceived to be, but rather in a regular *succession of transformations.* For example, when chromic hydroxide, which is grayish-green, is dissolved in acid, a green solution results, which turns to greenish violet or pure violet if allowed to stand a long time. Exactly what the chemical changes are that correspond to these color changes I do not know, and probably the information which chemists have on the subject is not exhaustive. At any rate modern chemistry conceives the phenomenon to consist in a succession of reactions and transformations, the various colors and shades being attributes of the compounds that exist in the various stages along the way, and not as due to individual bodies carried by the preceding stages for the express and exclusive purpose of producing the particular colors that are observed, as would be implied in such a metaphysical scheme as was the germ-plasm theory elaborated by Weismann.

Enzymic chemical action presents perhaps a still better starting point for imagining what the fundamental hereditary processes may be than does ordinary chemical activity like that just instanced. The essence of this kind of activity is, as everybody knows, that in some way enzymes cause or at least facilitate *transformation* in other substances. Thus the attribute of solubility of the sugar into which starch is transformed, through the action of the salivary enzyme ptyalin, is not held to be due to a determiner for solubility carried by the ptyalin and passed on into the sugar, but rather it is recognized that solubility is one of the attributes possessed by the kind of sugar into which starch is converted by the ptyalin. The solubility is thought of rather as an attribute of the sugar and not as something once latent in the ptyalin which produced the sugar. A few details of the action of the enzyme in this case illustrate the point still better. Maltose, which is the chief if not the only sugar resulting from the action of ptyalin, is not reached by a single bound, as one might say, but through a series of bodies known as dextrins, at least three of which have been recognized. These are amylo- erythro- and achroö-dextrin, named from the color they display when treated with iodine, the first mentioned turning blue, the second red, and the third remaining colorless. What modern chemist would think of explaining the blue of the amylodextrin by a "determiner" for that color in the ptyalin or even in the starch, the red of the erythrodextrin by another determiner for red, and so on? That is the sort of explaining chemists of a century ago did, but they have long since learned not merely the futility but the scientific evil of such explanation.

If it were germane to our present task we might go on and show that the *gene* conception in modern genetics is really a revival in biology to-day of the *gene* conception which passed muster in chemistry a hundred years ago, when oxy*gen* and hydro*gen* were named. Such an exposition would be appropriate to a history of scientific theory or to a treatise on the theory of natural knowledge, but hardly to the present work.

Endorsement of E. B. Wilson's Proposal to Drop "Determiner" From the Vocabulary of Genetics

In his Croonian Lecture having the title *The Bearing of Cytological Research on Heredity,* E. B. Wilson said, "In the meantime it would be well to drop the term 'determiner' or 'determining factor' from the vocabulary of both cytology and genetics."[5] If the facts and arguments set forth in the preceding pages are valid, they constitute a demonstration that not only would it "be well to drop the term 'determiner,' " but that it must be dropped, at least in its present application, before thought and investigation on the mechanism of heredity can be free and in very deed truth-seeking. "What we really mean to say," Wilson continues, "is 'differential' or 'differential factor,' for it has become entirely clear that every so-called unit character is produced by the coöperation of a multitude of determining causes." So far as these statements go they are in strict accord with the organismal standpoint maintained in this volume, and we may also say, with the physical-chemistry standpoint.

Where attributes of adult organisms have been so definitely correlated with particular chromosomes and possibly parts of chromosomes of the germ-cells as seems to be the case in the fruit flies, such chromosomes are unquestionably differential, and since they stand at the very beginning of a long and complex transforming and developing series, they may very properly be called differential factors even though they do not themselves participate substantively in the transformation. The general similarity of their mode of action to that of enzymes is certainly considerable: a minute quantity of the substance is capable of inducing or facilitating the transformation of a large amount of other substance in a perfectly definite manner, and the inducing agent is not itself consumed.

Advantages of Conceiving Germ-Cell Chromosomes as Initiators in Hereditary Development

This chemical way of viewing chromatin and chromosomes sanctions the idea that these are to be regarded as initiators of developmental processes which lead to hereditary attributes, rather than determiners of those attributes. If one wants to know in what way this conception would have an advantage over the determiner conception as a working hypothesis, my reply is that the advantage is two-fold. First, it would surely correct the tendency of genetic research under the guidance of the determiner hypothesis, to restrict its attention to attributes of adults at one end of the ontogenic series and to the chromosomes of the germ-cells at the other end, and to ignore or touch only in the lightest way all the intervening parts of the series. This correction would result because the new standpoint would bring the whole series of continuities and transformations alike into proper perspective, revealing thus that the members of the series intervening between germ and adult must be investigated in exactly the same way and with the same objects in view as the end members, if complete understanding of the hereditary process be the goal of research. It could not then happen that the egg-cell would be represented, as it now so commonly is, as a relatively large sac containing nothing significant for heredity except the relatively small chromosomes. Nor could nearly the whole mass of ontogenic phenomena, especially those of histogenesis, be treated so lightly in speculation and so largely neglected in investigation as they have been under the domination of the determiner theory.

The second advantage in the initiator conception is that since it would recognize the "differential factor" of the chromosomes to be in reality due to the fact that the whole ontogenic series to which the chromosomes belong is differential, that is, that it pertains to a particular species or kind of organism, it would put an end to the notion by which recent genetic science has been so largely dominated, that the problem of how the series came to be thus specific or differential may be solved by speculation, and it would incite geneticists to efforts to solve the problem by observation aided by experiment. It is impossible to refute the charge that genetics is to-day more interested in an elaborate system of conceptions—of speculation, in other words—than it is in observed or possibly observable phenomena.

We cannot keep too constantly before our minds the fact of our almost complete ignorance of how any substance becomes hereditary substance whether through the "inheritance of acquired characters" or in any other way. Hence mere speculation on the subject after the manner of the pangens idea is much worse than nothing if permitted to run into a bewildering and enslaving system like that of the germplasm theory as it came from Weismann's mind. Nevertheless it is quite germane to the present discussion to point out that whatever might be the nature of the chemical action, whether enzymic or some other, through which the series of ontogenic transformations should be accomplished, the character and subtlety of these processes seem to make them, more than any others we know, competent with some modification to serve as the go-between for impressing the germinal material with the latent attributes of the species.

Inconclusiveness of the Cytological Evidence Usually Appealed to in Support of the Chromosome Theory

And this leads to the concluding statements of this discussion. The three categories of cytological fact which have been weightiest in the formation and maintenance of the chromosome theory of heredity are the individuality and continuity, chiefly numerical, of the chromosomes from parent to offspring; the apparent equality (it should never be forgotten that the dogma of equality does not rest on rigorous quantitative investigation) of the chromosomes in the male and female germ-cells; and the assumption that in the male germ-cells the chromosomes alone are concerned in fertilization. Even if these groups of assumed fact were established with absolute certainty they would fall far short of being direct and final proof that chromatin is the only hereditary substance. That this is true must be apparent to all well-informed, carefully thinking biologists. The most important grounds for this inconclusiveness are involved in the facts and arguments set forth in the preceding pages, but they may be summarized here, and in addition two other grounds may be pointed out.

First and foremost, in my opinion, is the general truth that chromosomes or even chromatic substance can not possibly be recognized as the sole bearers of hereditary substance, because the evidence is enormous in quantity and direct and indisputable in quality that other substances participate actively in the production of hereditary attributes. There is no way of escaping this conclusion except by narrowing the definition of heredity for the very purpose of bringing it within the scope of the chromosome theory of hereditary substance. The scope and fundamentality of this aspect of the problem is sufficiently dwelt upon, we may hope, in what has gone before.

The two additional grounds for skepticism as to the conclusiveness of the cytological evidence will now be pointed out. First, as to the evidence from the individuality and continuity of the chromosomes. All that any careful thinker claims or can claim for this evidence is that the individuality and continuity of chromosomes as observed are what might be expected if they were the germinal depository of the organism's hereditary attributes. And the question is constantly and naturally asked, what other meaning can the whole remarkable series of phenomena of maturation and fertilization have than that attributed to them by the chromosome theory? My rejoinder to this argument may begin with a reply to the question. As long as knowledge of the chemistry and physiology of cells—germ-cells with the rest—is as fragmentary and inconclusive as it now is, certainty as to the meaning of the phenomena mentioned is out of the question. However, it would seem quite probable that they are concerned primarily with the nutritive and assimilative processes of the cell and only derivatively with heredity. Furthermore, the question asked may well be paired off with another of similar character, namely, what is the meaning of the almost if not quite complete breaking up and disappearance of the chromosomes in the so-called resting stages of the immature germ-cells, this being accompanied by the dissolution of the nuclear membrane so as to allow the freest possible commingling of the whole nuclear contents with the cytoplasm of the cell? Have we not at least as much factual right to suppose that during this mixing of substances the chromatin, and so later the chromosomes, are influenced by the cytoplasm, as that the reverse influence takes place? Is it not entirely possible that this process is one of the very means or occasions of impressing the chromosomes with the attributes of the organism which, as we have seen, apparently must take place whether acquired characters are ever inherited or not?

And now as to the argument from the assumed exclusive participation of the chromosomes of the male germ-cell in fertilization. First of all, it can not be admitted for a moment that the chromosomes are proved to be as exclusively the fertilizing agents as they are generally assumed to be. Even in such extreme cases of seeming exclusiveness of chromosomal participation as that claimed by Strasburger for the pollen cells of some plants, neither Strasburger nor any one else has claimed, so far as I know, that all other male substances than chromatin are excluded as rigidly as would be required by experiments in a chemical laboratory designed to ascertain the action of a particular chemical element or substance in its purity. It is certain, for example, that in almost if not quite all male germinal elements in animals, a thin outer layer from the cytoplasmic part of the spermatid is present on the head of the spermatozoön. Furthermore, it is well known that at least the "intermediate piece" of the sperm tail, which is not usually regarded as chromosomal in origin, remains in the egg at fertilization. Nor is there any good ground for supposing that the non-chromatinic portions of the nucleus are absolutely excluded. The almost certain presence in the egg at fertilization of at least these male substances other than chromatin can by no means be regarded as insignificant for heredity, especially if the initiator conception of germinal material is held. It seems to follow of necessity that if the fertilizing substances, whatever their source, be conceived to act in an organic system of the physical chemistry sort after the manner of enzymes, no such quantitative relation subsists between these fertilizing substances and the products of organic growth as the chromosome theory implies; nor can their action be so narrowly localized in the egg. Their action would be conceived to involve the entire ovum *ab initio,* and not the chromosomes alone.

Summing Up of the Findings Against the Chromosome Theory

The general result of our critique is that the whole attempt to interpret the physical basis of heredity in accordance with elementalist conceptions has failed and must continue to fail, so far as its main aim is concerned. We are led to see that the germ-plasm dogma, no matter how often or how completely it changes its nomenclatural habiliments, as in the shifting from determin*ants* to determin*ers*, or from determiners to *gens*, or from gens to *factors*, involves a rejection of the conception that the germinal elements of organisms after being discharged are literally detached *parts of* those organisms. This conception was well on the road to incorporation into the great body of established biological truth when it was headed off by Weismann's diametrically opposed hypothesis of germinal isolation.

I would insist that the defense of the organismal conception in this volume is really a carrying out of such a conception of the organism and its germinal products as is implied by the old view that the germ is a part of the parent organism. It would hardly be possible to express more satisfactorily in a single sentence the most inclusive theorem, as it might be called, the demonstration of which is the aim of the part of this volume devoted to the means by which organisms propagate their kind, than the following from E. B. Wilson: "To the modern student the germ is, in Huxley's words, simply a detached living portion of the substance of a preëxisting living body carrying with it a definite structural organization characteristic of the species." [6]

Coupling this statement by Wilson with another from one of his latest writings,[5] to the effect that we ought to drop the term *determiner* because in reality what it means is *differential*, I call attention to the fact that the "differential factor" of the later statement and the "definite structural organization characteristic of the species" of the earlier statement are in essence one and the same. The only difference is that in the earlier statement it is the whole germ-cell that is recognized to be a detached part of the organism, while the later statement can be brought down to the chromosomes because of the greater refinement of knowledge attained since the earlier one was made. The point I wish to make stand forth with the greatest possible boldness is that the germ-cell chromosomes may properly enough be said to be differential, if only one never loses sight of the fact that they are differential in no other sense than are any other particles or substances of the germ-cells or any other cells which participate in the production of species-attributes.

Brief Reference to the Untoward Implications of the Germ-plasm Conception of Heredity

The somewhat laborious task of exhibiting the difference between conceiving the physical basis of heredity from the elementalist and from the organismalist standpoints may well be brought to a close by calling attention to the implication of the two conceptions as applied to heredity in man himself. Looked at from this direction the germ-plasm dogma is seen to be chargable with the grave offence of having added its weight to a conception of human life, the overcoming of which has been consciously or unconsciously man's aim throughout the whole vast drama of his hard, slow progress from lower to higher levels of civilization—the conception that his life is the result of forces against which his aspirations and efforts are impotent. As applied to man this form of fatalism is no less sure and no less dire in its tendencies than have been any of the innumerable theistic forms of fatalism that have prevailed through the centuries. It is almost certain that the ardor with which Eugenics has been espoused by several biologists is due in considerable measure to the fact that they have felt more or less definitely this sinister implication of the theory, and have turned to Eugenics as the only weapon against its evil forebodings. The germ-plasmic eugenist virtually says, "Yes, indeed is man a reasoning, willing, aspiring animal, but all his activities in these ways are futile so far as the race as a whole is concerned, except as they are brought to bear, extrinsically and operatively rather than organically, on the Germ-Plasm." This form of the Eugenic idea corresponds in spirit to the propitiative offerings of primitive religion. It aims to mollify by human agency powers that act upon men's lives, but which are in themselves largely extraneous, largely evil, and wholly irresponsible.

What eugenists of this school have failed to see, evidently, is that even were unit-factors as differentiate from one another in heredity as the extremest Mendelist conceives them to be, and that even were the germ-plasm improved up to the level of his highest hopes, his results in terms of actual human lives and social conditions would be distressingly meager. They would be so, because whether unit-factors exist independently in heredity or not, they certainly do not exist thus independently in development and function. In these ways they interact upon one another in the most vital manner, as physiology, especially of the internal secretions and the nervous system, and as physiological and social psychology are rapidly and conclusively demonstrating.

We thus end our examination of the means by which organisms produce others of their kind with the conclusion that the material through which reproduction is accomplished is in the most vital way part and parcel of the organism, that is, that the germ-cells are somehow stamped through and through, potentially, with the characteristics of the kind, or race, or species to which the producing organism belongs. And with this we are ready to pass to the examination of those integrative phenomena of the organism generally, one manifestation of which is this very nature of the germ-cells.

REFERENCE INDEX

1. Marshall 292
2. Hertwig, R. ('12) 75
3. King 232
4. Riddle 10
5. Wilson, E. B. ('14)..... 351
6. Wilson, E. B. ('00)..... 7

PART II

THE CONSTRUCTIVE SIDE OF THE ORGANISMAL CONCEPTION

Chapter XVII

GROWTH INTEGRATION

The Field to be Covered by the Constructive Discussion

ACCEPTING the inevitable destructive result of our critique of the elementalist standpoint, that the attempt to interpret living beings in the terms of their constituent parts alone always leads to partial failure and disappointment, or to the worse result of illusionment as to the trustworthiness of the explanations proposed; and accepting the constructive result that everything in the critical study tends to show that no part of any organism can be rightly interpreted except as part of an individual organism, this individual being in turn interpreted as a member of a taxonomic group, it is revealed that we are only on the threshold of the positive, the constructive side of our general enterprise. Even though the conclusion be unescapable that the living organism somehow acts causally on its parts, the problem still remains as to the modus operandi of that acting. The "somehow" which came to us as an incident of our critical study has yet to be inquired into.

Stated more specifically the task now before us is that of examining closely and systematically the interdependences among the parts of the individual organism. Although these interdependences are among the most obvious and general of all organic phenomena such an examination of them biology has not yet made systematically. Indeed—and here is one of the most vital things for us to see—a cardinal charge against the elementalist standpoint is that in its very nature it not only does not encourage, it actually stands against such examination. Its opposition to comprehensiveness and systematization is profound and essential.

Our examination will begin with a single brief, two-parted definition: The structural and functional interdependence found to exist among the parts of an organism we call *bio-integratedness;* and the process of moving on from grade to grade of interdependence among the differentiating parts which constitutes ontogenesis in the individual we call *bio-integration.*

Four Types of Bio-integration to Be Treated

In the present state of knowledge and for the discussion now before us four types or kinds of bio-integratedness and bio-integration may be recognized as pertaining to the individual organism:

1. *Growth integration,* most obvious in graded meristic series, but also expressed in the "axial gradients" of Child.

2. *Chemico-functional integration,* known so far chiefly in connection with internal secretions.

3. *Neural integration,* comprising the interdependences among the parts of the nervous system, and the involvement with this of the muscular, glandular and other organs.

4. *Psychic integration,* very closely connected with neural integration, but approached from the side of the totality of activities of living beings rather than from the side of nerve-organ activity, and so taking cognizance of a vast number of phenomena not yet definitely correlatable with neural phenomena.

The full presentation of facts and arguments under these four heads would reach far beyond the limits set for the present work. We are, consequently, obliged to restrict ourselves to a small portion of the best established and most compelling evidence under each head.

Graded Repetitive Series as Integrative Phenomena

This, perhaps the simplest form of integrational phenomena known to biology, is seen almost everywhere, but shows itself most typically and strikingly in plants and in many lower animals. Reference is made to the gradation in the repetitive or meristic parts appearing in so many organisms. The most obvious criterion of such gradation is the relative size of the parts, but, as we shall see later, there is considerable reason for supposing the gradation is not restricted to size. The few examples to which space can be given are selected to represent as wide a range as possible of the phenomena under consideration.

Illustrations from Animals

The lancelets, fish-like animals of the genus *Amphioxus*, may be noticed first (Figure 55). It will be observed that

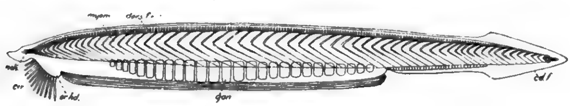

FIGURE 55—SIDE VIEW OF AMPHIOXUS (AFTER PARKER & HASWELL). nch., notochord. cir., cirri. or.hd., oral hood. myom., myomeres. dors.fr., dorsal fin rays. cd.f., caudal fin. gon., gonads

the creature tapers off toward both ends and that the series of metameres, *myom*, usually called myomeres because they compose the main body-musculature, diminish not only in dorso-ventral measurement from near the middle each way, but are also thickest in the mid-region and become thinner as they progress toward each end.

Something of this size scheme of body-parts is very common in the animal kingdom. Figure 56, a photograph of the skeleton of a python (see frontispiece), shows in a general way the size-relations of metameric skeletal parts in a higher vertebrate. Something of the extent to which the proportionality of parts of the individual metameres is carried out in this skeleton is shown by tabulating a series of measurements of the parts:

Position of vertebra	L.V.	T.V.	T.ex.zyg.	H.D.S.	W.D.S.	L.R.
5	6 mm.	5 mm.	11 mm.	15 mm.	4 mm.	25 mm.
50	11	16	23	11	8	48
100	13	17.5	28.5	9	7	79
150	13	16.5	26.5	7.5	7	82.5
200	11.5	15	22.5	5.5	7.5	69.5
250	10	10.5	16.5	5	5.5	52.5
300	6	5	7	4	2	0
327	4	3	4	2	2	0

"Position of vertebra" refers to the serial number, beginning with the head-end, of the vertebra measured. Legend: L.V., Length of vertebra, measured from the posterior edge of one dorsal spine to the anterior edge of the one next behind it. T.V., thickness of vertebral centrum in its thinnest part, i.e., near the middle. T. ex. zyg., thickness of vertebra at extreme of posterior zygopophyses (articulating processes). H.D.S., height of dorsal spine. W.D.S., width of dorsal spine. L.R., length of rib.

The starfishes are another class of animals which exhibit beautifully this size gradation of repeated parts, both their "tube-feet" and the calcareous skeletal supports being graded proportionately to the tapering arms of the animal. The following table presents a series of measurements of the two organ systems just mentioned, from a single arm of *Astrospecten californicus*. The dimensions are in millimeters, and the series proceed from the proximal to the distal end of the arm.

TABLE

Series number	Tube-feet Length	Ambulacral plates Length	Width	Thickness	Adambulacral plates Length	Width	Thickness
5	3.9	6.4	3.1	1.2	3.8	3.6	1.0
10	3.7	3.9	2.7	1.3	3.5	2.4	1.2
15	3.5	3.5	2.4	1.3	2.8	2.2	1.4
20	3.1	2.6	2.0	1.2	2.4	2.0	1.2
25	2.2	2.3	2.0	1.0	2.1	1.9	1.1
30	2.1	2.1	1.9	0.9	1.9	1.7	1.2
35	2.3	1.8	1.6	1.0	1.7	1.3	1.1
40	1.7	1.2	1.1	0.9	1.4	1.0	1.0
45	1.8	0.8	1.0	0.6	1.0	0.8	0.9
50	1.3	0.7	0.7	0.5	0.7	0.7	0.6
55	0.9						
60	0.8						
65	0.7						
68	0.5						

Not only do these graded meristic series appear in the individual makeup of a great range of animal species, but they occur in the colonies of many species in which aggregations are produced by budding. Sometimes, as in many alcyonaria, the size gradations are very obvious, while in other groups the distinctions are so small as to be discoverable only by close quantitative study. An example of this latter is furnished by the plumularian hydroids. A typical colony of the genus here studied, Torrey writes, "closely resembles a feather, of which the shaft is represented by the stem and the veins by the two ranks of alternating branchlets, or hydrocladia, corresponding to barbs. Each hydrocladium is divided by more or less definite nodes into internodes and bears on one aspect—the same in all hydrocladia—a compact series of hydranths, one to each internode." [1]

Without entering into the tabular and graphic details contained in this study, the author's summarized statement concerning one of the tables will suffice: "It will be seen from the table that, as the tip of the colony is approached, not only do the hydrocladia possess fewer and fewer hydrothecae, but the dimensions of the latter through the mesial nematophore reaches its minimum more and more rapidly. Since the hydrothecae, once formed, do not enlarge with age, it is clear that for such colonies as this, there is a limit of growth and a specific form."[1]

This correlation and proportionality among repetitive parts is frequently observed within the bounds of particular

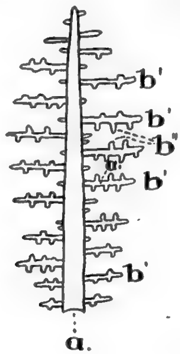

FIGURE 57—TENTACLE OF HALOCYNTHIA JOHNSONI (SCHEMATIC; AFTER RITTER).
a, axis. a′, axis of branch. b′, b″, primary and secondary branches.

organs, as for example, in the branching tentacles occurring in various groups. A detailed study of one of these cases was made by me some years ago on the tentacles of an ascidian. Figure (pl. 12, figure 13) of this study, supplemented by the following statement, illustrates the point. "Although this figure is diagrammatic in a way, it is accurate as to numbers of branches. The positions, too, of all the branches and length of the primaries were determined by micrometer measurements, and the secondaries were drawn as accurately as possible."[2] For the rest, the figure (Figure 57) tells its own story.

Illustrations from Plants

But it is in the plant world that these graded series of homonymous parts in individual organisms are most strikingly seen. It occurs in what is perhaps its most typical, least modified expression in the arrangement and size relation of parts in the leaves of many ferns and palms. But the compound leaves of innumerable flowering plants illustrate it very beautifully. Figures 58, 59, and 60 (*Acacia, Vicia* and *Cassia*) show three types of compound leaves based on the mode of gradation of the leaflets. These might

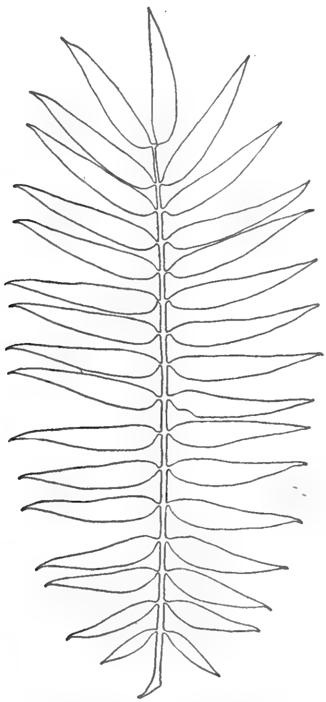

FIGURE 58. ACACIA ELATA.

be described as the bi-gradient, the direct gradient and the reverse gradient types, depending on whether the gradation is from the mid-region of the axis both ways (figure 58), from the proximal toward the distal end (figure 59), or from the distal toward the proximal end (figure 60).

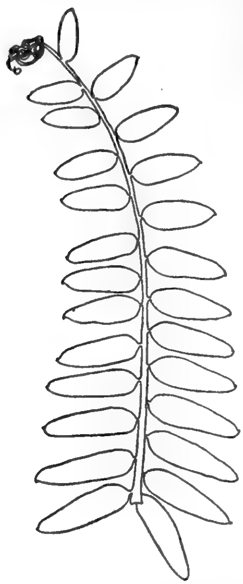

FIGURE 59. VICIA GIGANTEA.

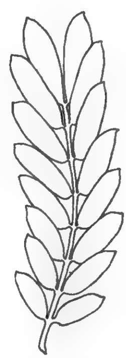

FIGURE 60. CASSIA SP.

Almost all simple leaves of seed plants show something of the same scheme. As examples, typical elliptical-entire leaves of the elm and poplar and such typical lobed leaves as those of most oaks may be pointed to.

Nearly every twig of a tree which represents a single annual growth impulse, in cases where the growing period is restricted to a small part of each year, presents a size gradient in the leaves distributed along the axis. A particularly striking illustration of this is furnished by the California coast redwood, *Sequoia sempervirens* (figure 61), where the new segment is short, is added end on to the one before it until a considerable succession of segments is produced, and where the leaves are retained for several years. That each segment in these leaves is an annual production is not certain, probably several segments being sometimes formed in a

FIGURE 61. SEQUOIA SEMPERVIRENS.

single season; but however that may be, that growth occurs in a series of impulses, each of which is sharply recorded in the size gradations of the repeated parts, is obvious enough.

It is a familiar fact, too, that in many plants a similar quantitative gradation of the reproductive parts along an axis occurs, but the extent to which this scheme pervades the constituents of the members of the series, even to the seeds, appears not to have attracted much interest on the part of botanists. To illustrate this point I present a single set of measurements, one of many which I have collected, of parts of the fructiferous organs of plants. These measurements are of *Frasera perryi*, an abundant annual in southern California and rather specially favorable for such a study in that the fruit stalk is single in each plant, stands up intact and rigid after it is fully ripe and dry, and is almost mathematically regular in the disposition of its parts. The table was compiled from measurements of a single plant, and three measurements pertaining to each seed vessel are given, namely, the length of the interval on the main axis between each two vessels, the length of the pedicels which bear the vessels, and the length of the vessels themselves. The measurements are all in millimeters. Several other dimensions might have been taken, which would almost certainly have produced similar results.

Series Number	Length of Internode	Length of Pedicel	Length of Capsule
1	27	36	21
2	19	29	18
3	19	32	18
4	22	34	16
5	23	29	16
6	19.5	28	17
7	18.5	25	17.5
8	17	25.5	17.5
9	19	25	18
10	18	25	16
11	18	21	16.5
12	15	21	16
13	15	22	13.5
14	15	22	14
15	15	21.5	13

That these gradations hold, at least in some plants, even to the seeds is certain as the following tabulation of the weight of seeds from different parts of the seeding axis of a wild mustard plant (*Brassica nigra*) shows. The figures were compiled from the weights of seeds taken from groups of twenty capsules from the bases, middle portions, and distal ends, respectively, of six such stalks. The weights given are in grams.

	Total weight	Number of Seeds	Av. Wt. per Seed
Base	.903	950	.00095
Middle	.694	733	.00088
Tip	.330	525	.00063

This mere glance at an exceedingly common phenomenon in living nature must suffice for the present.

Justification for Bringing All These Phenomena Under One Head

Probably about the first question that most persons would raise concerning what we have presented would be as to how far the series dealt with have anything to do with one another. Especially, we may apprehend, would most biologists question the justifiability of bringing together the meristic phenomena in animals and the repetition of parts in plants. If such a collocation of phenomena must be justified on the basis of known causal factors, then undoubtedly is justification impossible in the present state of knowledge. But justification of this sort is not called for by the point now occupying us. What concerns us at present is the quite formal fact that when any lot of homonymous objects fall into a quantitatively graded series the members of that series have a fixed relation to the series as a whole. They are not interchangeable with one another. Each is a function, mathematically speaking, of its set or series. Vertebra *m* of the python's skeleton, myotome *m* of the amphioxus body, tube-foot *m* of the starfish arm, branchlet *m* of the ascidian tentacle, leaflet *m* of the vetch leaf or of the redwood shoot, seed-vessel *m* of Frasera, seed-lot *m* of the mustard plant, and *m*, or any other member you choose from any other series whatsoever, is a determinate thing; it is what it is partly because of its position in the series regardless of whether the physical or other producing agent of the different series be the same or wholly different.

Even this purely structural formal basis establishes the fact of a measure of integratedness for all individual organisms in which the phenomenon appears. But to leave the subject at that would be superficial and unsatisfactory indeed. However, reflection makes it almost certain that there is some sort of causal basis for the phenomena. This conclusion follows first from the fact that the series result from the growth of the organism, and second from the certainty, at least in many cases, that the continuance of life of the individual involves the maintenance of the series, this in turn involving some measure of metabolic interdependence among the members of the series.

Attempted Causal Explanation of These Series

For establishing the general truth of this type of integration we need not, in strictness, go any further than we have gone. Nevertheless, the importance of the subject justifies a few remarks on attempts that have been made to explain the series causally. The best known of these comes from botanists, and conceives that the diminishing series of leaves and other structures, seen with more or less distinctness almost universally among plants, is due to the increasing remoteness of the successive parts from the roots of the plant, that is, from the main source of the plant's food. It is obvious, however, that this explanation is not of general application, since in animals the food does not come from a root system which anchors the organism to its food-yielding medium. Nor is it possible to bring the series in all animals into correlation with a blood circulatory system, as their existence in many coelenterates, hydroids and alcyonarians for example, where no circulation exists, shows. It is almost certain, too, that the series occur in many plants that have no sap system such as is assumed by the physiological explanation above indicated. Many of the marine algae come under this head, a striking example of which is the kelp *Macrocystis pyrifera* of the western coast of both Americas. That the laminæ of this plant fall into a beautiful direct gradient series is a fact which can not escape the notice of any one who sees them. The question of whether each streamer of laminæ reaches finally and necessarily a limit of growth in which the size series is present is not so certain, but from considerable attention to the question I am almost sure this is the case, although the point needs more study.

Another interesting and probably useful course of reasoning about organic growth attempts to connect the results of growth with autocatalytic chemical action. Although these attempts have not, so far as I am aware, taken special cognizance of the natural size series which are occupying us, but have been concerned with the weights or volumes of organisms at various stages of growth, there is little doubt that the phenomena we have been considering are closely connected with those dealt with in these attempts. T. Brailsford Robertson seems to have devoted more thought to this matter than any one else. The following, taken from the summary of conclusions found in his original paper, presents the most essential parts of his theory: "(1) In any particular cycle of growth of an organism or of a particular tissue or organ of an organism the maximum increase in volume or in weight in a unit of time occurs when the total growth due to the cycle is half completed. (2) Any particular cycle of growth obeys the formula $\log \frac{x}{A - x} = K(t - t_1)$, where x is the amount (in weight or volume) of growth which has been attained in time t, A is the total amount of growth attained during the cycle, K is a constant, and t_1 is the time at which growth is half completed." [3]

Assuming that the growth of an amphioxus, let us say, to adulthood represents a growth cycle of this statement, that the production of somites begins with the most anterior one and proceeds toward the tail, and that the successive growth-increments (corresponding to x in the formula) are registered in the somites as we find them, then the animal's body as exhibited by its myomeres would correspond fairly well to Robertson's statement under (1), as would several of the other growth series we have glanced at, and as would also great numbers of series presented by ordinary plants and animals.

The formula for growth contained in (2) is, according to Robertson, "such as would be expected to hold good were growth the expression of an autocatalytic chemical reaction." Assuming the general correctness of these statements, no one interested in the larger problems of organic growth could hesitate to believe that they must be important in some way.

However, that the relations shown do not prove that autocatalytic chemical activity is a cause of growth in anything more than a subordinate, contributory way, is obvious on reflection. In the first place, Robertson himself has pointed out, in substance, that such action says nothing about the particular shape which the mass of transformed substance takes, but since some characteristic configuration or shape is fundamental to all organic growth, the entities for which A and x stand in the formula are really only abstractions. Although the formula may apply approximately to a great many organisms, it will apply to none exactly, except by chance to a very occasional one. This is the general form of criticism, illustrations of which are seen in the fact that in the series of direct and inverse gradients shown in the vetch (figure 59) and *Cassia* (figure 60), respectively, the formula appears not to apply at all. The general purport of the strictures here placed upon the value of this explanation of growth is well brought out by Moeser, who says, probably with literal truthfulness, "One will not find two germinating plants (*Keimpflanzen*) which would have exactly the same growth curve, even though they proceed from seed of absolutely the same weight and grow under exactly the came conditions." [4]

The kernel of this criticism is that even though it should be established, as very likely it will be, that autocatalytic action is an essential factor in all growth, it can be a causal explanation in only a partial and subordinate sense. This is so because although K is a constant for a particular individual as observed, it assumes different values for different groups, partly at least because no account is taken of size or configuration. Moreover, even if these factors were considered, there would be nothing corresponding to a physical *constant* (depending only on autocatalytic action), since one of the most distinctive things about organic growth is that it is *differential*, the differentials corresponding to the taxonomic group to which the individuals belong.

Even though we are still much in the dark as to what the causal nexus is between the growth processes and the quantitatively graded series so widely seen in nature, it seems certain that some such nexus exists, and that its operation constitutes a true integrational factor in the individual. It appears, too, that this type of integration is about the simplest and that it accompanies the simplest type of differentiation, the two together constituting the simplest type of organization above the organization of the cell. But into this interesting subject we can not now go.

Axial Metabolic Gradients as Integrative Phenomena

For the most thorough and sustained experimental investigation of the primary integrative processes in growing organisms that has been made, biology is indebted to C. M. Child. In two recent volumes he has summed up and systematized the elaborate researches prosecuted by him in this field almost exclusively for fifteen years, and has presented his conclusions more fully than in any of his previous writings.

The limitation set for the present to the constructive part of our enterprise makes it impossible to do more than touch at a few points the great mass of experimental evidence on which Doctor Child bases his conclusions. Fortunately, however, the kernel of the conclusions can be stated rather clearly in a small space.

Although (surprisingly, it seems to me) Child refers hardly at all to the graded meristic series occurring in nature, to which the preceding pages have been devoted, it can hardly be doubted that the phenomena with which he deals, and calls "axial gradients," come under the same head as do those which have been occupying us. The phenomena which in the first instance Child has been concerned with, have been brought to light mainly through studies on regeneration in many lower animals. But the general conclusions reached are far broader than this; indeed they extend to well-nigh the whole scope of organic growth, but especially to growth which involves elongation either of the whole organism or of certain parts of organisms. Thus the head-tail type of individual, whether the body be segmented as in arthropods and in many worms, or unsegmented as in other worms and in molluscs, is perhaps the most striking exemplification of the axial gradations with which Child deals. The following quotation shows the generality with which he views the matter from the ontogenic side: "Gradients in rate of cell division, size of cells, condition or amount of protoplasm in the cells, rate of growth, and rate and sequence of differentiation are very characteristic features of both animal and plant development. Such gradients are definitely related to the axes of the individual or its parts, and are evidently expressions of axial metabolic gradients. While the existence of such gradients indicates the existence of gradients in activity of some sort, the various kinds of gradients are not all necessarily present where metabolic gradients exist. In some cases the visible gradient may be a gradient in rate of growth or in protoplasmic constitution; in still others a gradient in sequence of differentiation, etc., and sometimes metabolic gradients exist without any structural indications of their presence. At best these various kinds of gradients are merely general indications of differences in metabolic rate and undoubtedly in many cases the visible differences along an axis represent something more than differences in metabolic rate. The important point is that visible indications of graded differences in metabolic rate occur so generally in definite relations to the chief axes of the body.[5]

One phase of this general statement is the developmental correlation that various regions of the body in many lower animals have with the head or anterior end, these regions being developmentally dominated, in Child's expression, by the anterior end proportionally to the distance of the region from the end.

A typical case is furnished by flat-worms of the genus *Planaria,* animals especially favorable for experiments in regeneration, since they are very hardy to laboratory conditions and have great powers of reconstituting themselves from pieces of various sizes, shapes and positions cut from them. "Any piece of the planarian body," says Child, "is capable of giving rise to all parts posterior to its own level, whether a head is present or not, but no piece is capable of producing any part characteristic of more anterior levels than itself, unless a head begins to form first." [6]

From a great mass of experimental evidence produced by Child and others we have the following: "These facts force us to the conclusion that in such experimental reproductions there is a relation of dominance and subordination of parts. The apical or head region develops independently of other parts but controls or dominates their development, and in general any level of the body dominates more posterior or basal levels and is dominated by more anterior or apical levels." [7]

A really unique merit in Child's work is the fact that he has given special attention to the connection of these axial gradients manifesting themselves in various structural and functional ways, with the fundamental metabolism of the organism. Several methods of experimenting have been employed to this end, the one most frequently used being what he calls the susceptibility or survival-time method. The essence of this depends upon the fact, determined by many observers, "that a relation exists between the general metabolic condition of organisms, or their parts, and their susceptibility to a very large number of substances which act as poisons, i.e., which in one way or another make metabolism impossible, and that difference in susceptibility may be used with certain precautions and within certain limits as a means of distinguishing differences in metabolic condition, and more specifically, differences in metabolic rate." [8]

The demonstration of metabolic gradients by this method depends upon the fact that "death and disintegration of different parts of the body usually follow a regular sequence," this making it possible "to determine the time, not merely of disintegration of the whole animal, but of the various regions of the body." [9]

Another way of showing difference in rate of metabolism in different parts of the organism is by the use of the biometer, an apparatus for estimating minute quantities of carbon dioxide, recently devised by S. Tashiro in connection with his important researches on carbon dioxide production in nerves. By these methods it is shown, pointing to a single instance, that in pieces of a flat-worm isolated by cutting "the rate of metabolism is higher in long anterior pieces than in posterior pieces of the same length."[10]

Starting from this low but seemingly universal level of integrative phenomena in the individual, Child formulates views of the nature of organisms that agree very well with the organismal standpoint upheld in this volume. He writes: "The organic individual appears to be a unity of some sort. Its individuality consists primarily in this unity, and the process of individuation is the progress of integration of a mere aggregation into such a unity, for this unity is not simply the unity of a chance aggregation, but one of a very particular kind and highly constant character for each kind of individual. In all except the simplest individuals it determines a remarkable degree of uniformity and consistency, both in the special relations of parts and the order of their appearance in time, and also in coördination or harmony of functional relation to these parts after their development."[11]

Meristic Gradients and Metabolic Gradients Both Phenomena of Growth Integration

In view, then, of the exceedingly wide prevalence in living nature of axially disposed meristic series quantitively graded, and of the equally wide or even wider prevalence of axial gradients on the basis of metabolic activity, the gradients of both sorts arising as fundamental growth phenomena, it appears impossible to avoid recognizing our first category of integration, namely, *growth integration,* as about the most simple and primal and universal of all these categories, at least for multicellular organisms. It seems as though the other kinds of differentiation and integration are superposed, as one might express it, upon this primordial kind. To a consideration of the other, superimposed integrations we now pass, taking them again in their seeming order of obviousness.

REFERENCE INDEX

1. Torrey 139
2. Ritter ('09) 71
3. Robertson 612
4. Moeser 373
5. Child (2) 65
6. Child (1) 213
7. Child (1) 215
8. Child (1) 66
9. Child (1) 77
10. Child (1) 73
11. Child (2) 2

Chapter XVIII

CHEMICO-FUNCTIONAL INTEGRATION

Functional as Contrasted with Growth Integration

ASSUMING Child's theory of metabolic gradients to be well grounded, we are furnished thereby with one important insight into the chemical processes involved in the unity of the individual organism. But that process is concerned primarily with the *growth,* with the *production* of the individual. The question now is, are there chemical processes the object of which is to *maintain* the functional unity of the complete or nearly complete individual? Are there chemical operations the office of which is to preserve a proper interrelation among the parts of the organism as these perform their special offices?

That such is to some extent the significance of most if not all internal secretions as usually understood is indicated by the fact that the functional disturbances attending removal of the thyroids or other glands from various animals; and by the further fact that where internal secretions play a part in development, their action is rather that of stimulator, or at least modifier, than of true producer.

The conception of internal secretions as being at least as much regulators of physiological function as of growth is illustrated by cases of hypopituitarism of the post-adolescent type, like those described by Cushing, for example. In the series of cases of disease due to "pituitary deficiency" the first symptoms appeared when the subjects were from thirty to forty years old.

The Conception of "Internal Secretions"

The nature of the phenomena now to be considered, and their significance for our discussion make it desirable to think about these secretions from the broad standpoint first stated, according to Bayliss, by Brown-Sequard and d'Arsonval, namely, as materials produced by any living cells or tissues which are discharged into the blood or lymph and have specific effects on other parts or functions of the organism. Regarded thus it is now known that many cells of the organism produce internal secretions. Although we are more concerned with function than with structure in this discussion, our purpose will be best served by beginning with a morphological classification of the secretion-producing cells. They may be divided into two categories, those which are disposed into definite organs, or glands, the ductless glands of long standing in anatomy; and those which are not assembled in such organs. Knowledge of this second class of cells is of recent date, and is fuller from the functional than from the structural standpoint. The chief glands, to which the name *Endocrine* has lately been given by Schäfer, are now so well known as hardly to need mention. They are the thyroid apparatus, including the thyroids and the parathyroids, the suprarenal body, the pituitary body, and probably the thymus and pineal bodies. Cells now known to produce internal secretions but which are not arranged in glands are certain cells of the pancreas scattered among the pancreatic cells proper; certain cells of the alimentary mucous membrane; the interstitial cells of the ovary and of the testis, and probably certain cells of the placenta, of the mammary gland, and of the uterus.

Following our usual course of making the treatment merely illustrative rather than aiming at exhaustiveness, our selection will include one example from each of these groups. From the glandular category we take the thyroid apparatus, and from the non-glandular a portion of the alimentary mucous membrane, namely, that of the duodenum.

Effects of Removing the Human Thyroid for Curative Purposes

As definite knowledge of the great physiological importance of internal secretions begins with human surgery—with operations on the thyroid apparatus—we may well begin our study here. This is the better starting point in that there is no more striking illustration of how great a part of the whole organism may be implicated in the action of internal secretions than is afforded by the products of the thyroids and parathyroids.

The subject first came into clear light in the early eighties of the last century through the experiences of Swiss surgeons, Theodor Kocher and J. L. Reverdin especially, who removed the thyroids to cure goitre, this disease being specially prevalent in some parts of Switzerland. The patients operated on were found to improve rapidly for a time after the operation, but later untoward symptoms began to manifest themselves. Because the variety and pervasiveness of these symptoms in a typical case are highly instructive for us we present them in detail, selecting a description from *Human Physiology* by Luciani: "Patients who have undergone total thyroidectomy . . . experience the initial symptoms of glandular deficiency either at once or at latest some weeks after the operation. They feel weak, complain of heaviness of the limbs, and more or less diffuse dull pains, particularly in the legs, which may become acute and assume the character of pains in the bones.

"Other more serious symptoms are gradually associated with the preceding. After four or five months the face and the extremities swell and become cold, the muscles are torpid, sometimes rigid, often exhibiting muscular tremors, and are incapable of carrying out any delicate manual acts of precision. At first the swelling is variable; it is more pronounced in the morning than in the evening, but steadily increases until it becomes permanent. It is not ordinary *œdema*, in which percussion with the fingers leaves a depression; it is a hard and elastic swelling. It is specially localized in the hands, feet and face, where it produces a characteristic alteration of the countenance. The lower eyelids are the first to present a sacculated, semi-transparent swelling, which is hard to the touch; then the infiltration spreads to the folds of the face, which become smoothed out; to the nose, which gets rounded; to the lips which swell, and bulge outward, saliva dribbling from them. The features are coarsened and expressionless like those of a *cretin*.

"The mental functions accord with this appearance, since they are blunted, so that the patients lose their memory, become deaf, taciturn, melancholy, self-absorbed, and reply extremely slowly to questions. They further complain of slight but perpetual headache; feel an almost constant sensation of cold, which is most acute at the extremities; at times they are seized with vertigo, and may even lose consciousness.

"All the symptoms become still further aggravated. The whole body may grow more bulky from the extension of the swelling. The skin loses its elasticity, can only be picked up in large folds, and becomes dry owing to defective capacity for sweating. The epidermis desquamates in more or less extensive lamellæ, particularly on the hands and feet; the hair turns grey, falls out, and gets constantly thinner.

"The heart functions weakly, but with ordinary rhythm; the pulse is small and thready. Examination of the blood shows nothing constant; but there is often a more or less pronounced and progressive oligocythæmia, which undoubtedly contributes to the characteristic pallor of the skin, this being of the earthy, yellow-spotted hue peculiar to cretins.

"The respiratory rhythm is almost always normal; the digestive apparatus functions well, as also the urinary system. The spleen is not enlarged." [1]

This complex (*syndrome* in medical terminology) of manifestations is known technically as *cachexia thyreopriva*.

Later experience by other surgeons with the same operation discovered that in some cases the effects are much more acute and rapid, and may be replaced by what has been called "tetany" (though having little in common with ordinary tetanus), ending in death more often than otherwise.

Experimental Thyroid Excision in Normal Lower Animals

No sooner had the far-reaching influence of the thyroid for the human organism begun to be recognized in this way than experimentation on inferior mammals was invoked for further light on the subject. Moritz Schiff, the groundbreaker in this field, published in 1884 the results of the removal of the thyroid from a large number of dogs. In all cases where the whole thyroid apparatus was excised the dogs soon died after a run of such symptoms as tremor, spasms, and convulsions. Nor did Schiff rest content with merely ascertaining the effects of removal, complete and partial, of the thyroid apparatus. He found that these effects could be entirely prevented by grafting a portion of the gland under the skin or into the body cavity of the animal before the thyroid operation, or by injecting thyroid juice into the blood or lymphatic systems, or by feeding raw thyroid to the dogs. The story of how these experiments led to the now widely practiced treatment of myxœdema with thyroid or thyroid extract would be out of place here, though it should not be passed wholly unnoticed.

So an enormous mass of evidence, experimental, surgical and clinical, is now in court demonstrating that for some animals at least, among them being the human and the canine species, products of the thyroid apparatus are indispensable to the normal life, the symmetrical growth and balanced physiological activities of the organism. That the apparatus is essential to the "Hormonic Equilibrium" of the organism in some animals is beyond question.

While no pretense can be made at an exhaustive examination of this evidence two phases of our discussion make it desirable to cary the examination on the manifestational side a little farther. One of these is the importance of making as objective and emphatic as possible the extent of the manifestations in the individual; the other is the question of the generality, taxonomically speaking, of the thyroid apparatus.

In the interest of the first of these I present, verbatim, the report of a single case of complete thyroidectomy, the animal in this instance being a fox. The individual concerned was a female less than one year old.

"Oct. 28. Glands removed; good recovery.

"Oct. 29. Normal, but does not eat.

"Oct. 30. Salivation, rapid breathing, strong tremors and tetany from 7 A. M. to 2 P. M.; quiescent but weak during the rest of the afternoon.

"Nov. 1. Normal, but rather weak; eats; no sign of tremors or salivation during the day.

"Nov. 2. Restless; slight tremors; dyspnœa; does not eat.

"Nov. 3. Some depression, but no tremors or salivation until 4 P. M.; does not eat. At 4 P. M. spasms appeared and continued unabated as long as observed (7 P. M.).

"Nov. 4. Found dead at 4 A. M. Post-mortem examination revealed no parathyroids nor accessory thyroids." [2]

As to taxonomic range and character of manifestation of thyroid influence, much diversity might have been anticipated on general natural history grounds. As far as investigations have gone they realize these anticipations. A summary of results will serve our purpose, and this is at hand in Schäfer's volume already cited.

Concerning the effects of removal of the thyroid apparatus he says:

"The most acute symptoms are exhibited by carnivora such as dogs, cats, foxes, and wolves (Vincent), and the young of herbivora (v. Eiselsberg, Sutherland Simpson) and are of a nervous nature. . . . Some species exhibit no symptoms whatever—at least when the operation is performed on the adult. Horsley states that this is the case with birds and rabbits; but according to Gley, the latter are affected if care is taken to find and remove all four parathyroids, and Doyon and Jouty obtained typical tetany in hens which had been parathyroidectomized. . . . According to Vincent and Jolly badgers are totally unaffected by complete removal of both thyroids and parathyroids." [3]

From the anatomical characteristics of the organs, and from the known effectiveness of minute portions of them, such statements as the last must be taken with reserve. Although these results show by their diversity that an enormous amount of study remains to be done on the comparative side, they leave no question that the secretion of the thyroid apparatus is important for the general health and equilibrium of most animals in which it occurs. The measure of this importance in the eyes of some authorities is seen in such a statement as, "No cell anywhere in the body can reach morphological perfection without thyroid stimulus." [4]

The Internal Secretion of the Duodenal Mucous Membrane

We now pass to an examination of the effects of the internal secretion of the duodenal mucous membrane. This particular secretion is selected for the reasons that it is, according to Bayliss, one of its discoverers, "the most typical of all the chemical messengers"; that it was one of the first to be investigated; and that it is one of the few which have been isolated as definite substances.

The mode of operation of this secretion is tersely stated by Bayliss: "Food entering the duodenum causes the production of a special substance which enters the blood and excites the pancreas to pour into the duodenum a digestive juice." [5]

That the presence of various substances, especially acids, in the duodenum, induces a flow of pancreatic juice was known when Bayliss and Starling began their work in this field; but up to that time the excitation of the pancreas to such action was supposed to be through a nerve reflex. These investigators had reported in 1902 [6] that acid in the duodenum is able to cause the pancreas to secrete after nervous communication between the intestinal wall and the pancreas is excluded. They went further and obtained an extract from the duodenal mucous membrane which, being injected into a vein, induced a copious flow of pancreatic juice. The substance, whatever it is, which acts thus they call *secretin.* It has been surmised by a few physiologists that the effects are not due to the direct action of the secretion on the pancreatic gland-cells, but that the influence is exerted through the vaso-dilator mechanism. But this surmise is negatived by the demonstration that the secretin will induce the flow of pancreatic juice while it does not alter the blood pressure. The case seems, then, fully established, and is so clear-cut and relatively simple an instance of the coördinated functioning of two wholly distinct parts of the body through chemical means, that it is desirable to get sharply before us the known steps in the process. In the course of normal digestion, food acidulated in the stomach passes into the duodenum. Here, probably in virtue of its acidity, it acts upon the cells of the mucous membrane in such a way as to induce them to produce a substance which is discharged, not into the intestines, there to take its part in digestion, but into the blood. By the blood stream the substance is carried through its whole circuit, hence through the lungs and so on, around to the pancreas, the typical gland-cells of which it excites into activity, so that the pancreatic juice, an "external" instead of an "internal" secretion, is poured into the duodenum to exercise its digestive office on the same food which started the cycle of activities.

It was with this substance particularly before their minds that the authors adopted the name *hormone* to designate substances which act thus. "The group of substances referred to," says Bayliss, "which includes adrenaline and the various internal secretions, is characterized by the property of serving as chemical *messengers,* by which the activity of certain organs is coördinated with that of others. They enable a chemical correlation of the functions of the organism to be brought about through the blood, side by side with that which is the function of the nervous system." [7]

This reference to the side-by-side activities of chemical messengers and nervous system in integrating the organism touches a subject of the utmost importance. Consideration of it must, however, be deferred until we have looked a little more into the nature of hormones.

The Nature of the Active Substances in Internal Secretions

That the peculiar iodine-rich albuminous substance obtained from the thyroid by Baumann in 1895 and since observed by other investigators, contributes in some essential way to the action of the secretion of this gland is the belief of apparently a large majority of authors (Bayliss, Eppinger, Howell, etc.), but not of all (Luciani). In view of the uncertainty on the point Schäfer's proposal "to express our ignorance by a term which implies no theory" may well be accepted, with the proviso that the term proposed be really taken as evidence that *something* though not *everything* is known about the substance. A part of the proposal is worth quoting. "I propose therefore provisionally to apply the word *thyrine* to denote the active principle, whether it be identical with or contained in the iodothyrin of Baumann or not."[8]

Thyrine then becomes the name of a substance the *source* and *some of the activities* of which are known, but whose main physical and chemical attributes are unknown.

Concerning the mode of action of thyrine there are several divergent views, all based on some evidence and so perhaps not entirely antagonistic. Is the antitoxic theory of Luciani[9] partly right, right as regards the parathyroid secretion (Moussu, Vassale and Generali), and partly wrong, wrong as to the secretion of the thyroid proper, this being *trophic* rather than antitoxic?[10] May there not be more in the enzymic theory suggested some years ago, than later writings have been inclined to favor? Does the fact that internal secretions seem to be simpler than enzymes, as indicated by their greater resistance to heat, preclude the possibility that their normal mode of action is of the enzyme type after all? That is, may it not be necessary to extend the conception of enzymic action (which is surely generic anyway) to include the various sorts of activity presented by hormones, understood in the sense given it by its originators?

But neither can the resemblance of internal secretions to drugs, so far as their action is concerned, be overlooked. This has been dwelt upon by Schäfer[11] and has important bearings on the problems of the origin as well as on the chemical nature of the substances.

Another aspect of the mode of action of internal secretions is that of whether the effects are to stimulate or inhibit the activity of the organ or tissue on which they operate. Schäfer and others make a special point of this, directing the attention to the fact, by way of illustration, that the adrenaline of the suprarenal medulla causes contraction of the plain muscle of the blood vessels and inhibition of that of the intestines.[12] The distinction has an undoubted natural grounding, and so is in the interest of accurate description and clear conception.

As to the actual chemical composition of internal secretions, knowledge is exceedingly meager. More is known about adrenaline, the active principle of the suprarenal gland, than about that of the secretion of any other gland or tissue. This was isolated by the Japanese chemist Jokichi Takamine in 1901, and has since been more fully examined by several investigators, notably by T. B. Aldrich. It is described as a micro-crystalline substance occurring in at least five crystal forms. Aldrich assigns to it the empirical formula $C_9H_{13}NO_3$, this structure placing it not far from tyrosin in the benzene or aromatic series. Of special interest is the astonishingly minute quantites which produce physiological effects. According to Aldrich 0.000001 gram of an aqueous solution of the chloride per kilo of body weight injected into the blood system raises the blood pressure 14 mm. of mercury.[13]

The chemical nature of *Tethelin*, lately isolated from the anterior lobe of the pituitary of the ox, has been studied by its discoverer, Robertson. It is described as white or pale cream colored, readily powdered, highly deliquescent, and having a greasy odor and slightly acid reaction in aqueous solutions. It contains 1.4 per cent of phosphorus and four atoms of nitrogen for every atom of phosphorus. The phosphorus-nitrogen content of the substance is considered by Robertson as specially significant, since this seems to ally it chemically with "phytin," a substance found in the rapidly growing parts of plants, and in milk. The natural suggestion is that the growth-promoting substances in plants, milk, and the pituitary secretion are chemically related.

The Close Resemblances and Interrelations of the Different Internal Secretions

Even the meager, merely illustrative examination of internal secretions we have been able to make brings out the close resemblance there is between the several endocrine glands, and also between the physiological effects of the various secretions. These resemblances suggest an intimate organic interrelationship among all the internal secretion-producing parts of the body.

All investigators in this field, no matter to how restricted a section of it their efforts are primarily directed, seem to come upon the interdependence of the sources and activities of hormones. To illustrate, Cushing, whose central interest has been the hyphophysis, is led to conclude that experimentally induced hyphophyseal deficiency works histological changes in many if not all the other ductless glands. It is not surprising, consequently that far-reaching theories have been elaborated on the basis of these relationships.

Bayliss refers with approval to Elliott for the conservatism with which he sums up the present state of knowledge on this aspect of the general subject. But even so, features are pointed out "which suggest a common bond":

"(1) Carbohydrate metabolism is influenced, not only by the pancreas, but also by the thyroid in super-activity, in acromegaly, and by the injection of adrenaline.

"(2) Growth is affected by the testis and the cortex of the suprarenals, arrested by the absence of the thyroid.

"(3) Nervous implications.

"(4) The pituitary becomes hypertrophied when the thyroid is removed. Acromegaly may lead to enlargement of the thyroid." [14]

At the other extreme of what may be regarded as legitimate scientific theorizing, we have the views of Sajous, who believes research will finally demonstrate a relationship between all the ductless glands the combined functioning of which dominates most of the activities, normal and pathological, of the organism.

Sajous' elaborately worked-out theories of internal secretions, especially in their relation to disease and medical practice, are opposed at many points to prevailing opinion based on present day research. Nevertheless regarded from the standpoint of general biology there would seem to be much merit in his effort on the one hand to find a common ground in the metabolic processes for all the phenomena attributed to endocrinal activity; and on the other hand to find a more consistent morphological and physiological basis of definition and classification of internal secretions and the structures which produce them than has yet been recognized. For example, whether Sajous is right or not in contending that the pituitary body does not produce an internal secretion, certain it is that the non-glandular structure of its posterior part, extracts of which alone have slowing effects on the heart, is strongly suggestive to the critical naturalist that the inclusion without qualification of this part at least of the organ among the endocrine glands is an instance of what is known to taxonomists as "lumping" in classification —a kind of practice that advance in knowledge always finds to be inadequate for purposes that are critical. Sajous' late summary of his views is highly suggestive to the general biologist, even though it is excessively theoretical in some parts.

As far as a much interested outsider can judge, the present state of understanding of the relationships among the internal secretions is set forth with exceptional judiciousness by Waller. "There can be little doubt," the author opens his discussion by remarking, "that the various internal secretions are most closely correlated, yet perhaps the most difficult, and also the most fascinating problem of present day medicine, is to assign to each its proper and right share of importance."[15] This statement, coupled with the fact that one of the main objects of the discussion is to display the many contradictions which the author's large experience as a practitioner has found in the action of the internal secretions, is about the most striking, and from our standpoint most significant thing about this paper.

As one illustration of the agreement of action of the secretions, or at least of the influence of the endocrine glands, it is pointed out that changes in calcium metabolism have been observed after removal of the thymus; in disease of the pituitary and of the pineal bodies; after castration; after ovariotomy; after removal of the suprarenals, and after removal of the thyroids and parathyroids.

Of the numerous instances of contradiction which he brings out, we mention only that concerning tetany. This he shows may result from either removal of the thyroid or from an overdose of thyroid extract. The explanation of the contradictions in the action of a given secretion favored by the author is that of the "varying influence of the other internal secretions." But the descriptions given seem to leave no doubt that difference in type of individuals also comes into the explanation. Thus among children afflicted with enlarged tonsils and adenoids, the two distinct types dependent upon the character of the symptoms, is a case in point. One type is dull and stupid, stunted in growth, has dry coarse skin, and may display symptoms of rickets. The other type is vivacious physically and mentally, given to peevishness, irritability and quick fatigue, and always wanting a change of activity. This type is over-tall for its age, perspires readily and is fine-skinned. Both types of cases are benefited at least for a time, Waller says, by treatment with thyroid extract.

Concerning the general nature of the interdependence among internal secretions, this author's views seem to me so eminently sound that I cannot refrain from quoting them in some fullness: "Considerable stress has been laid upon the antagonism of different internal secretions by various authors. I believe we should gain a truer insight into their working if we dwelt rather upon their harmony. It does not strike me as a very high conception of the human organism that health should consist in the balance of dissentient or antagonistic forces. It would seem far more ideal that all the internal secretions should work together for the common good of the organism, and that when some special demand is made upon a particular gland the others will work in harmony with it. Every gland is probably necessary for the perfect activity of the rest, and the harmony between the glands is demonstrated by physiological experiments. . . . When it is found that the removal of an organ constantly induces either atrophy or hypertrophy of some other organ, we can reasonably deduce that in the first case the organ removed is essential to the welfare of the one that atrophies in its absence, and in the second case that the hypertrophying organ is endeavoring to replace the lost one, in some degree, and that therefore the two organs have a kindred function."[16]

As an example of the first case, the fact is cited that the removal of the thyroid or of the anterior part of the pituitary induces the atrophy of the testicles or the ovaries. The second case is illustrated by the hypertrophy of the suprarenal from the removal of the thyroids, and also by the hypertrophy of either the thyroid or the pituitary on removal of the other. "The demonstrated facts of hypertrophy," we read, "clearly point to an *entente* or even a triple alliance between thyroid, hypophysis and suprarenals. And the genital system is absolutely dependent upon the integrity of these three."[17]

Stating now, in a single paragraph, the results of investigations in this field, we have: The different parts and activities of the organism are maintained in their normal state, both as to the essential nature of each, considered individually, and as to their relation with one another, by a number of exceedingly powerful and subtle chemical substances (internal secretions) which are produced by certain parts, are passed into the blood, and by it are carried about over the whole organism to exert their appropriate influences on other parts and functions. Because of the peculiar way these substances do their work, they have been called *chemical messengers*, or to have a distinctive name, *hormones*.

Relation Between the Internal Secretory and Nervous Systems

But no physiological truth is better known than that one of the main offices of the nervous system is to correlate the organs and parts of the body with one another. It is but natural to suppose, therefore, that if there is a chemical scheme for accomplishing the same end, the two are in some way closely related.

That the relations which exist between the cerebro-spinal nervous system, the autonomic nervous system (including the sympathetic), and the internal secretions, constitute one of the most important subjects in the whole physiological domain, at the same time that it is one of the most recondite and difficult to investigate, has come gradually to view through the work of the last few decades. We will try to extract enough from the mass that has been written on the subject, to illustrate the principles involved. The modern period of knowledge of what was formerly but rather indefinitely included under the term sympathetic nervous system, has revealed that we have to do with a portion of the general nervous mechanism which in reality is a subdivision of a larger category.

Composition and Nature of the Autonomic System

The name autonomic was given to this category by Langley. "The autonomic nervous system," he says, "means the nervous system of the glands and of the involuntary muscles; it governs the 'organic' functions of the body"; and further: "The word implies a certain degree of independent action, but exercised under the control of a higher power."[18] Bayliss adds: "It is necessary to be quite clear that the autonomic system *includes* the sympathetic, since some writers abroad use the name as applying to all the visceral nervous system *other than* the sympathetic, speaking of sympathetic and autonomic."[19]

Perhaps the most important single fact which differentiates the autonomic from the cerebro-spinal system is the intercalation, everywhere in the autonomic system, of an extra neurone between the cerebro-spinal nerve and the part innervated. Cannon states this distinction very clearly: "The skeletal muscles receive their nerve supply direct from the central nervous system, i. e., the nerve fibers distributed to these muscles are parts of the neurones whose cell-bodies lie within the brain or spinal cord. The glands and smooth muscles of the viscera, on the contrary, are, so far as is now known, never innervated directly from the central nervous system. The neurones reaching out from the brain or spinal cord never come into immediate relation with the gland or smooth muscle cells; there are always interposed between the cerebrospinal neurones and the visceral extra neurones whose bodies lie wholly outside the central nervous system."[20]

Cannon's suggestion that these interposed neurones may function as transformers for impulses received from the cerebrospinal system should be noted here.

Three sharp subdivisions of the autonomic nervous system are recognizable. One is known as the vagal or cranial autonomic, because it is largely made up of fibers from the vagus, or tenth pair of cranial nerves. Another is the sympathetic, or better, the thoracico-lumbar autonomic, because its fibers originate from the great visceral sympathetic ganglia. This is by far the most extensive of the three subdivisions, and is the only one that is distributed to all parts of the body. The third is the sacral autonomic. As its name implies, it is quite restricted in extent, its fibers being distributed to the extreme distal end of the intestine, the urinary bladder, and some of the external genital organs. But the differences between the three which are most important for us are physiological, a particularly important difference being that the thoracico-lumbar division acts antagonistically to both the end divisions. Stimulation of the fibers of the sympathetic has just the opposite effect to the same stimulus applied to the fibers of the others. "The sympathetic fibers check, the vagal autonomic fibers excite, the movements of the intestines; the sympathetic dilates, the vagal autonomic contracts, the pupil; the sympathetic hastens, the vagal autonomic slows, the heart."[21] The sacral contracts the lower part of the large intestine and relaxes the outlet of the bladder, while the sympathetic relaxes the same part of the intestine and contracts the same part of the bladder. Cannon states the general principle thus: "*When the mid-part meets either end part in any viscus their effects are antagonistic.*"[22]

While the incompleteness of knowledge in this field needs emphasizing, yet that knowledge is sufficient to put some of the main features beyond question, and to make clear the great importance of the subject and of fuller knowledge on it. Touching these general aspects Professor L. F. Barker writes: "While we do not yet understand the exact mechanisms of association among the activities of the cerebrum, the endocrine glands, and the reciprocally antagonistic autonomic domains and their end-organs, we can begin to see the paths which must be followed in order that more exact knowledge may be gained." [23]

Experimental Evidence of Connection Between the Adrenal Glands and the Nervous System

Some of the most important information we have in this field is furnished by Cannon and his collaborators concerning the secretion of the adrenals and its relation to the autonomic and central nervous systems. It had been proved before Cannon began his investigations that adrenin injected into the blood has exactly the same effect on certain parts of the organism as does the sympathetic autonomic nerves with which the same parts are supplied, and that the effect of the secretion is direct and not through the nerves. In other words, it had been proved that the organism has two methods by which the same activity of certain of its parts can be induced, one nervous, the other chemical. Thus the dilation of the pupils, the erection of hairs, the inhibition of activities of the alimentary canal, and the liberation of sugar from the liver can be induced either through sympathetic autonomic centers or by the secretion of the adrenal bodies. This in itself was important evidence of interrelation between the nervous system and internal secretory system. But the experimental researches prosecuted in Cannon's laboratory have proved that a connection exists between the autonomic-adrenal phenomena and the cerebrospinal system through the sensory nerves, and with the psychic life of the animal; and have shown the probable significance of the entire scheme for the life of the organism as a whole.

To be a little more specific, they have proved:

(1) That strong excitation of sensory nerves stimulates reflexly the adrenal glands and causes them to pour an increased amount of adrenin into the blood.

(2) That emotional excitement (as the fright of a cat by a dog) similarly increases the flow of adrenin.

(3) That this increase of adrenin in the blood may increase the liberation of sugar from the liver into the blood to such an extent as to make sugar appear in the urine, thus demonstrating a true "emotional glycosuria."

(4) That the increased adrenin of the blood thus produced is probably advantageous to the organism in that it enhances its ability to meet special stresses that naturally accompany special excitement, as of fear, anger, or pain, this advantage consisting partly in augmentation of the working energy of the muscles, probably through the sugar delivered to them, and in increasing the coagulability of the blood, thereby reducing the danger from bleeding wounds.

Summing up his conclusions as to utility, Cannon writes: "These changes in the body are, each one of them, *directly serviceable in making the organism more efficient in the struggle which fear or rage or pain may involve;* for fear and rage are organic preparations for action, and pain is the most powerful known stimulus to supreme exertion. The organism which with the aid of increased adrenal secretion can best muster its energies, can best call forth sugar to supply the laboring muscles, can best lessen fatigue, and can best send blood to the parts essential in the run or the fight for life, is most likely to survive." [24]

But fear and rage are, one hardly need be reminded, in part psychic phenomena, and hence inseparably connected with the higher centers of the cerebrospinal nervous system. Though in the main reflex and automatic, they are nevertheless to some extent subject in man to intelligent control. Thus the way is open for a measure of rational understanding of the structural-functional means by which human beings "tap," as William James would say, and bring under direction those remarkable "reservoirs" of ordinarily unused energy about which everybody knows something from his own experience, and upon which nobody has written more intelligently than James.

An excellent beginning has been made, then, in the experimental demonstration of the integration of the endocrinal and common glandular systems, the blood circulatory system, the autonomic and cerebrospinal nervous systems, and the emotional-psychic life of animals.

Clinical Evidence of Adrenal-Nervous Connection

But important knowledge and general views in this field are also coming from clinical medicine, and pharmacology. A general presentation of the results reached down to 1913 is contained in *Innere Sekretion und Nervensystem*, by H. Eppinger and others. A particularly significant body of evidence coming from this source concerns the relation between the sympathetic or middle autonomic nervous apparatus and the two end autonomic systems, the cranial and sacral. These two groups act, it will be recalled, antagonistically to each other. Eppinger and others have shown that the thoracico-lumbar, or sympathetic, and cranial, or vagal systems differ in susceptibility to stimuli in different individuals, and perhaps in the same individual at different times, thus making the two groups what is called *sympatheticotonic* and *vagotonic* with reference to each other, depending on whether the sympathetic or the vagal is the more susceptible to stimuli. This difference can be demonstrated by the administration of various drugs, as adrenin and pilocarpin. But it is known, according to Eppinger, that the thyroid toxin stimulates both the sympathetic and the vagal. From this it results that over-stimulus of either may occur through this source, and go to the extent of producing the characteristic symptoms of Basedow's or Graves's disease (rapid heart beat, exophthalmia, diarrhea, etc.). These symptoms may occur in varying degree, depending on whether the patient is sympathetico- or vagotonic, the sympatheticotonic type of the disease being characterized by marked protrusion of the eyeballs, especially rapid heart beat, absence of sweats, diarrhea, and disturbance of the respiration; while the vagotonic type is characterized by slight protrusion of the eyes and increase of heart action, by outbreaks of sweat, diarrhea, and by faultiness in the respiratory rhythm.

While some observers, like Falta, do not believe the facts now known can be definitely classed in this manner, the effort, justified by some positive knowledge, has at least the merit of specifying to some extent the intricate reciprocal action between the thyroid apparatus and the nervous system, and also between the different portions of the autonomic system; and to this extent all students of the subject bear witness. Thus Falta: "In my opinion everything speaks for the fact that in Basedow's disease the *entire nervous system* is in a condition of over-excitement and that the pictures presented by the vegetative nervous system are as uncommonly manifold and always changing." [25]

The indication of prime importance in this is that in these antagonistic divisions of the autonomic nervous and endocrinal glandular systems, operating together with the other portions of the organism, there is a balancing-off or equilibrating apparatus through which the whole complex of vegetative functions is carried on, all of which in turn are connected with the psychic functions. Probably no better illustration can be found of the conception of the organism as fundamentally dynamic. According to this conception normality, both in function and in structure, consists not in rigid, invariable activities and organs, but in a ceaseless play of constitutively antagonistic forces and structures. By this conception the whole life of the organism, physical and psychical, may be crudely likened to the performance of the tight-rope walker, which depends on numberless balancing activities. Let the performer be really motionless in every part for one instant, and he falls.

The treatment of tetany and its relation to internal secretions, especially to that of the thyroid, by Eduard Phleps in the work now under consideration, is another excellent illustration of how interpretation may run in accordance with this conception of the animal organism. The essence of the section, as touching this question, is contained in the following:

"On the ground of clinical symptoms authoritative clinicians like Eulenburg, Kahler and Nothnagel explained tetanus as a disease of the entire nervous system. Later it was proved that this disease was not due to primary organic changes of the nervous system, but to secondary functional disorders. . . . The view of those authors, who refer the disease to the simple effect of a substance of the epithelial granules acting normally and continuously on the whole nervous system, finds here a further development of that old theory because for them the clinical picture of a regular grouping of nervous stimulus- and response-phenomena arises from an impairment of the close functional relation between the nervous system and the epithelial granules (MacCallum, Chvostek jun., Biedl, Eppinger, Falta, Rudinger, Jonas, et al.). In agreement with these authors we conceive the action of the epithelio-secretive substance as that of a hormone in the sense of Starling and Bayliss, which must have its essential point of attack on certain reflex stations of the central nervous system."[26]

Taking cognizance, now, of the fact that most if not all the cells known to produce internal secretions arise embryonically from epithelium, as does also almost all nervous tissue, we have the suggestion of a deep-seated combination scheme, chemical-and-nervous, for integrating the organism. The best investigated example of what is here referred to is the suprarenal bodies. It is now fully established that the inner or medullary portion of the organ, the part which produces the adrenin, is developed from the same neuroblastic mass out of which the sympathetic autonomic ganglia arise. Furthermore, it seems beyond question that the so-called chromophil, or sometimes the adrenin granules in the chief cells of the functioning gland play a fundamental rôle. Combining these facts with the equally well ascertained facts that the cortex of the gland is derived from the same embryonic mesoblastic mass which gives rise to the genital glands; and that in adults cortical changes in the suprarenals are intimately correlated with reproductive changes, and a general view of the factual basis on which the suggestion rests is before us. We may, I think, regard the suggestion not only as justified but as revolutionary in comparison with any theory that was scientifically justifiable until recently. The following further quotation from Phleps brings the idea into still clearer view:

"The unqualified dependence of the nervous system on the epithelial bodies (this last used in the sense of general physiological considerations, and only by way of illustration), and the absolutely vital significance of this enables us to see many things in a new light. We learn that many motor, sensory and vasomotor-trophic functions of the central nervous system even up to the highest reflex stations having the most complicated cortical functions, are in constant functional coöperation with organs which heretofore have not received sufficient consideration from this standpoint. The results compel a change of our views concerning the constantly dominating position of the nervous system. *We may see in many of its activities only the most manifold intermediary rôles between glandular functions which are in the closest relation to metabolism, and the sum total of all reactions which follow external stimuli.*" [27]

Summary of Present State of Knowledge In This Field

Speaking generally, we may say that the trend of all results, experimental and clinical, is unquestionably toward a demonstration of the closest interaction between the entire internal secretory system and both the autonomic and cerebrospinal nervous systems, this interaction affecting the whole of both the growth and the functioning of the animal organism.

But we must remind ourselves again how fragmentary knowledge is in this great realm. Unanswered questions meet one on the very threshold of any portion he enters. By way of illustration, take the phenomenon of abnormal growth known as acromegaly. This malady is characterized, as the name indicates, by a "peculiar non-congenital hypertrophy of the upper and lower extremities and of the head." [28] Such, according to Schäfer, is the definition given by Pierre Marie, who first fully described the disease. The main visible symptoms consist in the enlargement of the bones of the head, hands, feet, chest, etc., especially in their terminal portions. Through such growth the nose and lower jaw, especially the chin, become protrudent. But the whole skeleton is more or less affected, and there is a corresponding over-development of the muscles, the affected person becoming abnormally strong. That acromegaly is constantly associated with an abnormal condition of the hypophysis is recognized by apparently all authorities. Whether the abnormality of the gland is a cause or only an accompaniment of the disease is an open question in the minds of some. However, the view of a large majority is that such a causal relation does exist. "That the acromegalic skeletal growth," says Schäfer, "is produced by hypertrophy and oversecretion (or perverted secretion) of the anterior lobe is highly probable, both as the result of partial extirpation in animals and from the effect of operative removal of the pituitary tumours in man." [29] The particular issue here, it will be noticed, is the vitally important one of what might be called the functional as distinguished from the hereditary cause or at least incitement of growth.

In illustration of the importance of understanding the unification among these complex systems, the manifestation of which is in turn dependent upon the organism as a whole, the following from the address by L. F. Barker already referred to, is impressive.

"In how far these sudden and violent excitations of the autonomic nervous system which accompany strong emotions are due to the intervention of the glands of internal secretion, and in how far they depend upon direct neural conduction from the brain, we are as yet but ill-informed. I need only remind you of the vasodilation of the face in the blush of shame, of the palpitation of the heart in joy, of the stimulation of the sudoriparous glands which precedes the sweat of anxiety, of the stimulation of the vasoconstrictors, the pupil dilators and the pilomotors in the pallor, mydriasis and goose-skin of fright, to illustrate some of these violent autonomic excitations." [30]

The references here, it will be noted, are primarily manifestations pertaining to the surface, the integumentary parts of the body, and their scope is what especially interests us. Very nearly the whole list of these parts is involved, and probably a complete inventory would be still more inclusive.

Now notice the range of manifestations at a deeper level that are involved. "The balance maintained normally between the two antagonistic systems the vagal and sympathetic autonomic is one of the most interesting of physiological phenomena. Think, for example, of the rate of the heart-beat—how constantly it is maintained at a given level in each individual when the body is at rest; the impulses arriving through the vagal system just balance those arriving through the sympathetic system, so as to maintain a rate of approximately seventy-two beats per minute. And a similar balance is maintained in other autonomic domains (e.g., the pupils, bronchial musculature, gastric glands, gastro-intestinal muscle, sweat glands, bladder muscles, etc.)." [30]

And Barker then calls attention to the extent to which the normal processes of the body depend upon temporary upsets of these equilibria, examples of which are watering of the mouth at the smell or sight of food which appeals to the appetite through these senses, the flow of gastric and pancreatic juices at the proper time, through indirect stimulation; the sudden relaxation of the sphincter and contraction of the detrusor of the bladder in micturition; the violent contractions of all the muscles concerned in parturition in the female, and so on.

We may summarize the results of this chapter thus:

(1) The researches of recent years on the internal secretory system and its connection with the great subdivisions of the nervous system, and with the blood, muscular, and reproductive systems, have laid a solid foundation for an understanding of the chemico-functional basis of the animal organism's unity.

(2) The emotional phase of the psychic life of the animal is proved to be in direct organic connection with this basis.

From these results there naturally springs the important question: What relation has human consciousness to this same basis? An attempt to answer this question will be an unavoidable part of our treatment later of the psychic integration of the organism.

REFERENCE INDEX

1. Luciani ....II, 13
2. Carlson and Woelfel..... 49
3. Schäfer (1914) 18
4. Hertoghe 194
5. Bayliss 707
6. Bayliss and Starling 325
7. Bayliss 706
8. Schäfer 28

9. Luciani ....II, 22
10. Luciani ....II, 35
11. Schäfer 9
12. Schäfer 11
13. Aldrich 457
14. Bayliss 721
15. Waller 277
16. Waller 280
17. Waller 281
18. Langley 240
19. Bayliss 484
20. Cannon, W. B.('16) 22
21. Garrison 144
22. Cannon, W. B. ('16) ... 34
23. Garrison 146
24. Cannon, W. B. ('13) ... 372
25. Falta 59
26. Eppinger, et al. 214
27. Eppinger, et al. 217
28. Schäfer 106
29. Schäfer 69
30. Garrison 146

Chapter XIX

THE ORGANISMAL SIGNIFICANCE OF THE INTERNAL SECRETORY SYSTEM

IT remains now to consider the real purpose for which the presentation of facts and views on internal secretions has been made, namely that of showing critically the significance of this secretory system for the organismal conception.

General Inability of Elementalism to Interpret the Phenomena

There is perhaps no better way of approaching this part of our task than by noticing the inability of elementalistic biology to deal in a really intelligent and consistent manner with the phenomena in this field. The breakdown of bio-elementalism when confronted with the phenomena of "chemical messengers" nowhere finds more cogent illustration than in the effort to identify internal secretions with the organ-forming substances hypothesized by Sachs and others.

Although in what follows the exposure of inconsistency and fallacy will have to be drastic and may seem personal, the truth is it is wholly impersonal in spirit and is directed at a system of bad reasoning born of what might be called a juvenile metaphysics of the living world.

The objective achievements of Jacques Loeb and others of the school he represents, in experimental biology, merit the admiration of all lovers of observational truth. One may be, too, more tolerant of their faults as reasoners than he could be but for his recognition of their high service against all the traditional forms of supernaturalism. The real case against the school is, as I see it, two-fold. First, in their zeal to substitute naturalism for supernaturalism they fail to notice that supernaturalism is in its very essence finalistic, and they are led to imagine they have attained, or can attain, natural explanations that fully supplant the old supernatural explanations. This results in the conversion of their supposed naturalism into something which is essentially another kind of supernaturalism. The second part of the case against the school is its abuse of the most common principles of the knowledge-getting processes in objective biology. For the general good of the biological sciences the urgent need of reformation touching both aspects of the case has led me to examine the particular instance of Loeb's treatment of internal secretions at greater length than would otherwise be justifiable.

Critique of the View That Internal Secretions are "Formative Stuffs"

Although Loeb is the only author, so far as I know, who has expressly contended for the identity of internal secretions with Sachs' organ-forming substances, the assumption is so accordant with the spirit of elementalism, and Loeb is so typical and eminent a protagonist of this philosophy, that his proposal will probably find many adherents. It is consequently desirable to see what there is in the effort to bring hormones into such a historical setting.

The statement of Loeb's views is contained in "The Organism as a Whole from a Physico-Chemical Viewpoint," 1916. Referring to his espousal twenty-five years ago of Sachs' hypothesis to explain heteromorphosis, he writes: "At that time the idea of the existence of such organ-forming substances was received with some scepticism, but since then so many proofs for their existence have been obtained that the idea is no longer questioned. Such substances are known now under the name of 'internal secretions' or 'hormones'; their connection with the theory of Sachs was forgotten with the introduction of the new nomenclature."[1]

A case to which Loeb makes special reference as proof that internal secretions are the same as Sachs' organ-forming substances is that of the effect of thyroid on the metamorphosis of the tadpoles of frogs and toads, demonstrated by Gudernatsch. The author's mode of using this case in illustration of his contention is highly instructive. He refers to the legless condition of the tadpoles and calls attention to so much of Gudernatsch's results as pertain to these members. Gudernatsch found, Loeb points out, that whereas in the usual course of things the tadpoles live from four months to a year before the legs grow out, by feeding them on thyroid gland these members can be induced to appear at any time. "We must, therefore, draw the conclusion," Loeb says, "that the normal outgrowth of legs in a tadpole is due to the presence in the body of substances similar to the thyroid in their action (it may possibly be thyroid substance) which are either formed in the body or taken up in the food."[2] When the case is presented in this way and nothing more said about it, it certainly looks considerably as if the thyroid substance or something like it is leg-forming substance; and such an interpretation would be enticing were it really true, as Loeb says, that no other substance seems to have such an effect.

When, however, one consults the account given by Gudernatsch himself as to what his experiments were and what they established, the whole matter stands in a quite different light. First of all, the fact that in these experiments mammalian thyroid was largely used as food for the tadpoles, thus bringing it to pass that if thyroid substance was specific organ-forming substance, then frogs' legs were produced by mammalian substance, shows at once that there is something badly askew in the theory. So we are incited to examine the facts, and particularly the reasonings, carefully.

The following is taken from one of Gudernatsch's papers: "The most striking and at the same time unquestionable results were attained by thyroid feeding. . . . The influence of the thyroid food was such that it stopped any further growth but on the contrary led to an abnormal diminution of the size in the animals treated, while simultaneously it accelerated the differentiation of the body immensely and brought it to a premature end." [3] In other words, the effect of thyroid food was to stop the increase in size of the frog's *larva* and start, almost at once, its transformation into the *adult*. Now this transformation does not consist merely in the production of legs, but in a whole series of changes, some of which, like leg transformation, are progressive, while others are regressive. For example, one of the regressive changes to which Gudernatsch gives particular attention is the resorption of the tail. "Reduction of the body mass (resorption of the tail, loss of water, therefore an increasing compactness of the body, etc.)" [4] more than in normal development, the author says, goes hand in hand with the progressive changes. That is, when the entire series of results of thyroid feeding are considered, and not merely one result picked out arbitrarily, then in case we choose to say the thyroid substance is organ-forming as regards legs, we should have to say it is organ-destroying as regards tail. Furthermore, if we call the thyroid substance organ-forming, consistency would compel us to recognize that one and the same substance is not only formative of legs but of numerous other organs and parts, as of the skin, mouth, respiratory and blood systems, all of which undergo, as is well known, progressive changes during metamorphosis. Nor is this complex series of morphological changes the while story. Striking and characteristic changes in the habits of the tadpoles resulted. "Towards the end of metamorphosis the animals hardly moved about in the water. They were always lying quietly, generally on their backs. When disturbed they would move for a few seconds in a somewhat convulsive manner and then drop again to the bottom of the dish, while tadpoles fed on other material would swim about for a long time."[5] So mammalian thyroid substance is not only organ-forming for a whole series of frog organs but it is habit-forming for a variety of frog habits!

But we must not let the ludicrousness of this veer us away from the reasoning in the case. Taking the facts actually brought out by Gudernatsch, what becomes of the specificity of the substance, which according to Loeb's statements was what Sachs hypothesized? "Sachs suggested that there must be in each organism as many specific organ-forming substances as there are organs in the body."[6] The truth appears to be that thyroid substance in this case is organ-forming in much the same sense that water is organ-forming for the leaves, flowers, and fruit in a squash vine, which could not develop without water. Indeed the analogy suggested goes further than appears at first sight. As everybody knows, the effect on young plants of a scant water supply is to stunt the plant as to size and to hasten its blossoming and fruiting. That is, an under-supply of water has an effect on immature plants similar to that of an over-supply of thyroid substance on immature frogs, namely that of retarding growth and hastening metamorphosis. The total effect in each case is systemic. In other words, the real significance of the instance used by Loeb is just the opposite of his interpretation of it. Thyroid substance is *organ-forming* only through being *organism-transforming*. Full justification of this way of interpreting the part in development played by thyroid substance is furnished by the recent studies of B. M. Allen. This investigator has, like Gudernatsch, experimented with frog larvæ. He has, however, supplemented Gudernatsch's work by depriving the larvæ of thyroid altogether instead of giving them an extra allowance through feeding. Allen extirpates the entire embryonic thyroid from tadpoles long before any indications of metamorphosis appear. What he finds of special importance for the present discussion is contained in the following:

"While Gudernatsch showed that thyroid feeding accelerates development, this work shows that the total absence of the thyroid gland produces complete cessation of somatic differentiation at a certain stage but does not hinder continued growth in size." [7]

The first part of this statement taken alone might be looked upon as confirmatory of the "formative stuff" theory of thyroid substance. But the phrase "at a certain stage" implies, as Allen's paper as a whole conclusively shows, that the exact opposite of such a conception is alone tenable. What actually happens, Allen brings to light, is that in spite of the complete absence of thyroid substance transformation of the larva is begun but is not carried through. That is, the organs and parts of the adult frog are laid down but (with the exception of the reproductive glands) are not completed. "It is evident," Allen says, "that the thyroid gland is in no wise essential to the earlier phases of development, but that at a certain definite stage, further development of the soma is dependent upon it." [7] It should be mentioned that Allen makes a rather special point of the accordance of his results with those of Gudernatsch.

If the greatly hastened and modified metamorphosis of the frog tadpole observed by Gudernatsch is inducible by no other means than by thyroid substance (which while possible is not at all certain), then is the substance causal in the important sense of being not only competent but indispensable. But even so it falls far short of being a complete causal explanation of the phenomena under contemplation, as Allen's results directly prove.

A defect in Loeb's reasoning is his ignoring the truth that thyroid substance lacks in the case cited the prime attribute of a sufficient cause, namely *full* competency. The actual substance which enters into the new legs, and as far as that goes, into the other new parts, is probably provided very little, if at all, by the specific substance or hormone of the thyroid. The causal rôle played is the relatively humble one of excitor or stimulator to an activity not essentially new but only exceptional as to time. Of course Loeb does not need to be told that hormones incite growth rather than provide the substance itself out of which the organs and parts are built. Indeed portions of his account of this very case show positively that he is aware of this. We read: "Thus we see that the mesenchyme cells giving rise to legs may lie dormant for months or a year but will grow out when a certain type of substances, e.g., thyroid, circulates in the blood. There may exist an analogy between the activating effect of the thyroid substance and the activating effect of the spermatozoön or butyric acid (or other parthenogenetic agencies) upon the egg." [8]

This suggestion of analogy between the action of thyroid secretion and "butyric acid (or other parthenogenetic agencies)" is well taken. The resemblance between the two agencies, as judged by their effects on development, is certainly rather close. Very well; would Loeb, then, call butyric acid organ-forming substance, and identify it with the "formative stuff" of Sachs? Certainly any substance which will rouse the latent developmental capacities of an egg into activity is in a minor sense formative, especially if these capacities are wholly unable to start without some such agency. But since butyric acid, or some one of the other dozens or scores of parthenogenetic agencies, may activate the eggs of many, many species of animals; and since the eggs of many, many species may be activated by any one of scores of such agencies, what about the specificity of the formative substances which Loeb himself expressly says was part of Sachs' conceptions?

Again, how reconcile the contention that thyroid substance or something like it is organ-forming for the legs of a tadpole with the statement quoted a few paragraphs back about mesenchyme cells which give rise to legs?

The Metaphysical and Logical Weakness of the View

Obviously Loeb's treatment of this subject contains irreconciliable contradictions. Is it then worthless? My answer is, no, not by any means. But how comes it that a scientist of his great experience and merited distinction can run into such self-destroying speculations and statements, seemingly without rational discomfort to either himself or others of the school which he represents?

The answer takes us back to some of the most fundamental issues between the elementalist and organismal standpoints, and though not requiring us to palliate in any degree such offenses against scientific reasoning, it partly explains how tolerance for such offenses is begotten, and discovers a nucleus of genuine merit in Loeb's position. Bringing the matter to as basal a statement as possible, what we find is that this whole book on the Organism as a Whole is written on the theory that the only alternative to the assumption of supernaturalism is materialism. Instead of supernatural forces of some sort (Platonic Ideas, entelechies, psychoids, "supergenes") for explaining the organism when regarded as alive and whole, his assumption is that material elements as known to us in *inorganic nature* are the sufficient causal explanation of organic phenomena.

Were this theory correct—were it true that the "vital principle" must either be conceived as supernatural or that the inorganic elements taken by themselves are competent to produce organisms, then it would be impossible for biology to do much better in its reasoning and general attitude than Loeb and other elementalists do when they undertake to construct a philosophy of organisms. I agree wholeheartedly that all supernaturalism, no matter what nomenclatorial garb it takes on, must be repudiated by the sciences of organic beings. Ideas, or psychoids, or entelechies, or "principles" of any kind conceived as independent of, or even separable from, sensible objects are quite as repugnant to me, an organismalist, as they are to any elementalist. The essence of my contention is that the natural substitute for these imponderable things are the *living, individual organisms themselves*, and not the particles of which they are composed. Each and every individual organism is a natural reality by exactly the same criteria that the atoms, molecules, cells and tissues of which it is composed are natural realities. And since each individual is to some extent different from every other, and maintains its individuality in full possession of these differences, by its power of transforming foreign substance into its own substance, it is ultimate both as to structure and as to causal power in as deep and literal a sense as the material particles of which it is composed are ultimate.

Loeb's considerable attention to the views of Claude Bernard is fortunate for us, since it affords a chance to show from still another angle the inevitable breakdown of elementalist reasoning when it is brought face to face with organic phenomena as actual nature presents them to the modern student. Loeb calls attention, properly, to the fact that one of the things on which Bernard placed special emphasis, as Bichat before him had done, is the organizing syntheses which go on in the living being. The real advance, it seems to me, which Bernard made over any of his predecessors, was the positiveness of his rejection of a vital force as something supernatural—as something, using his own words, "under the government of a special principle, a peculiar power what name soever be given it, whether soul, or archeon, or psyche, or plastic intermediary, or guiding spirit, or vital force, or vital properties"; [9] and his rejection of these conceptions because of his recognizing that the unitariness of the organism removes the necessity for assuming any such extraneous principle. "When we say," Bernard writes, "that life is a guiding idea, or the *evolutive force of the being*, we merely express the thought of a unity in the succession of all the morphological and chemical changes effected by the germ, from the beginning to the end of life. Our mind grasps that unity as a conception it cannot escape. . . ." [10]

Up to this point the position held by Bernard is entirely satisfactory for the organismal conception as I am trying to develop it, and is, according to my view, unassailable. But the unquoted part of the last sentence contains a statement which reveals Bernard on a by-road leading away from the promised land toward which he was headed as long as he was speaking in terms of biology proper. The rest of the sentence follows: "and explains it as 'a force'; but the mistake is in supposing that this metaphysical force acts after the manner of a physiological force." [10] Stated in a nutshell, the by-road which Bernard is entering here is that of a kind of separatedness, but inevitable concomitance, or parallelism between the phenomenal and neumonal worlds which, according to the views upheld in this volume, does not exist. "We need here," Bernard says, "to draw a distinction between the metaphysical world and the phenomenal physical world, which serves as its basis, but which can borrow nothing from it." This Leibnitzian theory, according to which "everything takes place in the soul as though there were no body, and in the body everything takes place as though there were no soul," Bernard says science "recognizes and adopts in our day." But in *our* day, this year nineteen hundred and eighteen, science, at least so much of it as speaks through this volume, though understanding fully what this dualist theory is, *rejects* it. It denies that things take place in the soul as though there were no body; and that things take place in the body as though there were no soul. On the contrary it affirms that what in rather uncritical language we call "the body" and "the soul" are in the most intimate and indissoluble connection with and dependence upon each other, and together constitute "the Organism."

The Form of Metaphysical Absolutism Involved

A full, systematic justification of this position is beyond the province of this volume; and in this chapter we are concerned solely with the "body," the strictly morphological and physiological aspect of the subject. Nevertheless, this much of contact with the "soul" aspect was unavoidable for the reason that Bernard and also Loeb have run into it in such fashion as to color deeply their discussions and outlook. This coloring is the more unfortunate and the more insistent in requiring attention from the fact that the authors, especially Loeb, are apparently unaware of such coloring. For example, Loeb writes on the first page of his book, after saying that the atomistic theory of matter and electricity are now in all probability on a "permanent basis": "This permits us to state as an ultimate aim of the physical sciences the visualization of all phenomena in terms of groupings and displacements of ultimate particles, and since there is no discontinuity between the matter constituting the living and non-living world the goal of biology can be expressed in the same way."

Statements like this, many of which can easily be quoted from Loeb's writings, leave no question about his metaphysical affinities. The conception of "ultimate particles" as explanation of all phenomena, is exactly what I mean by elementalist absolutism.

Confusion of Theory of Organisms and Theory of the Knowledge of Organisms

Although the thoroughgoing metaphysical character of these statements is evidenced by the finalism which crops out at several points, this is not the aspect of the matter which chiefly interests us here. Rather what we are concerned with is the fact that affirmations about the "aim of the physical sciences" and the "goal of biology" do not belong, properly speaking, to the provinces of physical science and biology at all, but to quite a different science, namely that which deals with the nature of knowledge itself. The "physical sciences" are the vast accumulation of man's positive knowledge, theories, hypotheses, and so forth, *about* physical nature; they certainly are not physical nature itself. Consequently a statement of the character and aims of that knowledge is not a statement about the phenomena to which the knowledge pertains. And the same reasoning applies to the affirmation about the goal of biology.

All this is only another way of saying what Loeb himself virtually tells us, namely that his entire discussion of the organism as a whole is made from the standpoint of *one particular theory* of the *ultimate* nature of living beings, that theory being the mechanistic. Recall the complete title of the book *The Organism as a Whole From the Physico-Chemical Viewpoint.* To treat the subject from this viewpoint is of course perfectly legitimate. When, however, the assumption is made that such a treatment is the only really legitimate one because it rests on *ultimate truth,* then sound science is bound to protest, chiefly because of the obvious biological inadequacy, and at some points, perversion and contradiction displayed in the treatment.

This brings us back to the way internal secretions are dealt with by Loeb. His failure to distinguish between the two very distinct fields of theory above indicated, namely theories about the phenomena of living beings, and theories about knowledge of these phenomena, largely explains the defects of the theoretical parts of his work. And we are now in position to give our criticism greater explicitness. Consider for example in the light of what has just been said about confusion relative to kinds of theory, the irreconcilable statements, cited on an earlier page, that the mesenchyme cells give rise to the legs of the tadpole, and that thyroid substance is organ-forming substance for the legs. Stated briefly the case seems to be thus: Loeb recognizes, as every one must, that internal secretions constitute a physico-chemical agency for bringing about that harmonious development and functioning so characteristic of the organism. But this harmony is one of the very things which has seemed to some biologists inexplicable without the assumption of supernatural influences of some sort; hence Driesch's attempt to modernize the ancient entelechy. But since internal secretions play the rôle that entelechies are supposed to play, namely that of establishing and maintaining the unity and equilibrium of the organism, the need for entelechies no longer exists; at least, this would be so for all persons who do not contend that "ultimate explanation" is the "goal of science."

To those who hold these absolutist beliefs as to the power and aims of science three favorite courses are open and are followed by different representatives of the school, depending on the taste, training and outlook of the person. One course consists in pointing out, taking an illustrative case, that internal secretions, being only contributing causes, do not constitute an ultimate explanation, so that entelechies or something similar are as necessary as before. This would be the course followed by the vitalistic wing of the absolutist school. For them internal secretions would be, in so far as they contribute to the harmony of the organism, merely agencies produced and used by supernatural causes.

Another course, and perhaps the one most frequented by elementalists, would be to contend that internal secretions are sufficient as a causal explanation of organic unity to make the entelechy or any similar notion quite superfluous, even though these substances are far from a complete explanation. The reasoning of this group of elementalists as to this situation is substantially as follows: although internal secretions fall far short of fully explaining organic unity and harmony, the action of these being merely that of incitors and inhibitors, they are yet genuinely causal, genuinely physico-chemical and so are on the road toward complete explanation of the phenomena. All that is necessary consequently, is to believe that still further advance in the same direction will reach finally a full elementalistic explanation; that is, an explanation which will have no need of either supernatural elements or the organism as a whole. The attitude of this large class is one primarily of faith rather than of reliance on positive knowledge; they are inspired more by what they believe they will do in the future than by what they actually have done. They are preëminently men of promises. Although their achievements in experimental science are indeed large, the results reached by them are prized more on account of what they are believed to augur for the future than for their present meaning and worth.

Then, finally, there is the group of elementalistic absolutists, of whom the author of *The Organism as a Whole from the Physico-Chemical Viewpoint* is one of the most eminent in our day, who, as we have pointed out, by confusing theories about objective phenomena with theories about the *knowledge* of such phenomena, are led to affirm that such phenomena as the unity of the organism are fully explained by internal secretions, when as a matter of fact they are only partly explained thus.

The surprising thing about the confusion of this group is that wholly irreconcilable positions are held with impunity, such for instance as those according to which thyroid substance is held to be the organ-forming substance of frogs' legs in one part of a discussion, and mesenchyme cells are acknowledged to be of this nature in another part. The contradiction is, to be sure, often of such character as easily to escape the uncritical reader; but as to the authors of such contradictions no other explanation seems possible than that of wrong habits of scientific thought begotten of untenable *a priori* conceptions. For example, a hasty reading of the discussion under review might lead one to suppose that thyroid substance is not, after all, regarded by Loeb as anything more than one contributing cause of frogs' legs, mesenchyme cells being recognized as another cause. Close attention to the text does not, however, warrant this generous interpretation of the author's position. Going back to his espousal of the theory of Sachs and other botanists as to organ-forming substances, we read: "Specific shoot-producing substances are carried to the apex, while specific root-producing substances are carried to the base of a plant. When a piece is cut from a branch of willow the root-forming substances must continue to flow to the basal end of the piece, and since their further progress is blocked there they induce the formation of roots at the basal end." [11]

If this means anything it means that the shoots and roots are actually built up by material carried about in the willow branch. There is nothing in the language that can be interpreted as meaning that shoot-and-root-forming substances are mere stimulators of some other substances which become the actual shoots and roots. Yet it is with formative substances of this sort that Loeb in some parts of his discussion explicitly identifies internal secretions. "At that time the idea of the existence of such specific organ-forming substances was received with some skepticism. . . . Such substances are known now under the name 'internal secretions' or 'hormones'; their connection with the theory of Sachs was forgotten with new nomenclature."[1] Then follows the reference to the tadpole legs; so that were consistency really a jewel to the author, he could not escape meaning that the substance of the legs was actually derived directly from the particular internal secretion in question, in this case thyroid substance; and the later statement about the formation of legs from dormant mesenchyme cells through the mere activating effect of thyroid substance is by implication contradicted.

This brings up again a matter about the interpretation of development which we have dwelt upon in several other connections, that of protest against rejecting the indubitable evidence of the senses in favor of *à priori* conceptions. The crucial question in the present case is this: which is the more fundamentally organ-forming substance for frogs' legs, the mesenchyme cells which "though giving rise to legs may lie dormant for months," or the thyroid substance which may stimulate these cells into premature activity? While Loeb's discussion does not raise this question definitely, the implication is unescapable that thyroid substance is for him the more fundamental. What else is the meaning of the contention that this substance is organ-forming while nowhere do we find the mesenchyme cells so designated? Yet the observational evidence is that the production of legs is accomplished through the transformation of mesenchyme and other cells which in the larva are not leg-substance, but in the adult are leg-substance. Hence it follows that so far as actual observation is concerned the mesenchymatous and other larval substances are more entitled to be called organ-forming than is the thyroid substance.

An Illustration of Neglect of Fact By Elementalist Theory

These reflections lead to a still deeper level of the inherent faultiness of elementalistic absolutism in biology, the tolerance which it engenders for ignoring relevant facts; or stated otherwise, of arbitrarily selecting from a great complex of facts just those which suit the argument, and disregarding all the others. For example, recall Loeb's reference to precocious leg-production in frog tadpoles as though the effect of thyroid feeding stood alone rather than as one among a great concatenation of effects, some constructive and some destructive, this complex of phenomena constituting the metamorphosis of the young into the adult. Loeb's use of Gudernatsch's results amounts to a positive obscuration for the reader of what these important experiments really teach. Only by the culling of facts to suit the argument and the use of certain words and phrases, as "influence," "responsible for," and so on, with equivocal meanings, can these results be made to support the contention that thyroid substance is specific organ-forming substance for frogs' legs. The patent fact is that certain mesenchyme and other cells of the larva are organ-forming for legs, and there is no straightforward way of talking about the causes of the transformation of a given group of more or less undifferentiated tadpole cells into the much enlarged and highly differentiated group called a leg, without recognizing the whole organism as causal of the particular transformation. Probably no set of discoveries concerning the development of the individual has ever been made which so objectifies the means employed by the whole in producing and correlating its constituent parts as those on internal secretions; and not the least significant fact is that these substances are themselves produced by the organism.

Even were Loeb's contention valid that thyroid substance is specific organ-forming substance, the indubitable fact that in normal development this substance is itself a product of the organism's activities, throws it into a very subordinate place as a cause of development. The truth is, the main upshot of the effort to explain ontogeny on elementalistic principles amounts to an effort to avoid recognizing the most positive and definite entity in the whole situation, namely the organism taken alive, normal and untampered with.

A Peculiar Elementalist Objection to the Organic Whole

We now come to the last point to be noticed in connection with the elementalist attempt to deal with internal secretions as related to the organism as a whole. Instancing the familiar way in which a particular part of a flat-worm will give rise to a new head after being cut away from the original animal, when no head would have been formed at this place had not the animal been cut, Loeb writes: "How does the 'whole' suppress all this formative power in the part before the latter is isolated? It almost seems as if the isolation itself were the emancipation of the part from the tyranny of the whole. The explanation of this tyranny or of the correlation of the parts in the whole is to be found, however, in a different influence."[12]

Then follows the statement previously quoted about the specific organ-forming substances of Sachs and other botanists, and the assumed identity of these with internal secretions.

Without raising the question concerning the evidence for the assumption that the production of a flat-worm's head as indicated is dependent upon internal secretions, let us consider a moment the interesting conception thrown into the treatment that the whole flat-worm *tyrannizes* over its parts. Why this? Is it "mere rhetoric"? We are not permitted to judge it thus, for no one has pronounced against this sort of thing in science more frequently than Loeb. The conception that the "whole" exercises a "tyranny" over the parts we must accept as being seriously scientific with Loeb. Well then, since tyranny is "absolute power arbitrarily or unjustly administered," according to the dictionary, it is certainly interesting to an organismalist to find so eminent an elementalist acknowledging the organism as a whole to be truly causal relative to its parts—for it is hardly conceivable that even the extreme pliability of elementalist practice as regards the definition of words would venture to hold the absolute power which constitutes tyranny to be without causal efficiency. Power thus potent but which could cause nothing, not even the destruction of the parts (for surely the tyranny of the whole does not destroy the parts), would be too queer a conception for anybody to father deliberately.

But the most interesting thing about this idea of the "tyranny" of the whole over the parts remains to be noticed. Tyranny is not merely absolute power; it is such power exercised unjustly or "in a manner contrary to law or justice."

Here we come, I think, to about the last ditch of the elementalist position. On what ground does one conceive the power exercised by the whole organism over its parts, to be contrary to law or justice. According to what legislature or court is there a law of the parts of an organism more just than the law of the whole? None whatever *in nature*, it must I think, be admitted. The only ground for the elementalist's pronouncement, that the whole acts tyrannously toward the parts, that it acts in a manner "contrary to law or justice" is in the *mind of the person* who makes the pronouncement.

The truth is—and it is of great importance since its influence reaches far beyond the confines of scientific technicalities—any scientist, especially any biologist, who is through and through an elementalist, is necessarily a protestant against all law except the law of elements.

The scientific elementalist is inevitably anarchistic toward all the most common, most objective, structures and laws of nature.

His faith is in the laws of the obscure or invisible world and against those of the everywhere visible world. Atoms are more real to his mind than are lands and waters, plants and animals.

REFERENCE INDEX

1. Loeb ('16) 155
2. Loeb ('16) 156
3. Gudernatsch 475
4. Gudernatsch 476
5. Gudernatsch 476
6. Loeb ('16) 159
7. Allen ('18) 515
8. Loeb ('16) 156
9. Bernard 516
10. Bernard 519
11. Loeb ('16) 154
12. Loeb ('16) 154

Chapter XX

NEURAL INTEGRATION

Neural and Not Psychical Phenomena the Subject of This Chapter

THE fact should be firmly fixed in mind at the outset that in this chapter we have nothing to do with the organism's consciousness and volition. We are to deal with the nervous system purely on the basis of its physical activities. Whether or not consciousness or something of the essential nature of it appertains to part or all of the activities to be treated we do not have to decide, so far as this discussion is concerned. Our task here is to be strictly anatomical and physiological, and not at all psychological.

Distinction Between Developmental and Functional Integration

The discussion may be opened by calling attention again to a matter noticed incidentally in the last chapter, namely the relative parts played by internal secretions and the nervous mechanism in integrating the *developing* organism on the one hand, and the *functioning* organism on the other. We saw that the rôle of internal secretions in preserving developmental equilibrium in the individual is so conspicuous as to throw the strictly physio-logical rôle of the substances somewhat into the background. Nevertheless as evidence, particularly clinical evidence, has made clear, their part in functional equilibrium is far-reaching.

We must now point out specifically what appeared only incidentally in the previous discussion, namely, that the integrative action of hormones and of the nervous system are to a considerable extent the reverse of each other as regards their relation to development and to adult function. That is, while harmones are, perhaps, more important in development and become relatively less significant with the advancing age of the individual, the nervous system plays a minor part in integrating the developmental processes but becomes of supreme importance in this way for the functioning of the adult. Or, stating the generalization in another way, integration of the organism is accomplished by chemical means chiefly, and by neural means little or not at all, during early life, and by neural means chiefly and by chemical means secondarily in later life.

One can hardly fail to see, in a general way, the bearing of this on the familiar truth that the life of the individual among the higher animals, man especially, is successively vegetal, animal, emotional, and finally rational and intellectual in its dominant characteristics, with the successive stages of earlier and later childhood, youth, and earlier and later maturity.

The Author's Indebtedness to Sherrington's Work

My dependence upon Sherrington's work in this field will be so great as to make it impossible to acknowledge it at every point. I therefore make at the outset the general statement that a large part of my discussion consists of a re-wording and rearranging of facts and ideas contained in this physiologist's important book, *The Integrative Action of the Nervous System.* But while my chief reliance here is on Sherrington's work, the writings of Cannon and his collaborators have been the most important source of what I have to say about the autonomic nervous system. Cannon's recent book, *Bodily Changes in Pain, Hunger, Fear and Rage*, has been especially drawn upon.

The Fundamentality of Cellular Integration in the Reflex Arc

In no department of physiology do cells keep themselves more persistently in the attention of the student than in the physiology of the nervous system. But likewise nowhere is the fundamental dependence of cells on other cells more clearly seen; for while, speaking from the standpoint of general functions, cells may be looked upon as individual units, when it comes to the study of nerve cells as such, that is, as constituents of the functioning nervous system, the individual cell is found to be no longer the basal unit. Viewed thus the reflex-arc and not the individual cell is the unit.

The distinction thus indicated is important from the organismal standpoint and must be considered a little more fully. The general functions of cells to which reference is made above are those common to all cells, even those of the tissues of fully differentiated multicellular animals. No matter what tissue be under consideration, muscle, gland, epithelial or what not, so long as it is truly living, each and every cell assimilates, breathes, excretes, and carries on all the metabolic processes. Thus far each cell is an independent unit in a high degree. Now while all tissue cells, using the term tissue in its common histological sense, have a relational or integrational function in addition to these individual functions, it is in the nervous system that this integrative aspect of cell life is most positive and definable. The reflex-arc, as the unit of the nervous system, is itself a combination of three indispensable parts or elements: the receptor, the conductor and the effector. Typically these structures contain at least four cells, one for the receptor, two for the conductor, and one for the effector. An illustration would be a reflex-arc consisting of a tactile cell of a touch organ, the two cells constituting the conducting path, and an effector cell in a muscle. A point of special interest in connection with such an arc is that the dependence of the parts on one another is such, in the highest development of the arc, that the specific action of each part is dependent on the specific and connected action of the other parts. "The optic nerve itself," to quote Sherrington, "is unable to enter into a heightened phase of its own specific activity on the application of light. Initiation of nervous activity by light is the exclusive (in this instance) function of cells in the retina, retinal receptors." [1] And of course without brain cells as effectors for vision the specific activities of receptors and conductors would be impossible. Not only would an optic nerve fiber's conducting ability be useless without a retinal cell on the one hand and a brain cell on the other, but the very conductive act itself would not be fully performed.

This specific dependence, as it might be called, of the parts of a reflex-arc is so significant that another example may be profitably noticed. A striking one is afforded by the effects of the passage of gall stones through the gall ducts, instanced by Sherrington, partly on the basis of his own studies. The excruciating pains associated with this malady are due to the distention of the wall of the gall duct by the passage through it of the mineralized organic concretions which constitute the gall stones. The point in this for us is the fact that though the stimulus which produces the pain is mechanical and acts upon the wall of the duct, this stimulus is so peculiar that other sorts of mechanical stimuli of the same tissue, even to cutting and wounding, give no sense of pain. Though the duct may be cut without causing pain, pain may be produced by injecting the duct to distention with neutral fluid. "Marked reflex effects can be excited [by fluid] from the very organs the cutting and wounding of which remains without effect."[2] The reflex mechanism involved is adjusted to respond only to a stimulus of a *special kind.* In this it is comparable to the optical mechanism referred to above.

These illustrations show something of the general nature of the reflex-arc as an integrated structure. But we want to know something about the part contributed by the different constituents of the arc to this nature. The specific office of sense organs viewed as the receptor members of the arcs will first engage attention. "The main function of the receptor is therefore *to lower the threshold of excitability of the arc for one kind of stimulus, and to heighten it for all others.*"[3] This principle is so important that we must allow no vagueness as to its meaning. It means that while more than one sort of stimulus may put a particular sense apparatus into operation, there is one and only one kind, namely, that to which the sense organ is naturally subject, that elicits the reflex in its normal or type expression. A sense organ may be looked upon as a group of receptors attuned to a special stimulus, as contrasted with that of the general stimuli to which an organism is always subject by being always in contact with its environment.

In illustration Sherrington instances the fact that the threshold for the touch-sensation is lower for a mechanical stimulus than for an electrical stimulus. Having regard to the whole lot of reflex-arcs of the body, we may say that the different sense organs constitute mechanisms of *selective excitability* for the different stimuli, each organ being so adapted to its natural stimulus that it responds to this better than to any other. This reference to the sense organs as adaptive makes it desirable to notice the fact that, according to Sherrington, electricity is never an *adequate* stimulus of any sense organ because it is not a *natural* stimulus for any, since it does not enter into the natural environment of the organism. This is important in view of the large use made of electricity in experimental work on nerve physiology. To what extent may accepted generalizations concerning various reactions be influenced by the inadequacy of the stimuli applied?

We glance next at the physiological character of the conducting member of the arc. It is hardly necessary to say that typically this part is found in the nerve cords everywhere present in the body. The most general truth of significance for us is the fact that having regard to conduction through the arc as a whole, the process is different in many important respects from conduction in the nerve cord alone. Conduction in the arc as an integrated whole is quite different from conduction in the part of the arc specially devoted to that office. To illustrate by a quotation: "Another remarkable difference between reflex-arc conduction and nerve-trunk conduction is the irreversibility of direction in the former and the reversibility of the latter." [4] This is only another way of expressing the well-established fact that nerve impulses are incapable, in the higher organisms especially, of running in more than one direction. Sensory excitations can go only centripetally and motor excitations only peripherally. As Sherrington points out, this is part of the "law of forward direction" of the neural system. It is fully established that nerve impulses may run in both directions when a stimulus is applied at any point or a nerve trunk, whether the trunk be motor or sensory. Taken by itself this fact might encourage a somewhat careless observer to think of the nervous system as more or less hit-or-miss in its structure and action, it being able to work one way as well as another, the final result being determined by where the stimuli happen to be applied. As a matter of fact, though, when we come to consider the real unit of neural organization, the reflex-arc, instead of any of the constituents of that arc taken separately, all suggestion of haphazardness disappears. Nerve conduction in a nerve trunk as an isolated phenomenon has neither existence nor meaning for the actual organism. The conducting element *par excellence* of the real unit of nerve organization is differentiated with reference to the other elements of that unit, and normally acts only in such relation. The circumstance that this element is found to have the ability to act somewhat differently under different but abnormal relations signifies little or nothing, so far as one can see, for the normal workings of the nervous system. What it does show is something of the diversity and plasticity of the organism because of its latent ability to act otherwise than in nature it does act when conditions are imposed upon it which are wholly new to it.

Although the explanation of this irreversibility of direction appears not to be known with certainty, the suggestion that it may be connected with a difference in permeability of the synaptic membrane between cells of the reflex-arc to certain ions, depending upon which side of the membrane is presented to these ions, is plausible and indicates the resourcefulness, as one might say, of structural and functional method by which the organization of the living being is accomplished. Should this suggestion prove to be correct, the question would arise, how comes it that the synaptic membrane is thus differential in its action? And no adequate answer would be forthcoming that did not take cognizance of the fact that the membrane assumed this differential mode of acting as part and parcel of the differentiation and integration as the reflex-arc as a whole.

Sherrington deals with a whole series of other differences between nerve trunk conduction and reflex-arc conduction, such as the phenomena of summation of stimuli, that is, the adding up of excitations too slight taken singly to produce reaction, until the aggregate brings response. Such are rhythmic activity in response to stimuli which are not rhythmic, or at least not of the same time-intervals as the excitations, gradation in intensity of reaction, and so on. To these he devotes more than a hundred pages, all under the common heading, "Coördination in the Simple Reflex."

It is unnecessary to follow the matter further, sufficient having been given to show something of the variety of ends, all definitely and specifically contributory to the needs of the organism as such, and all accomplished through differentiation and integration of one relatively simple organic structure, the reflex-arc.

The Integration of Reflex-Arcs

Our next task is to learn something of the combinations among the myriads of reflex-arcs of which the higher nervous system is composed. The definition of the reflex-arc given on a preceding page presents this mechanism in its simplest terms, and so far as the definition indicates it would seem possible for it actually to exist and be operative in this simplicity. In fact, physiologists are accustomed to assume such an entity and to call it the simple reflex-arc, and its activity the *simple reflex*. But while such a conception is convenient and helpful for didactic purposes, especially as an aid to visualizing the earliest stage of cell integration in the evolution of the nervous system, as a matter of fact, according to Sherrington, it probably never exists in nature. "A simple reflex is probably a pure abstract conception, because all parts of the nervous system are connected together and no part of it is probably ever capable of reaction without affecting and being affected by various other parts, and it is a system certainly never absolutely at rest." [5]

The factual basis for the conception that one of the most essential distinctions between the nervous system and other systems of the body is the integratedness of the former, is well expressed by Donaldson: "Isolated groups of nerve-cells do not occur. Indeed, a group of nerve-cells disconnected from the other nerve-tissues of the body, as muscles or glands are disconnected, would be without physiological significance. It is desirable, therefore, to emphasize the fact that by dissection the nervous system is found to be connected throughout its entire extent." [6]

I would ask the reader to consider these statements in the light of the cellular conception of the organism maintained in this volume and especially dwelt upon in the chapters on the cell-theory, namely, that the true way of viewing the organism is not as being built up of cells in the sense of having been constructed by the bringing together of previously isolated cells, as a brick house is built up of bricks; but rather as being composed of cells through resolving itself into these as it increases in size and differentiates itself into its organs and functions. With such a conception Sherrington's formulation, based on the findings of vast observational and experimental research on the structure and action of the nervous system, is in perfect accord. If we see in the completed nervous system a complex mechanism developing as a unit subservient at all stages to the needs of the organism as a whole, the myriads of reflex-arcs then present themselves as final, as end-stages in the differentiation and not as initial states; and the universal organic and functional connection with one another, affirmed in the quotation, would be just what we might expect.

If, on the other hand, the integral nervous system were built up in a literal sense, that is, by the actual coming into connection with one another of previously isolated simple reflex-arcs, such arcs ought to be demonstrable both in the ontogeny of higher animals and in the adults of the lower metazoa.

No one should be beguiled into the notion that the readily observed facts of ontogeny of the nervous system, the various processes, dendrites and axones, do actually grow out on nerve cells and bring cells into connection with one another and with receptor and effector cells, and that a functional coördination is thus finally reached does not exist in any way or degree in the early stages. Proof that the completed stages of neural integration cannot be accomplished without the production of cell-growths which put cells into connection with one another is a very different thing from proof that these cells were once wholly isolated from one another, and that the outgrowths which establish the final connections were initiated by impulses which originated wholly *within* the cells; as, by way of illustration, the branches of two young trees standing not far apart might come into contact as the trees increased in size. It is a matter of elementary knowledge of animal development that in this sense the cells of the nervous system are never isolated either from other nerve cells or cells of certain other parts of the organism. Until we know vastly more than we do know of the chemical nature of intercellular substances and of the chemical and physical activities which go on at the planes of contact between cells, nothing could be more gratuitous and unscientific than to assume that nerve cells differentiate as they would were they not in some measure vitally associated with one another from the very beginning. Indeed, such positive knowledge as we have tends strongly against such an assumption.

Recall, for example, the fundamental part chemical messengers play in development. And Sherrington's insistence on the rôle of intercellular substance and "surfaces of separation" between cells in the functioning of the adult nervous system is much to the point for this contention. *Intercellular* as well as *intracellular* conduction must be, he maintains, expected in the reflex-arc on the basis of the cell theory.[7]

The Spreading and Compounding of Reflexes

As a practical matter study of the integrative action of the nervous systems never gets away from integration *among* reflex-arcs and reflexes any more than it does from the cellular intergration fundamental to the reflex-arc itself. The general nature of the study always involves the "spread of reflexes over a wide range of nervous arcs." The idea is more fully stated in the following: "This compounding of reflexes with orderliness of coadjustment and of sequence constitutes coördination, and want of it inco-ordination. We may therefore in regard to coördination distinguish coördination of reflexes simultaneously proceeding, and coördination of reflexes successively proceeding. The main secret of nervous coördination lies evidently in the compounding of reflexes."[8] For the rest, all we can do or need do to meet the requirements of this discussion is to get at the main principles as illustrated by examples of this compounding of reflexes.

As a starting point for the discussion of this larger aspect of neural integration, Sherrington takes what he has called "the principle of the common path." Basal to this conception is the familiar fact that a stimulus applied at a single point, on the surface of the body, for example, gives rise to a nerve impulse which may reach a great number of muscles or glands. A single receptor with its neurone must be in communication with a great number of effectors, some of which are very different in kind. Looking at the arcs from the effector ends, as one may say, it is obvious that the impulses reaching innumerable effectors must come over a single conducting course.

On the other hand, since a given muscle or gland may be reached by impulses from a great number of reflex-arcs, the effector and its neurone must be the "common path" for all these impulses, often very different in quality and source. "While the receptive neurone forms a private path exclusively serving impulses of one source only, the final or efferent neurone is, so to say, a public path, *common* to impulses arising at any of many sources of reception." [9]

An example which illustrates the general principle of the common path, and several phenomena incidental to this, and one which has been much investigated by Sherrington, is that of the scratching reflex in dogs. It has long been known that in several land vertebrates which have the habit of scratching the side and back of the forward part of the body with the hind foot, the scratching movement may be elicited by appropriate stimuli applied to the area reached by the foot as a pure reflex; that is, in the absence of any chance for impulses from the brain to reach the parts involved in the activities. Since the scratching movement consists in bringing the hind leg forward and upward, and for scratching, a rhythmic movement of the foot, the muscles, both flexor and extensor, of the thigh, leg and foot must be involved. And since the reflex can be induced by a stimulus applied at any point within the large *receptive field* (i. e., nearly the whole side and back of the body), impulses started from various parts of the field must pass through one and the same neurone in the muscles concerned.

And here comes in a fact showing another aspect of the integration of reflexes in this case. A stimulus at a given point in the field too weak by itself to elicit the reflex may bring it on when acting in combination with weak stimuli at other points in the field. Sherrington calls reflexes which act together in this way allied reflexes.

Still another kind of combination of reflexes involving the common path principle, even more significant than allied reflexes, are what are known as *proprio-ceptive reflexes.* The kernel of this class of reactions is the existence of receptors in the deep tissues of the body, that is, not belonging to the surface and hence not subject to stimuli from the external world, but because of their situation subject only to stimuli "given by the organism itself." Specifically the sources of the stimuli are the muscles, joints, blood vessels and so on, which by the regular activities are always in more or less movable contact with one another and with other parts and organs. Stimuli from the environment acting upon the skin receptors give rise to reflexes which put muscles into activity, and these activities serve in turn as stimuli to receptors in the deep parts; and the impulses arising from these deep receptors may pass to still other muscles over the same effector neurons used by impulses coming from skin receptors. The proprio-ceptive field is a sort of relay and supplement and extension of the field of contact with the environment, or *extero-ceptive* field.

A simile used by Sherrington helps us to understand the import of this compounding of reflexes: "The receptor system bears, therefore, to the efferent paths the relation of the wide ingress of a funnel to the narrow egress. Further, 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. The simile to a funnel will therefore be bettered by supposing that within the general systemic funnel, of which the base is five times wider than the egress, the conducting paths from each receptor may be represented as a funnel inverted so that its wider end is more or less coextensive with the whole plane of emergence of the final common paths. This gives some idea of the enormous formation of common paths from tributary paths which must take place."[10]

And Sherrington forces home the truth of the scope of the combinations by calling attention to the fact that under strychnine poisoning "a muscle can be excited from practically any afferent nerve in the body." This is equivalent to saying, he remarks, "that each final common path is in connection with practically each one of all the receptors of the body."[11]

Even though this statement should prove to be too strong, it certainly contains truth enough to show "the profusion in which common paths exist."

The functional side of the total reflex system, the structural side of which was visualized by the funnel simile stated above, may be regarded as pretty well presented by certain phenomena known as irradiation. What is meant by this is quite clear, in its general outlines, from the following:

"The more intense the spinal reflex . . . the wider, as a general rule, the extent to which the motor discharge spreads around its focal area. Thus, as stimulation of the planta causing the flexion-reflex is increased there is added to the flexion of the homonymous hind limb extension of the crossed hind limb, then in the homonymous fore limb extension at elbow and retraction at shoulder, then at the crossed fore limb flexion at elbow, extension at the wrist, and some protraction at shoulder; also turning of the head toward the homonymous side, and often opening of the mouth, also lateral deviation of the tail. According to circumstance, especially according to intensity of stimulation, the field of end-effect of the flexion-reflex may vary from a minute field occupying part of a flexor muscle of the knee to a field including musculature in all four limbs and neck and head and tail."[12]

Antagonistic Reflexes in Skeletal Muscle Groups Finally Integrative

So far what has been said about the interaction between reflexes has dealt only with interactions which are harmonious with one another in various modes of combination. But there are antagonistic or competitive as well as harmonious interactions. These must now receive attention. As an illustration we make use again of the scratching reflex. "If," says Sherrington, "while stimulation of the skin of the shoulder is evoking the scratch-reflex, the skin of the hind foot of the *same* side is stimulated, the scratching may be arrested."[13]

Then the author proceeds to show, by description and diagrams, how the two excitations here involved, one from the skin of the side of the body, the other from the hind foot of the same side, have the "same final common path" to the muscles of the hip and leg, which, however, they "*use to different effect*"; that is, the one to excite, the other to inhibit, contraction of the muscles concerned.

At once there arises the important question: What is the meaning of such antagonisms, such seeming want of harmony, as this? How deep-seated is the competition thus shown? Does it amount to an "ultimate truth" in the nature of the organism, thereby furnishing an argument in favor of the "struggle of the parts" as an explanation of the organism? Or is it possible that the antagonism is secondary to the wider needs and activities of the organism as a whole?

A partial answer to this inquiry is found in the character of the stimuli operative in the two opposing fields. "Stimulation of the skin of the hind foot by any of the various stimuli that have the character of threatening the part with damage causes the leg to be flexed, drawing the foot up by steady maintained contraction of the flexors of the ankle, knee and hip."[14]

In other connections Sherrington dwells on the peculiarities of effect and importance from the standpoint of adaptation, of stimuli of this class, as for example pricking, strong squeezing, injurious heat, and so on.

From the descriptions we notice that the actions of the hind leg involved in the scratching-reflex are considerably different from those involved in withdrawing the hind foot from stimuli of harmful portent. But since the same muscles are necessarily involved in the two sorts of action, and since the stimuli reaching these muscles from the two sources must use the same final paths to those muscles, back of the antagonism between the two sets of reflexes is the question, which, in a given case, is more important to the organism, the scratching action or the withdrawal-from-danger action?

Since the scratch-reflex in the dog is probably connected primarily with flea and other insect bites, and since on the whole it may be assumed that these are rather annoyances than real dangers to life and limb as the "nocuous" excitations are by fundamental nature, it would be fair to infer that though either reflex might under certain circumstances inhibit the other, the threshold for the injury-escaping reflex would be lower. I do not know that there is anything in the evidence which bears directly on this point, but the question is one that would surely arise were the whole subject of antagonistic reflexes to be looked at from the standpoint of the needs and adaptations of the normally living organism.

The way certain other reflexes, antagonistic in a sense, are yet correlated in a larger sense, is more obvious than in the case just given. Thus the reflex appertaining to two limb muscles which oppose each other does not merely activate the muscle which contracts; it simultaneously causes depression of the opposing muscle. Coöperative antagonism, as it might be called, of this general sort is widespread among higher animals, and applies to glandular, circulatory and various other mechanisms as well as to the muscular. Indeed, it being undoubtedly true, as Sherrington repeatedly points out, that the "outward behavior" of animals involves a great variety of movements which proceed in an orderly sequence, *if they are normal*, it seems almost necessary to suppose that really all normal reflexes must be coöperative and harmonious with reference to the organism as a whole, even though when viewed one by one or in secondary groups they are antagonistic.

Such a conception of the real nature of antagonistic reflexes is favored by the seemingly general fact that these reflexes are seldom if ever really destructive of one another, since they do not act upon one another *simultaneously*. Their antagonism consists in a *successional* opposition to one another. As they follow one another one acts in the opposite direction to the other, and the antagonism is the more real in that frequently they overlap to some extent. But as already said this overlapping probably never amounts to complete coincidence. Such overlappings and other forms of partial opposition constitute the phenomena of inhibition which play a great rôle in the sum total of reflexes of the organism. This is part of the method by which transition is accomplished from one reflex to another, where the same muscles, for example, execute both. But the fact that the transition is accomplished normally "without confusion," to use Sherrington's phrase, shows the subordination of the inhibitions to the organism as a whole.

Another important fact to which Sherrington calls attention is that inhibitions which reflexes produce upon one another never, so far as is known, result in injury to the tissues involved. Genuine opposition of reflexes, as of any other sorts of physical or chemical action, would, according to all our conceptions of natural bodies, have deleterious effects on the opposing bodies. As a matter of fact, inhibiting reflexes not only do not injure the mechanisms involved, but actually prepare them for greater functional activity later on.[14]

This beneficent effect, as it might be called, of inhibition is perhaps illustrated by certain forms of compensatory reflexes. Thus, stimulation of the *central* end of the nerve to the extensor muscles of the dog's knee results in contraction of the flexors of the hip and knee. But on removal of this stimulus contraction of the extensor muscles immediately succeeds, this "rebound" being especially marked in the *vasto-crureus* muscle, the main knee extensor.[15]

The Antagonisms within the Autonomic System Finally Integrative

It was mentioned above that "coöperative antagonism" in nervous action is widespread in the animal mechanism. The illustrations given pertained to the cerebrospinal system and the innervation of skeletal muscles. We must now follow this subject farther and deal with the somewhat similar phenomena presented by the autonomic nervous system. This subject was treated to some extent in the chapter on internal secretions, to the extent, that is, that it implicated the endocrinal glandular system. The presentation here will involve some repetition of what was said in the previous discussion, but the nature and importance of the subject will justify this.

Cannon appears to have been the first to make clear the similarity between the opposing action of the subdivisions of the autonomic nervous system and what Sherrington calls the reciprocal innervation of antagonistic skeletal muscles. "As the above description has shown," Cannon writes, "there are peripheral oppositions in the viscera corresponding to the oppositions between flexor and extensor muscles." [16]

The description referred to is summed up in the statement that many of the viscera and other parts of the body are innervated by either the cranial or sacral, i.e., the terminal autonomics, and also by the thoracico-lumbar or middle autonomic, this double innervation being such as to be statable thus: "*When the mid-part meets either end-part in any viscus their effects are antagonistic.*" [17] The heartbeat is slowed by the cranial autonomic and quickened by the thoracico-lumbar. Contraction of the smooth muscular layer of the stomach and small intestine to produce "tone" is increased on the whole by the cranial autonomic, while gastric peristalsis and secretion are inhibited and the arterioles of these organs are contracted by the thoracico-lumbar.[18] The pupil of the eye is contracted by the cranial autonomic and dilated by the thoracico-lumbar. The lower part of the large intestine is contracted by the sacral autonomic and is relaxed by the thoracico-lumbar. The discharge tube of the urinary bladder is relaxed by the sacral and contracted by the thoracico-lumbar. The blood vessels of the erectile tissue of the external genitals are dilated by the sacral autonomic and contracted by the thoracico-lumbar; and so on.

Now it is especially important, as Cannon says, to notice the kind of service these subdivisions of the autonomic system perform for the organism. On considering the functions of the cranial division, one recognizes that they have to do with bodily conservation. "By narrowing the pupil of the eye they shield the retina from excessive light. By slowing the heart rate, they give the cardiac muscle longer periods for rest and invigoration. And by providing for the flow of gastric juice and by supplying the muscular tone necessary for contraction of the alimentary canal, they prove fundamentally essential to the processes of proper digestion and absorption by which energy-yielding material is taken into the body and stored. To the cranial division of the visceral nerves, therefore, belongs the quiet service of building up reserves and fortifying the body against times of need or stress."[19]

Pasing to the sacral division, one sees that as concerns its distribution to the digestive and urinary viscera, its office is that of accomplishing the discharge of refuse material from the body. Hence, "like the cranial division, the sacral is engaged in internal service to the body, in the performance of acts leading immediately to greater comfort."[20]

So much in illustration of the service of the two end divisions of the autonomic nervous system.

What, exactly, we now inquire, is the nature of the service performed by the thoracico-lumbar division? Since this part is, as already seen, antagonistic in its action to both the cranial and sacral parts wherever it innervates an organ or part also innervated by these end-parts, the inference is readily drawn that its service to the organism as a whole would also be in a sense opposed to the services of the cranial and sacral divisions. Only a moment's reflection is necessary to recognize that at some points at least this is so. For instance, quickening of the heart beat through innervation by the thoracic autonomic nerves, thereby sending the blood stream more rapidly and strongly through the whole body, is clearly opposed, so far as these acts in themselves are concerned, to the slowing of the heart and hence of the blood stream by the vagus nerve, i.e., by cranial autonomic innervation. Now this can mean nothing else than that whereas the vagal (cranial) autonomic action is in the interest of upbuilding and conserving the organism in its whole normal, wonted life, the speeded-up action through the thoracic autonomic is in the interest of some special, more or less temporary need or condition. But obvious and important as is this opposition between the two subdivisions as concerns heart innervation, of not less importance, though less easily observable in their full scope, are the oppositions and reciprocations brought about in the peripheral blood vessels of the whole body through the innervation of the muscles of the arterial walls by the thoracico-lumbar autonomic in connection with the heart action. These innervations, coupled with those of the smooth muscles of the gastro-intestinal canal, of the sweat and probably other peripheral glands, of the smooth muscles of the hairs in mammals, of the adrenal medulla for the secretion of adrenin, and of the liver for releasing stored carbohydrates into the blood, by the thoracico-lumbar autonomic, produce an equilibrating mechanism of well-nigh inconceivable complexity and delicacy, the workings of which are, however, beginning to be revealed to us, both as to details and as to rationale for the life of the organism as a whole.

The whole scheme is adjusted, we may say, for these two necessities of the organism: (1) to serve the organism in its uninterrupted and uninterruptible, and therefore normal, growth and existence; and (2) to secure the perpetuity of the organism through the recurring times of special demand and stress which are inevitable from the external conditions of life under which all living beings exist.

The supreme significance of all this from our standpoint is that we get a glimpse of the means by which an extensive and fundamental number of parts of the organism are subordinated in their special activities to the special needs of the whole. The following from Cannon's chapter, ***Fatigue and Blood Pressure,*** gives concreteness and some particularity to this statement: "In connection with the foregoing considerations [of facts bearing on the value of increased arterial pressure in pain and strong emotion] the action of adrenin on the distribution of the blood in the body is highly interesting. By measuring alternations in the volume of various viscera and limbs, Oliver and Schäfer proved that the viscera of the splanchnic area—e.g., the spleen, the kidneys, and the intestines—suffer a considerable decrease of volume when adrenin is administered, whereas the limbs into which the blood is forced from the splanchnic region actually increase in size. The action of the adrenin indicates the relative degrees of sympathetic innervation." [21]

This last sentence is, of course, what specially concerns us in this discussion. Continuing, and having in view his own researches into the beneficial effects on fatigued striated muscles of adrenin in the blood, Cannon says: "At times of pain and excitement sympathetic discharges, probably aided by the adrenal secretion simultaneously liberated, will drive the blood out of the vegetative organs of the interior, which serve the routine needs of the body, into the skeletal muscles which have to meet by extra action the urgent demands of struggle or escape. But there are exceptions to the general statement that by adrenin the viscera are emptied of their blood. It is well known that adrenin has a vasodilator, not a vasoconstrictor, action on the arteries of the heart; it is well known also that adrenin affects the vessels of the brain and the lungs only slightly if at all. From this evidence we may infer that sympathetic impulses, though causing constriction of the arteries of the abdominal viscera, have no effective influence on those of the pulmonary and intracranial areas and actually increase the blood supply to the heart. Thus the absolutely and immediately essential organs—those the ancients called the 'tripod of life'—the heart, the lungs, the brain (as well as its instruments, the skeletal muscles)—are in times of excitement abundantly supplied with blood taken from the organs of less importance in critical moments." And Cannon concludes with the very pertinent remark: "This shifting of the blood so that there is an assured adequate supply to structures essential for the preservation of the individual may reasonably be interpreted as a fact of prime biological significance." [22] Indeed so! Even more significant, I believe, than Cannon has shown us—as we shall see in a later chapter. But the limitations set at the outset of this chapter, namely that of dealing only with neural processes strictly, especially as these manifest themselves in reflexes, permits us to go no farther than to call attention to the fact that while Cannon finds "the most significant feature" of the reactions he has studied to be "that they are of the nature of reflexes—they are not willed movements, indeed they are often distressingly beyond the control of the will," [23] he yet has coupled them in the most positive way with the emotions, especially with those of fear and rage, and with pain. Otherwise stated, he has coupled them, more definitely than this has been done before, with the conscious psychic life of the organism.

Concluding Remarks on the Significance of Neural Integration for the Organismal Standpoint

Perhaps enough illustration and general discussion have been presented to convince the reader not only that "the nervous system functions as a whole" [24] but that this functioning is strictly subservient to the needs of the organism as a whole, whether the normal individual, living normally, or the normal individual living under special stress, be considered. It is hoped the reader will not have failed, despite the brevity and inadequacy of the presentation, to perceive the fundamental truth that the organism's totality of activities, executed to so large an extent through the agency of the neural mechanism, are in turn subordinate to the needs of the organism *as related to its natural environment*. This is only another way of expressing the truth, which has become almost trite since the idea of evolution has been an essential part of biological philosophy, that all, or at least by far the major part, of the activities of organisms are adaptive.

In connection with no other organ-system than the nervous does the truth come out so patently that the unity and wholeness of the particular *system*, the unity and wholeness of the *organism*, and the *adaptiveness* of the organism to its environment are bound together inseparably; indeed, may almost be called different aspects of one and the same truth of living beings. The very conception of adaptation, at least as touching neural activity, seems dependent upon the correlatedness, the unitedness, of the differentiated parts. The conception of adaptation, so far at least as reflexes are concerned, depends essentially on the mode of relation of parts. It is non-existent except through a particular form of integration.

Every specific act of every part of the nervous system is primarily in the interest of some other part and function of the organism than itself.

This is particularly true for all adaptive acts of the system.

Considerations of this sort seem not only to justify, but to render necessary some such view of purpose in the organism as the following: "The infinite fertility of the organism as a field for adapted reactions has become more apparent. The purpose of a reflex seems as legitimate and urgent an object for natural inquiry as the purpose of the colouring of an insect or a blossom. And the importance to physiology is, that the reflex reaction cannot be really intelligible to the physiologist until he knows its aim." [25] No biologist whose mind is both open and reasonably penetrating will hesitate to accept these views as correct.

What I wish to call particular attention to is that the last sentence quoted calls for an additional clause: "the reflex reaction cannot be really intelligible to the physiologist until he knows its aim," *and he can know its aim only by considering it in the light of the organism's entire complex of normal activities;* i.e., in accordance with the conception of the organism as a whole.

REFERENCE INDEX

1. Sherrington 6
2. Sherrington 12
3. Sherrington 12
4. Sherrington 38
5. Sherrington 8
6. Donaldson 605
7. Sherrington 16
8. Sherrington 8
9. Sherrington 116
10. Sherrington 145
11. Sherrington 146
12. Sherrington 151
13. Sherrington 136
14. Sherrington 136
15. Sherrington 205
16. Cannon, W. B. ('16) 35
17. Cannon, W. B. ('16) 34
18. Cannon, W. B. ('14) 262
19. Cannon, W. B. ('16) 31
20. Cannon, W. B. ('16) 34
21. Cannon, W. B. ('16) 107
22. Cannon, W. B. ('16) 108
23. Cannon, W. B. ('16) 185
24. Sherrington 114
25. Sherrington 236

Chapter XXI

IMPLICATIONS OF THE TROPISTIC AND SEGMENTAL THEORIES OF NERVE ACTION

HAVING now carried our study of the nervous system far enough to enforce the conception that the very essence of this system, even the wholly reflex aspect of it, is its unifying, its integrating office for the organism, it remains to see what the elementalist mode of reasoning can do with the system in this aspect of it.

As in the case of internal secretions, we can get at this subject in no way better than through the writings of Jacques Loeb. This way of approach will be seen to be the more advantageous when Loeb's long-continued and distinguished experimental studies on some of the activities of several kinds of lower animals are borne in mind.

Neglect of the Works of Sherrington and Cannon by Jacques Loeb

Despite Loeb's express statement in the preface to *The Organism as a Whole* that the volume is intended to be a companion to *The Comparative Physiology of the Brain,* on which account a discussion of the central nervous system is omitted from *The Organism as a Whole,* the omission seems very unfortunate. Much important work on the physiology of the nervous system, especially on its integrating function, has been done since *The Comparative Physiology of the*

Brain was published,* Sherrington's really epochal book being especially notable among these later productions. Some of the most fundamental views and arguments presented by Sherrington and others are irreconcilable with conceptions which are at the very center of the elementalist philosophy, and which find repeated expression in Loeb's writings on the nervous system. It is, consequently, a matter of surprise and disappointment that a volume devoted expressly to the organism as a whole should leave almost unmentioned the very organ-system which is most distinctive of the organism thus viewed, especially since this leaves untouched clearly formulated theoretical issues of cardinal importance.

Likewise the disposal of Cannon's work, upon which we have drawn so largely in the preceding chapter, by five lines in *The Organism as a Whole* [1] is truly astonishing!

Indeed, so striking is the defectiveness of *The Organism as a Whole* in this respect that one can not help recognizing that despite its author's statement in the preface as to his intentions, the omission amounts to an evasion even though not so intended.

However, the expressed statement by Loeb of the place he wishes the *Comparative Physiology of the Brain* to hold in his interpretation of the organism has great usefulness to us: it is tantamount to an assurance that the views set forth in the *Comparative Physiology* have undergone no essential change because of the discoveries and arguments produced since the publication of that work. So our examination of Loeb's position on this important matter must concern itself largely with facts and views contained in the *Comparative Physiology of the Brain.*

* The copyright of *Comparative Physiology of the Brain* bears the date 1900, while *The Integrative Action of the Nervous System,* by Sherrington, than which few more important books on the system have been written, was copyrighted in 1906.

The Real Importance of Loeb's Conception of the Nervous System

First of all, it is with genuine satisfaction that I recognize the eminent service rendered physiology and general biology by this author's experimental investigations on animal activities. I have long considered that one of physiology's foremost achievements is the clarification of the conception that nerve phenomena, the most characteristic features of which are response to stimuli and the conduction of the effects of stimulation, are not wholly unique attributes of nerve tissue, but are elaborations of attributes inherent in all protoplasm. Perhaps no single physiologist has contributed more to this clarification than has Loeb. Particularly important has been his demonstration that nerve centers in the sense of ganglionic masses which determine reflexes are not fundamental to the coördinated and adapted movements of animals; that reflexes take place in many animals normally and in many others experimentally, where no such centers exist, as for example in the ascidian after the single ganglionic mass is cut out.

The pertinacity and technical skill with which Loeb has followed up these ideas are highly commendable. By focussing attention on the fact that since plants have no nervous system, they are, in so far as they exhibit movement induced by stimuli, illustrations of the principle of protoplasmic response in actually differentiated organic beings; and by carrying the same conception over into the animal world and demonstrating that many activities here also are dependent upon direct protoplasmic response and not necessarily on a specially differentiated ganglionic mass, he was led to formulate the tropism theory. This theory, as we shall show presently, has a significance which Loeb himself seems not to have fully comprehended, his general standpoint being unconductive to such comprehension. The theory must, consequently, hold a large place in our effort to see what Loeb as a representative of the elementalist school in biology is able to do with the integrative action of the nervous system.

Let it be understood that with the general controversy which has gone on so long and warmly concerning the value of the tropism theory for explaining animal behavior, we are little if at all concerned in this discussion. The point of cardinal importance for us, and the thing about the theory which, as mentioned above, Loeb and his followers seem not to have perceived clearly, is the fact that the theory is really dependent for so much of validity as it has *on the conception of the organism as a whole.* In other words, the theory is in essence an organismal and not an elementalistic theory. Hence it results that since Loeb is the author and chief proponent of the tropism theory, but is at the same time an extreme elementalist, his general position as an interpreter of animal activities and the nervous system contains fundamental contradictions. Indicating these contradictions and exploring, though incompletely, the course along which the truth lies, will form a natural conclusion to our study of the integrative office of the nervous system.

The Organismal Character of Tropisms Partly Recognized By Loeb

I first call attention to the fact that Loeb himself has seen, though "as in a glass darkly," the organismal character of the tropism theory. Speaking of the lack of a sharp boundary between reflexes and instincts, he says some authors are wont to speak of reflexes when the responses to stimuli pertain to single parts or organs, but they "speak of instincts where the reactions of the animal as a whole are involved (as is the case in tropisms)." [2]

And the same idea comes out still more definitely in a proposal to discard the term reflex as applied to such a response as the clasping act by the arms of the decerebrated male frog, and substituting the term tropism. "It is better," he says, "to call them tropisms since the organism as a whole is involved." [3]

But Loeb's most illuminating recognition of the organismal character of tropisms is found not in any positive acknowledgements but in his statements of what a tropism consists of essentially. This he has told us many times and in varied phraseology during the last twenty-five years. The following from *The Organism as a Whole* will serve well as the basis of our consideration: "Animals possess photosensitive elements on the surface of their bodies, in the eyes, or occasionally also in epithelial cells of their skin. These photosensitive elements are arranged symmetrically in the body and through nerves are connected with symmetrical groups of muscles. The light causes chemical changes in the eyes (or the photosensitive elements of the skin). The mass of photochemical reaction products formed in the retina (or its homologues) influences the central nervous system and through this the tension or energy production of the muscles. If the rate of photochemical reaction is equal in both eyes this effect on the symmetrical muscles is equal, and the muscles of both sides of the body work with equal energy; as a consequence the animal will not be deviated from the direction in which it was moving. This happens when the axis or plane of symmetry of the animal goes through the source of light, provided only one source of light be present." [4]

Reduced to its lowest terms, the tropistic mechanism described here and in many other places consists of: first, portions of the body surface having specific irritability, as receptor organs, these being usually paired and usually symmetrical, though often unsymmetrical; second, muscles as effectors, almost always paired and usually symmetrical; third, nerves, variously constituted, as conducting paths connecting more or less directly the sensory areas with the muscles. In a word, tropistic movements are determined by a definite number of definite kinds of parts or organs, definitely arranged with reference to one another and to the whole; that is, upon an organized body called a living being, or briefly an organism. A *morphological entity* is fundamental to the tropism conception. Tropisms are *explained*, partly, by these *organizations.* Loeb does not avoid, in fact at times appears not to try to avoid, recognizing this: "The irritable structures at the surface of the body and the arrangement of the muscles determine the character of the reflex act." "The explanation of them [tropisms] depends first upon the specific irritability of certain elements of the body-surface, and, second, upon the relations of symmetry of the body." [5]

The Organismal Character of the Segmental Theory of the Nervous System

Another province in which Loeb has done distinguished work and into which the organism as such constantly obtrudes itself and will not accept relegation to a secondary place, is that of the coördinated action of the central nervous system in those animals in which that system consists of an axial series of ganglia. Here again, as in the tropism theory, the basal perception underlying the segmental theory of nerve physiology is the truth that nerve "centers" as the "seats" of particular activities and functions in the old sense (in the sense, that is, of being the real source of these activities) do not exist. According to the segmental theory the real unit of activity of an animal made up of metameres or segments is the segment itself, the nerve ganglion of the segment being only one element in the complex. The idea is set forth with fullness and perspicacity in the chapters of the *Physiology of the Brain* dealing with worms and arthropods, and especially in the one on the segmental theory in vertebrates.

The starting-point chosen by Loeb for the treatment is the earthworm. It is generally known that this animal is jointed, or segmented, that every segment has a pair of nerve ganglia situated underneath the intestine on the floor of the body wall, and that the pairs of ganglia are all connected with one another fore-and-aft, so that the whole constitutes a chain of ganglia extending the entire length of the body. In addition to this series of ventral ganglia, there is a pair of large ganglia above the gullet at the extreme front end of the creature, these being connected with the ventral nerve chain by commissures. These dorsal nerve masses are usually called the supra-esophageal ganglia, but are often spoken of as the brain.

On the basis of the "center" theory of the nervous system, it was very natural to raise the query: Is the earthworm's brain the "seat", the primal source of the coördinated activities, the crawling and burrowing which make up so large a part of the life activities of these creatures? Loeb's own formulation of the question is good. "Does coördinated progressive movement in which all the segments of the body participate, depend upon the brain?"

The first attempt to answer this question experimentally seems to have been made by Friedländer. Having made a small opening in the side of the body of a living worm and cut out a piece of the nerve cord so that a complete hiatus in the cord was produced, the author fully expected, he tells us, that after the healing of the wound in the body wall, which took place very quickly and completely, the parts of the body in front of and behind the section of the cord would either behave like two separated individuals, or that the front part would draw the rear part passively about. But neither result followed. "Animals from the middle of which a piece of the central nervous system is wholly wanting, crawl exactly like normal animals except as to a small difference to be noticed later." [6] The wave of muscle contraction and other movements, starting at the anterior end, passed over the point at which the ganglionic chain was interrupted and on to the hind end of the body just as though there were no such interruption. "The movements were coördinated exactly in the same way as in the normal animal."

These experiments, together with others that have since been performed by other investigators, overthrow, as Loeb says, "the idea that coördination in these animals is determined by a special centre of coördination which is located in the brain." [7]

But if the brain is not the coördinating center and does not contain it, what and where is that center? The answer is there is no such center. Coördination is not a central matter at all. Rather it is a matter of the working together of many parts, each duly balanced off with many others, and properly subordinated to the whole animal.

The details of the truth thus stated in general terms can be illustrated more advantageously by referring to some of Loeb's own observations on a group of annelids considerably higher in the scale of life than the earthworm. I refer to the pile worms familiar to many persons accumstomed to marine shore fishing. This group of worms, known collectively as nereid annelids, are far more "heady" than earthworms in that the useful body member named head is more definitely set off from the rest of the body than it is in the earthworm, and is outfitted with eye-spots, touch appendages, and so on, wholly lacking in the earthworm. Nereis is much more highly "cephalized" than is the earthworm, so brain centralization would be expected to present features here not occurring in the more lowly worm. When nereids are deprived of the supra-esophageal ganglia their behavior is such as to suggest at first sight support for the conception that the brain is a fundamentally originative and coördinative member. For example, an individual thus depleted will crawl about unceasingly on the sand of the aquarium bottom, without, however, showing any signs of burrowing into the sand, though this operation is very characteristic of the normal animal. This looks as if the brain were indeed the seat of the burrowing instinct. But not so. The burrowing activity can be induced not only in animals deprived of the brain, but even in parts of animals from which the heads have been cut away altogether. Loeb placed a brainless piece of a Nereis on the sand, and as usual it remained quiet. "I then gradually covered the forward end with sand. The rest of the animal immediately began to make the typical movements which the animal makes in forcing its way into the sand. At the same time the glands began to secrete the sticky substance which cements the particles of sand together, forming the wall of the burrow-hole." "But, why," Loeb asks, "does the Nereis not burrow when deprived of its brain? For the simple reason that it makes use of the organs of the mouth in burrowing, and these are amputated with the head. Hence it is the loss of the peripheral head-organs which keeps the decapitated Nereis from burrowing, and not the loss of the brain." [8]

Since the coördinated activities involved in burrowing can be accomplished through the acting together of various superficial and deeper body parts—sense organs, muscles, and so on—without the intermediation of the brain, these peripheral members are the real "seat" of such activities. The author then goes on to inquire what part the brain does play in the creature's normal life. "If we compare," he says, "the conduct of a Nereis whose brain has been amputated with that of a normal worm, the difference seems to be of the same nature as that between an insane and a rational human being. . . . The peculiar irritability by means of which the Nereis draws its head back and moves backward out of the tube depends upon organs which are located in the forward end of the body and whose sensory nerves go to the supra-esophageal ganglion." [9]

One office of the Nereis brain is, therefore, to make a great complex of activities normal, or "of the same nature" as sane activities in "rational human beings." Not the activities themselves but their *normality* are dependent on the brain. This distinction is undoubtedly general, but it is definite so far as it goes. Beyond question to the student who should compare the movements of the normal and the brainless Nereis with sufficient care, the distinction between the two would be as positive, as indubitable, and as definable as that between almost any two complexes of organic phenomena. The unsatisfactoriness of the distinction is not its vagueness but its generalness. It is sufficiently definite though not sufficiently analytic.

Just what the brain does to normalize the activities; in other words, how the change of character of the worm's behavior—its "restlessness" and general insane-like conduct—results from the absence of the brain, is not known. Loeb makes a suggestion toward an explanation, but recognizes what he offers as nothing more than a suggestion; so to go into it is not worth our while.

But there is an obvious usefulness of the ganglia and whole neural chain in such animals aside from the general one of normalizing behavior. It serves as a common conducting and relay system between the peripheral sense organs and locomotor muscles. Loeb's statement of the matter is concise and may be adequate: "The central nervous system does not control response to stimulation; it merely serves as a conductor from the point of stimulation to the muscle through which weaker stimuli may pass, and pass more rapidly than would be possible if the muscle were stimulated directly.

"In the Annelids each ganglion is a relay station for the sensory and motor nerves of the corresponding segment. If the head exercises a stronger influence upon the behavior of the animal than any other segment, as in Nereis, for instance, I believe it is due to the fact that in the oral end more kinds of irritability are present and more peripheral organs are differentiated (sense organs, mouth, etc.) than in the other segments." [10]

Loeb has done good service in bringing together the evidence for, and emphasizing the truth that, the segmental make-up of the vertebrate head, so long and so ardently studied by anatomists and embryologists, is as important a matter for neural physiology as it is for morphology, and that as a matter of fact the vertebrate spinal cord works more or less on the segmental principle, and in this respect falls into line with the central nerve axis of annelids and arthropods.

The first, or one of the first, investigators to recognize this truth was Schrader, in his studies on the activities of frogs whose brains had been wholly or partly removed. The frequently quoted and later fully confirmed statement by this investigator is of prime significance for the general truth of what we might call fore-and-aft neural integration in vertebrates. "The series of experiments we have given teaches us that the central nervous system of the frog can be divided into a series of sections, each of which is capable of performing an independent function. It brings the central nervous system of the frog into closer relation with the central nervous system of the lower forms, which consist of a series of distinct ganglia that are connected by commissures. It speaks against the absolute monarchy of a single central apparatus and against the existence of different kinds of centers, and invites us to seek for the centralization in the many-sided coupling of relatively independent stations." [11]

As statements much at variance from this concerning the "central" function and power of different parts of the frog's brain are not uncommon, and as the facts are specially important for us, it is desirable to consider the evidence a little more in detail. To this end the following from Holmes may be taken as an authoritative summing up of results so far reached in this field. "A frog with the brain removed as far back as the medulla is still capable of performing regular leaps and swimming movements of the limbs. When thrown on its back it rights itself, and still performs compensatory motions when tilted or rotated. Breathing is normal, and the animal swallows pieces of food that are placed in its mouth."[12]

If the removed portion of the brain is made yet larger, by including the anterior part of the medulla, the animal is still capable of performing a long series of regular activities. "Locomotion is effected mainly by creeping, but the frog is nevertheless capable of springing in the ordinary manner. In the water it swims by alternate movements of the limbs. . . . The breathing and swallowing reflexes are still normal, but the croaking reflex is no longer performed. The reflex of snapping at food is not destroyed. . . . If a piece of meat is rubbed against the frog's nose, the animal snaps at it and uses the fore legs to stuff it into the mouth. The same reaction may be brought about by using the finger, but after the finger is seized and it is found that the object is too large to be stuffed into the mouth, the frog begins to reject the morsel, and uses the fore legs to push it away. Truly a remarkable combination of reflexes!"[12]

And further: "There is no center for coördinated locomotion in the medulla. Disturbances of locomotion begin with the fore limbs. If the medulla is cut across at the tip of the calamus scriptorius, the animal sinks on its breast, and the fore limbs are for a considerable time helpless, although the hind limbs are capable of performing vigorous coördinated movements. The reason for this is that the injury lies so near the region from which the nerves of the fore limbs arise that the movements of these members are very naturally affected."[13] It is the interdependence fore-and-aft of reflex activities like those here indicated, that is, of the fore limbs acting with the mouth, and the hind limbs with the fore limbs, that suggested to Loeb the idea of "chain reflexes," a conception of special interest from our standpoint.

That the segmental mode of functioning of the vertebrate nerve axis is not limited to the lower portion of the great vertebrate phylum is, as Loeb points out, shown by such investigations as those of Goltz, and of Goltz and Ewald on the dog. These are given so fully in the larger text-books of physiology and are thereby so readily accessible to all specially interested in the subject, that a very brief presentation will suffice.

Two main groups of reflex centers in the nerve cord are made out, the cervico-thoracic and the lumbo-sacral. The first group contains the center for movements of the anterior limbs, for respiratory movements, for acceleration of heart action, for dilation of the pupil of the eye, and for several other activities at the anterior end of the body. The lumbo-sacral group contains the center of movements for the posterior limbs, for control of the anal sphincter, the activities of the generative organs, and several other parts.

The existence of such centers in the dog's cord was proved by section of the cord at different levels under the most careful operative conditions. Had the investigation stopped with experiments of this sort the facts brought out might have been interpreted as confirmatory of the "seat" notion of a nerve center. When, however, it was discovered that a dog from which most of the cord is removed may live for many months in a good state of health, all the vegetative functions being carried on almost typically, it became clear that the various centers of the cord are not the seats of life activities in any such fundamental sense as was earlier supposed.

For one thing the results seemed to show that the sympathetic nervous system shares with the central system more profoundly than had been known before, in determining life processes.

The conclusions which flow with apparent certainty from the observations are well summed up in the following: "Undoubtedly all such activities [of visceral life] may subsist and function in a comparatively normal fashion after removal of all spinal influence. The office of the spinal system in regard to the visceral life seems to consist in endowing these functions with greater energy, and in conferring greater stability and more solid equilibrium on the general constitution of the animal." [14]

The reader will hardly fail to recall, on reading this statement about the function of the mammalian spinal cord, Loeb's statement, quoted some pages back, about the normalizing function of the Nereis brain. "Stabilizing" the dog's function means much the same as "normalizing" the annelid's functions. Nor should the reader neglect to notice that he might substitute the word "integratedness" for "equilibrium" in the quotation without change in the essential meaning of the sentence.

Critique of the Elementalist Attempt to Interpret Tropistic and Segmental Theories of the Function of the Nervous System

We now return to the central point of the present subtopic, namely that of what a genuine bio-elementalist is able to do when confronted by the facts in possession of modern physiology bearing on the tropistic and segmental theories of the nervous system.

First we are compelled by the evidence to recognize the general soundness of the doctrines. Second, we recognize that probably no biologist has dealt with the conception more comprehensively and illuminatingly than Loeb. Third, we find that his espousal and able treatment of the theories has led him into positions so thoroughly organismal in both essence and expression as to be quite irreconcilable with his own more general elementalistic philosophy.

It remains to point out more specifically than we have wherein the organismal implications of his teachings as exemplified by his writings constitute a refutation of the elementalistic implications of his teachings as exemplified by his strivings after an "ultimate explanation" of organic phenomena in the terms of physics and chemistry.

The essence of the irreconcilability of the two positions may be put into a general form thus: in innumerable statements and definitions found in his discussions of these theories, Loeb is compelled to introduce the *organism* either as a whole or in considerable portions, as a causal explanation of particular phenomena with which he deals; and the compulsion to such introduction makes it impossible for him or any one else to dispense with the causes thus introduced by resolving them into ultimate elements of any sort, whether organic or inorganic.

Illustration and justification of this general statement must be given. The following typical sentence may introduce the discussion: "The irritable structures at the surface of the body, and the arrangement of the muscles, determine the character of the reflex act." [15]

Notice what it is that "determines" the character of the act. "Structures" and "muscles" do it, these being "arranged" so-and-so, the irritable structures definitely on the surface of the body and the muscles within the body. The point needing special attention is that not chemical compounds or even living substances but *structures*—organs—and these *arranged;* that is, entities which neither exist nor can exist except through the agency of an organism, enter into the definition.

That the structures referred to and all others of the organism are composed of chemical substances and nothing else, is beyond question. Why, then, does the author not bring into the definition this truth about the ultimate make-up of the structures? The central aim of his researches on animal activities being what they are—"a physico-chemical analysis of behavior," is it not surprising that he should be satisfied with even a working description or definition of a reflex act which takes no notice of the results of such analysis? If the understanding of complicated life phenomena consists in resolving them into their simple elementary components (see the first sentence of the *Physiology of the Brain*), then a definition of a tropism that would contribute largely to an understanding of it should not be content with such proximate constituents as "irritable structures," "muscles," and so on, but should go right back to the ultimate physico-chemical elements.

The typical elementalist answer to these restrictive criticisms is well known. It is, substantially, that the understanding and hence the definition of tropism, or for that matter of any other life phenomenon, is final or ultimate only when expressed in physico-chemical terms. The criticism suggested concerning the mere proximateness and hence inadequacy of a definition that uses such terms as "structures," "muscles" and "body surface" is allowed to have a large measure of validity, the usage being justified mainly, it is claimed, on practical grounds. The morphological conceptions involved, it is held, are so strongly intrenched in biological terminology, indeed are so necessary in a historic and subsidiary sense, that it is very inconvenient, if indeed it is not impossible, to dispense with them. In a word, the contention is that while morphology and general physiology are necessary, their necessity is secondary or subsidiary to physics and chemistry. But what should be seen in this with the greatest possible clarity is the error, the fallacy, not of *objective fact* but of *reasoning.* Stated in general terms, the fallacy consists in the tacit assumption that the indispensability and adequacy of physico-chemical elements to explain organic behavior are of a sort that exclude the indispensability and adequacy of "structures," as sense organs and muscles, from the explanation of the same phenomena.

To go into the epistemological and logical necessities involved in the situation with which we are here confronted is entirely beyond the scope of this volume. However, in the interest not so much of Truth in the abstract as of healthy, wholesome, useful science, biologists will have to cease employing such deprecatory epithets as "anthropomorphism," "metaphysics," "rhetoric" and the like to gain for themselves a sense of security in the use of language and reasoning which can not endure for an hour the searchlight of really careful thinking and expression.

To focus the general statement of the fallacy on the particular matter in hand, one must see that both factually and epistemologically the organs and other morphological and general functional elements, or factors found by the analysis of the organism, are "fundamental" or "ultimate" for the phenomena to be explained by exactly the same criteria that the physico-chemical elements are fundamental or ultimate.

A reflex act or a trophism can no more be intelligibly expressed or understood or conceived as an objective fact without sense organs, muscles, etc., than without physico-chemical substances. If one questions the truth of this affirmation let him test the matter by trying to express a reflex act in the terms of the physico-chemical elements known from analysis to be "behind" such an act. To begin with, he finds it necessary to fix upon *some particular* reflex act, scratch-reflex, perhaps. Such particular act must be taken as the starting point, otherwise the experimenter will be committed to the Idea of the act rather than the act itself. And I take it for granted that no truly present-day biologist is willing, even though he may not be able to justify fully his unwillingness, to commit himself to that ancient position. Very well then, the scratch-reflex is going to be expressed in terms of the oxygen, nitrogen, carbon, phosphorus, and so on, which we are sure are "behind" it. Having recognized the necessity of starting with some particular reflex act, and having settled upon the scratch-reflex, true to his recognition of the necessity for particularity he sees he must go still further in the same direction. If his analysis is to be exhaustive and his expression adequate and final, it will have to particularize still further and concern itself with the reflex of some particular animal species, very likely even with some single individual or small group of individuals. A vast mass of evidence makes it almost certain that a dog's scratch-reflex is different from a cat's, and both are different from an ox's, a frog's, and so on. Suppose, then, the dog's reflex settled upon, and defined so fully that it is distinguished from every other reflex whatever. The student is now ready for his main undertaking, that of expressing the dog's scratch-reflex in terms of the physico-chemical elements to which the reflex is reducible.

The next step is to examine the chemical elements concerned, as these are treated in inorganic chemistry, for the purpose of seeing what their "terms" are; that is, what their attributes or properties are, the special purpose of this examination being to ascertain what terms, i.e., what attributes, answer to, or correspond to, and so can be used to express, the dog's scratch-reflex. The outcome of this examination is unequivocal. It finds no terms, no attributes whatever, which by themselves suggest even remotely the reflex under consideration.

But since the examiner has satisfied himself that no other *simple elements*, material or immaterial, are "behind" the reflex, and since the reflex is an indubitable reality, there is no escape from the conclusion that something other than the original inorganic simples must have intervened between these simples and the reflex. And what is that something? Does common experience and common sense hesitate with its answer? If it does its hesitancy is probably from surprise that so obvious a matter should be made the subject of serious questioning. "The dog is what has intervened between the chemical simples and the reflex." That is what common experience must answer and will unhesitatingly answer once it recovers from its surprise at being questioned on a subject so open and daylight clear.

But then science comes forward with its criticism of this common-sense answer. There is no gainsaying, it admits, the truth of the naïve answer thus given. But this answer, science says, is a mere truism. It leaves the case just where it was before science began its analysis, so is worthless for scientific purposes, however useful it may be for ordinary purposes.

This rejoinder by science must be looked into carefully; otherwise its weakness will be missed. It must be exacted of science that she show more explicitly than she has what she means by explaining the dog's scratch-reflex by referring it to the physico-chemical elements at the basis of the act. Let us, we must insist, hear you express a dog's scratch-reflex in the terms of oxygen, carbon, *et cetera*.

That such expression is possible is freely granted. But how can it be done? That is a key question. It can be done in one and only one way, namely by adding to the attributes, that is, to the "terms" which inorganic chemistry recognizes in the chemical elements concerned, just those attributes and terms which the dog's scratch-reflex requires in order that the elements may explain the reflex. We can say that besides the specific gravity, combining weight, and other well-known attributes of the elements, they possess dog's-scratch-reflex attributes. But, the name thus suggested for the newly discovered attributes being cumbersome, we may devise for them some term more simple and convenient—for example, do-sca-re-x, *doscarex.*

In virtue, then, of the doscarecious powers of oxygen, carbon, and so forth (the fact that these powers can only be assigned in the lump and not distributively to the several elements should not be lost sight of though it may be neglected for the present argument), a complete physico-chemical explanation of the phenomenon under consideration is reached.

Does this discussion advance the interpretation, the understanding of biological phenomena beyond the familiar sarcasm about explaining drug-induced sleep as due to a dormitive principle of the drug? Yes, I maintain, it does, because in place of a "principle" attached to no particular drug but to any sleep-producing substance, the doscarecious powers recognized by us are definitely assigned to the chemical elements known to be the *sole* constituents of organisms. The new powers take their place perfectly, definitely and positively among the other attributes of the elements, the assignment being based on the solid ground of analyses, laboratory and other, made through years of scientific research. But an exceedingly important point which comes in sight here is that though these doscarecious powers are proved to be real ones, they are latent and wholly unknown to inorganic chemistry for the reason that they never manifest themselves under any other combination of things than just that which in its totality Zoology has named dog. A dog, and a dog only, is able to cause oxygen, carbon and the other elements to reveal these particular scratch-reflex powers. The dog comes in as a *sine qua non* to the production of, and hence to the causal explanation of, the particular group of activities under consideration. We have no evidence that the chemical elements operating themselves can actualize their own latent doscarecious powers.

The reader will hardly fail to connect what we are saying with the familiar phenomena of the assimilation by organisms of nutrient substances. All our argument really does to the usual conception of this phenomenon is to focus attention upon the fundamental importance of the *individual organism* as a factor, as a cause, of the chemical transformations wrought in the nutrient substances. The insufficiency of statement of the assimilative, or anabolic, or synthetic aspect of the metabolic cycle in the organism, lies in its failure to bring out clearly enough the indubitable fact that the final results are innumerable activities and substances which pertain solely to the living, normal *individual*—which are strictly personal and private, as one may say.

The current mode of expression according to which the assimilative syntheses take place *in the organism* is quite misleading in that it permits or even encourages a conception that the syntheses have a measure of detachment from, and independence of, the life of the organism as a working unit, which as a matter of fact they do not have. Again, the usual statement that the syntheses result in organic *substances* of more complex, higher make-up is inadequate in that it diverts attention from the fact that these new substances belong to, are fundamentally part and parcel of the organism. They are not "any old organic substances" but are exactly those substances necessary to maintain the normal life of the particular individual organism. Hence despite the indubitable fact that the final results are reached by way of innumerable purely physical and chemical operaations, the organism itself, acting as an integer no matter how complex, is always to the fore as a controlling, dominating factor.

The Bearing of This Critique on "Analysis" in Biological Reasoning

The considerations thus briefly set forth lead to certain still more general ideas of the utmost importance. The natural entities to which we apply the descriptive terms living and organic are specially distinguished by the chemical and physical syntheses which they accomplish by virtue of their inherent constitution. So as concerns the most characteristic of these syntheses, especially the chemical ones, they are to a very considerable and fundamental extent definitive of the organic individual, species, genus, and so on, of taxonomic biology. This is equivalent to saying that the synthetic operations regarded each by itself terminate in results which are in large measure unique and so unforeseeable from anything we know about the original elements *as such;* that is, before they have actually been subjected to the particular synthetic transformations under consideration.

And this again is equivalent to saying that synthesis—transformatory synthetic processes and products—is more distinctive of living beings than are analytic products and processes.

Finally, it follows from these facts about the synthetic nature of organisms, and from the established principles of thought, that analysis alone is incapable of interpreting, of understanding organic beings. No natural object which in its nature is more distinctively synthetic than analytic can be understood by knowledge-processes which are more analytic than synthetic.

This conclusion goes to the very heart of the elementalistic position, and, as stated in the discussion on internal secretions, is really as much an epistemological as a biological problem.

Reverting again to Loeb's writings, the conclusion to which we are forced joins issue with the very opening sentence of the *Physiology of the Brain*, as we have already indicated. "The understanding of complicated phenomena," particularly as presented by organisms, can not be accomplished through "an analysis by which they are resolved into their simple elementary components." Unquestionably analysis is essential to, but it is not adequate for, full understanding of the phenomena.

The very nature of organic synthesis and of knowledge-getting precludes the possibility of attaining the kind and degree of understanding of organisms which elementalist biology claims to have attained and promises to attain.

"A complete explanation of life in terms of physics and chemistry" is impossible for the sufficient reason that physics and chemistry *as such* do not contain any of the really distinctive *terms* of life. Those terms can only be brought into physics and chemistry *after* and not *before* the phenomena of life have been searchingly scrutinized; that is, analyzed and found to involve physics and chemistry. The *terms* of life are in the original data on the phenomena of life, and no sort of analysis can possibly make this otherwise.

Our position, it should be noticed, touches the hackneyed controversy over vitalism and materialism only in so far as the course of reasoning we have pursued involves the recognition that each organism (the dog, for instance) is a natural object possessed of certain causal powers, by exactly the same logical and epistemological criteria that any simple chemical element or chemical compound is a natural object.

Stated in a brief and common-sense way, our contention is that the attributes which make a dog a *living* body are no less natural than are the attributes which make carbon a *chemical* body. The tiresome and meager-fruited controversy between Materialists and Vitalists may be characterized as due to the fact that neither party has taken the trouble to establish clearly, even in their own minds, the meaning of the word *natural.* As a consequence of this slipshodness the two groups agree tacitly, in treating the inorganic and the organic worlds as though *natural* does not mean the same thing in the two realms. The implication is that if the inorganic world, for instance, be held to be natural by both parties, for the Vitalists the living realm is largely *super*-natural, while for the Materialists the same realm is largely *infra*-natural.

Theories of Animal Behavior in Relation to the Science of Zoology

This somewhat protracted though wofully insufficient treatment of neural integration may close with a brief section on some of the still larger biological and methodological implications of the conclusions reached. Special attention is called to the fact that the culminating part of our argument has involved data and conceptions which are as unequivocally zoological, morphological, and physiological, as any of the data and conceptions are unequivocally physical and chemical. Physical chemistry, or any other aspect of inorganic chemistry, is utterly powerless, so far as we can see, to discover such facts, as for example, that oxygen, carbon, nitrogen, etc., possess latent "doscarecious" powers. This final section is in the direction, consequently, of establishing the parity, to claim the least, of zoology, botany, morphology, and general physiology, with chemistry and physics, in the great complex group of biological science.

We may first allude to a favorite mode of expression of materialistic elementalists. Whenever fuller analysis has proved some group of animal phenomena not hitherto connected directly with physico-chemical substances and forces, to be in reality dependent on such agencies, this school is wont to remark in substance that investigation has finally "transferred" the phenomena from the provinces of zoology, morphology, general physiology and the other sciences of animal life, to physics and chemistry. Our argument puts beyond question the *logical* inadequacy of such a statement. Analysis does not by any means *transfer* the phenomena from zoology, etc., *into* physics and chemistry. Neither analysis nor any other agency can any more take the study of animal phenomena away from zoology and put it into physics and chemistry than it can take bread-making away from the baker's art and put it into physics and chemistry. The chemist may undoubtedly take to bread-making and find that his new employment has much in common with his old; but in so far as he really succeeds at the new, he is more a baker than a chemist. He has not transferred bread-making to chemistry, but if anything has done just the reverse. What analysis actually does in these cases is to *extend the bounds* of physico-chemical forces and laws into zoology, morphology, etc., and to prove that *if* zoological, morphological and physiological undertakings are to move into ever greater fullness, aid from physics and chemistry is indispensable.

Thus critical examination of the reasoning of elementalist biology reveals the *logical fallacy* in any sort of statement which involves the assumption that the older sciences of organic beings, like taxonomic botany and zoology, geographical distribution, morphology, general physiology and so on, are not and never can be relegated to places of minor or secondary importance in biology. But it is the *practical harmfulness* of such assumptions rather than the logical fallacies underlying them which chiefly concern us in this volume, and no part of our whole subject is more vitally affected by such harmfulness than this of the behavior, even the purely tropistic behavior, of animals. The whole round of animal biology—attitude toward research problems and undertakings, valuations and interest in different fields of knowledge, educational aims and method—all are affected.

In the light, for instance, of such a complex of animal behavior as that presented by the northern fur seal, its mating, breeding, migrating and other habits, the contention that the myriads of complicated phenomena which go to make up animal life can be understood by converting zoology into a laboratory and experimental science, to the end of analyzing the phenomena into their "simple elementary components," is so ludicrous as hardly to need argumentative refutation. Indeed, it seems as though persons obsessed by a theory to the extent of being impervious to the ludicrousness of the contention, are likely to be also impervious to the true reasoning involved in it. It may, I think, be assumed that so much of zoology as has formed this remarkable conception of itself will before long drop into the background by scientific gravitation despite its present great vogue.

One of the leading motives, consequently, of this constructive part of my enterprise is to establish the essentiality of general zoology and its time-sanctioned departments on so solid a basis of philosophic reasoning that the necessary methodology of the regenerated science of the future will be clearly seen in broad outline.

If the considerations inadequately presented in these last pages and in other parts of this volume once get secure lodgment in biological thought it will become manifest that the "behavior" of any animal species (as of the fur seal, to take at random any one of thousands of species that would illustrate the point quite as well) can mean nothing less to a really scientific biology than the whole series of activities of at least one individual animal from its birth to its natural death. Consequently an "understanding of the complicated phenomena" thus presented can not be secured by any amount whatever of analysis, but only through an endless series of never-divorced analyses and syntheses, this series running on through years of effort by scores of investigators in the field (on the Pribilof Islands, on the Behring Sea and far down into the North Pacific Ocean) and in the laboratory, by general zoologists, mammalogists, anatomists, embryologists, physiologists, comparative psychologists, bio-chemists and physical chemists.

How, let one ask himself, would the resolving of a fur seal's behavior into reflexes as the simple elementary components of that behavior, contribute to an understanding of the annual oceanic migration of the animal or the fighting of the males for the females, if that analysis accomplished nothing beyond proving a certain measure of identity between the reflexes of a fur seal and, for instance, those of an earthworm? Or how would the understanding sought be enhanced by carrying the analysis to a still deeper level —to the physico-chemical level—and discovering just how oxygen, or some particular proteid substance, participates in the reflex? We must never lose sight of what the biologist's task is as regards understanding. It is to investigate for the purpose of understanding the facts presented by *living beings in nature.* The behavior of the fur seal as the animal lives its normal life is what is to be studied in the case chosen, and one of the most characteristic things in this particular behavior is a yearly journey of several thousand miles through the Pacific Ocean. *That* is one of the phenomena to be studied and understood, and no amount of analysis resulting in discoveries which do not apply *particularly* to that particular phenomenon can be admitted as an explanation of that phenomenon. As compared with any of the other natural sciences, biology is preëminently the science of *individuals*—or natural objects which though alike in innumerable attributes are unlike in innumerable other attributes, and in no aspect of organisms do the differentials which make individuals, species, genera and so on come out so importantly as in behavior.

The evidence being now overwhelming that all organic phenomena, including behavior, are dependent upon physico-chemical substances and forces, one of the most pressing questions of procedure in biological research is that of bringing the older and, generally, less exact natural history aspects of the science into closer, more vital coöperation with its newer experimental and more quantitatively exact aspects. Specifically stated, work of the type long prosecuted by exploring expeditions, botanical and zoological gardens, museums, botanical, zoological and biological societies, and government biological surveys; and that of laboratories in the strict modern sense, the morphological, physiological, and bio-chemical laboratories, must join hands more closely and effectively than they have heretofore to insure continued progress in the organic sciences. Several movements of the day in biology could be mentioned whose meaning, viewed from our standpoint, can hardly be mistaken. Perhaps the most conspicuous of these is that congeries of research activities known as ecology. In spite of frequent depreciative comments about ecology, especially because of its indefiniteness as to both content and delimitation, it has the merit—from our standpoint the very great merit—of facing organic nature as it actually is, that is, of having for its subject matter the modes of life of organisms as nature presents them, and hence of recognizing the laboratory as an agency, but only as one among other agencies, for dealing with its subject. As to method, while ecology recognizes the indispensability of the laboratory and experimentation in the narrow sense, it refuses to let such experimentation usurp the whole of its interest and effort.

So our study of the organism's integratedness as exemplified by its activities, that is, by its behavior, and by the mechanism through which these activities are carried on, leads to the somewhat unexpected though entirely natural result which may be summarily stated thus: To gain understanding of the behavior of living beings is admitted by everybody to be the chief reason for investigating such activities. Due consideration of the nature of the activities and of the nature of understanding makes it certain that the phenomena themselves are highly integrative and integrated, or synthetic, and that understanding of them depends as much on synthetic knowledge-getting as on analytic knowledge-getting. Perception of this last truth necessitates, again, a sort of synthesis, or integration, of the numerous research agencies.

REFERENCE INDEX

1. Loeb ('16) 285
2. Loeb ('12) 70
3. Loeb ('16) 284
4. Loeb ('16) 257
5. Loeb ('02) 7
6. Friedländer 363
7. Loeb ('02) 85
8. Loeb ('02) 91
9. Loeb ('02) 94
10. Loeb ('02) 101
11. Schrader 177
12. Holmes ('06) 313
13. Holmes ('06) 315
14. Luciani .... iii, 355
15. Loeb ('02) 5

Chapter XXII

PSYCHIC INTEGRATION

Preliminary Remarks

(a) Absolute Discrimination Between Reflex and Psychical Phenomena Not Necessary

IT will be recalled that in our discussion of neural integration we limited ourselves strictly to those manifestations and activities of organisms which are, so far as observation can determine, strictly reflex, that is, show no evidence of intelligence and volition, or even necessarily of instinct. What we have to do next is to consider the unity, the integratedness of the animal organism as manifested in the vast array of its activities which by universal consent are designated by such terms as instinctive, emotional, volitional, conscious and intelligent.

Be it noted that this aim will not exact of us, any more than did the last, a sharp delimitation between reflex or purely mechanical acts and psychical or conscious acts. Just as in the former discussion we were concerned with the integrative character of those acts which are indubitably reflex, so here our object is to study the integrative or synthetic character of those acts which are indubitably psychical. We shall now be dealing with acts which have unquestionable psychical attributes, that is, show something of individual plasticity and something of correspondence to individual needs; which are, in other words, to some degree individually determined to meet individual requirements either of external or internal imposition and intention.

(b) *The Organism an Original Datum in All Problems of Psychic Life*

Another preliminary remark of high importance concerns the question of what, precisely, it is with which we have to do—of what we start from and what is ever in sight, in the discussion. Our fidelity to the organism, living in its natural setting, as the foremost objective reality in this treatise, prevents us *ab initio* from being satisfied with a Body, and a Mind or Soul, as these have figured from time immemorial in discourse about the higher animals, particularly about man.

If in all the world there is such a thing as objective truth, what we start with and have ever to deal with in studying psychic phenomena, just as in studying all other phenomena of animals, are individual objects or bodies of very particular construction and activity. And by no possibility can consistent thought and statement avoid acknowledging that that vast assemblage of acts and other manifestations which are called psychical are yet only part and parcel of the still vaster assemblage of acts and manifestations presented by the very same living objects, that is, by organisms. Our occupation will be basally with *an* object, some particular organism, having innumerable attributes, which being classified fall rather roughly into two great groups, one of which we name physical or material and the other psychical or spiritual. For short, the physical or material group is called the Body, while the psychical or spiritual group is called the Soul or Mind.

Our discussion, then, will never lose sight of the fact that the acts with which we deal are acts of the *organism* and not of any of its parts merely, whether these be conceived as material or psychical. No matter how far particular acts may be dependent upon, and so explicable by, particular parts, this dependence can not in reality be the whole story, for the sufficient reason that the parts are, finally, non-existent except as derivatives of and dependencies upon the organism, as our whole treatise has abundantly shown.

Discussion of the Nervous System, the Brain, the Cerebral Cortex, Neurones, Reflexes, the Senses, Responses, Emotions, Consciousness, Will, Reason, and so on, as though any of these are now or ever have been or ever will be independent entities, or things to which the organism is subordinate, is from our standpoint one of the deep inadequacies and misfortunes of much biological and psychological thinking. To the whole attitude of the zoological naturalist, who by native endowment and by training is imbued with the spirit of his motto "neglect nothing" in the study of animals, this habit of the special sciences of animal life is intolerable.

The ancient problem of the relation of "Mind" to "Body" is one of those problems which run on endlessly in discussion simply because the partisans of one theory or another never know exactly what they are discussing—never know just where they start from, in what direction they are going, or what the end would be like if they reached it.

Lest this statement be taken as foreshadowing both a right statement and a "final solution" of the problem, I affirm very positively that it foreshadows nothing of the sort. All I hope to do is to add considerably to a clear statement of the problem, to add a little to our comprehension of whither we are going, and to contribute a bit to the "final solution," whatever that may mean.

The real problem of psychic integration formulates itself, for us, in a two-parted way: Given any particular act or action-system* which is unquestionably psychical, (1) how many and what parts of the organism are essentially involved in it? and (2) does the act or action-system bear such

* This phrase I borrow from Jennings (*Behavior of the Lower Organisms,* p. 107) and mention incidentally here that we shall find it extremely useful later on.

relation to other acts or action-systems and to material parts of the organism as to warrant the ascription to these acts of causal influence on other acts and on the material parts?

(c) Provisional Classification of Psychical Facts

A third and final introductory remark touches on the question of what shall be recognized as contained in the organism's system of psychic attributes. Following our regular custom of beginning with the phenomenon under contemplation at its fullest, most indubitable expression, we shall not go far amiss if we accept the time-honored triumvirate of feeling, will, and intellect as the most obvious subgroups of highest psychic attributes; for only a hopelessly sophisticated philosophy and psychology can hesitate to acknowledge that every full-grown, normal, civilized human organism, at least, is at once a sort of reservoir of feeling, sentiment, and emotion; a dynamo of resolution and execution; and a granary of intelligence and reason. (See, for example, *Thinking, Feeling, Doing*, by E. W. Scripture.)

Perhaps the only thing that needs saying about these sub-systems of mind is that our general standpoint aligns us squarely with the tendency in present-day psychology to accept them for what they actually are, striving to become acquainted with them and to assess their importance on this basis. To ascertain first of all the facts on the psychic side of the living animal, then next to interpret, to correlate, to explain these facts, are cardinal principles of procedure in our enterprise. For one thing, as an evolutional zoologist of many years' practice in speculating on how animal parts originated (even those of almost infinite simplicity as compared with the mind of man), I am too familiar with the limitations and pitfalls of the genetic method to be beguiled into making some one theory of the origin of mind the corner-stone of my interpretation.*

Furthermore, as a naturalist faithful to the mandate "neglect nothing," I am in full accord with psychology's abandonment of the earlier supposition that a leading aim of psychology must be to prove that some one psychic province is all-dominant, the others being merely secondary and tributary.

Thus all forms of the theory that the psychic empire is at bottom Intellect, the heaven-ordained monarch of which is Unconditioned Ideation, are incompatible with our standpoint. Something of the weight and variety of authoritative sanction which are pitted against us here is indicated by such names as Descartes, Locke, Leibnitz, Hume, Kant, and Herbart of the proximate past, and Wundt, Royce, Howison and Bradley of the immediate past.

And theories like those of Fichte, Schopenhauer, Hartmann, Nietzsche, and divers pale, fitful present-day lights, which would accord to Will hegemony over the entire psychic realm, are still less tolerable to us.

Feeling has won its rightful place in psychology so recently that there seems little danger of its pushing its claims to position and power beyond reason. Except perhaps in the form of sensationalism, or the theory that all knowledge actually does originate in sensations, psychology of the western world appears never to have attempted seriously the deification † of Feeling as it has of Reason and Will. When

* I count it as one of the pieces of good luck in my scientific career that, through no merit of my own, a technical memoir of mine containing an elaborate theory of the origin of the vertebral column has lain in editorial keeping unpublished for a decade and a half.

† It is possible, as my friend Professor G. M. Stratton suggests to me, that the German philosopher Fr. H. Jacobi, came nearer doing this than any one else. His teachings gave, however, a definite place to positive knowledge, so that, according to Höffding, his faith and his knowledge constituted two distinct philosophies. What he wrote probably does not, consequently, contradict the statement in the text. Jacobi seems not to have exerted much influence on the main current of German philosophy and life.

Thomas Hobbes identified imagination with fancy, "original fancy" with sense, and "decaying sense" with memory, and held sense to be caused by "so many several motions of the matter, by which it presseth our organs diversely," and when he defended the general doctrine that "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," [1] he did indeed blaze a trail which might easily lead to an over-exaltation of the sensuous and emotional side of life. But the eminently practical character of Hobbes' undertaking being remembered (he was writing not for the love of speculation but to save his country from political chaos and misery begotten as he believed from false theories and impossible desires and ambitions) one may expect to find in him elements of steadiness and restraint which would make for safety in speculation.

One such element is clearly seen in his opinion that he who is "born a man" and lives "with the use of his five senses" has all the native equipment necessary to realize the best in him both for himself and for his country.[2] The necessity of being "born a man" is the point to be especially noticed. That, with all it may imply, is on a par with the necessity of using the five senses. So whatever of scientific hobby-riding under such captions as "sensationalism," "empiricism," "associationalism," may have followed in the wake of Hobbes' writings, Hobbes himself, I am quite sure, was at heart a genuine organismalist and is entitled to high esteem as one of the very first moderns to speak strongly for the importance of the body generally to the psychic life of man. Listen to this:

"Natural sense and imagination are not subject to absurdity. Nature itself cannot err; and as men abound in copiousness of language, so they become more wise, or more mad than ordinary." "Between true science and erroneous doctrines, ignorance is in the middle." [3]

Having completed a reconnoissance of the field in which we are to work, of its expanse, its contents, and its main subdivisions, we are prepared to take up the task proper. Approaching it from our standpoint, one naturally surmises that between the organism's neural unity as manifested by its reflexes studied in the last chapter, and its psychical unity known to psychology and to be considered presently, a vital unity of still higher order exists. By unity I mean a unity so intimate, so reciprocating, so mutually constitutive that the term *parallelism*, with the meaning given it in much of recent psychology, is wofully inadequate for it. Such a unity, if it exists, must be sought by inspecting the entire gamut of psychic life, from the simplest responses to stimuli on up through simple reflex responses, the tropisms, the primal affective and emotive responses, through the perceiving, the imagining, the conceiving, the reasoning operations, to the very highest constructive human mental achievings.

Likeness Between Tropistic and Higher Psychic Activity

An important move, starting from the highest phase of rational mind, has been made toward recognizing the nature of this unity. This move is the more significant for our enterprise in that the investigator who has made it is neither an elementalist nor an organismalist, but an eminent subjective idealist. Josiah Royce is the student who has performed this service. Stated in a single sentence, the advance he has made toward discovering the union consists in the recognition of certain fundamental resemblances between some of the very highest operations of man's mind and the pure tropistic operations of lower animals.

Royce's contribution to this subject is contained in his *Outlines of Psychology*, and nowhere else so far as I know. From the preface of this book I gather the following, partly by way of quotation and partly from obvious inference.

"To my mind," says Royce, "an interesting side-light has been shed upon the well-known controversies between the associationists on the one hand, and the school of Wundt and the partisans of 'mental activities' generally, on the other, by the stress that Professor Loeb has recently laid upon the part that what he calls 'tropisms' play in the life of animals of all grades." [4]

Then after telling in a few sentences what tropisms are, Royce continues: "Now it is especially notable that the 'tropisms' of Loeb are not, like the 'reflex actions' of the theories, modes of activity primarily determined by the functions of specific nerve-centres. Furthermore, they are more general and elemental in their character than are any of the acquired habits of an organism." (At this point Royce takes up, for a moment, a matter to one side of my main purpose, namely the problem of "self-activity" and "spontaneity"; so I venture to change somewhat the order and emphasis of his argument.) "Now it has occurred to me to maintain, in substance, that the factor in mental life which Wundt's school defines as 'Apperception' . . . may well be treated, from the purely psychological point of view, as the conscious aspect or accompaniment of a collection of tendencies of the type which Loeb has called 'tropisms.'" [5]

Then we have: "Wundt has insisted that his 'Apperception' is no disembodied spiritual entity. I conceive that Loeb has indicated to us, in the concept of the 'tropism,' how a power more or less directive of the course of our associations, and more general than is any of the tendencies that are due, in us, to habit, or to specific experience, can find its embodiment in the most elemental activities of our organism." [6]

What, now, is the bearing of this idea of Royce's on the main theme of this chapter, the organism's unity as manifested in and influenced by its psychic life? As an initial step toward answering this question, the reader is asked to recur to the chapter on tropistic activities and their anatomical groundwork, recalling that it was the special aim of our discussion to show the inevitable organismal trend of the whole doctrine of tropisms. It should be remembered also that tropisms are, all of them, probably, beyond question adaptive in fundamental nature; i.e., they work in the interest of either the individual as a whole or of the species to which the individual belongs. The circumstance that under occasional more or less artificial conditions the activities of an animal may result in injury to it or even in its death, is not proof that on the whole those activities are not advantageous. A horse or man may make himself sick now and then by taking the wrong kind of food or too much food, but this does not prove eating to be useless on the whole, or non-adaptive.

Another important thing to bear in mind about tropisms is their automaticity, or preferably their intrinsicality. They are rooted in and partake of the very essence of the organism—so much so that they manifest themselves inevitably when the right external and internal conditions are present, whether the general ends which they normally serve are attained in the particular instance or not. The flight of the moth toward the flame, even at the sacrifice of its life sometimes, is a manifestation of a tendency that works, on the whole, for the good of the animal. That the moth follows the impulse even to death merely shows how tremendously deep-seated this type of reaction is. That the activity may result in injury or death in a special case is just because the case is special, i.e., it is a departure from the regular conditions under which the reaction-type became incorporated in the organization of the creature. Being always poised for a particular kind of action, and having a supply of energy to execute the action, are unquestionably among the most distinctive attributes of animal organisms. Such organisms are distinguished from plant organisms not only by the present fact of inherent activity of the animal, but by their inherent preparedness for acting to meet new and more or less unusual situations. This action and action-readiness are the real meaning of the neuro-muscular system. All biotic organization is anticipatory in various ways, but animals are almost exclusively anticipatory in action.

It is just these attributes that Royce recognizes as common ground between certain of the highest psychic activities of man and tropistic activities. With this overplus, and in some cases useless or even injurious activity (instanced by the flight of the moth toward and around the flame), let us now pass to the upper end of the gamut of animal activity for illustrations. A very few must suffice. The first chosen is one of exalted creativeness in art.

From the vast domain of art a more instructive illustration of over-wealth of self-activity can hardly be found than is afforded by William Shakespeare. A recent investigation of his works undertaken with a view to finding what they tell about the "native endowments of the author" and prosecuted with that love for accurate, exhaustive knowledge which is the very soul of modern science, leads to the result that of these endowments "the most outstanding perhaps is his exuberant vitality." This characteristic of the man is exhibited in the "reckless volubility of almost every character, the piling up of fancy upon fancy, of jest upon jest, the long embellishment of humor and foolery and horseplay for no other reason than the delight they afford." [7] And incidentally, the strict individualism of this sort of thing is exemplified by one of these same Shakespearian characters: "Come, come," says Mercutio to Benvolio, "thou art as hot a Jack in thy mood as any in Italy. . . . Nay, an there were two such, we should have none shortly, for one would kill the other." "What has Queen Mab to do with the action of the play of *Romeo and Juliet?* Nothing; but Mercutio mentions her, and before any one can stop him he has poured forth fifty lines of purest fantasy. . . . Whole scenes," this student declares, "exist for no other reason than that the author's brain is teeming with situations and humor and infinite jest." [8]

Now I hear in imagination expressions of astonishment, rising to protest, even to ridicule, on the part of some biologists, and to horror on the part of some literateurs, at the idea of suggesting that there is anything really in common between the two groups of phenomena here placed side by side—the activities of a moth and of Shakespeare! For the moment I do no more in reply than ask the reader to take cognizance of the fact that the whole training and occupation of the naturalist consist largely in comparing all sorts of things, inorganic as well as organic, which to cursory observation seem unlike, for the purpose of finding whether closer and broader examination can not discover resemblances and affinities which may throw light on the ever-insistent problem of origin and causal relationship. From that procedure, and that alone, initially, came the theory of organic evolution. It is the quintessence of the organic method. To him who is so instructed and disciplined that the recognition of likeness and kinship between, for example, the prothallus of the fern and the flowering plant, or between a horse's fore-foot and a man's hand, will receive no shock from the comparison. The intrinsic justification of the comparison will be deferred until we have a few other illustrations before us. Another illustration will be taken from an author, J. J. Rousseau, whose activities stand about midway between art proper and science proper.

"I felt," Rousseau says in his *Confessions*, "that writing for bread would soon have exhausted my genius, etc.," Again: "Nothing vigorous or great can come from a pen totally venal." And finally: "In a severe winter, in the month of February, and in the situation I have described, I went every day, morning, and evening, to pass a couple of hours in an open alcove which was at the bottom of the garden. . . . It was in this place, then, exposed to freezing cold that without being sheltered from the wind, I composed, in the space of three weeks, my letter to D'Alembert on theatres."

If this sort of thing, one may note in passing, is a case of "struggle for existence," the existence struggled for is on the highest plane of psychic life and not on the plane of mere brute continuance.

The only other example will be one of activity in science proper, i.e., "pure intellect" as far as there is such a thing. The case of some man devoting the best of his life to the working out of a great germal idea—an Aristotle, a Copernicus, a Galileo, a Kant, a Darwin will serve our purpose best. Of these we choose the case of Darwin. Consider first the youth and the young man keenly alive to the flood of sense impressions pouring in upon him from external nature, and mentally—"internally"—"restless," as Royce would say, from an undefined though strong dissatisfaction with the stereotyped school and university curricula and modes of dealing with subjects. Later comes the set of environmental influences (chiefly through the naturalist Henley), quite incidental to his regular, prescribed environment, to which he responds with eagerness and effectiveness—an almost automatic choosing of fields of intellectual activity. Out of all these fragmentary and by-the-way experiences—"contents of consciousness"—there is organized a body of natural knowledge, and such definiteness and promise of tendency as to justify an appointment to a post of considerable responsibility and unique opportunity, that of naturalist to H. H. Exploring Ship *Beagle*.

During the voyage and from the new and strange contacts with nature afforded by it, there arises another state of "restlessness," this time concerning the origin of organic species, the "mystery of mysteries," as Darwin himself put it. A matter deserving special notice is that the truly forward, the creative step came *after*, and was conditioned upon, a period of dissatisfaction with the prevalent teaching on the subject. Then the considerable time of semi-constructive observation and thinking and feeling under guidance of the general surmise that species arise naturally and not supernaturally, as all his earlier experiences—"contents of consciousness"—had taken for granted. And at last the final, for him, great conception, the hypothesis of the "struggle for existence" and "survival of the fittest" as a cause of the transformation of species. The suddenness and spontaneousness with which this idea emerged into consciousness should be specially noticed. Once the merest suggestion of it hove in sight, the whole hypothesis formed itself, organized itself, rapidly and completely.

The sense in which the process may justly be called spontaneous is important. Although we well know that the famous hypothesis was suggested by the reading of Malthus' work on population, we know equally well that the most essential features of the hypothesis were *not* contained in the teachings of Malthus. There was something genuinely *new* in the hypothesis. Out of the former total of experiences came that which did not actually exist in those experiences. Although the hypothesis was clearly a product of something which went before, it was a synthetic product in the strictest sense, in essentially the sense that a chemical compound is a synthetic product of its interacting elements, the sense that the most distinctive attributes of the compound can not be found in the elements taken separately, but only *after* the interaction has actually occurred.

We must not fail to consider the long period of Darwin's strict "self-activity" in collecting evidence, pro and con, bearing on his hypothesis; and the activity *designed*, notice, to ascertain whether or not there is a process going on in the outer world of plants and animals corresponding to the process he had *conceived*, i.e., had pictured in his "inner world" of consciousness. The genuineness of the individual, the personal, the unique character of mental life and mental creation can hardly be more strikingly illustrated than by such cases as this of Darwin's when the conception, the hypothesis, is kept to one's self so long in order "to prove" whether it is "true" or not.

Now I want to call particular attention to the indubitable fact that these illustrations are only extreme manifestations of attributes which are universal in the human animal at least. There is no normal human known to anthropology which has not some measure, no matter how small, of creative impulse in art and in science.

As a conclusion to this presentation of instances I must again insist upon one of my cardinal points: that the *individually* active and creative power of the human organism on its psychical side is not a whit less real, less objective, less a natural phenomenon to the natural historian than is the *individually* creative power of physical growth and variation, and reflex and tropistic action. Indeed, the thorough-going, consistent *zoological* naturalist, the substance of whose science is largely animal behavior in all its aspects, can not possibly approve the effort to separate completely the two sorts of creation.

First Move Toward Showing the Organismal Character of the Higher Psychic Life

Now for the further scrutiny of such psychical facts as those typified by the examples presented, for the purpose of seeing what has been done and may yet be done toward bringing them into accord with the organismal conception, the pole star of all our previous discussions. This examination will begin, as others have begun, by showing how elementalistic attempts to interpret organic phenomena soon reveal their inadequacy and finally break down as the efforts come to face the increasing complexity which progress of objective research always finds in such phenomena.

Associationist Psychology a Special Case of Elementalist Biology

In the particular psychical realm we are now to examine, elementalist theory has appeared most prominently as what is called Associationism. This flourished first in England as the school of English Associationists, David Hartley, near the middle of the eighteenth century, being usually considered its founder. Psychologists of this group hold that ideas, which for them appear to be identical with sensations, are the "ultimate elements" of psychic phenomena. "According to this theory, rigidly carried out, all genesis of new products is due to the combination of pre-existing elements." [9] Even the passions, according to Hartley "must be aggregates of the ideas, or traces of the sensible pleasures and pains; which ideas make up, by their number and mutual influence upon one another, for the faintness and transitory nature of each singly taken." [9] The "piling up of fancy upon fancy, of jest upon jest, the long embellishment of humor and foolery and horseplay" which Professor Manly shows characterize many of the Shakespearian plays, would be explained, according to this kind of psychology, not really by the author Shakespeare but by the "aggregation," in some way, within him of ideas.

And, similarly, the works which in popular language are said to be by a Darwin, a Humboldt, a Copernicus, an Aristotle, are in reality not *by* but merely *in* these men. The men were only the places of aggregation of the elements—the ultimates—by which the teaching on the origin of species, on the general character of the earth, on the solar system, on the deeper meaning of external nature, were produced. Again the old story with which we have become familiar: not the organism, but elements of, or perhaps merely in it, are the causal explanation of whatever occurrences are associated with the organism. It is, I think, safe to assume that both the merits and the demerits of associationist psychology have been made patent enough, at least to English-speaking students, by the writings of James and others.

If only the doctrine of "association of ideas" can be satisfied to do what it is really able to do, and not insist upon trying to do what it can not do, its usefulness is great and its permanence in psychology assured.

As indicated above, the "huge error," as James expresses it, by which the "whole historic doctrine of psychological association is tainted" [10] is only another miscarriage of the elementalist mode of reasoning, and so is subject to the general type of criticism which the reader has met in every chapter in this book.

In order to divest the criticism as much as possible of personal flavor I shall make large use of James' language. "All these writers," says James, referring to Hobbes, Hume, Priestley, Hodgson and the later English associationists, "hold more or less explicitly to the notion of atomistic 'ideas' which occur. In Germany, the same mythological supposition has been more radically grasped, and carried out to a still more logical, if more repulsive, extreme, by Herbart and his followers, who until recently may be said to have reigned supreme in their native country." [11]

Now the objection to the doctrine of "atomistic ideas" does not so much concern the conception of ideas as atoms as the nature attributed to these atoms, namely in assuming them to be immutable, and sufficient in their isolate capacities to account for the thought and other products arising from their "association." The following two quotations illustrate the form this criticism, the essence of which is now very familiar to us as biologists, takes when it appears in garments of a psychologist's making. The "huge error" of the association doctrine, mentioned above, James explains, is "that of the construction of our thoughts out of the compounding of themselves together of immutable and incessantly recurring 'simple ideas.'" [10]

If there be any doubt as to the meaning of this surprisingly un-James-like wording, there certainly can not be as to the following: "For Herbart each idea is a permanently existing entity, the entrance whereof into consciousness is but an accidental determination of its being. So far as it succeeds in occupying the theatre of consciousness, it crowds out another idea previously there. . . . The ingenuity with which most special cases of association are formulated in this mechanical language of struggle and inhibition, is great, and surpasses in analytic thoroughness anything that has been done by the British school. This, however, is a doubtful merit, in a case where the elements dealt with are artificial; and I must confess that to my mind there is something almost hideous in the glib Herbartian jargon about *Vorstellungsmassen* and their *Hemmungen* and *Hemmungssummen*, and *sinken* and *erheben* and *schweben*, and *Verschmelzungen* and *Complexionen*." [11]

The long and short of the "huge error" of associationist psychology is that ideas are no such independent, immutable, simple entities as the doctrine supposes; that in their origin and in all they are, and all they do, and all that comes forth from their association, they are in some sort and measure dependent upon—what? Something. Search after this something has been a large motive of more recent psychological inquiry.

One way of supplementing and rectifying associationist doctrines is to epitomize the shortcoming of these doctrines in the statement that they recognize only the objective side of the association process, whereas the subjective side is equally important. Thus Pillsbury: "It was a neglect of the subjective conditions and the insistence upon the objective side of the problem that has led the English Associational School into disrepute. The explanations that they gave were true as far as they went, but their incompleteness vitiated the conclusions as soon as they laid claim to universality." [12] And the author then shows, convincingly enough, how two sets of subjective factors, attention and choice, play a large and important rôle in determining the "associative train." And further, "A complete explanation of association demands that both sets of factors [objective and subjective] be taken into account; to omit either is to fail in the solution of the problem." [13]

Preliminary Examination of Objective and Subjective

There is undoubtedly a real gain in having proved, as Pillsbury and others surely have, that a "side" other than the objective in association does exist. It is highly advantageous, also, to have learned enough about this other "side" to make the term subjective an appropriate name for it. But any one coming to a study of the associational activities of the organism's psychic life as we have, namely as naturalists, can not avoid, if true to his traditions and methods, wanting to know how these two "sides," the objective and the subjective, go together—what the nature is of their relation. For the very fact that they are two *sides* of one thing is to the naturalist *prima facie* evidence that they are in vital organic connection. Even the two sides of an inanimate thing, like the earth with its two hemispheres, have a relation to each other too important for the earth sciences to ignore or even to put off as merely "parallel." But when it comes to an entity like a live animal, the quintessence of which is organization, the question of how two of its "sides" so important as its objective and its subjective are related, becomes most fundamental, especially when a subject like that of psychical association is up for consideration.

It is, then, fundamental to our enterprise to find out all we can about the connection between the objective and subjective aspects or sides of the organism. It would be folly to expect results of any value from an effort of this sort without having first given attention to the nature of each of the sides. Now the objective "side" comes to much the same thing as the physical or material organism as we are conceiving the "sides." But since this has been the subject of our whole treatise up to the last two chapters, and even of the greater part of these, we are already possessed of enough understanding of this "side" for our present purpose.

As to the subjective "side" the case is different. Into its nature, its makeup and activities, we have looked very little—only in a bird's-eye-view fashion thus far in the present chapter, and into its marginal or transitional zone in the preceding two chapters. We are, consequently, obliged to penetrate further into the subjective realm itself before the main problem, that of the relation between the sides, is attacked.

The Essence of Wundtian Apperception

This carries us back to the point at which we brought Royce's suggestion of a relation between Loeb's tropism theory and Wundt's apperception theory into the discussion, the return to this point being for the purpose of using Wundt's conception to induct us further into the nature of mental life. The importance of examining Wundt's conception is two-fold. In the first place, we want to know whether or not it is genuinely descriptive of man's highest psychical life. If it is, nothing can stand in the way of its acceptance, so far, by the anthropological zoologist. But in the second place we must know whether or not it carries, as some critics believe it does, transcendental or supernaturalistic implications. If this charge be true it is of course to this extent unsanctionable from our standpoint.

Wundt's most concise characterization of apperception which I have found is, "The process by which any content of consciousness is brought to clear comprehension we call apperception." [14]

A content of consciousness is any definable experience we may have. All consciousness whatever is *consciousness of* something or other. This "something or other," no matter what, nor whether regarded as a whole or in part, is a content of consciousness, according to my understanding.

What is most distinctive about Wundt's characterization may be regarded as centering around the word *clear*. When a particular content gets itself into the lime-light of consciousness—when it becomes the center of attention—the process by which it does so is *apperception*. On the other hand, the process by which contents, though brought into consciousness, come only into its outer zones or edges, and do not monopolize attention, is *perception*. Though this getting of a content into clearness in consciousness, this monopolizing of attention, may take place passively or actively so far as the mind as a whole is concerned, the active way seems to be the more distinctive and important, at least for mental life as a whole.

It is apparently this positive activity of apperception, directed toward making particular contents of consciousness clear, which has brought criticism upon Wundt's conception. "Wundt talks," says Pillsbury, "almost as if there were a faculty or force of apperception, something behind and superior to consciousness, which brings about the change in clearness of the impressions. There is in the brain a definite centre of apperception, and in consciousness a force very closely related to will, that in and of itself chooses certain ideas for elevation to the high places of consciousness, and equally arbitrarily rejects others." And then follows this in Pillsbury's criticism, which brings out unmistakably its real purport: "It is very much like the self-conscious unity of apperception of Kant, which gives the final form and order to the various disconnected elements of the mind, and is in so far something inexplicable, a factor in experience that must be assumed without any further discussion of its nature, origin, or laws of action." [15] A suspicion, obviously, that the transcendentalism of Kant broods over Wundt's theory. As to the justification of this suspicion we need not be concerned here. Enough for us at this point to recognize that from the standpoint of description as natural history practices the art, or aims to practice it, Wundt's account of the way the mind works in a vast range of its activities seems true, and as far as it goes is satisfactory.

Not only the matter of clearness of the contents of consciousness, but their makeup as well is important. Although "psychical elements" figure largely in Wundt's system, one finds no intimation that the whole mind and its contents can be "explained" by reducing them to "ultimate elements" after the familiar manner of elementalist explanation. "All the contents of psychical experience," Wundt says, "are of a composite character." And it follows from this that "*psychical elements*, or the absolutely simple and irreducible components of psychical phenomena, are the products of analysis and abstraction." [16]

The two words, "analysis" and "abstraction," need particular consideration. The psychical elements found by analysis do not exist, as such, in nature. Analysis, in this case, is logical or thought-analysis, and not objective analysis. We should do well to recall what was said in the discussion of reflexes, namely that the "simple reflex," though legitimate and useful as an aid to interpreting the phenomena of reflex nerve action, has no actual existence in nature. It, like the "psychical element," is an abstraction. This is only another way of saying that psychical elements are what they are because they are parts of the mind as a whole, just as we have seen over and over again physical elements of the body are what they are because they are parts of the body. "The specific character of a given psychical process depends for the most part not on the nature of its elements so much as on their union into a composite psychical compound. Thus, the idea of an extended body or a rhythm, are all *specific* forms of psychical experience. But their character as such is as little determined by their sensational and affective elements as are the chemical properties of a compound body by the properties of its chemical elements. *Specific* character and *elementary* nature of psychical processes are, accordingly, two entirely different concepts." [17]

We must not miss an essential point in this, that since with psychical elements just as with chemical elements we never know exactly what or how much each particular element contributes to the compound, we are obliged to conceive the attributes of the compounds as pertaining to the elements *collectively* even before the compounding has been done.

So it comes about that because all contents of consciousness are fundamentally composite, but are also resolvable into components of various grades of complexity, synthesis and analysis have a prominent place in the Wundtian system. Of these, synthesis is the more definitive and fundamental, since it enters into the very nature of consciousness itself. Consciousness *is*, according to Wundt, the "inter-connection of psychical compounds." "It is the name for the general synthesis of psychical processes, in which synthesis the single compounds are marked off as more intimate combinations." [18] The meaning of this is made clearer by the statement that unconscious states like deep sleep, faint, and so on, are the interruption of these interconnections.

Remarks On Analysis and Synthesis

This brings us to where we can see the important distinction between an aggregation and a synthesis—in a psychical sense particularly—and likewise between a fragmentation and an analysis. No mere aggregation,* as of ideas or emotions, would make consciousness. Only a synthesis of constituents can do that. And, contrariwise, while the mere severance of the synthesized components produces unconsciousness, an analysis of them results, not in unconsciousness, but in a consciousness of the constituent parts of the *contents of consciousness.* The essence of consciousness is unitariness—integratedness, in our general terminology—as regards the contents of an individual organism's psychical nature, so that whatever analytical processes the mind performs must move *within* the bounds of its own unitariness or integratedness. Were we to conceive the analytic operations of the mind to exceed or even quite to equal its synthetic operations, we should have to conceive it as utterly negating consciousness, i.e., as destroying itself. A man could analyze his own mind in an elementalist sense only by suiciding. In other words, he could never do it, simply because he would have killed himself by the very process of analyzing before he had completed his job.

These remarks on the distinction between synthesis and aggregation, and between analysis and fragmentation, are not quite what Wundt says. They go somewhat beyond his actual expression, but are legitimate inferences, I am quite sure, from his discussion as a whole. And they help us toward what we want to accomplish, namely to discover still more

* Recall our previous remarks on this subject, e.g., pp. 182 and 268, and also the quotation from Hartley, p. 228.

than Royce discovered about the relation between apperceptive processes, as Wundt conceives them, and the processes known as tropisms.

Anticipating our results, and stating them in the most general terms possible, we may say that the "apperceptive synthesis" of Wundt, and what may be called tropistic synthesis, have a common ground in the kind of synthesis which is the very essence of that kind of organization to which the term *life* is applied. To be alive is to be an organic individual; to be an organic individual is to be an individual that perpetually synthesizes itself from substances extraneous to itself (food, in the narrower sense, and oxygen); and to be a psychically endowed individual is to be an individual which in addition to synthesizing a physical nature from the substances mentioned, synthesizes a psychical nature from physical and chemical contacts and interactions between the individual and the external world, the physical contacts being called stimuli. Viewing the matter thus, it is seen to be highly probable that in its ultimate essences the dependence of the psychical nature of the organism on stimuli is connected, directly and inseparably, with the dependence of its material nature on material nutriment.

We should, I think, be surprised were a demonstration to be produced that psychic life has as little connection with metabolic processes as the text-books of psychology would lead one to suppose. Every modern psychologist, like every modern biologist, accepts, unquestioningly I presume, the conception that *in some way* the psychic life is no less dependent on the nutritive substances and processes than is the physical life. Yet that "some way" appears to be generally regarded as so remote and obscure as to be beyond the reach of profitable treatment by psychological science, judging from the considerable number of standard text-books which I have consulted on the point. In only one of these

(*Elements of Physiological Psychology*, by Ladd and Woodward) do I find the word "metabolism" in the index.

Our task may then be restated as that of making out more fully and clearly than Royce did the connection between apperception and tropisms, which is involved presumably in the whole problem of organic synthesis from its highest manifestations in psychic synthesis to its lowest manifestation as metabolic synthesis; of bringing to more specificity the general statement made above. But such statements are altogether too sweeping and abstract to satisfy scientific description and explanation in our day. A chapter must now consequently be devoted to making them more definite.

REFERENCE INDEX

1. Hobbes	15	10. James ('90)	I, 553
2. Hobbes	21	11. James ('90)	I, 603
3. Hobbes	25	12. Pillsbury	106
4. Royce	ix	13. Pillsbury	112
5. Royce	x	14. Wundt	229
6. Royce	xi	15. Pillsbury	270
7. Manly	3	16. Wundt	32
8. Manly	4	17. Wundt	33
9. Baldwin	I, 80	18. Wundt	223

Chapter XXIII

ORGANIC CONNECTION BETWEEN PHYSICAL AND PSYCHICAL

A Still Closer Look at the Organismal Nature of Tropisms

HAVING selected tropisms as a strategic point in our program of search for the vital connection, if such exists, between the physical and the psychical, we must turn again to this subject. Our previous treatment of the tropism theory brought out the essential organismal character of the type of activity to which the term tropism has been applied. The result of that treatment might be epitomized by saying that in so far as the theory rests on accurate and adequate description, it is genuinely organismal and genuinely sound, but in so far as it rests on causal explanation that is elementalistic in spirit and expression it is genuinely unsound. Our present aim will be furthered by illustrating this epitomized stricture on the theory in a little different way from which we objectified our criticism in the earlier treatment.

Every one familiar with current explanatory discussion of tropisms must have noticed the large and free manner in which the word substances is made use of in the explanations. Thus, to illustrate: The larvæ of certain butterflies emerge from their winter nests under the influence of the warm spring sunshine, crawl to the tips of the branches of some shrub or tree, eat the buds and tender leaves there; then, after feeding to satiety "turn tail" and crawl down the branches. This rather complex and, to the insects, highly useful performance Loeb and others have proved to consist of a series of reflexes so interconnected as to come under the tropistic type of activity. And Loeb, e.g., in the chapter, *On the Theory of Animal Instincts (Physiology of the Brain)* uses the case to good effect in support of his contention that the traditional instinct-and-nerve-center explanation of such phenomena is utterly inadequate.

So far, good. As to the straightforward presentation of facts, Loeb's position seems unassailable. But what about the causal explanation of the facts? What, exactly, is it that sends the larvæ up the branches? What causes the eating activities? What makes the creatures then turn about and finally sends them down the branches? That several environmental factors, the warm weather, the sunlight, the character of the plant buds and leaves, and so on, are involved is brought out clearly enough. But what about the factors pertaining to the larvæ themselves? The body-shape, the skin, the sense organs, the muscles are, as was emphasized in the previous discussion of tropisms, freely recognized after a fashion by the tropism theory. But deeper still than these—what? Chemical substances "according to requirement," in the language of the cook books. Until the caterpillars have taken food they are positively "heliotropic," that is, literally, are induced by sunlight to move toward the sun, after the higher spring temperature has caused chemical changes in their bodies essential to such movement. But by eating to satiety the chemical changes essential to the positive heliotropism are inhibited and a negatively heliotropic state comes on. "We can imagine," writes Loeb, "that the taking up of food leads to the destruction of the substances in the skin of the animal which are sensitive to light, upon which substances the heliotropism depends, or that through the consumption of food the action of these substances is indirectly prevented."[1] In the total scheme, then, actual and imagined, various "substances" are indispensable.

That the imaginary constituents constitute a very important part of the explanation is obvious. This fact is, however, not specially objectionable. It is not if its true character is never forgotten. But here comes the point I wish to make focal just now. If imagination is to be given a place at all in the argument it must have a larger place than Loeb has accorded it. Otherwise the teachings of evolution, i.e., the genetic continuity in biology, are tacitly repudiated. Attention has previously been called, especially in the chapter on the organism and its chemistry, to the deep current of virtual anti-geneticism which runs through physiology generally, and particularly through bio-chemistry. Undoubtedly we can imagine "substances" produced and destroyed in such a complex of activities as that described, to meet exactly the needs of the larva; but can we legitimately imagine them to be so produced and so destroyed by any other means than just by the particular animals in question, that is, by the organisms? Various of our discussions, but particularly those in which the specificity of protoplasm have been dwelt upon, constitute a decisive negative answer to this question. No causal explanation of the requisite "substances" imagined can stop short of the organism, alive and normal, as an essential and "causal factor" in the phenomena presented. Causal explanation of tropisms which aims to reach a physico-chemical basis is really organismal as well as are tropisms seen through the medium of pure description.

The Automatic and Anticipatory Character of Tropisms and Other Reflexes

Nor should the reader fail to note the intrinsicality, the adaptiveness, and the anticipatoriness, of tropisms as illustrated in this example. Conscious will and choice seemingly do not come into the operation at all. Given the right conditions, internal and external, the caterpillar goes through the concatenation of operations necessary for its existence, willy-nilly. Moreover, the actions initiated by the warm weather, the larvæ being yet down at the base of the shrubs or branches, have in organic prospect, as one might say, a supply of food peculiar to the species. And this supply, be it remarked, is several inches at least, and several minutes at least, away from the larva at the beginning of its round of activities. Its future, even more obviously than its present, existence is involved in the acts. Anticipatoriness is perhaps the most conspicuous attribute of the adaptiveness of such activities. C. Lloyd Morgan has well expressed the truth that one of the most important lessons to be learned from the study of animal behavior "in its organic aspect" is the fact that "living cells may react to stimuli in a manner which we perceive to be subservient to a biological end, and yet react without conscious purpose—that is, automatically." [2]

But from our examination of the cell-theory we conclude that "living cells" in this statement ought to read "living organisms."

So much by way of further preparation, in the reflex and tropistic phases of animal life, for our search after a vital connection between the physical and the psychical. It will now be advantageous to return to that supremely important aspect of human psychic life already examined somewhat, namely that of Wundtian apperception.

A Still Closer Look at the Likeness Between Higher Rational Life and Tropisms

As Royce's statement of the objections to the conception of Wundt contains several points that will be useful to us, we reproduce more of his sentences. "It has been objected to the partisans of Wundt that the term 'apperception,' as thus used, seems to signify a factor in mental life which can be explained neither in terms of what we have called sensitiveness, nor in terms of the law of habit. It has also been objected that the conception of a conscious process, engaged in influencing its own states, is a conception which confuses together metaphysical and psychological motives. The psychologist, engaged as he is, not in studying how Reason forms the world, but in observing and reducing to rule the mere phenomena of human mental life as they occur, is not interested, it has been asserted, in a power whose influence upon mental phenomena seems to be of so ambiguous a character as is that which the Wundtian 'apperception' possesses." [3]

Again: "This is the place," Royce writes, "neither to expound nor estimate Wundt's theory. But it does here concern us to point out that *what occurs in mind whenever we are actively attentive is attended with a feeling of restlessness, which makes us dissatisfied with all those associative processes that do not tend to further our current intellectual interests.* On the other hand, the cerebral processes that accompany active attention are certainly such as *tend to inhibit many associative processes that would, if free, hinder our current intellectual interests.*" Meanwhile, "*our active attention itself is always the expression of interests which possess the same elemental character that we have all along been illustrating in the foregoing paragraphs.* The attentive inventor is eager about the beautiful things that he thinks of while he is trying to invent. The attentive hostess is eager about social success. The attentive caged animal is eager about whatever suggests a way of escape." [4]

The discussion from which these sentences are taken is contained in a chapter near the end of the book, entitled

The Conditions of Mental Initiative, and in order that the reader may get the full force of what Royce is talking about, he is earnestly recommended to read the entire chapter. Only thus can the "foregoing paragraphs" mentioned be adequately appraised. But we must try, in our own way, to get the essence of the matter. Royce's presentation is his way of insisting upon the facts of psychical life and activity, high and low, which have given rise to the Wundtian conception of apperception, these facts being the indubitably initiatory, directive and selective qualities of mind in all its grades. Furthermore, Royce dwells on the homogeneity, as one may express it, of this intrinsicality of mental life—its initiative, its persistence, and its selectivity—with the individual or fluctuating variations which have played so large a part in theorizing about organic evolution and heredity during the Darwinian era of biology. And he goes back still further in good modern biological fashion, and connects these variations with organic growth itself, thus calling attention to the fact that variations of this particular sort can not be referred to environmental influence.

At this point we may stop, as biologists, to supplement Royce's argument by pointing out that variations of the sort indicated, are referable to environmental influence only in the sense that growth is so referable. An organism's securing and taking in of its nutritive substances are undoubtedly a kind of response to contact with its environment, and in that broad sense growth may be said to be due to environmental influence. If the organism had no nourishment, if it received no environmental influence of this kind, it certainly would not grow. At the same time, since the organism manages somehow to build a great variety of tissues and organs out of one and the same supply of nourishment; that is in response to one and the same "environmental influence" (as we are agreeing to use the phrase here) there is no course open but to recognize that the organism is a very important, because indispensable, factor in its own growth and differentiation. "Self differentiation," so far as the whole organism is concerned, is a fact than which no other in the whole domain of biology is better established. Indeed, self differentiation is really a special form of self growth and surely no one would contend that environmental influence is more than an essential factor in the growth of an organism. To hold it to be a complete explanation of the phenomenon would be too manifestly absurd to receive serious consideration. It would be to contend, in effect, that *one* of the processes of the organism (its growth) is more than *all* the processes of the whole organism. But since most if not all variation depends, either directly or indirectly, upon growth, what is more natural than that the living, growing organism should display much self variation?

That such variations are among the most common phenomena presented by organic beings, there is no shadow of doubt to any one who views the problem broadly and critically, and with no domineering preconceptions as to what ought to be and ought not to be; who, in other words, views the organic world as a natural historian, guided by the mandate "neglect nothing," instead of as a physicist in the mathematico-laboratory sense, guided by the mandate "neglect everything which can not be made to conform to general mathematically statable law."

These remarks about the relation of mental activity to growth, differentiation and variation of the organism, and to environmental influence would apply throughout, *mutatis mutandis,* to tropistic activity.

A Still Closer Description of the Subrational Moiety of Psychic Life

And this brings us to where our final return may be made for purely descriptive and comparative purposes, to the subrational moiety of psychic life, the purpose of the return this time being to characterize this moiety as faithfully but as briefly as possible on the basis of the total results of researches in the field up to the present time. So bulky and varied are these results that to examine them exhaustively is hardly possible for any one person even though he be a specialist in the field. Much less possible is it, then, for a general zoologist to make such an examination. Nevertheless it is, I believe, possible to give an epitome of the present state of knowledge that shall be true in all fundamental respects and highly significant for our enterprise.

Remarks on the Classes of Subrational Life

In giving this epitome we shall not try to maintain a sharp distinction between reflexes, whether of the tropistic or any other type, and instincts. To begin with, as always, when a large and intensively cultivated domain of science is entered for the purpose of extracting from it its most certain major results, we may take it for granted that the extremists touching any portion of the field over which divergence is wide and warm, are unsafe guides for the general student. Thus, the student who enters the realm of animal behavior for such a purpose as that for which we are now entering it soon sees that those specialists who find nothing but tropisms, and these of the most uncompromising sort, in the activities of much of the animal kingdom, are not the ones to whose guidance he can entrust himself, no matter how voluminous, and perhaps excellent in quality, their experimental researches may be.

For instance, such a view as that of Bohn's, according to which the word *instinct* ought to be eliminated from the terminology of science "as a legacy of the past, the middle ages, the theologians and the metaphysicians," [5] is so obviously unjustifiable to any well-informed zoologist as to make him suspicious of such a writer all along the line, especially wherever his judgment and scientific poise are implicated.

This question of the reality of instincts I use to illustrate the peril to the general student of the unpoised specialist, because it is germane to the present discussion. In general zoology the type of animal behavior to which the term *instinctive* is applied is not less conspicuous and real, to say the least, than is the type described as tropistic. For an experimentalist to come out of his laboratory and tell a broadly experienced entomologist or ornithologist, for example, that the familiar achievements of young insects of many species, and of numerous young birds should not be called instinctive because (as the experimenter asserts) they are reducible to the tropistic or perchance the simple reflex type of reaction, may justly be characterized as scientific impertinence. It is as though an embryologist, having discovered that a bird's wing is the genetic counterpart of a salamander's forelimb, should instruct the ornithologist that it is wrong for him to call the bird's wing a wing, because the member may be reduced to a lower type of limb. Unquestionably the experimental specialist often produces results which necessitate changes in the general zoologist's conceptions and nomenclature. But it is not his province to take into his own hands the revision of the fundamental terms of zoology. Any one moderately instructed in the history of zoology knows that "instinct" is a hardly less well-grounded zoological term than "birth" or "intelligence," or many another indispensable term.

Our inquiry is not as to whether there are such things as instincts, but how they operate and what they signify for the animals possessing them. Perhaps of highest interest to us is the fact that innumerable instincts, if indeed not all, are as indubitably hereditary as are any animal endowments whatever. This comes out especially convincingly in those numberless cases where the instinctive operations develop strictly *pari passu* with the anatomical development of the young, there being absolutely no opportunity for them to learn, even subconsciously.

Take as an example the crustacean *Amphithoë longimana,* in which Holmes compared in detail the activities of the newly hatched young with those of the adult. "Amphithoë lives in tubular nests which are usually lodged among sea weed. The nests are somewhat longer than the animal, and are spun of a web-like material into which bits of sea weed are often incorporated which help to conceal the occupant. In its nest Amphithoë lies in wait for prey, ready to dart out upon any small creature which touches the ends of its long antennæ.

"The activities of the adult Amphithoë, with the exception of those concerned in reproduction, are almost exactly paralleled by those of the young. I have taken the eggs from the maternal brood pouch shortly before hatching and kept them isolated in individual dishes. For some time after emerging from the egg the young were weak and had imperfect control of their movements, which were jerky and irregular. Soon the minute creatures would crawl and swim much like the adults, and the next day they began constructing nests which were the same shape as those formed by their parents." Then comes a part of the description to which the reader's special attention is called because it brings out, partly by implication, a richness of detail in behavior which defies full expression, and which every carefully observing zoologist knows to be characteristic of the activities of nearly all animals. Especial attention is invited to this, because this elusive wealth of behavior is usually overlooked by the cursory observer on the one hand and by the experimentalist on the other. "The attitudes in the nest," Holmes writes, "the waving of the antennæ, the beating of the swimmerets, the restless movements of the legs and mouth-parts, springing after food, belligerency toward passers by, the little unobtrusive signs of timidity, the reversal of position in the nest on the approach of danger and the general behavior outside of the nest, were, on the next day after hatching, almost exactly the same as in older individuals. The only differences in behavior were due to the feebleness of the young and their imperfect control of their movements." One never reads a description like this by a typical experimentalist, especially if he be a pure tropist, or by a meagerly trained zoologist! Then the final statement: "The young are hatched with all the instincts necessary fully to equip them for the business of life. No experience is necessary to teach them what is advantageous for them to do." [6]

The impossibility should be noticed of drawing a sharp line in this description between instinctive and purely reflex acts. "Reversal of position in the nest on approach of danger" is clearly instinctive. But "beating of swimmerets," and especially the "restless movements of the legs"—are these instinctive or wholly reflex? Probably they are reflex, though the leg movements may well be partly instinctive. A whole volume of examples as unquestionable as this could be compiled, and all groups of animals from mammals down to worms at least would be represented.

Four Certainties About the Adaptiveness of Subrational Psychic Activities

Concerning the purposefulness or adaptiveness of activities of this general type, I think four things may be regarded as absolutely certain.

Generally Useful to Individual and to Species

First, a vast majority of them are recognizably contributory to the perpetuity of both the individual in its normal life, and of the species. But for them neither individual nor species would continue to exist. This is so obvious that further remark upon it is unnecessary.

Many Useful to Species Primarily

Second, in a large number of instances particular acts by particular individuals are in the interest of the species primarily and of the individuals only secondarily or not at all. This is shown most conclusively in cases like that of several species of salmon, where the individual normally goes through activities which secure the continuance of the species but which end in the death of the individual. A large and varied number of cases of this type occur, especially among insects. But the supremacy of species over individual needs appears under various other forms. Thus almost certainly such tropistic activities as that of the moth going to its death or injury in the flame is of this sort. This case may be stated in general terms thus: Owing to lack of any ability on the part of an individual to modify its inherited mode of action to meet a special situation, it acts in the old way even though the new situation, while in general like the old, yet differs from it enough to make it perilous to the individual if it acts unmodifiedly in the old racial way. The preëminently racial utility and hereditary character of instincts is certainly one of the most interesting things about them for the present discussion.

Variability of Subrational Activities

The third certainty about reflex and instinctive activities is that they are by no means so stereotyped and invariable as older cursory observation or as much theorizing, especially about tropisms and instincts, has held them to be. Darwin, in the notable chapter on Instinct in *The Origin of Species*, was the first to attack seriously the notion of such invariability in dealing with instincts. He undertook to show that the instinctive type of activity is subject to variation just as are all other aspects of animal life.

A telling set of recent investigations under this head is by the Peckhams. That on the solitary wasp, *Ammophila urnaria*, is particularly to the point because the earlier writers had used its habits of paralyzing caterpillars by stinging them and storing them up as food for its young to illustrate the undeviating and unerring character of instincts. But the extensive studies of these entomologists led them to write: "The one preëminent, unmistakable and ever-present fact is variability. Variability in every particular—in shape of the nest and the manner of digging it, in the condition of the nest (whether closed or open) when left temporarily, in the method of stinging the prey, in the degree of malaxation, in the manner of carrying the victim, in the way of closing the nest, and last, and most important of all, in the condition produced in the victims by stinging." [7]

No present-day authority so far as I know contends that instincts operate in a hard-and-fast manner comparable to the workings of any man-made machine. They are now universally recognized to be subject to the same general principles of variation to which all organic phenomena are subject. Furthermore, under the searching investigation and criticism of numerous workers, notably H. S. Jennings and his followers, the tropism theory has been deprived, for most biologists, of its inorganically mechanistic character. The principles of "random movements," "avoiding reactions," "trial and error," and others, are thoroughly established and the recognition of them may be said to have so modified the doctrine of tropisms as to make it one of organic mechanism rather than of inorganic mechanism—as it virtually would be according to the thoroughgoing elementalistic conception of it. The "mechanistic conception of life," one may remark, has very much to commend it if only the machines conceived are recognized to be *alive*. My remarks under this head * may be consulted by the reader who wishes to follow this point.

What is meant by random movements is made clear by the following: "In the earthworm and the larvæ of blowflies which are negatively phototactic it has been shown by the writer that movements which bring the animal toward the light are checked or reversed and only those which happen to direct the animal away from the light are followed up. Whatever immediate orienting tendency the light may have in these cases is relatively unimportant as compared with the element of selection of favorable movements in directing the animal away from the light."[8]

Here it will be noticed that the end, beneficial to the animal, is reached through a combination of orienting reactions of the rigidly tropistic type, i.e., the type dependent on the movement of the animal directly toward or away from the source of light by the symmetrical plan of the body, and a sort of reaction in which the particular body-form and the direction of light rays are of only secondary

* See "Machines, living," in the index of *The Probable Infinity of Nature and Life.*

significance. But this latter type of activity, wholly divorced from a direct-orienting reaction, and even from a bilateral body symmetry, is of wide application among the lower animals. It was first brought clearly to the attention of biologists by Jennings in his now well-known investigations on Paramecium and other protozoans. These investigations formed the bases of the "avoiding reaction" and the "trial and error" conceptions now generally recognized to be of much importance in the behavior of all animals, especially of those in which a high measure of bodily activity occurs but in which there is little or no intelligence. Jennings' lucid account of his results in the chapter *Behavior of the Infusoria; Paramecium* (*Behavior of Lower Organisms*) is strongly commended to the reader.

The following paragraph must suffice for our reference to this work. After describing the behavior of Paramecium, Jennings writes: "This method of behaving is perhaps as effective a plan for meeting all sorts of conditions as could be devised for so simple a creature. On getting into difficulties the animal retraces its course for a distance, then tries going ahead in various directions, till it finds one in which there is no further obstacle to its progress. In this direction it continues. Through systematically testing the surroundings, by swinging the anterior end in a circle, and through performing the entire reaction repeatedly, the infusorian is bound in time to find any existing egress from the difficulties, even though it be but a narrow and tortuous passageway."[9] And this complex and highly useful behavior is performed by an organism which, so far as the best anatomical researches have been able to determine, is entirely devoid of a nervous system, and consists of a single cell!

But the "trial and error" scheme here exemplified is by no means confined to unicellular, non-nervous animals, nor to experimentally produced conditions. That it is operative in nature, and among animals with rather highly developed nervous systems I shall illustrate by describing briefly a performance witnessed by me some years ago. This was the capture and engulfment of food by a nemertean worm.*

These marine worms are of considerable size, some reaching a length of many inches, even a few feet, and ranging in thickness from less than an eighth of an inch to nearly an inch. Externally they give the impression of being very lowly in organization, the body being devoid of limbs or other appendages, and without segmentation. However, when they are examined internally a surprisingly high grade of organization is found, the muscular, digestive, blood and nervous systems being on a par, probably, with those of any invertebrates below the crustaceans and insects. The nervous system, particularly the brain, is relatively large, though not differentiated into diverse ganglionic masses and connecting strands to the extent found in jointed worms. The creatures are poorly equipped with external sense organs, there being no tentacles nor any certain olfactory or auditory organs. And eyes, when present, are so minute and simple as to be without power of sight in the ordinary sense; almost certainly they are mere light-perceiving organs.

The most distinctive anatomical feature of the nemerteans is a very long and thin though muscular and flexible hollow tube situated at the anterior end of the animal, which is usually carried stowed away in a pouch within the body. While thus retracted the tube has some such relation to the rest of the animal that a glove-finger would have to the

* Greatly to my regret I am unable to say what the species or even the genus was of either the nemertean or the annelid here referred to. The observation was made at the Shumagin Islands, Alaska, and under circumstances that rendered it quite impossible to "look up" the species. And my knowledge of the taxonomy of these groups of worms is altogether too meager to enable me to identify genera even, offhand.

glove were it completely inverted into the hand of the glove. This tube is used in the capture of prey, the animals being carnivorous and highly voracious. The mode of employing the apparatus consists essentially in thrusting the tube out with almost the speed of lightning, the object being to bring the organ into contact with the prey at many points. The lash is not used as a lasso for catching or as a spear for piercing the prey, but for paralyzing it, probably by a toxic secretion spread over the whole surface. The more effectually to accomplish this, the lash is shot out at a victim again and again.

Now for the aspect of the whole operation of food-taking which specially concerns us. Being quite sightless and touchless in the usual sense, the lash must be used as an exploring or finding as well as a paralyzing or killing organ; and since its great length and limberness preclude it from being used as an ordinary tentacle is used, the finding operation is accomplished by repeated out-thrustings of the tube. In the instance witnessed the prey was an annelid worm, a creature well provided with locomotor organs, and a good crawler. On this account the victim-to-be was able, in the early stages of the onset, to move out of contact with the nemertean now and then. At such times the prey could be relocated only by darting out the lash at random, except as to general direction. So it resulted that many of the thrusts missed the mark; but they were instantly repeated with a little variation of direction, till the victim was located again. The whole performance reminded one of the game of blind-man's-buff, a game in which the seeker paws around in the general vicinity, as he believes, where the one sought was last touched.

The effectiveness of the try, try again method was attested in this instance by the fact that the annelid was hit and the paralyzing dose administered times enough to put the annelid into so helpless a state that the nemertean was finally able to get its mouth into contact with its prey. Then the victim, itself but little smaller than the nemertean, disappeared down the latter's "throat" with almost the rapidity with which the lash was retracted into and thrust out of its pouch. How much of this highly complex performance, so eminently useful to the nemertean, was purely reflex, how much chemotactic, and how much instinctive? And who will assert positively that there was no trace of consciousness, even of intelligence, in it?

An extremely interesting line of inquiry is suggested by cases of "trial and error" like this where at one extreme the "errors" are not much less numerous than the successes, and, at the other extreme, are cases in which the errors are reduced almost to nil. A type case of this last would be the poise-and-spring of a cat after its prey. With little doubt a closely graded series could be made out running through from one extreme to the other. A cardinal interest in the inquiry would be as to the extent to which the simple reflex, tropistic reflex, instinct, and intelligence figure in the different grades. Would it not turn out that the gradual diminution of error through the series would be, generally speaking, concomitant with the increase of intelligence? I suspect so.

Tendency of Subrational Activities to Excessiveness

The fourth and last certainty about reflex and instinctive activities to receive attention is their tendency to excessiveness—their way of going beyond what is necessary or even really safe for the welfare of the organism. Although from several points of view this is one of the most important aspects of the whole subject, it has received surprisingly little attention, especially by the modern school of experimental zoology.

Probably every one who has observed animals widely and thoughtfully has been impressed with the exuberance of their performances. That they are ever wont to overdo things, even operations which are when done in measured fashion absolutely essential to their existence, is matter of common knowledge. Holmes has some comments under this head which may fitly introduce our presentation. "With all their wonderful adaptiveness instincts are far from ideally perfect. Much of Mark Twain's remarks on the futility and imbecility, the wasted effort and labor at cross purposes shown in the behavior of ants may easily be verified by any observer." [10]

A common form taken by excessiveness of action is repetition. Very many, perhaps all, animals are notorious repeaters. A few out of the many available instances will suffice to fix the phenomenon in mind. Some time ago a small whale (probably a half-grown Humpback, *Megaptera versabilis*) came near shore at La Jolla, California, and remained in the same small area for days. While there it went through a particular set of movements known to whalers as "breaching" scores of times, each set being exactly, so far as one could see from the shore, like every other set. The performance consisted, in this case, of a leap out of the water, which carried the body clear of the surface of the sea, the direction of emergence being probably thirty degrees from the perpendicular. During the ascent the animal turned with a characteristic twist to the left and came down on its head and left side with a great splash. Once back in the ocean the creature reversed the course it was going when making the leap, returned to some distance from where it had emerged, reversed its course again, and repeated the leap identically, to all appearances, even as to the spot of emergence and direction of travel. Why so many times the same performance in the same spot? That is the problem which concerns us here. Even though we conceive it to be somehow adaptive—connected in some indirect way possibly with feeding or reproduction or migration or some other vital function—the question still remains, why so much of it? And to this no probable or even rational answer is forthcoming from the standpoint of adaptation and utility, taking these terms in their usual meaning.

Here is another case from the mammalia, the possible adaptive significance of which is still more remote, if anything, than that of the behavior of the whale. Many individual mice of the genus *Peromyscus* being used by Doctor Sumner and Mr. Collins in their researches on heredity and environmental influence at the Scripps Institution take to throwing back summersaults in their cages. The more common performance consists in a run along the floor of the wooden cage and up its side to near the top, then a quick, strong jump backward clear across the cage, the feet being uppermost during the first part of the leap but coming to rights again by the time the landing is made. Here again the question of why the mice do this seemingly useless thing is not so interesting for the present discussion as that of why they do it so much.

The high flight of some species of birds, the great elevations being reached by long, regular upward spirals, would appear to come under the head of non-adaptive, superfluous action. The sand-hill crane, *Grus mexicana*, may be taken as an instance of a bird given to this habit. Surely such flights by this species can have nothing to do with food-getting, since in the excursions the bird is going directly away from, instead of into, the region where its food abounds. It eats snakes, frogs and other creeping animals, and various seeds and roots. Nor is there any evidence that the flights are concerned with the mating function, nor yet with migration, though one might possibly imagine that while on the excursions the birds learn, after a fashion, the topography of the surrounding regions.

The high-diving and booming of the night-hawk, *Cordeiles* *virginianus*, repeated time after time in the early evening and occasionally in midday when an approaching storm cools the air, would seem to be another performance of the non-adaptive sort. The suggestion that this is a courtship affair can hardly stand, in view of the fact that at least as often as otherwise the birds which do it are entirely alone. Nor can one see how so extensive and swift a dive, with so much noise, can be advantageous for the capture of flying insects.

And reflect on the quantity of movement of many animals. Can any one believe that mammals and lizards run, birds and insects fly, and fishes swim just exactly so much as and no more than, they must in order to survive? Would it be contended that the Golden Plover, to take a well known case of extensive migration, would certainly succumb in the struggle for existence on anything less than a journey from the high latitudes of the northern hemisphere well into the southern hemisphere and back, each year? There is a vast difference between a necessity for migration to *some* extent and a necessity for migration of a particular quantity. One of the great weaknesses of the natural selection theory has been, I am very sure, its slight regard for quantity; quantity of need, quantity of performance, quantity of benefit.

These examples serve to illustrate the fact that among the higher animals at least, much muscular activity occurs which is not at all, or only partly, adaptive. But by far the more common occurrence of excessive activity is in connection with behavior which is more or less obviously adaptive. "A good thing carried to excess," in the familiar phrase, expresses well what is in mind here.

This excessiveness of adaptive activity is naturally more easily recognized in animals which are most easily observed and most active generally. Thus it is from birds and insects that examples can be most readily drawn.

Let the current view be accepted that the song of passerine birds is associated adaptively with the mating function. Even so, no one who has given careful attention to the matter can have failed to recognize that with many species much more singing is done than actual pairing and breeding call for. I have kept almost daily notes for several years on the singing of the Western Meadow Lark, *Sternella magna neglecta*, in the vicinity of La Jolla. The birds are resident the whole year through, and as they come familiarly around my home and laboratory, the observations can be quite full. Although the breeding time is restricted to late February, March, April, and sometimes May, there is not a month in the year when songs may not be heard, most of the time in full volume. Significantly, I believe, the song is at its ebb during some weeks just before the nesting period begins. Nor does the singing of the males seem to be connected in any close way with mating. The birds do not pair off closely and permanently, even for the breeding season. Most of the singing, which occurs chiefly in the morning and early forenoon and again toward evening, is done while the singer is, more commonly than otherwise, quite alone on some telephone pole or wire. And the mode of singing does not change at all when mating begins. Another interesting fact about the singing of this species is the considerable range of temperature and light conditions over which the song is invariable, so far as these factors are concerned. The song may be as full and frequent on cloudy, misty mornings as on sunny ones; and over a considerable range of temperature the song is quite independent of the particular degree marked by the thermometer.

While the song habits of this bird are undoubtedly somewhat exceptional in their looseness of correlation with mating and with environmental conditions, certain it is that much this sort of thing is observable with several resident species which I have observed. The house finch, *Carpodacus*

Mexicanus, and the California towhee, *Pipilo fuscus*, may be specially mentioned in this connection. The fact that domesticated song birds, like the canary, may be brought to sing almost perpetually is only an extreme manifestation of tendency among song birds to sing in excess of any strict utility of song.

Think of the monotonous repetition in the croaking of frogs, the chirping of crickets, the stridulations of cicadas, and so on! I have counted more than five hundred consecutive chirps of a cricket in about half an hour, with only a little variation as to notes or intervals. And this is surely a very moderate example of what actually occurs—as any one can easily convince himself by listening and counting almost any still night, almost anywhere where crickets live. Probably the chirping of crickets is employed in mating. Very well. But are the thousands of chirps uttered by a given individual each night for many nights, the smallest number upon which the species can survive? Even asking of the question reveals the monstrosity of a theory that would necessitate an affirmative answer to it—as strict adherence to the natural selectionist meaning of utility undoubtedly would.

In place of bringing forward additional instances, which could easily be done, to show that vocal sounds and bodily performances of various sorts more or less obviously connected with mating among higher animals are produced in excess of what the strict application of the rule of physiological economy would dictate, I shall do no more than utilize the conclusions of two investigators who seem specially qualified to speak on the subject, and assume that these conclusions would receive the sanction of all zoologists who have given serious attention to the matter and have formed their judgments unbiased in favor of any explanatory theory.

The first of these investigators is W. H. Hudson, who represents a period a little antecedent to the present specially critical experimental era. I quote from his well-known *The Naturalist in La Plata*, published in 1892: "I wish now to put this question: What relation that we can see or imagine to the passion of love and the business of courtship have these dancing and vocal performances in nine cases out of ten? In such cases, for instance, as that of the scissor-tail tyrant-bird, and its pyrotechnic evening displays, when a number of couples leave their nests, containing eggs and young, to join in a wild aërial dance; the mad exhibitions of ypecahas and ibises, and the jacanas' beautiful display of grouped wings; the triplet dances of the spur-winged lapwing, to perform which two birds already mated are compelled to call in a third to complete the set; the harmonious duets of the oven-birds, and the duets and choruses of nearly all the wood-hewers, and the wing-slapping aërial displays of the whistling widgeons; will it be seriously contended that the female of this species makes choice of the male able to administer the most vigorous and artistic slaps? . . . There are many species in which the male, singly or with others, practises antics or sings during the love-season before the female; and when all such cases, or rather those which are most striking and *bizarre*, are brought together, and when it is gratuitously asserted that the females *do* choose the males that show off in the best manner or that sing best, a case for sexual selection seems to be made out. How unfair the argument is, based on these carefully selected cases gathered from all regions of the globe, and often not properly reported, is seen when we turn from the book to Nature, and closely consider the habits and actions of all the species inhabiting any one district. We see then that such cases as those described and made so much of in the 'Descent of Man,' and cases like those mentioned in this chapter, are not essentially different in character, but are manifestations of one instinct, which appears to be almost universal among the higher animals. The explanation I have to offer lies very much on the surface. . . . We see that the inferior animals, when the conditions of life are favorable, are subject to periodical fits of gladness, affecting them powerfully, and standing out in vivid contrast to their ordinary temper. And we know what this feeling is—this periodic intense elation which even civilized man occasionally experiences when in perfect health, more especially when young. There are moments when he is mad with joy, when he cannot keep still, when his impulse is to sing and shout aloud and laugh at nothing, to run and leap and exert himself in some extravagant way." [11]

The reader is asked to note what Hudson says about picking out such evidence as will help the case for sexual selection, and saying nothing about evidence which will not help it. Beyond question the dogma of natural selection, especially the Weismannian perversion of it, has flourished largely on this sort of thing. Nor has natural selection alone among biological theories had the benefit of assorted evidence. Indeed the whole elementalistic mode of interpreting living nature may be characterized as one whose doctrines depend largely upon "special privilege," to adopt a phrase lately much used in the economic world, as to evidence for their support.

The other investigator upon whom we draw is Prof. Julian S. Huxley, whose work is that of a field zoologist imbued with the exacting spirit of the present day. Huxley's studies are devoted to the mating habits of birds, so there can be no question that the activities he describes are intimately connected with reproduction. Of the numerous species dealt with in the paper now before us, we notice first the Great Crested Grebe. It is highly significant that in this species mating takes place *before* the so-called courtship performances begin, so this latter process can not be essential to securing a mate. The female is "courted" after she is got possession of. The courtship activities begin soon after pairing, two entirely different sets of ceremonies being involved in the activities. One of these Huxley calls ceremonies of mutual display, the other, ceremonies of coition. The highly elaborate mutual display performances are fully described but can not be reproduced here. They consist in a variety of body attitudes, head and wing and feather movements, swimmings and divings, and call-notes, the whole lasting some minutes. Concerning this preliminary operation, Huxley writes:

"The most noticeable thing about all these ceremonies is that they are 'self-exhausting'—they do not lead on to anything further. Looked at from the physiological point of view, they seem to me to be nothing but 'expressions of emotion': the birds act thus because they are impelled to do so, because they enjoy it. Looked at, on the other hand, from the evolutionary point of view, they seem to have been developed as a bond to keep the pair together." [12]

Following these preliminaries, the ceremonies of coition take place, these being less striking, though characteristic.

Speaking of his studies on the mating habits of some of the warblers, and referring to differences of interpretation between himself and W. P. Pycraft, another observer in the same field, Huxley writes: "In this, Mr. Pycraft and myself are, I think, agreed; to both of us the 'display' of the male Warbler is nothing but a *direct* expression of sexual excitement, scarcely, if at all, modified by Darwinian Sexual Selection—nothing but the way in which nervous disturbance caused by sexual excitement happens to liberate itself. General nervous discharge will cause general muscular contraction; and something approaching this is here seen—rapid hopping, extension and fluttering of the wings, spreading of the tail, bristling up of the feathers on head and throat, and utterance of a series of quick sounds. This expresses a condition of readiness to pair, and doubtless to the female comes to be a symbol of the act of pairing.

Hence, as far as the female is concerned, the act of pairing has come to depend upon this stimulus (acting of course on a suitable internal physiological state). This is no more strange in the bird than it is that in ourselves thoughts and emotions of love well up at the sight of some tangible object connected with the beloved." [13]

But it is in the sex function itself that the tendency to overdo manifests itself with greatest force. In fact, the familiar and ominous expression "sexual excesses" as applied to the human animal, indicates very truthfully what is before us. The whole phenomenon of competing and fighting among the males of all higher animals for possession of the females, with its momentous consequences in dozens of ways, may truly be said to rest back on the excessiveness of the sex impulse and instinct. Since as a general rule the males and females of animal species are approximately equal in numbers, pairing off two by two, after the manner of the population of Noah's ark, might occasion but little and mild competition could each male and each female be satisfied with *one* mate, in accordance with the allotment which the numerical equality would make. And the pertinent question may be raised in passing, would not such a mode of pairing secure the perpetuation of the species quite as well as, possibly better than, the method which is so largely in vogue?

Highly suggestive seems to me in this connection, observations I have recently been able to make on the mating habits of one of the California "surf perches" (*Cymatogaster aggregatus*). This is one of the numerous viviparous bony fishes peculiar to our coast. The species under attention lives quite normally, as far as one can see, in the aquaria of the Scripps Institution; so what may be assumed to be its typical habits can be observed continuously.

Strict monogamy appears to prevail in the species. At least this is true with the specimens—three males and four females under observation, and so far as a particular breeding period is concerned. Each male begins his attentions while his fiancee, so to speak, is heavily gravid from the previous mating (when and how accomplished we unfortunately know nothing about beyond the fact that it must have been before the individuals under observation were brought to the aquarium from the sea, about six weeks before the mating began.)

In the case of one pair, the amours of the male continued more than two weeks, the first few days of which were before the family of young began to be born, the period of parturition extending over three days. Although there was no indication on the part of the other males of intentions or even desires toward the spouse (as she may now be called) of this male, he was quite pugnacious, directing his seemingly unnecessary operations against the other females as well as against the other males. It should be said, however, that his antipathies were considerably greater against the males than against the other females. The other two males took partners after much the same fashion; but since both of these were somewhat smaller, and fully acknowledged the over-lordship of the one singled out in our account, their performances were less clear cut.

Specially noteworthy is the character of the amours of the male, which alone or almost alone, seems to take an interest in the performance. No contact, or at least only the slightest, of the male with the female was seen though the fish were under observation much of the time. A peculiar downward darting of the male first on one side then on the other of the female, close to her but not quite touching her, was one of the favorite manoeuvers. But various rapid circlings about, up and down, head-on and tail-on, over and under, and in nearly all possible ways, may be witnessed.

The full meaning of this monogamic (temporarily so, at least), largely non-tactual type of mating we do not know partly because we have not yet all the facts; but I suspect it to be important. But this much is clear as to its bearing upon the point uppermost in this discussion: There is an excessiveness of activity in a variety of ways, particularly in the driving of other females, the presence of which in the vicinity of the mate is merely incidental and utterly harmless.

Obviously it is the demand, instinctive or organic or both, for *more* sexual gratification than the natural numerical scheme of the two sexes provides, and the actual necessities of race perpetuation demand, which is largely responsible for the contests to secure mates, so characteristic of all higher animals. The bull fur seal must have forty or fifty mates, instead of the one which the numerical equality of the two sexes would naturally give him; hence the fierce combats among the males, with the result that a great majority of the whole male population at any one time is forced to remain outside the "harems" during the mating season. And some such eliminative process must occur in all species where the sexes are about equal in numbers, and where promiscuity in pairing is practised.

Nor are the injuries and disasters which may result from the driving power of the sex-impulse restricted to competing individuals of the same sex. The mates sought after not infrequently suffer seriously from the excesses of the seeking males, the females being usually more passive and hence the more liable to injury in this way. Thus, J. S. Huxley has lately told of the exhaustion and actual death of the female mallard duck from being repeatedly "tread" by the males, the same and different individual males participating in the strangely destructive performance.

Finally, the individual itself is not safe from self-injury through its own sex impulses. Some of the forms which this sort of thing may take in the human species are too familiar, too disastrous and too repugnant to need illustration in proof of their reality. That they occur also more or less among animals is well known to all who have had considerable experience with domestic animals.

Excessive activity in connection with the alimentary function must now be glanced at. That there is no nice quantitative balance between the food necessities of the animal and the food gathering instincts and impulses and efforts on the basis of the principle of natural economy and parsimony, is shown conclusively it would seem by many animals which have the storing habit. The honey bee is an example of this among insects. Given a sufficient supply of flowers to work on, in the wild state these bees seem always to store away more food material than they consume.

The extent of their honey-making is limited rather by the raw material available and by their own restricted physical powers than by their nutritional needs. This is the impression I have from my observations on wild and tame bees and I find it to coincide with that of other naturalists whose opportunity for observing wild bees has been much greater than mine. For example my esteemed naturalist friend, Mr. Frank Stephens of San Diego, California, reminds me that the view is confirmed by the fact that in "bee trees" a portion of the comb containing honey is not infrequently black and shows signs of being old.

Darwin made quite a point, it may be recalled, of the economy in some aspects of the bee's work. "The comb of the hive-bee," he says, "as far as we can see, is absolutely perfect in economising labour and wax." (*Cell-Making Instinct of the Hive-Bee*, in *The Origin of Species*.) [14] But a thoroughly economic adjustment between different parts of a given complicated operation, and economy of the operation as a whole, are very different.

As an instance of excessive repetition in the food-getting activities among the insects, the following from Fabre may be taken as fairly typical. A solitary wasp of the genus *Sphex* captures and slays a locust, but instead of using it at once for food, or of taking it directly into her home, she sometimes leaves it on the road, and runs to her home, even though this is threatened by no danger. Then after a time she returns to the game. This going-and-coming may be performed repeatedly before the carcass is finally taken into the dwelling. If by chance the game is removed during the absence of the wasp, the wasp returns to the spot where her load was left, but, not finding it, she, nevertheless, keeps up the going-and-coming for some time. The first back-and-forth journey from game to dwelling is explicable, Fabre shows. "But what is the use of the other visits, repeated so speedily one after another?" Fabre inquires.[15] Something like this almost every one must have seen, who has watched insects at all.

I am quite certain that the acorn storing habit of the California woodpecker, *Melanerpes formicivorus bairdi,* is quite beyond any use the bird makes of the acorns. In the first place, despite much discussion of the question whether the acorns are used at all, and if so how, the case is by no means clear. But the point I particularly wish to make is that whatever use, if any, the birds make of the acorns, whether as food directly or as culture media for worms or insects, these in turn to be eaten by the birds, they store up many more than they utilize. This seems to me highly probable from the fact, which I have ascertained by numerous examinations at different places and times, that many holes contain dried up and wasted acorns which show no signs of having been picked at or otherwise moved after they were inserted into the holes. Furthermore, the great extent of the hole-drilling and filling in itself seems to exceed the bounds of necessity, especially in view of the certainty that the bird's chief food supply is from quite another source. A pine log fifty feet long and one hundred thirty-six inches in girth at the middle, which I found in the San Jacinto Mountains, contained on a fairly careful estimate 31,800 holes, many of them containing acorns.

But even were it certain that the acorns are utilized in any manner and to some extent in connection with the feeding function, there are still other evidences than that just adduced of the imperfect and excessive operation of the acorn-storing instinct. As is well known, the bird sometimes extends its drilling operations to wooden buildings to the extent of making itself a great nuisance. I have seen a case where the birds had pierced the rustic of an uninhabited house, so that when the acorns were inserted, instead of filling the puncture as they would fill holes in a tree, they would drop down into the space between the rustic and the inner wall. Apparently the failure to stop the hole, and failure also to perceive why, or to recognize that the hole could not thus be stopped, "fooled" the birds into putting one acorn after another into the same hole, endlessly almost, judging by the great quantity of nuts piled up at the bottom of the space.

While the storing habit of the California woodpecker is undoubtedly exceptional as to extent, it is by no means wholly unique. At least one species of blue-jay (*Cyanocitta cristata*) has much the same habit, in the opinion of most ornithologists who have studied the habits of the bird. An experienced naturalist, E. H. Forbush, has recently said concerning Mark Twain's "Baker's Blue Jay Yarn," in *A Tramp Abroad*, "All of this is not merely amusing; it is good ornithology in so far as it reports the way a Jay acts." [16] This story, it may be said for the benefit of any reader so unfortunate as not to know it, turns upon the performance of a jay similar to that narrated above about the California woodpecker, the acorns, and the old house.

The habit of the shrikes (genus *Lanius*) of impaling their victims and leaving them, almost certainly operates more or less independently of, and often in excess of, the food requirements of the birds. "My observations," says Forbush, "have led me to believe that it rarely returns to eat what it has thus cached, unless driven to do so by hunger resulting from adverse fortunes of the chase." [17]

Nor is there much if any question that something of the same sort occurs among mammals which have the food storing habit. E. T. Seton quotes the following from Dr. John Wright concerning the big eastern chipmunk (*Tamias striatus griseus*): "It is a most provident little creature, continuing to add to its winter store, if food is abundant, until driven in by the severity of the frost. Indeed, it seems not to know when it has enough, if we may judge by the surplus left in the spring, being sometimes a peck of corn or nuts for a single squirrel." [18] There are many other statements by the best authorities, especially concerning numerous species of mice, which strongly suggest a like superabundance of storing activities. But for the rest I will mention a case that has come to my own notice.

I am indebted to Mr. Frank Stephens for information about and the opportunity to witness to some extent for myself the operations of the storing instinct and feeding habits of the Antelope Ground-Squirrel (*Ammosperinophilus leucurus*). This chipmunk-like little squirrel proves to be so readily domesticable that it becomes almost as familiar a household member, at least for Mr. Stephens' household, as a domestic cat. Although an account of the habits of the single individual in Mr. Stephens' possession can not yet be told fully by a long ways, a few points of much interest for the present discussion are positive enough.

In the first place the genuinely instinctive character of the storing habit is established by the fact that although the specimen under observation was taken soon after birth, and has lived all its life in complete isolation from parents and all its kind and has been furnished artificially with an abundance of food, its storing operations are carried on constantly and almost as perfectly, so far as one can judge.

as though it were living in the natural state. This fact in itself is evidence that the instinct is not determined solely by immediate needs of the individual. But much more convincing evidence furnished by this case to this effect is in the particular way the instinct works. For example, this species possesses cheek pouches for carrying food as do so many rodents which have the storing habit. When nuts, grain, etc., are presented to the animal she very rarely eats them immediately even though manifestly hungry, but carries them away to some distance, one at a time; going back and forth and placing the articles in her two pouches till these are quite full. And these little pre-storage journeys, as they may be called, are often definite in character. At any given time they end at nearly the same spot, and the animal takes nearly the same position while the article is being prepared for and inserted into the pouches. This is clearly the typical procedure in filling the pouches, though it is varied considerably from time to time.

As to what follows the pouch-filling there is considerable variation—normally so it appears. In case the animal is hungry she may quietly extract the nuts from the pouches and eat them. Or she may run about for some time with her cheeks bulging full. Or she may take her load off somewhere and lay it away either in some cache previously established or in a new one. The cache may be in a bed of sand if this is at hand; or it may be in or under some old garment or piece of cloth or paper which the surroundings may present.

An especially interesting fact noticed by Mr. Stephens is the tendency shown on the part of this squirrel to carry the articles to as distant a place from where it gets them as can well be reached.

On the whole there is no doubt that we have here a variedly illustrative example of activity over and above need in the operation of an instinct.

This bare touch, so far as instances are concerned, of overactivity in connection with reflexes, and especially with instincts which are on the whole useful, leads naturally to the great field of animal play. Space limitations prohibit us from taking more than a bird's eye view of this field. Fortunately, however, even such a view can be quite effective for our purpose because of the well-known work of Karl Groos, *The Play of Animals.* Our sole purpose here, as in the rest of this discussion, is to answer the question whether animals do or do not carry their activities which on the whole are fundamental to their existence beyond what is necessary for their own individual requirements. With Groos's explanatory theory of play we are concerned only so far as it involves the question of fact upon which our present interest centers. That most if not all animal activity which can rightfully be called play, and which is not intelligent, is instinctive, we believe Groos has conclusively shown. The explanation adopted by Spencer and others that play is the useless imitation by young animals of useful activities performed by their seniors, the imitative acts being useless because merely the overflow of "surplus energy," is certainly inadequate, as Groos has insisted. That animals constantly go through performances playfully which they have had no chance to see or to have otherwise impressed upon them from without, is as certain as that they constantly perform useful acts in this way.

It consequently results that a source of energy for play, that is, for actions which are not immediately essential to the existence of the organism, must be an endowment of the organism no less certainly than that a source of energy must exist for actions which are essential to its existence. So Groos's statement: "A condition of surplus energy still appears as the *conditio sine qua non* that permits the force of the instincts to be so augmented that finally, when a real occasion for their use is wanting, they form their own motive, and so permit indulgence in merely sportive acts,"[19] becomes a statement of fact if by "surplus energy" we understand energy available for, and upon occasion used for, acts which are not indispensable to the existence of the individual.

The quantity and generality of play performed by animals may be taken as one important measure of the extent of the energy possessed over and above what is essential for their normal individual existences, and this without reference to whether or not the play may be useful as a preparation for future essential activities, or for recreation only. The fact can hardly be too much insisted upon that ulterior usefulness of the organism's acts, whether to the species generally, to offspring, or to the individual's own future, cannot possibly be a sufficient explanation of the energy immediately required for the act itself. Even though an animal does nothing whatever except by reason of its hereditary endowments, or in the interest of its offspring; and though the real purpose of much that it does looks to its own future, it must nevertheless continue to eat, digest and assimilate, and breathe. The subdivision of biology which has come to be known as physiology has for its distinctive task exactly that of studying the *present* activities of the organism. With the organism's past, whether individual or racial, and with its future, whether individual or racial, physiology can be concerned only indirectly.

Summary of Organismal Character of All Subrational Psychic Life

Having now examined broadly though far from exhaustively the psychic life of the animal in each of its most obvious phases, the highest rational phase, the emotional phase, the instinctive phase, and the reflex phase (in which tropisms are included) for purely descriptive and classificatory purposes, let us briefly summarize what we have learned.

In each phase we have found the *organism*, living, whole and normal, indispensable to a comprehension of the phenomena examined. Or, expressed in a different way, we have found it possible in each phase to reach only a very imperfect understanding of the phenomena by referring them to the elements which can be discovered in them. For example, the theory of association of ideas is inadequate to explain rational life, in such manifestations as apperception and mental initiative and creativeness.

In the emotional phase, in such emotions as fear, rage and sex passion, not only does cursory observation recognize the involvement of a large part of the organism, but physiological investigation is able greatly to extend our recognition of this involvement by showing how the nervous system in its cerebro-spinal and its autonomic divisions, the circulatory, the alimentary, and the internal-secretory systems, are essentially and reciprocally involved.

As to the organismal character of psychic life in the phase of instinct, it suffices to recall that one of the most widely accepted criteria for differentiating instinctive from reflex activities is that the former involve the organism as a unity, a whole, while reflexes, according to this criterion, pertain only to limited portions of the organism. "An instinct is a more or less complicated activity manifested by an organism which is acting, first, as a whole rather than as a part." [20] To this statement of the matter may be added that when the instinctive act is in the interest of the individual performing it, the act is not only *by* but *for* the individual as a whole.

As to the reflex phase (if that is to be reckoned as psychic) the organismal nature of tropisms has had so large a place in our discussion that surely no more need be done in this summary than to remind the reader of our discussion of tropisms. And even reflexes of a simpler form than the tropisms—indeed the abstract conception of the "simple reflex," though not, perhaps, involving the conception of the organism as a whole, yet is not comprehensible on elementalistic principles, as our examination of Sherrington's investigations revealed. And such phenomena as those of the spreading and compounding of reflexes are quite incomprehensible except on the organismal principle, even though the whole organism may not be involved, observably at least, in particular reflex acts.

Specificity of Subrational Psychic Life

The concluding section of this descriptive chapter on psychic integratedness must be devoted to the specificity, not to say individuality, of animal behavior in all its phases. The vast body of trustworthy detailed knowledge now in our possession justifies, I am quite sure, the following generalized statement under this head: ***It is exactly on the psychic side of animal life, psychic being taken in the broadest sense, that animals are most differentiated from one another, both as to individuals and as to species.***

Taxonomic zoology is based almost entirely on structural attributes of animals. This results from reasons that are obvious, speaking generally, and constitutes a justification of the fact from a practical standpoint. Nevertheless the purely practical advantages of the classificatory systems as they have been built up have been, and are, gained at the expense of several rather serious disadvantages. One of these is, as advance of knowledge leads us to realize, that our well-nigh exclusive attention to structural differences and likenesses has tended strongly to divert attention from functional differences and likenesses. It is of fundamental importance for a truly comprehensive science of organic beings, that is, for a philosophical biology, to regard our synoptic classifications not as a final result of knowledge-getting, but rather as a life-sized sketch, as one may say, of the whole living world, to facilitate the gigantic task of completing the picture through the coöperation of numberless artists, the completion to be accomplished by filling in the sketch with the entire round of attributes, structural and functional, presented by the natural lives of organisms. I have dwelt somewhat at length on this matter elsewhere,* and can refer to it here only as a background for what I wish to say about psychical specificity.

Two extracts must suffice. "No biological phenomenon is adequately interpreted or dealt with experimentally, until it has been considered with reference to the place that the organism to which it pertains holds in the system of classification." And further: "What I affirm is that the inductive evidence has now gone so far toward proving every sharply differentiated species to contain some differentia in all the main provinces of their structure and function, that to assume the absence of such differentia in any given case is unwarranted." [21]

I want to utilize these earlier general statements about organic specificity, as a basis on which to rest a generalization concerning the specificity of psychic attributes. So enormous is the observational data available for illustration here, that in lieu of presenting any of them I am going to state in a wholly dogmatic fashion the generalization toward which we are certainly being led by modern crucial researches on animal behavior. Let us imagine ourselves possessed of an approximately exhaustive descriptive knowledge of the behavior of the whole animal world, this knowledge being, however, unaccompanied by any knowledge whatever of the corporeal nature of the animals. This behavior-knowledge would fall naturally into categories larger and smaller, the

* *The Place of Description, Definition and Classification in Philosophical Biology,* in *The Higher Usefulness of Science* (Boston, 1918); also *The Scientific Monthly,* November, 1916.

smaller ones being for the most part subdivisions under the larger. Then let us imagine this system of behavior-knowledge compared exhaustively with a later-acquired, equally exhaustive knowledge of the corporeal nature of all animals. The two systems would be found to match each other very nearly as closely as though the two had been worked out together, much as they are being actually elaborated by structural and functional zoology. In other words, the species, genera, orders, and so on, of animals are differentiated from one another and coördinated with one another by their "behavior," that is, by their whole round of psychical and reflex attributes, much as they are by their corporeal attributes.

The inductive evidence for such a generalization is being produced at the present time by three quite definitely marked-off kinds of research. These may be designated as (1) qualitative field researches, (2) laboratory experimental researches, and (3) quantitative field researches, the definitely quantitative method being statistical.

The first-mentioned class of investigations is typified by the earlier field zoologists, whose aim was to learn, as exhaustively as possible in a purely qualitative way, the habits of animals in nature. Workers of this class are the typical zoological naturalists of the history of animal biology. Aristotle, Conrad Gesner, John Ray, Charles Linné, P. S. Pallas, Gilbert White, J. J. Audubon, J. H. Fabre, A. R. Wallace and A. Forel may be named as conspicuous examples of pre-modern members of this class; and Charles Darwin stands out sharply as a representative of it, but as a transition to the modern period, the transitional character of Darwin being seen not only in the doctrines he proposed but as well in his intimate combination of the experimental method with the older method of observation.

By the modern period of research in field zoology I mean the period during which, while the natural history point of view and attitude are retained, the critical rigor of modern science generally is practiced, and experimentation in one form or another is employed as a supplementary agency wherever and whenever possible. One of the best examples of this type of zoological research and writing is *Ants*, by W. M. Wheeler. But a considerable portion, and fortunately an increasing portion, of experimental research in animal behavior is being done quite in the spirit of field zoology. The work of R. M. Yerkes deserves mention as perhaps the most definitely purposed and executed combination of the field and experimental methods for investigating the behavior of mammals and birds, that has yet been made. But much of the research recently named animal ecology tends strongly toward rigor in field investigation. This kind of study is specially adapted to bring out the specific nature of behavior, since the *group* of organisms, species, etc., as a whole, occupies a central place in the student's interest, so that if behavior is attended to at all its differential features are likely to receive attention along with the differential structural features.

(2) Laboratory experimentation on animal behavior has, as previously indicated, been prosecuted more intensely and widely in the modern period than either of the other classes of investigation. In fact, it may be said to be distinctive of the period, and to have set the standard as regards rigor for the other types of investigation. From its very nature, however, it is not calculated to bring the specificity of behavior to a central place in the student's interest. Singling out as it does one or a few attributes at a time as they are exhibited by one or a few individuals of one or a few species, breadth and penetration of comparison are liable to be sacrificed. This kind of research tends to be extremely particularistic in every way. Nevertheless, painstaking and judicious workers, like Englemann, Forel, Binet, Wasmann, Romanes, C. Lloyd Morgan, Verworn, Jennings, Loeb, Holmes, and Parker, generally state what species their in-

vestigations have dealt with, so a reader interested in the question of specificity can usually detect evidences of differences in the behavior of different species, even though the investigator himself was obviously little interested in the subject, and so took no pains to bring out such evidence.

Indeed, the fact that species-differences in behavior can so frequently be recognized in descriptions even though the writer's general attitude may, if anything, militate against the disclosure of the differences, is rather strong evidence of the general prevalence of such differences.

(3) Although statistical investigation of animal behavior has been much less prosecuted than has either of the other types it is, nevertheless, within the limits of its availability, a very valuable method for revealing species differentia, its efficacy consisting in the fact that species may be compared with reference to different behavior traits taken one by one, and on the basis of quantitative data covering considerable samples of whole populations. The method is specially applicable to the minute floating life of the seas and lakes, known as plankton, and is being much employed to this end at the Scripps Institution for Biological Research. It cannot be described in detail here, but consists essentially, as employed at this Institution, in collecting great quantities of organisms by agencies as nearly quantitatively constant and accurate as possible, in counting the organisms thus secured, and in correlating the biological values thus obtained with quantitative studies on the physical environment of the organisms, these environmental determinations being made simultaneously with the collection of the organisms. By this means one element in the behavior, that namely of the up-and-down journeys in the sea, long known to be performed by many oceanic species, has been studied with a fair degree of quantitative accuracy as to the extent of travel, time required for each journey, and environmental influence. A considerable series of species have been compared on this basis.

Two quotations are all that space will permit us to give for showing what these investigations are bringing out on the subject of specificity in behavior. The animals referred to in the first quotation constitute a group of small, arrow-shaped worms known as the Chaetognatha—bristle-jaws. "Each species occurring in the San Diego region has its own definite and specific manner of vertical distribution, just as truly as it has its own morphological characteristics." [22]

Similar results have been obtained by the same methods applied to a very different group of animals, minute crustaceans of the ubiquitous order Copepoda. The investigations on this group have been made by Dr. C. O. Esterly, and the results are specially interesting in this case, because Doctor Esterly has applied laboratory experimentation, to some extent, to the same animals, and has found a good degree of concordance in the results of the statistical and the experimental investigations. "A heterogeneous assortment of forms may be obtained in the same collection but each has its own way of reacting toward the elements of its environment." [23]

It is the indubitable trend in one direction of the vast evidence from these three quite different classes of research on animal behavior that to my mind justifies such a conception of specificity of psychical and reactive animal life as that formulated above. Something of the probable meaning of this specificity we shall see in the next chapter.

REFERENCE INDEX

1. Loeb ('02) 189
2. Morgan, C. L. 10
3. Royce 328
4. Royce 329
5. Holmes ('11) 92
6. Holmes ('11) 93
7. Peckham, G. W. and E. S. 53
8. Holmes ('11) 21
9. Jennings ('06) 49
10. Holmes ('11) 95
11. Wallace, A. R. 555
12. Huxley, J. S. 151
13. Huxley, J. S. 156
14. Darwin, C. ....I, 353
15. Kellogg, V. L. 648
16. Nature Lovers' Library II, 218
17. Nature Lovers' Library III 100
18. Seton ....I, 360
19. Groos 15
20. Wheeler ('10) 518
21. Ritter ('18) 119
22. Michael 46
23. Esterly 11

Chapter XXIV

SKETCH OF AN ORGANISMAL THEORY OF CONSCIOUSNESS

Remarks on the Hypothetical Character of This Chapter

HYPOTHESIS and theory will dominate in the task upon which we now enter and in this respect the present chapter will differ sharply from the preceding chapters. Fact, description, classification, and restrained generalization have been the leading motives up to this point. One main and several subsidiary hypotheses will be central in the discussion. Into the presence of these will be summoned many of the facts and generalizations previously set forth. The purpose in this summoning will be on the one hand to test the hypotheses by the facts and generalizations and on the other hand to see how the facts will look in the light of the hypotheses.

This announcement of the hypothetical and theoretical character of the task now before us, will give us two advantages: It will justify a dogmatic form of expression at times which we should not otherwise feel privileged to use; and will justify a brevity of treatment which would not be possible were we aiming at thorough generalization and demonstration. Hence the justification of undertaking to deal with so vast and vital a subject in the limits of a sketch.

The Natural History Method and the Study of One's Self

Insistent as I have been on the importance of the natural history way of approaching the phenomena of the living world, in entering upon the present discussion I must emphasize this more than ever and must call attention to the particular character of this importance in our present undertaking.

The natural history method of viewing organic beings is *per se* the comprehensive method, one of its best mottos being, as we have repeatedly seen, "neglect nothing." That knowledge of organisms separates itself sharply into departments is no deterrent to the naturalist against utilizing any knowledge he may come upon that will contribute to his main aim—that of understanding organisms. Who or what shall restrain me from observing and carefully thinking about any fact of my own being which promises to help me on my road to such understanding? The foremost zoologists, of modern times especially, have amply recognized and freely used this principle so far as all physical and some of the lower psychical attributes are concerned. But when it comes to man's higher psychical attributes, zoologists have usually said, sometimes expressly, sometimes tacitly, that these belong to a wholly different realm, a realm with which we have little or nothing to do. And their position of "hands off" as touching man's higher psychic life, has received the readier, fuller sanction in that it has accorded well with the prevalent views and practices of those students, anthropologists, economists, sociologists, and ethicists who have made these higher reaches of human life their special fields of inquiry. But the course of nature can not be permanently thwarted. Such an attempt to wrench human life asunder is bound to fail finally. In the several subdivisions of biology, normal advance has tended to stay the wrenching process, comparative psychology being notable in this tendency.

The opposition to such organic disunion consistently maintained throughout this book reaches its culmination in these chapters on psychic integration. In what follows we shall pass more freely than ever from one phase or aspect to another, over the entire gamut of psychic life both in the individual and in the animal kingdom. If facts of my own subjective life will serve my purpose, I shall be as free to requisition them as to requisition facts of any phase or aspect of my objective life. If the ethical or esthetic or social attributes of the human animal will best illuminate a point, these shall be brought in with as little misgiving as will be anatomical or embryological or physiological or instinctive attributes.

So great store do I lay on this catholicity of attitude toward psychic life, that I shall show by a single instance that at least a few other present-day zoologists have somewhat similar feelings about the zoological character of psychical phenomena. Referring to the controversies which have inevitably arisen over the problem of instinct, W. M. Wheeler says that such controversy "is pardonable, at least to some extent, since the subject itself presents no less than four aspects, according as it is studied from the ethological, physiological, psychological or metaphysical points of view." "From the first two of these," the author continues, "instinct is open to objective biological study in the form of the 'instinct actions.' These may be studied by the physiologist merely as a regularly coördinated series of movements depending on changes in the tissues and organs, and by the ethologist to the extent that they tend to bring the organism into effective relationship with its living and inorganic environment. But that these movements have a deeper origin in psychological changes may be inferred on the basis of analogy from our own subjective experience which shows us our instincts arising as impulses and cravings, the so-called 'instinct-feelings'; and these in turn yield abundant material for metaphysical and ethical speculation." [1] From the context of these sentences we may infer that Wheeler recognizes that the four aspects mentioned under which the subject of instincts presents itself, represent the same number of valid departments of man's mental life. The point I wish to make is that although a zoologist may recognize without cavil that speculation on psychological, ethical, and metaphysical problems which arise in connection with instincts, are legitimate activities of man, and might properly deny that it is incumbent upon him to do much speculating of this sort, yet it would be incumbent on him to take due cognizance of these speculative attributes of the human animal. A truly scientific zoology can not justify itself in issuing a manifesto to the effect that certain attributes presented by some animals do not fall within its province. It may more or less constantly neglect or refuse on practical grounds, to deal with certain attributes; but that is a very different matter from a formal declaration such as many present-day zoologists make, that with these attributes zoology has nothing to do. Such a declaration is self-stunting, if not self-stultifying, in that it is a virtual self-inhibition by zoology of its own growth.

These reflections may be terminated by defining the motives and the mental attitude with which I approach the great problem of consciousness. I come to it not as a metaphysician, not as a psychologist, not as a physiologist, not even as an anthropologist, but as an anthropological zoologist; as a zoologist who in course of his regular professional work takes up the animal group of which he himself is a member, chancing as he does to possess among other attributes that of knowing his own life *directly*, that is, through subjective or self-conscious experience, as well as *indirectly* through objective experience.

Approaching the problem of consciousness in such an attitude and for such a motive, it is impossible to view it otherwise than as one aspect of the larger problem of life generally. For while the psychologically and metaphysically important question of whether consciousness is coextensive with life need not be raised by the naturalist, the indubitable fact that at least a large sector of life is conscious; in other words, the fact that consciousness is a part of life, he can not ignore if he is to deal with consciousness at all. For the naturalist, then, no hypothesis or theory of consciousness can be satisfactory which is not clearly and expressly embedded in and an essential part of an hypothesis or theory of life generally. Our central hypothesis, drafted in accordance with these principles, may now be given.

Formulation of the Central Hypothesis

All the manifestations which in the aggregate we call Life, from those presented by the simplest plants to those of a consciously psychical nature presented by man and numerous other animals, result from the chemical reaction between the organism and the respiratory gases they take, oxygen being almost certainly the effective gas for nearly all animals. An essential implication of this proposition is that every living individual organism has the value, chemically speaking, of an elementary chemical substance.

Let us be promptly explicit in recognizing the character of the two propositions contained in this hypothesis. They are manifestly chemical in large part, and a complete demonstration of their truth is impossible without the aid of chemical research focussed directly upon them. But though clearly chemical, equally clearly they go beyond—far beyond—present chemical knowledge. To speak of a whole organism as equivalent to a chemical element seems at first sight not only unwarranted by positive chemical knowledge, but opposed by such knowledge. Furthermore, the term "reaction" as used in the first proposition undoubtedly seems quite foreign to the technical meaning which chemistry has attached to the word. Indeed so remote to say the least, are these fundamental propositions of the hypothesis from definite chemical knowledge, that if they are entitled to rank as constituting a legitimate scientific hypothesis, this must be on grounds other than those of present-day technical chemistry quite as much as on those of such chemistry. In attempting, consequently, to establish the propositions on a true and useful hypothetical basis, it will be permissible to notice these other grounds first.

Preliminary Justification of the Hypothesis as Such

The proposition that each living individual has the chemical value of an elementary substance, will receive attention first, and the initial step will be to inquire what, in general, the criterion is of an elementary chemical substance. Here, for instance, is a lump of phosphorus. In virtue of what is it declared to be such a substance? Not primarily, let us specially notice, because the phosphorus is *simple*, that is to say, is an element in the sense of not being reducible to still simpler substances. Rather the basal criterion of its being a chemical substance is that upon its being brought into contact under certain conditions with certain other chemical substances, oxygen for instance, there is produced a third substance having very different attributes from either of the original substances. Transformation of substances chiefly through interaction upon one another is the foundation fact which has brought it to pass that substances are described as chemical. That is the fact upon which the science of chemistry primarily rests. Facts and problems of simplicity and complexity, relative and absolute, are later and secondary. The task of chemistry "consists in the investigation of substances and those of their processes by which the physical attributes of the substances undergo permanent changes." (*Handwörterbuch der Naturwissenschaft.*)

Every adequate definition of chemistry and chemical substance and chemical action contains the idea of transformation in one form or another. Clearness on this point is indispensable to our purpose. Chemistry is too often defined, even in elementary text books and in dictionaries, as though the "composition of matter" were its initial and most essential function. But this conception is surely contrary to the history and most essential nature of the science. There is, it seems, entire agreement among competent writers that scientific chemistry is a direct descendant of Alchemy, and a very imperfect knowledge of the history of Alchemy reveals the fact that the every-where present, normal transformations in nature, particularly in inorganic nature, were the foundation phenomena of this old art. One has only to recall the place held by the idea of the transmutation of metals, this idea having usually the practical aim of changing the "base metals" into "noble metals." The "philosopher's stone" and the "great elixir" were magical somethings by which the transmutations could be accomplished.

Greatly significant from our standpoint is the fact that one of the objectives of Robert Boyle (middle of the seventeenth century), who, perhaps as much as any one man, is entitled to be called the father of experimental chemistry, was to rectify the false and mystical notions prevalent in his time about "Elements," "Principles," "Essences," etc. "Tell me what you mean by your Principles and your Elements," Boyle demanded, "then I can discuss them with you as working instruments for advancing knowledge." [3]

What is "behind" the transformations—forces, elements, principles, essences, spirits or what not—is indeed an important and, properly asked, a legitimate question. But—and here is the most vital fact of all—it is a question which can not be raised even, until *after* the transformations have been observed, nor can an answer of objective value be given unless the whole round of observed phenomena, the substances previous to transformation, the transformatory processes, and the new substances, be accepted at their face value, that is to say, at a value which is as near to ultimate truth as any truth whatever, connected with the phenomena.

The elemental constitution of bodies is an inference, always and solely, drawn from their observed corporeal attributes. And chemistry is the science which assumes the task of drawing, elaborating, and systematizing these inferences on the basis of the transformation of the attributes. The meaning of the statement that chemistry is one of the natural sciences is that chemistry is the science which uses its natural history observations to penetrate still more deeply into the constitution of bodies. *Natura a natura vincitur*, nature is surrounded by, is contained in nature, is as fundamental a truth for chemistry as for any other natural science. A living being is as much a natural body as is a piece of phosphorus, and its obvious attributes, its outer-layer attributes, are as essential to its nature as are its inner, its hidden attributes. So any genuinely transformatory changes, and genuinely new products arising through the reaction between the living body and some other body is so far chemical in nature, and the reacting bodies are so far chemical.

A long step toward justifying the proposition that each individual living organism has the value, chemically, of an elementary substance, will be taken if it can be shown that any qualitatively new product whatever results from the interaction between the organism acting as a unit, as one, as an element, and some other element. Having regard to the entire world of living beings, the chances for finding new products which may have arisen in this manner are practically if not theoretically infinite. Manifestly, then, only a very small sector of the entire range of such possible productions can be searched. It must, consequently, be our aim, as always in handling inductive natural history evidence, to choose for examination evidence which shall be most clear-cut, most illustrative, and most convincing.

The sector of organic phenomena best capable of yielding such evidence is, I believe, exactly this of psychic life. And within the great range of this life, the higher conscious life of man is most replete with the evidence we seek. Again within the range of man's higher life, each individual's own private life, even his subjective life, his consciousness, is the evidence most certain and convincing. Translating this last statement into familiar language, one sees that it is only another way—the scientific way—of affirming the truth, that the greatest of all certainties of which man is capable is that of his own existence. I am saying, virtually, that when we analyze, after the manner of objective science, this old familiar affirmation about certainty, and carry the analysis as far as we are at present able to, we find that the sense, or better, the feeling of certainty of self-existence and self-identity *is* in last analysis one of the effects of a transformatory interaction between ourselves and some substance (oxygen?) in our breath, as stated in the first of our two propositions.

That proposition seems then to be hardly more than a recognition that psychic phenomena containing at least the germ of consciousness is a kind of chemical product which has not heretofore been clearly recognized as such, the lack of recognition being due to the strangeness of the product as compared with any chemical products with which experimental chemistry has hitherto occupied itself. But looked at in a really broad and deep way, is it any more difficult for me to interpret a state of consciousness in myself to be a result of chemical action between me and the air (oxygen?) I breathe, than for me to interpret the dim greenish-white luminosity of a piece of phosphorus to be a result of the chemical action between the phosphorus and the air essential to the glowing? From a purely chemical standpoint I do not believe we have any ground for holding that some products of chemical reaction are more comprehensible or less comprehensible than are others.

Chemically viewed the problem now on our hands is entirely one of *fact*—fact as determined by observation alone, and by observation with the aid of experimentation. If it can be shown that each individual conscious being really does behave like a chemical substance in the process of reacting; and if the result of such reaction can be shown to have even one of the essential marks of a chemical product, both propositions of my two-parted hypothesis are warrantable and the hypothesis becomes genuinely scientific—a genuine "working hypothesis"—one, that is, for bio-chemistry to take seriously.

More Systematic Justification of the Hypothesis

That the propositions are demonstrable to the extent of the demand just indicated is my contention. This contention I will now try to make good and will begin with a few remarks on a question concerning the hypothesis which ought to arise instinctively in the mind of every one. That question is: Does such a conception of psychic life and consciousness as that contained in our hypothesis imply any real infringement upon or derogation from *me*, in the deepest sense a real entity properly designated by the terms *person* and *personality?*

On saying that this query ought to arise *instinctively*, I do not mean *ought* in the ethical sense, but in the organismal sense. That is, in a sense which implies that the very nature of the conscious organism is that it is not only self-existent in a measure like every natural object, but that it is self-identifiable, and within certain bounds, self-determinative of its own acts. Now recognizing it to be thus by its "very nature" is only another way of recognizing that it is so in its instincts as well as in its physical organization. But since instinct is more fundamental, more deep-rooted in the organism than is intellect, as phylogenic and ontogenic psychology make clear, if a pronouncement implying a derogation from the reality and natural prerogatives of the individual be issued from the intellect, a response of protest and antagonism would be expected from instinct. This would be expected as an ordinary organic impulse to self-defense and self-preservation.

The Nature of "Outer" or Objective and "Inner" or Subjective

What we have to do consequently is to scrutinize the conscious individual in order to see if it presents any uniqueness of attributes and of transformatory power in reacting with other bodies that is on a par with the uniqueness of an ordinary chemical substance in the same respects. Now it is, as suggested some pages back, exactly in the conscious, the subjective life, that such uniqueness is most easily demonstrable. There are several ways in which the conscious individual manifests this uniqueness. A particularly convincing way, I think, is in the relation between what are commonly known as the objective, or "outer," and the subjective or "inner" sides of mental life. This, consequently, will be the approach to the subject chosen by us and we will enter upon it by returning to Royce, first to his "Outlines of Psychology," then a little later to some of his specifically philosophical writings.

In the first chapter of the Outlines, devoted to initial definitions and explanations, Royce states, simply and clearly, a distinction "between our physical and mental life," which elsewhere he has worked out with great elaboration. Thus: "Physical facts are usually conceived as 'public property,' patent to all properly equipped observers. All such observers, according to our customary view, see the *same* physical facts. But psychical facts are essentially 'private property,' existent for one alone. This constitutes the very conception of the difference between 'inner' psychical or mental, and physical or 'outer' facts."[4]

Ever-present, and obvious as is the comparison here made, it nevertheless is of so great importance that we must stop and reflect upon it, for we shall surely fail to grasp the full measure of what is to follow if we are lukewarm toward one of the elements of it. The element I refer to is the *uniqueness*, the essentially *personal* character of inner as contrasted with outer facts. Every normal person is ready enough to insist that his thoughts, his feelings, his emotions and all the rest of his higher psychical experiences are *his* and his *alone*. The tremendous reality and force of the rights of "private opinion," of "personal conscience" and so forth, among civilized men, hardly need to be expatiated on.

The character of the uniqueness of these experiences, however, concerns practical living less vitally, so we give it less attention. The whole vast range of my mental life, from the lowest, simplest, vaguest sensations to the highest, most bewildering complex emotions, passions, imaginings and thoughts, are my own, absolutely, so far as other persons are concerned. I cannot share them to the least extent with another person. Of course I can let others, especially my most intimate associates, my dearest friends, know a good deal *about* these experiences of mine. But after all, gladly as I would share many of them with these friends, it is utterly impossible for me to do so. *My* experiences must remain wholly outside of *their* consciousness. No two persons can have the same experience any more than they can have the same hands or stomachs. Nor is this all. If mental life is subject to the general biological laws of variation into which we have latterly gained much insight, I am obliged to suppose that these experiences of mine, the whole retinue of sensations, feelings, emotions and thoughts, differ somewhat from the corresponding experiences of other persons. And all observation confirms this supposition—much of it strongly. Inferential evidence could hardly be stronger than that my particular emotional response to opera singing, for example, is quite different from that of many other persons.

Obviously we are here skirting the edge of what modern realism in formal philosophy calls pluralism, and deals with in part as the question of whether percepts are strictly individual and personal. No philosopher with whose views I have become acquainted, has discussed this question so fully, and in my opinion, so illuminatingly as Sellars. The following sentences taken from his chapter, *The Advance of the Personal*, show clearly, it seems to me, that the conclusions he has reached, working from the purely philosophical side, are essentially the same as those arrived at by me, advancing from the biological side: "What may be called the sensory content of our percepts is important,—I do not wish to be understood to belittle it,—but so are the meanings which arise in conection with our bodily activities and motor adjustments to stimuli. Here again, we are face to face with individual factors in perception which even the idealist must recognise and somehow explain. Evidently, perception is not a mere passive presentation, but a construction whose genetic elements can be partially traced. Finally, let us call to mind that percepts are continuous with feelings and with the so-called organic sensations. . . . Once vaguely objective, feeling is now considered subjective or personal."[17] Many other sentences and paragraphs of like purport could be quoted from this author. I have selected this for the twofold reason that it indicates the measure of my agreement with his view as to the personal character of percepts and the rest of conscious life; and at the same time indicates wherein I shall have to out-do him in the matter of validating the individual. A part of our task, to be reached a little later, will be to show that although feeling and all the rest of psychic life is indeed subjectively personal, it is also objectively personal. In other words, it will be my task to remove, or at least to show the way to remove, the vagueness which Sellars asserts, rightly, has hitherto clouded this side of personality. To do this thing is, indeed, one of my most important chances to contribute to a "better philosophy of life."

But since our psychical life, especially our conscious life, is a vast—incalculably vast—complex of experiences, of "contents," sounds, sights, memories, feelings, ideas, many of which are set off very sharply from the rest, are clearly characterizable, and are wonderfully persistent; and since innumerable of these are coming along all the while which have much of genuine newness about them; and since further, these contents of consciousness are intertwined with and are determinative of a vast complex of other contents called volitions which in turn are linked up with and are more or less directive of bodily activities of many kinds, some purely reflex and some instinctive, it seems impossible to escape recognizing, even if one wanted to, that if the verb "to create" has any definite meaning at all the normal, self-conscious animal organism is about the most creative thing we know or can conceive. Indeed it is altogether likely that the very notion of creation, whether natural or supernatural, came initially from the creative activity and the impulse to such activity, of man himself.

We may justly say, I think, that we know all creativeness, chemical creativeness with the rest, through being in our own deepest natures creative, that is, transformative and transformative in the way which we call chemical. We learn *about* the processes of life and call some of the most essential of them chemical just by *performing* those processes as some of our most essential attributes. A portion of the process which goes on within us, together with the corresponding product, constitutes what we call the science of bio-chemistry. This means that according to our hypothesis "objective" and "subjective," or "outer" and "inner" as applied to life, are something quite different from what they have been either in traditional philosophies, or in most, at least, of recent psychology. "When we speak," Royce writes, "of our physiological processes as internal, the word 'internal,' although it here generally implies 'hidden, in whole or in part, from actual outer observation', does *not* imply 'directly felt by us ourselves.' "[5] My hypothesis implies a denial of the correctness of this statement. I say that in the sum total of the "contents of consciousness," a nether segment, as one might call it, of physiological processes is "directly felt by ourselves." *There is no content of consciousness which does not contain an element that is internal or subjective in whatever sense any other content of consciousness is internal* or *subjective*. And per contra, *there is no content of consciousness which is not objective to some extent, in whatever sense any other content of consciousness is objective.* The mind, according to this conception, is not something which uses the brain or any other part of the organism merely as a tool with which to make thoughts and other contents of consciousness. Nor on the other hand is consciousness of the nature of a secretion, the gland for which is the brain, though unquestionably the brain has an essential part in the production of thought and the higher contents of consciousness. Among the consequences of the reaction between the organism and the air we breathe are consciousness with its marvellously rich and varied contents.

But at this point I must specially request the reader to notice that I am not pretending to describe and explain all the contents of consciousness. In other words it is not a *theory of knowledge*, but a *theory of consciousness* that I am sketching; and knowledge in the strict sense, and consciousness are very different. They differ, according to my understanding, much as the fully developed, physical organism differs from the living substance, or protoplasm, of which the organism is composed. Consequently I am not even concerned primarily with sensation in so far as this implies sense organs or even nerves and nerve terminals of the simplest kind. Rather I am dealing with the stages and conditions antecedent to consciousness and in which it is latent, in much such way as the cytologist when he studies the living substance of all sorts of tissue-cells is not dealing with organs and the organism in the full sense, but only with their substrata. But although it is not knowledge, properly speaking, either in its conceptual or perceptual aspect that I am discussing, since my enterprise does take me across the border line and a short distance into the realm of knowledge, I must, in the interest of historical continuity and setting, say a little more than I have said about the general nature of knowledge.

My assertion should be taken literally that there is no content of consciousness which is purely either subjective or objective, inner or outer, conceptive or perceptive, ideational or impressional, or whatever form of expression be given the antithesis here implied. That every content of consciousness which exists or can be conceived has an essential element of both members of the antithesis is exactly what I mean. To illustrate, even the axioms, postulates, or whatever else may be counted as most ultimate in mathematics contain an element of the outer, or objective, as well as of the inner, or subjective. These mathematical contents of consciousness I single out to illustrate my meaning because they have been clung to by philosophers and scientists more tenaciously than any others as purely subjective or mental. And further there is a strategic gain in this reference to mathematics in that it brings into the open the fundamental opposition of my hypothesis to one main root of Cartesian philosophy; the philosophy, that is, from which the modern doctrine of psycho-physical parallelism has grown. Our thinking, which Descartes held proves our existence, really proves it only in so far as it shows that among the activities essential to the human organism thinking is one. In other words the "therefore" in "I think, therefore I am," is true only because "I am, therefore I think," the reverse proposition, is also true and includes the other truth. The lesser truth is true because it is an essential part of the larger truth, much in the same way that the cells of a multicellular organism are alive because they are essential parts of the organism.

We need not inquire how, from this serious shortcoming of Descartes' description of psychic life Descartes went on to the conclusion that "there is nothing really existing apart from our thought" and that "neither extension, nor figure, nor local motion, nor anything similar that can be attributed to body, pertains to our nature, and nothing save thought alone; and, consequently, that the notion we have of our mind precedes that of any corporeal thing, and is more certain, seeing we still doubt whether there is any body in existence, while we readily perceive that we think."[18] Nor need we concern ourselves with the voluminous and tedious reasonings by which a considerable number of moderns, following Descartes's lead, have convinced themselves that they have "reduced" all reality or at least all reality that really amounts to anything, to quantity. Enough now to remark that every modern biologist who really accepts the basal data of his science, must agree that "Psycho-physical paralellism . . . stands to-day as the scandalous but irrefutable consequence of postulating a material world without qualities and a world of minds that lack spatiality and exists—*nowhere*." [19] One way of characterizing my hypothesis would be to say that it is an effort to remove this scandal by showing wherein the postulation noted by Dr. Montague is not true.

The genetic relationships of my hypothesis can be still farther indicated by coming on down from Descartes to Hume then from Hume to Huxley and finally to G. F. Stout and John Dewey as philosophers of to-day. Hume's nomenclature for the subjective and objective sides of man's psychic life is "Relations of Ideas" for the first, and "Matters of Fact" for the second. Of the first kind says Hume, "Are the sciences of Geometry, Algebra and Arithmetic; and in short, every affirmation which is intuitively or demonstratively certain." . . . "*That three times five is equal to half of thirty*," is a simple illustration of the relation of ideas. And, "Propositions of this kind are discoverable by the mere operation of thought, without dependence on what is anywhere existent in the universe."[20] And further on, Part 2, same section, we read: "It must certainly be allowed, that nature has kept us at a great distance from all her secrets, and has afforded us only the knowledge of a few superficial qualities of objects; while she conceals from us those powers and principles on which the influence of those objects entirely depends." Then Hume goes into a discussion of the operations and relations of the "superficial qualities" and "secret" powers of objects which is so similar to my treatment of the relation of the organism to the attributes of certain objects (chapters 20 and 21 this book, and, more particularly, my essay *Is Nature Infinite?*[21]) that it seems as though his words must have been in my mind when I thought out what I have there written, though I certainly was not conscious of Hume's views. And this subconscious influence appears the more probable in that I have almost conclusive proof of having read his argument not long before my own was written. I am certain, however, that if his statements were in my mind they were only in its proconscious part and were not nor ever had been in its full-conscious part. In other words, if I had read his words I had not grasped their full significance. This probable instance of the "sub-" or "pro"-conscious I refer to not so much because of its interest in this instance, as because of its bearing on my conception of the nature of consciousness. The discussion by Hume to which I refer is that in which he talks about the sensible qualities and the "secret powers" of the bread we eat. "Our senses inform us of the color, weight, and consistence of the bread," he says, "but neither sense nor reason can ever inform us of those qualities which fit it for the nourishment and support of a human body." The particular puzzle upon which Hume comes in this matter is the fact that although the examination here and now of a natural object gives us absolutely no clue as to what latent attributes ("secret powers," he calls them) the object may possess, when we examine a second object of the same kind we assume that the same secret powers are possessed by the second object. "If a body of like colour and consistence with that bread, which we have formerly eat, be presented to us, we make no scruple of repeating the experiment, and foresee, with certainty, like nourishment and support. Now this is a process of the mind, of thought," Hume goes on to say, "of which I would willingly know the foundation." "The bread," he says, a little farther on, "which formerly I eat, nourished me; that is, a body of such sensible qualities was, at that time, endued with such secret powers: but does it follow that other bread must also nourish me at another time, and that like sensible qualities must always be attended with like secret powers? The consequences seem nowise necessary. At least, it must be acknowledged that there is here a consequence drawn by the mind; that there is a certain step taken; a process of thought, and an inference, which wants to be explained." Then after a little further argument to show the necessity of recognizing such a process we find this to me exceedingly interesting passage: "There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument. What that medium is, I must confess, passes my comprehension; and it is incumbent on those to produce it, who assert that it really exists, and is the origin of all our conclusions concerning matter of fact."

The great merit here shown by Hume is his ability to push the analysis of his problem to the very limit of the positive information he had to go on, recognise exactly wherein his information was lacking, and then stop without running off into a purely speculative substitute for his deficient knowledge. According to my hypothesis the unknown "medium" which he saw must exist, the researches of a century and a half since he wrote, in chemistry, physiology, general zoology and botany, and psychology, have enabled us to see is the individual animal organism reaching with the respiratory substance (oxygen?) it takes in. In this one particular and, from the standpoint to which we have been accustomed, very peculiar case, the reaction is at one and the same time part of the essence of both ideas and impressions in the Humean sense, the reaction being the "medium" or the "certain step" by which the inference is drawn, this inferring being possible because of the continuity of the organism as a person, or self, and the persistence of the respiratory substance as the same identical thing from the past through the present into the future.

We will now notice how Huxley, because of his much more extensive knowledge of the structure and function of animals than Hume possessed, was able to draw still closer than Hume could to the heart of the old Mind-Body puzzle. The gist of Huxley's position on, and contribution to, the problem can conveniently be presented through his remarks on the question of innateness of various aspects of psychic life, these remarks occurring in his essay on Hume. After pointing out that neither Locke nor Hume seemed to know exactly what Descartes, the originator of the modern conception of innate ideas, meant by his phrase "idées naturelles," Huxley quotes Descartes as follows: "I have used this term in the same sense as when we say that generosity is innate in certain families; or that certain maladies such as gout or gravel, are innate in others; not that children born in these families are troubled with such diseases in their mother's womb; but because they are born with the disposition or faculty of contracting them."[22] Then after further quotations to the same effect Huxley writes: "Whoever denies what is, in fact, an inconceivable proposition, that sensations pass, as such, from the external world into the mind, must admit the conclusion here laid down by Descartes, that, strictly speaking, sensations, and *à fortiori*, all the other contents of the mind, are innate. Or, to state the matter in accordance with views previously expounded, that they are products of the inherent properties of the thinking organ, in which they lie potentially, before they are called into existence by their appropriate causes."

The upshot of this clearly is that innate for Descartes and Huxley means hardly anything else than hereditary, as applied to the psychical as well as to the physical attributes of animals. The ample justification in our day of the view that psychical attributes are hereditary should, it would seem, restore to full standing in biology, the conception of innate ideas—only, of course, in a very different sense from that into which later Idealists have perverted it.

It is in this discussion that Huxley makes one of the most direct and unanswerable arguments against materialism that can be made: "The more completely the materialistic position is admitted, the easier it is to show that the idealistic position is unassailable, if the idealist confines himself within the limits of positive knowledge."[23] That is to say, if the materialist insists that all traces of innateness of ideas and other contents of the mind must be repudiated, he virtually contends that heredity of whatever sort, whether of physical or psychical attributes, must be repudiated. With this conception of innateness in the entire psychic aspect of the organism before him Huxley asks: "What is meant by experience?"

"It is the conversion," he replies, "by unknown causes, of these innate potentialities into actual experiences."[24] Now these "unknown causes" are, according to my view, essentially the same as the "medium" which Hume recognized must exist for making the "step" possible from the "superficial qualities" to the "secret powers" of natural objects and from the "secret powers" of one object to those of another. They are, to repeat, the reaction of the organism in its latently psychical aspect, with "the breath of life," that is, with the oxygen, or whatever be the gaseous constituent of the air which is active in respiration. And I believe we can see to a considerable extent why Huxley considered these causes as wholly unknown. It was because physiology and bio-chemistry in his day were not yet able to view the organism from the standpoint of physical chemistry. Because of this inability Huxley nor any other physiologist of his period had an adequate structural ground-work for thinking organismally about living things. They were consequently obliged, really, to think of all psychic phenomena, and consciousness with the rest, as being restricted to the nervous system. That such was Huxley's view at any rate, we know from his own words: "No one who is cognisant of the facts of the case nowadays doubts," he writes, "that the roots of psychology lie in the physiology of the nervous system." The important revision of this statement which our hypothesis calls for is that while the roots of psychology are indeed in the nervous system they are by no means in that system alone. They pass through it to a much deeper level, so to speak, and in passing draw great nutriment from it.

In a brief but important paper starting off with the proposition that a philosopher can not legitimately question the existence of the external world—that all he can rightly do is to inquire what that world is and how we can know it at all, G. F. Stout comes to the kernel of the problem in considerably the same way that Hume and Huxley came to it. "For primitive consciousness and for our own unreflective consciousness," he says, "sense experience and the correlative agency which conditions it coalesce in one unanalysed total object. They coalesce in such a way that the sense-presentation appears as possessing the independence of the not-self, and the independent not-self seems to be given with the same immediacy as the sense-presentation." And, "this complex but unanalysed cognition," Stout continues, "is the germ from which our detailed knowledge of matter develops."[39] If proved true my hypothesis would be a considerable forward step, I believe, in analysing this "unanalysed cognition." For although Stout's assertion "the independent not-self is not matter" seems at first sight to exclude oxygen or any other constituent of our breath from such a place in the external world of his conception as that which it has in that world according to my conception this exclusion is, I think, only seemingly so, for a sentence farther on the author says matter "essentially includes the qualification of the independent not-self by the content of sense-experience." The seeming discrepancy is probably due to the generality of the term matter. I too would say that the "independent not-self" is not matter were I to mean by matter the total substance of the external world. But in the sense that the effective respiratory gas (oxygen supposedly) is matter, my hypothesis would require me to hold that the not-self has an essential material component, which component is really the attribute of the gas in virtue of which it reacts with the organism in the peculiar way it does to produce consciousness. It seems to me that what Stout seeks in the "qualification of the independent not-self by the content of sense-experience" is the *immediately* consciousness-producing attribute of the respiratory gas. We might state the point this way: Oxygen (or the effective respiratory gas) has a double status in human consciousness. First and most fundamentally, it has the status of an immediate and essential participant in producing all consciousness whatever; and second it has the status of an indirect participant in producing the particular consciousness which we call observational knowledge of the gas. Our knowledge of this one gas is due to two things, (1) to our reaction to it through our sense organs in the usual psychological meaning of react; and (2) to our reaction with it through the protoplasmic basis of all consciousness, reaction in this case having the meaning which chemistry has given the word. What the relation is between the attributes of the gas in virtue of which it reacts with the organism in these two ways, and also what the relation is between the attributes of the organism in virtue of which it reacts with the gas in these two ways, are questions with which a theory of knowledge would deal but which lies outside of the scope of this sketch, which, as has already been said, restricts itself to a theory of consciousness. I may, however, refer in passing to the fact that chemistry appears to be all at sea on the problem of the relation between the chemical and the physical attributes of all substances whatever; so the difficulties about oxygen in this one particular are not an unshared difficulty.

Finally, to bring this exposition of the historical setting of my hypothesis down to the present hour, I call attention to the way the hypothesis connects with the best that formal philosophy in our own day has done, or as I suspect is competent to do, towards making out what "experience" is. No philosopher with whom I have met has gone farther in this direction than John Dewey. In his recent essay, *A Recovery of Philosophy,* we read: "Dialectic developments of the notion of self-preservation, of the *conatus essendi,* often ignore all the important facts of the actual process. They argue as if self-control, self-development, went on directly as a sort of unrolling push from within. *But life endures only in virtue of the support of the environment.*"[26] The italics are mine and mark the most vital part of the quotation for us. And a page farther on: "Experience is no slipping along in a path fixed by inner consciousness. Private consciousness is an incidental outcome of experience of a vitally objective sort; it is not its source. Undergoing, however, is never mere passivity. The most patient patient is more than a receptor. *He is also an agent—a reactor.*" . . . Again the italics are mine. I take the liberty to end the quotation at "reactor" though the remaining part of the sentence is important for Dewey's particular purpose. But my aim is different. I want to fix attention on the two statements italicised for the purpose of showing how my hypothesis connects with Dewey's general conception of experience. When Dewey says life endures only as supported by the environment, he is speaking in very general terms, having reference, I imagine, more to social and other bulk aspects of environment. My hypothesis, on the contrary, makes the dependence of life on environment exceedingly specific in that it undertakes to show the particular thing in the environment, namely, the respiratory part of the atmosphere, which is physiologically basal to self-development and self-preservation. The Self which traditional philosophy has struggled so hard to understand *is* literally, the human organism, according to my hypothesis. And when in this discussion I speak of it as reacting with the respiratory air to produce consciousness, I am using the verb to react in a very specific, physico-chemico-biological sense, while Dewey is using it in a general sense, and explicitly at least, with only a psychological implication.

The "self" which I am suggesting does indeed imply "another" no less unequivocally than does the "self" of advanced social psychology. But the "self" and the "other" implied by my hypothesis differ from those of current philosophical theory in that the roots of both are not only in the social relationships of the human species, but extend right on through these into sub-human relationships, even down into the very constitution of inorganic nature. The "self" and the "other" of my conception are more personally objective, and more cosmic in their affinities, than are the "self" and the "other" of social psychology.

Continuing now with our examination of the foundation of my hypothesis I find it convenient, especially because of my reference a few pages back, to Huxley's unanswerable contention for an essence of truth in both materialism and idealism, to call attention to a natural history fact in the higher mental life of man which I take to be a strong confirmation of the contention. This fact concerns the general difference between what are commonly known as the materialistic and the idealistic attitudes of mind. This difference comes, I believe, to the same thing finally, as the difference between the objective and subjective attitudes, and is also the difference, at bottom, between what in rather loose though prevalent expression, is called the difference between the scientific and the philosophic attitudes. It would seem that the philosopher who declares himself to be an Absolute Idealist, as Royce does, is under heavy obligation, especially if he enters the field of psychology, to explain the fact that the originators of great interpretative ideas of nature have invariably recognized that their hypotheses must be "proved"; that is, that the subjective experience which constitutes the hypothesis must be found to have its counterpart in the external world of sense. If "Reason creates the world," even in the recondite meaning of Royce's philosophy, how happened it that Newton should have been so "restless" for evidence of an objective, an external counterpart to the subjective result he had reached by mathematical reasoning, that he held back his reasoned creation for sixteen years, waiting for the proof, the sense-perceptual or at least the sense-percep*tible* experience, that should round out his reasoned truth? May not, I ask, the very kernel of the difference between science at its best and philosophy at its best be in this, that the typical scientist is somewhat deficient in "restlessness," adopting Royce's terminology, for internal or subjective reality; while the philosopher of the schools is somewhat deficient in restlessness for external or objective reality? We could say with almost literal chemical accuracy that the curiosity and eagerness of the naturalist for yet unobserved objective truth is due to an unsatisfied affinity which is weak, or in some instances, wholly lacking, in the subjective idealist.

The facts which seem to justify our chemico-organismal hypothesis of conscious psychic life, seem also to imply a complete interpenetration of objective science and idealistic philosophy.

As to the Lowest Terms of Self-Consciousness

Let us now veer our course in examining self-conscious life, and see what can be made out about its roots and rootlets instead of about its fruitage.

We are often reminded that our knowledge about our internal organs, our heart, liver, lungs, et cetera, comes only through observations by the anatomist and physiologist; that we are quite unconscious of these organs in our own bodies, especially if they are working normally. Now I point out that to be perceptually conscious of a liver, let us say, as a specialized morphological entity performing its appropriate functions, is a very different matter from being conscious of those primal, undifferentiated processes which are basal to life itself, and so are common to all the tissues whether liver, muscle, brain, or what not, so long as they are actually living. That that which is truly organic, in the sense of pertaining to the fully constituted organism, must be regarded from this standpoint as well as from the standpoint of their final state of differentiation, is one of the common-places of modern biology. Let a person in as nearly perfect health as he ever experiences, do his best to eliminate all external and internal stimuli of his specialized sensory parts; also all remembering, all feeling of the usual kind, all imagining, and all thinking. Then let him answer the question: How do I know I am alive? An undertaking of this sort is wholly introspective in the sense of being such that each person must engage in it for himself alone. He can not show his results to anybody else. A good bit of ingenuity may be exercised on it and the outcome will be found to be rather surprising if not very conclusive as to the purpose for which the experiment was tried. But the results as reported may be of some value. Personally, I believe I can follow my consciousness down to where I can recognize its most basal remaining "content" to be an awareness of what I may call extension without definite limitations. It seems to me I can detect something to which I could not, from its nature alone, apply the terms "I" or "me" as something differentiated from everything else. Possibly what I note is wholly fanciful, but I seem to feel myself in about the condition of psychical life which I imagine a star fish is in.

Of course I realize how far such a statement is from being purified of all thought and other ordinary mental elements. Nevertheless, I believe it to be of some value as evidence that consciousness is an attribute of the organism as a whole, and can neither be held to contain an element which can exist separately from the organism, nor be restricted to any particular part of the organism as the brain or the nervous system. There seems to be *some* evidence "directly felt by us ourselves," and that evidence points to this conclusion as to the nature and "seat" of consciousness. The point is susceptible, I am quite sure, of rather rigid experimental examination. However, the further experiments which have suggested themselves to me involve difficulties more formidable than I have thus far been in position to attempt.

The reader acquainted with James's notable Chapter X, "The Consciousness of Self" (*The Principles of Psychology*, Vol. 1) will recognize the difference between such introspective experimentation as that here indicated, and that so illuminatingly described by James as tried on himself. While James's undertaking was to give an account of the *thought and other processes* in consciousness as he could observe them in himself, what I want to accomplish requires me to get rid of, to ignore as far as possible, the very things which James was studying. I want to find whether any "content of consciousness" remains after thought and the other usual mental contents are out of the reckoning. I believe, however, that James opens the way to such an hypothesis as mine. Thus in a footnote we read, "The sense of my bodily existence, however obscurely recognized as such, *may* then be the absolute original of my conscious selfhood, the fundamental perception that *I am*. All appropriations *may* be made *to* it *by* a Thought not at the moment immediately cognized by itself. Whether these are not only logical possibilities but actual facts is something not yet dogmatically decided in the text."[6]

Except for a little misgiving arising from uncertainty as to the exact meaning of "Thought" in this quotation, I believe my hypothesis does what James says his text leaves undecided.

This foot-note of James's may serve as a switch key to shift the current of our discussion from the psycho-conscious phase of life through the psycho-physical to the purely physico-chemical phase. The course along which this shifting will run can be designated thus: full-fledged intellect (already examined), instinct, emotion, bio-physico-chemical organization.

Instinct and Physical Organization

The discussion from which we have just turned of the relation between "inner" and "outer," between "subjective" and "objective," must be regarded as meeting the requirements of this sketch so far as the first member of the series is concerned; and the relation between instinct and physical organization will now receive attention. The evidence of vital connection here is so abundant and clear-cut, and the views of competent observers are so unanimous that the subject can be disposed of quite summarily. Probably the most indubitable single block of evidence comes from nest-building and cocoon-spinning insects. Many of the facts from this field have been so much exploited for the very purposes to which we now invoke them that a few quotations from and remarks upon the writings of naturalists generally acknowledged for learning and judicious thinking will suffice.

We turn first to W. M. Wheeler, and take to begin with, words which he in turn quotes from Bergson: "As Bergson says," we read, " 'It has often been remarked that most instincts are the prolongation, or better, the achievement, of the work of organization itself. Where does the activity of instinct begin? Where does that of nature end? It is impossible to say. In the metamorphoses of the larva into the nymph and into the perfect insect, metamorphoses which often require appropriate adaptations and a kind of initiative on the part of the larva, there is no sharp line of demarcation between the instinct of the animal and the organizing work of the living matter. It is immaterial whether we say that instinct organizes the instruments which it is going to use, or that the organization prolongs itself into the instinct by which it is to be used.' " And Wheeler continues: "The spinning of the cocoon by the larval ant is a good example of the kind of instinct to which Bergson refers. From one point of view this is merely an act of development, and the cocoon, or result of the secretive activity of the sericteries and of the spinning movements of the larva, is a protective envelope. But an envelope with the same protective function may be produced by other insect larvæ simply as a thick, chitinous secretion from the whole outer surface of the hypodermis. Here, too, we have an activity which, though manifested in a very different way, is even more clearly one of growth and development. And when the workers of *Œcophylla* or *Polyrhachis* use their larvæ for weaving the silken envelope of the nest, as described in Chapter XIII, we have a further extension and modification of the cocoon-spinning activities. In this case the spinning powers of the larva are utilized for the purpose of producing an envelope, not for its individual self, but for the whole colony. In conventional works this latter activity would be assigned a prominent place as a typical instinct, the spinning of the cocoon might also be included under this head, but the formation of the puparium, or pupal skin, would be excluded as a purely physiological or developmental process, yet this last, no less than the two other cases, has all the fundamental characteristics of an instinct."[7]

Then immediately follows this statement, especially significant for the proposition of our hypothesis which assigns to the individual organism the chemical value of an elementary substance: "Viewed in this light there is nothing surprising about the complexity and relative fixity of an instinct, for it is inseparably correlated with the structural organization, and in this we have long been familiar, both with the dependence of the complexity and fixity of parts on heredity and the modifiability of these parts during the life-cycle of the individual. Fixed or instinctive behavior has its counterpart in inherited morphological structure as does modifiable, or plastic, behavior in well-known ontogenetic and functional changes."

The statement that surprise is largely taken away from such elaborate manifestations of instinct as those here depicted, by recognizing that the instincts are "inseparably correlated with structural organization" and have their "counterpart in inherited morphological structure," will, no doubt, receive the assent of most zoologists, as will also the statement that our long familiarity with structural organization and morphological inheritance is what makes us regard these without surprise, and, by inference, as comprehensible. It is not that the corporeal form and structure of the worker ants and of the larvæ which they manipulate as spinning instruments and shuttles for making the nest, are necessarily simpler and, on that account, more comprehensible than are the instinctive acts of the workers, but that during our whole lives we have been familiar with structure, and ourselves exist as "structural organizations." This is equivalent to saying that we have always been not only learning but directly experiencing interdependences and correlations among the common body-parts and body-acts, and so regard them as comprehensible, as explicable. To comprehend really an external complex of structures and activities is to live the counterpart of it. To understand such a complex scientifically is to understand it through a course of observation and reasoning; that is, rationally. To explain such a complex is to bring in, or recognize consciously one by one the constituent elements of the complex, and recognize all these as parts of the *ensemble*. It is to recognize the elements in both their isolate and integrate capacities.

So much for the evidence of integration between instinct and physical organization as presented by one carefully philosophical naturalist. Several other naturalists have gone nearly as far, but this single instance is so typical and conclusive as to the objective facts that it will suffice. In commenting on the significance of being surprised at such rarely witnessed performances as those furnished by these ants, while we are not surprised at common structures and acts of equal or greater complexity furnished by more familiar animals and by ourselves, I go beyond, though only a little beyond Wheeler.

The only other zoologist to whom I turn for evidence of vital relation between instinct and structure is C. O. Whitman. His testimony supplements Wheeler's in that it is more exclusively and radically objective than is Wheeler's; that is, it verges less toward the subjective-type of presentation and draws nearer to the bio-chemical ground work. Although Whitman wrote relatively little on animal behavior, that little seems to me to contain some of the most important observations and conclusions which have been produced in this branch of zoology. What I utilize is taken from his address *Animal Behavior*. The animals upon which Whitman's chief studies were made were leeches of the genus Clepsine; a salamander (Necturus); and pigeons of several species. Our purpose will be best served by quoting a few sentences which go direct to the heart of the question in hand, that namely of the vital connection of instinct and basal physical structure. "The view here taken," Whitman writes, "places the primary roots of instinct in the constitutional activities of protoplasm and regards instinct in every stage of its evolution as action depending essentially upon organization".[8] Then, apparently to clarify and emphasize the last clause about the dependence of instinct or organization, he adds a footnote thus: "Professor Loeb refers instinct back to '(1) polar differences in the chemical constitution in the egg substance, and (2) the presence of such substances in the egg as determine heliotropic, chemotropic, stereotropic, and similar phenomena of irritability.' According to this view, the power to respond to stimuli lies in unorganized chemical substances, and the same powers exist in the adult as in the egg, because the same chemical substances are present. Organization serves at all stages merely as a mechanical means of giving definite directions to responses.

"The view I have taken regards instinctive action as *organic* action, whatever be the stage of manifestation. The egg differs from the adult in having an organization of a very simple primary order, and correspondingly simple powers of response. Instinct and organization are, to me, two aspects of one and the same thing, hence both have ontogenetic and phylogenetic development."

These statements show, as do those given in our discussion of the cell-theory, how far Whitman went away from full-fledged elementalism and toward organismalism. But his treatment of instinct and animal behavior reveals what his treatment of the cell-theory does not, at least so clearly; namely, how far he also went on the way to the natural history mode as contrasted with the mechanistic mode of philosophizing on biological phenomena. And this gives me a pleasant opportunity to testify to the genuinely naturalist current that ran through his life and work. An unforgettable visit which I had with him among his pigeons not long before he died, permitted me to see something of the character and depth of his interest in those animals. His whole attitude toward them—his wonderfully broad information about, and understanding of their general ways of life and personal idiosyncrasies, his solicitude for them, and his measured affection for them—was such as is never displayed by any one who has not very much of the real naturalist about him, in his personality as well as in his knowledge. The individual pigeons, many of them at any rate, appeared to be realities to him in a deep sense and not merely "mechanical means for giving definite directions to responses" of chemical substances. But after all this is said, it must also be said that there is no evidence that Whitman ever grasped fully the conception that the "constitutional activities of protoplasm" in which he believed instincts to be rooted, must be the constitutional activities of *protoplasms* (protoplasm in the plural number), because no individual pigeon is either any other individual nor even exactly like any other; and also that the existence of protoplasms is dependent upon the organisms to which they belong as well as upon the chemical substances of which they are composed. Whitman went so far on the road toward organismalism as to believe genuinely in the *organic* and *organisation*, but not far enough to make him accept unreservedly *individual organisms*.

We are able to state definitely wherein lies the great and rather unique merit of Whitman's investigations on animal behavior. (1) By a judicious combination of pure observation and observation aided by experiment and conception, he pushed psychic phenomena in the form of instinct down almost to the physico-chemical level; that is, to the protoplasmic level. (2) He at the same time remained positively within the organic, the living realm. His merit is that of restraint as well as of positive achievement. He did not permit his enthusiasm for physical explanation to betray him into adopting a phraseology which, while *sounding* like an explanation of instinct, amounts in reality to a *denial* or a *repudiation* of it.

So much for the evidence of vital connection between instinct and organization. According to the schedule indicated a few pages back for reviewing systematically this connection through the entire range of psychic life, we have next to glance at the connection between the emotions and organization.

Emotion and Physical Organization

Approaching this subject as we now are from the direction of psychology proper, the well-known James-Lange interpretation of emotion comes immediately to mind. It will be advantageous for our sketch not to focus attention too closely on any theory or discussion but to take in as much as we can of the entire field, keeping in the foreground our own personal experiences and observations as contrasted with the descriptions and views of authorities. What I mean is that the reader shall take himself in hand for serious study as to his emotional life, watching himself from hour to hour, day to day, and year to year under all the varied conditions, happenings, purposes, and impulses to which he is subject. In doing this a special point should be made of looking back scrutinizingly at experiences of particular satisfaction, elation, joy, sorrow, irritation, anger, fear, dread, humiliation, and shame, as soon after their occurrence as possible that they may be fresh in memory. But incidents and episodes of one's remoter past which stand out with special vividness from the intensity of the particular emotions when they were experienced, or because of results which flowed from them, will be found illuminating.

To what extent and in what particular fashion was our *bodily organization* implicated in the feelings and emotions we experienced, is our problem. Fortunately one can "live over again" as we say; can "work himself into" rather pronounced emotional states, through a combination of memory and imagination. That is, he can be much of a genuine dramatist when all alone, as touching events and scenes of his own past experience. What happens to your body when you do that sort of thing? is the central question before us. The very criterion by which you answer this question you will find will be that of how far the body-manifestations appropriate to the particular emotions are elicited through your efforts. If your hands do not clinch somewhat, if many of your arm, leg, and abdominal muscles do not contract somewhat, if your respiration does not quicken somewhat, and other manifestations, various corporeal indices of anger, do not appear quite independently of direct intention on your part, you will be sure you have not "worked up" a genuine state of anger. The only *real* knowledge of an emotion is a *lived* knowledge of that emotion. In order to be a true actor your body parts must *act,* directly, automatically, spontaneously, so far as any rational purpose is concerned. And what is true of anger is clearly true of all other emotions.

Our emotional activities may be described as instinctive and reflex activities, the feeling-impulse of which comes *through* intelligence, but is not *of* intelligence—is not under the direct guidance and control of intelligence. According to this interpretation no animal, no matter how highly constituted as to instincts and reflexes, could have emotion unless it had intelligence. Emotional activity is instinctive and reflex activity of an intelligent organism, with, however, the element of intellect eliminated or in abeyance for the time being as regards these particular acts. This is what I would call the natural history description of emotion. And I believe it is in essential accord with James's conception of emotion, but his description is a psycho-physiological rather than a natural history description. I am quite sure that what I have just said means virtually the same as the following: "*If we fancy some strong emotion, and then try to abstract from our consciousness of it all the feelings of its bodily symptoms, we find we have nothing left behind,* no 'mind-stuff' out of which the emotion can be constituted, and that a cold and neutral state of intellectual perception is all that remains."[9]

I will now point out wherein I believe the natural history description and interpretation of emotion are somewhat truer and better than those given by James and other physiological psychologists—and, I may add—very much truer and better than those given by certain writers who approach the subject from the physiological side pure and simple. James's epigrammatic statements about being afraid because we tremble when we meet a bear in the woods; about being sorry because we cry; about being angry because we strike, do his own position some injustice, I think. This is an instance in which his gift for piquant writing succeeded too well. But the fact ought to be noticed that what he actually says is that as between the usual statement, namely, that we tremble because we are afraid, cry because we are sorry, strike because we are angry, and his way of stating the case, his way is "more rational." It is only relative, not absolute truth, he is aiming at in these statements. Nevertheless, after due allowance is made for an expressional miscue to some extent, there is yet substantial defect in his presentation. Speaking in general terms, the defectiveness is not so much in the antithesis set up as in the restrictedness implied. Or, bringing the criticism around toward our particular standpoint, the statement falls short of being organismal.

Cannon has, I believe, indicated the direction in which the adequate statement lies. He writes: "We do not 'feel sorry because we cry,' as James contended, but we cry because when we are sorry or overjoyed or violently angry or full of tender affection—when any one of these diverse emotional states is present—there are nervous discharges by sympathetic channels to various viscera, including the lachrymal glands. In terror and rage and intense elation, for example, the responses in the viscera seem too uniform to offer a satisfactory means of distinguishing states which, in man at least, are very different in subjective quality. For this reason I am inclined to urge that the visceral changes merely contribute to an emotional complex more or less indefinite, but still pertinent, feelings of disturbance in organs of which we are not usually conscious." [10] What Cannon's criticism amounts to, expressed in other language is: while freely granting that organs and functions in the usual physiological sense play an essential part in emotion, neither the visceral nor any other single set of organs is sufficient to account for the whole of *any* emotion. Visceral changes contribute to the "emotional complex," but the real source of the feelings involved is embedded elsewhere and more broadly in the organization. Cannon suggests: "the natural response is a *pattern reaction*, like inborn reflexes of low order." [11] "The typical facial and bodily expressions," he writes, "automatically assumed in different emotions, indicate discharge of peculiar groupings of neurones in the several effective states."

Without stopping to examine this language in detail, our aim will be achieved by pointing out that the more closely the various emotions are scrutinized, and the more effort there is made to refer them to their causes, the more varied are they found to be, and the more widely are we led to search in the organization for causal factors. The mental attitude of perfect openness toward any and all facts, both of effect and cause, which may occur in a given organic situation, is one of the leading characterizations of the organismal conception. The assertion that the organism as a whole is the causal explanation of an emotion or an "emotion complex" is justified by two considerations: (1) Except for the organism viewed alive and whole and under both its ontogenic and phylogenic aspects, the emotion would not exist; and (2) so wide-spread and subtle does common observation recognize the parts of the organism involved to be in many of its emotional activities that for practical purposes, it is better to work on the hypothesis that *all* parts of the organism are implicated than to adopt the alternative hypothesis that certain parts *only* are involved; that is, that some parts are *not* involved.

As a matter of fact, I believe that in spirit James' hypothesis is organismal even though, probably from his training and career in formal anatomy, physiology, and psychology, he never became entirely free from the Body-Soul antithesis and the dogmatisms of "nerve physiology," which have so dominated modern physiology and psychology. This opinion I base on the general tenor of his discussions particularly of the emotions, rather than on his direct formulation of his theory of emotion. I will quote a few passages that seem particularly to trend in this direction. "No reader of the last two chapters [*The Production of Movement, and Instinct*] will be inclined to doubt the fact that *objects do excite bodily changes* by a preorganized mechanism, or the farther fact that *the changes are so indefinitely numerous and subtle that the entire organism may be called a sound-*

ing-board, which every change of consciousness, however slight, may make reverberate. The various premutations and combinations of which these organic activities are susceptible make it abstractly possible that no shade of emotion, however slight, should be without a bodily reverberation as unique, when taken in its totality, as is the mental mood itself. The immense number of parts modified in each emotion is what makes it so difficult for us to reproduce in cold blood the total and integral expression of any one of them. We may catch the trick with the voluntary muscles, but fail with the skin, glands, heart, and other viscera."[12] I ask the reader to make special note of the part of the quotation beginning, "The various permutations" as we shall have more to say about it a few pages farther on.

Again we read: "Our whole cubic capacity is sensibly alive; and each morsel of it contributes its pulsations of feeling, dim or sharp, pleasant, painful, or dubious, to that sense of personality that every one of us unfamiliarly carries with him. It is surprising what little items give accent to these complexes of sensibility."[13] I hope the reader will notice how easy it would be for me to contend that these statements come near to my statement about "inner" and "outer," or subjective and objective; and also to my formal hypothesis as to the nature of consciousness. However, I do not wish to make too much of such a contention, though I shall bring up the point again presently. All I want to do just here is to make still clearer the meaning of my view that James was organismal in spirit, though not wholly so in formal statement. To me one of the strongest evidences of this was his obvious effort, as indicated by these and many other passages in many other writings than his *Psychology*, to *describe fully* the phenomena with which he chanced to deal. As I have remarked in substance so many times in this book, one of the most unmistakable signs of the elementalist attitude in biology is incomplete and more or less perverted description. And nowhere, perhaps, in the whole biological realm is there a better chance for description of the genuinely natural history, organismal kind—the kind a cardinal motto of which is "neglect nothing," than in this very field of human emotions, especially of one's *own* emotions. Nor can I refrain from reminding the reader that one of the master works in this field is Darwin's *The Expression of the Emotions in Man and Animals*,[27] and that while a leading motive of its author was to interpret the emotions in accordance with the theory of descent and the natural selection hypothesis, probably the most lasting value of the work is from its fullness and excellence as a natural history description of the emotions and their objective expression.

As to the fact of vital interdependence between psychic life and physical life through the emotions, personal experience and observation, backed up and supplemented by many authoritative writings, among which those of Darwin and James stand out strongly, there seems no longer any room for question. The rôle of the emotions as between "Body" and "Soul" may be crudely likened to the splice which a skillful sailor weaves into two pieces of rope in joining them so that there shall be no knot and as great strength as in any other part of the rope. In the recent period of psychology —of so-called physiological psychology—we have frequently heard about psychology "without a Soul;" and such an idea has seemed repugnant to many persons. But if we could show that this modern psychology is "without a Body" by the same token that it is "without a Soul," the legitimate misgivings about the soullessness of the psychology ought to be allayed. And really the organismal conception of psychic life is seen, especially when we examine it in the phase of the emotions, to amount to such a composition of the Body-Soul antithesis. "Body" we can see, as it figured in the old psychology, virtually signified what we usually mean by corpse, or cadaver. "The Body," in that sense was not alive at all.

It was not alive because all the life was taken out of it (by the theoretical antithesis) and put into "The Soul."

Glance at the Equilibrative Interaction Between "Body" and "Soul"

Going forward from such predominantly observational descriptions of psychic life in its emotional phase as those of Darwin and James, to such experimental descriptions as those being produced by the investigations of Pawlow, of Crile, and especially of Cannon, we are getting considerable insight into the rationale of how "Body" and "Soul" vitalize each other. Modern researches on the physiology or the psychology (which one calls it depends entirely on the direction of his approach) of psychic life is revealing something of the why and how of the poet's instinctive perception, "Soul needs Body as much as Body needs Soul." Only one aspect of this "why and how" need be noticed in the present discussion. That is the fact of the balancing off of antagonistic emotions to make the normal emotional life just as reflex-actions and instinctive actions are largely phenomena of equilibration, or balancing-off.

It should be recalled that we have found this antagonistic-equilibrative principle to run through the entire neuro-psychic life. In the strictly reflex phase the mode of operation of the opposing muscles, the flexors and extensors of the limbs, as brought out by Sherrington, was cited as a good illustration of the principle. A manifestation of the principle in a broader way, as measured by the extent of organic parts involved, was seen in the relation of the vagal (cranial) and splanchnic (thoracico-lumbar) autonomics, as emphasized by Cannon (Chap. 19, this book) this illustration being chiefly in the reflex phase. In a yet higher phase we saw, again from Cannon's work, the principle in operation through the emotions (Chap. 23) thus bringing it up to the phase of lower conscious life.

The reader should not forget the insistence throughout our presentation of these antagonistic phenomena, that always the oppositions and antagonisms and competitions are fundamentally constitutive as to the normal organism. Even the most pronounced of them are yet in the interest of the organism as a whole. They are always partial phenomena relative to the whole organism. They have evolved in strict accordance with and sub-ordination to the fundamental nature of the organism in its totality. The opposing muscles of our limbs can not break or tear one another under normal conditions. Even antagonisms among the parts of the organism are possible because the parts belong to the organism. The antagonisms of the parts do not produce the organism, primarily, but are themselves produced by the organism, or at least, are a portion of the means or methods by which the organism lives and enlarges, develops and functions. All this, be it noticed, holds not merely as touching purely physical organization * but as to the entire gamut of psychic life, at least up to and including instinctive and emotional life.

Support of the Hypothesis by the Physico-Chemical Conception of the Organism

This prepares us for the final step of switching the discussion from the psycho-conscious aspect of life to the bio-physico-chemical aspect. The place in our discussion to which this return naturally takes us is that wherein we considered the organism's chemical nature as interpreted by physical chemistry. That interpretation has been presented by several physiologists but with special insight and cogency by F. G. Hopkins. For example, our citation in Chapter 4 of the statement that the conception of the organism as a

* Recall the discussions of growth and chemico-functional integration, chapters 17, 18, and 19.

chemical laboratory "is rapidly gaining ground," should be recalled, as should also the opinion of Hopkins: "the chemical response of the tissues to the chemical stimulus of foreign substances of simple constitution is of profound biological significance," and that further study of the phenomena "must throw vivid light on the potentialities of the tissue laboratories."[14] So far this chemical laboratory conception of the tissues may be said to be strictly chemical; but let us recall what the interpretation is when it passes from chemistry in the exclusive sense to physical chemistry and becomes more specific as to the laboratory apparatus, as one may say, through which the "tissues" work. In other words, recall the conception of the cell and its mode of operating, as viewed by physical chemistry. The quotations given in Chapter 4 may well be repeated in part: ". . . the living cell as we now know it is not a mass of matter composed of a congregation of like molecules, but a highly differentiated system; the cell in the modern phraseology of physical chemistry, is a system of coexisting phases of different constitutions." [15] Then from this review our own contention, set forth especially in Chapter 7, that wherever in such statements as those just quoted from Hopkins "the term *cell* occurs the term *organism* really ought to be used."

It is important for our cause generally that the full weight of our argument in support of the view that on the strictly physical plane, the *organism* rather than the *cell* is really the equilibration system toward which physico-chemical knowledge is tending, should be in the reader's consciousness. At this point if, consequently, this is not so, he is urged to read what is said on the point in Chapters 4 and 7 especially.

Our central purpose now is to show that the organismal hypothesis of consciousness articulates directly and naturally with the same conception of the organism. Undoubtedly it is in the emotional phase of psychic life that this articulation is most open to common observation. Compare, for example, James' "Our whole cubic capacity is sensibly alive; and each morsel of it contributes its pulsations of feeling, dim or sharp, pleasant, painful, or dubious, to that sense of personality that every one of us unfamiliarly carries with him," with Hopkins' "On ultimate analysis we can scarcely speak at all of living matter in the cell; at any rate, we cannot, without gross misuse of terms, speak of the cell-life as being associated with any one particular type of molecule. Its life is the expression of a particular dynamic equilibrium which obtains in a polyphasic system . . . 'life' as we instinctively define it, is a property of the cell as a whole, because it depends upon the organization of processes, upon the equilibrium displayed by the totality of the coexisting phases."[16] Also compare Hopkins' statement that among the different "phases" of the cell in which its life inheres, "are to be reckoned not only the differentiated parts of the bio-plasm strictly defined (if we can define it strictly), the macro-and-micro-nuclei, nerve fibers, muscle fibers, etc., but the materials which support the cell structure, and which have been termed metaplastic constituents of the cell," with James' "each morsel" of our cubic capacity "contributes its pulsations of feeling, etc."

The congruity of these statements is apparent even when taken as here exhibited; that is, each as standing by itself at about the two extremes of the scale of life. When, however, they are viewed in connection with my general argument that "cell" in Hopkins' statement ought to be replaced by "organism"; and in connection with what we have learned from Cannon and others about the mechanism by means of which the organism operates in the phase of conscious emotion, it seems as though our organismal hypothesis of consciousness comes near to a demonstration. And so far as ordinary descriptive natural history is concerned, I believe this to be true. However, I recognize, keenly enough, that from the standpoint of bio-chemistry, and physiology, and also from that of philosophy in the traditional sense, that demonstration is not only far away, but is attainable, if at all, only by surmounting very formidable difficulties. So I reassure the dubious reader that all I am claiming is that my two propositions about the nature of consciousness together constitute a legitimate scientific hypothesis.

Personality and Elementary Chemical Substances

With both the physico-chemical aspect and the psychical aspect of our hypothesis now before us more fully and sharply than they have been hitherto we will examine an objection to it which I apprehend will be the most serious the hypothesis will meet; namely that to the proposition that each individual organism has the value in a chemical sense of an elementary substance. And since this objection will probably be more intolerant and stubborn from the side of physics and chemistry than from that of natural history and psychology I will adjust my remarks with reference to the opposition as thus anticipated.

The considerations I am going to present might have been, in strict expository coherence, presented as a part of my discussion of the uniqueness of the individual consciousness as marked by its necessary privacy and its difference from all other individual consciousness. What we are now to emphasize is the fundamentality of objective as contrasted with subjective personality of such highly developed animals as song birds, domesticable animals, and civilized man.

A complete definition of "personality" is not obligatory for our purpose. Only this much need be said about the meaning we shall give the word: First, we deny the right claimed by some authors to make personality purely psychical, or spiritual—a thing of the "inner," or "deeper" self; "Self" that is, in a thorough-going subjectivistic sense.

It is on this ground, as I understand, that some psychologists, as G. F. Stout, and apparently C. Lloyd Morgan,[29] deny personality to animals. All I will say on this question here is that I am quite sure that every close observer of the higher animals will recognize that if he undertakes to give a truly full report of his observations on their behavior he will have to speak of the personality of some at least of them just as he would of the personality of *observed* human beings, or he will be obliged to call the same thing by some other name—a kind of procedure against which we have spoken strongly throughout this volume. For us, whatever personality may be, we must conceive it to be founded upon, and conformable to, the organism. "Organism" must be the more inclusive term. "Person" must stand to "Organism" in the logical relation of species to genus.

Another meaning of personality in this particular discussion will concern the uniqueness of each organism as to its psychical attributes regarded in their totality. By uniqueness I mean not merely the fact that each organism is *itself*, perceptually regarded, but that it is not a replica, a duplicate of any other. It is not only another organism but it is in some measure a *different* other organism. For the benefit of those physical- and metaphysical-minded readers who have never informed themselves much about the facts of natural history and have never tried seriously to think in the natural history manner I would remark that what I have just said concerning the uniqueness of the individual organism is only re-asserting in a more refined way what botany and zoology have recognized more or less definitely since Darwin's time at least, and have partially expressed in the terms "individual difference" and "individual variation."

With this we come to the cardinal point: *If individual animal organisms, especially individual humans under civilization, be contemplated with due heed to the motto "neglect nothing" the conviction will be reached that each and every* *one has literally as much of uniqueness about it as has an elementary chemical substance.*

In order to bring out the truth of this statement we must exhibit, in the regular natural history manner, the resemblances and differences between chemical elements on the one hand and the resemblances and differences between human beings on the other, and then pool the results of these comparisons.

To the carrying out of this enterprise the so-called periodic law in chemistry is of very great importance. The essence of this law, stated from the natural history standpoint, is that the chemical elements range themselves into natural species and genera after much the fashion that plants and animals do; and that the classification is based mostly on the chemical attributes of the substances, but partly on their physical attributes also. Thus the "halogen group," that to which lithium, sodium, and potassium belong, is a genus in the sense of descriptive natural history, its species being the substances mentioned with others not enumerated. Also the group often spoken of in chemical laboratories as "the iron group"—the genus containing the species iron, cobalt, nickel, platinum, etc., illustrates the point. Two species of the last genus, iron and nickel, will be used in our study. Let us compare some household utensil made of iron with a similar one made of nickel. For the ordinary uses to which these implements would be put the difference between the substances of which they are made would hardly be noticed. The higher specific gravity of nickel (8.5 plus) is so slight as compared with that of iron (7.8) that the greater weight of the nickel implement would probably not be noticed. Nor would the slightly lower melting point of nickel nor its much lower magnetic capacity be recognized. The most available distinguishing difference is in color, the ordinary housekeeper answering you, if you ask how she knows a nickel from an iron implement, that the nickel piece is silvery bright while the iron piece is black.

See now what this means. Actually, as is well known to every beginning student in analytical chemistry, these two metals are very similar in color as well as in other physical attributes—so much so, in fact, that some authors apply the same term "silver white" to both. What a housekeeper really means when she says she knows one implement to be of nickel because it is bright and the other to be of iron because it is black, is that she is depending on a chemical rather than a physical attribute for a distinguishing mark; the attribute, that is, in virtue of which iron is acted upon much more readily by oxygen in the presence of moisture than is nickel. The much greater liability of iron than nickel to tarnish and rust is a chemical rather than a physical difference between them. This fact, namely that of the dependence of distinguishing differences between substances more upon chemical than upon physical attributes is of very wide applicability in nature, and is greatly important both scientifically and philosophically.

Now turn from comparing these two elementary chemical substances to a comparison of any two human organisms, or persons who might be members of a household to which the implements might belong. And make the comparison first on the basis of the physical attributes just as we began comparing the implements of nickel and iron. Does any reader doubt that he would find it much easier to distinguish the persons than the metals? As to purely morphological, that is, physical differences between almost any two persons (with the possible exception of certain rare instances of "identical" twins), there is no room for question. General shape of head, face and features, and the size and proportions of the various parts of the body furnish many unmistakable distinguishing attributes.

On the Psychology of Subjective and Objective Personality

But unerring as are the differentiating marks on the physical side, such marks are few as compared with those on the psychical side. Noting first certain merely physico-psychical differences think of the manners of speech and of hand writing, to mention only two items! Undoubtedly these differences are to a considerable extent physical but no one would seriously question that psychical factors come in all along the line. This is perhaps most obvious in speech as evidenced by voice modulations, intonations, gesticulations, and facial and bodily expressions. Again, differentials are everywhere recognizable in responses to sensory stimuli, especially in the matter of reaction-time. There are the quick and accurate persons, and the quick and inaccurate ones; and there are the slow and accurate and the slow and inaccurate types, to go only a step in description and classification on this basis.

Then we proceed to compare the unequivocal psychical phases of life: the feeling, the emotional, the esthetic, the religious, and the intellectual phases. Here we pass into a realm of what might properly be called objective privacy in psychology, individuals for the study of which would be largely the student's most intimate and most enduring friends and associates, human and animal. Such a psychology would be undeniably so particular and intimate that much of it would be unpublishable even if it had an interest beyond the few persons concerned. At the same time there are some portions of it of great public importance, one such portion being exactly what we are in need of in the present discussion. I refer to the exceedingly familiar but scientifically much neglected definite and sustained psychical differences of individuals who by reason of being members of the same household or same small community are subject to nearly identical influence so far as concerns such fundamental environic factors as food in the narrow sense, drink, air, light and temperature. The duty before us is that of testifying to, of *viséing*, the objectively psychical individual as we did the subjectively psychical individual earlier in this sketch. "What is needed," writes Sellars, "is not vague statements to the effect that individuals cannot be separated or that they are aspects of one another, but definitions and analyses." [17] Sellars is here raising his voice against the tendency in present-day social psychology to make the individual a kind of incident in the social order, a by-product of Society. It is a satisfaction that the regular course of my psychological argument has brought me to where I also may contribute something to the definition and analyses essential to checking the tendency indicated by Sellars. If it can be shown biologically and psychologically all in one that personality is indubitably objective, both substantively and kinetically, not only the Individual but Society will be the gainer, I am very sure. For my contribution we will examine in outline what may appropriately be called the action-system (adopting and expanding Jennings' term) as it manifests itself in a small homogeneous group of human beings. Our study will be, in other words, one in domestic and neighborhood psychology.

The "material" in this instance must be my own household and the handful of persons constituting the colony of the Scripps Institution for Biological Research. This group is rather specially favorable for such a study in that its geographic severance from other groups, and its strictly rural habitat give it an exceptionally natural, simple, and uniform environment. The analysis might run along any one or all of several axes; but our purpose will be accomplished by following one only. That one shall be the reaction, the behavior, of individual members of the group in response to the stimulus of the world war. Were completeness to be aimed at in the analysis, every individual in the group would have to be considered. Such a treatment would be highly instructive but space limitations forbid us going to such length. We must restrict ourselves to a few of the more pronouncedly individualistic behaviors and must treat even these in a very sketchy fashion. To be remarked at the outset is the fact that every member of the group is deeply loyal to America and to the cause of the Allies. On the very door-sill of the examination we recognize two well-differentiated aspects to each person's action-system, namely an aspect of commonality for nearly all members of the group; and an aspect of very pronounced differentiality for many of them.

Behaviors-in-common will receive attention first. In the uniform growth, from the very beginning of the struggle in August, 1914, of belief in the general rightness of the cause of the Entente; of realization of the meaning of the struggle; and of sentiments and resolutions of devotion to the foreign nations with which our nation is finally joined, these experiences have been very much at one. To be sure this commonness has fallen far short of identity. But as to essentials resemblance has been far greater than difference. For example every adult has accepted unhesitatingly his and her obligations to the Red Cross; to the appeals for aid from Belgium, France, and the other despoiled countries; to the increasing cost of living; to the buying of Government Bonds; and to the appeals and regulations of the Food Administration. Naturally there has been difference in the particular way and extent of response of each in these matters; but in essence there has been nothing differential.

We turn now to behavior-not-in-common; behavior, that is, which has differentiated the members personally with great sharpness. This examination is much more important for the subject in hand. The reference here is to each one's "bit" as the common phrase had it when our country was first entering the conflict. The "war work" (as the expression has gradually become with the advance toward the climax of the gigantic struggle) into which each has gravitated has much the appearance of the naturalness and inevitability presented by the falling of a stone or the flowing of water. The case grows so significant at this point that I must particularize somewhat more than I have heretofore. *A* becomes an acknowledged leader in "drives" for Red Cross funds, Liberty Bond sales, etc. *B* becomes a regular consultant on the knitting of Red Cross articles. *C* is a highly skilled deviser and maker of dishes from "substitute" foods. *D* is appointed an official of the National Food Administration. *E* becomes an official teacher of girls and women as to the peculiar duties and obligations of their sex in war times. *F* concentrates nearly the whole of his physical energy upon an elaboration of the view that a victory over Germany and her allies cannot be really complete without being spiritual as well as material—that the philosophy or theory of life being fought for by Germany must be overthrown as well as her armed forces. Of the forty adult members of the group fully one-half have been incited in a special degree to some activity that has a distinct personal character, some of these, as above indicated, being very pronouncedly so. The personality of these reactions comes to view most distinctly in the fact, absolutely certain to an observer whose acquaintance with the persons has been intimate and has extended over some years, that no one of those who has settled into one of the special, definite, and important pieces of work could wholly replace any of the others in their special tasks. Probably each could do *something* at the "job" of any of the others were conditions such as to force him to try; but success under such conditions would surely be partial, very much so in some of the cases.

This automatic definition and classification of persons subject to a common major stimulus, with nearly the same general environic conditions, and with almost complete freedom of action so far as concerns the particular stimulus, seems to me a phenomenon of very great importance since it depends upon principles of organic beings, especially upon principles of civilized man's "being," which are well-nigh if not entirely universal, I am sure. Undoubtedly the phenomenon is often much obscured through counteracting elements in the environment, especially in social customs, economic conditions and general education among civilized men. But in spite of all these, attentive observation will nearly always be able to recognize it. Highly significant is it as bearing on this particular aspect of the matter, that the niches finally found by most of the persons were obviously determined to some extent by long continued previous activities and unmistakable natural "gifts."

Another noteworthy fact is the clear indication of not mere acceptance, but positive satisfaction on the part of most if not all the persons, once they are "settled" to their "jobs," this satisfaction prevailing despite the strenuousness, perplexity, and wear-and-tear entailed. During the first weeks of America's plunge into the maelstrom the anxious psychical casting about in our little group, as throughout the whole land, presents to the anthropological biologist as he looks back upon it a case of trial and error on a gigantic scale, the scene being replete with jumbled elements of noble zeal, splendid efficiency, mis-expenditure of strength and funds, and ludicrous proposals. But out of this, as out of this unprecedented instance of world-wide "struggle for existence," there is quite sure to come, indeed is coming, as one of its first fruits, *personality* more real and powerful and fuller of grandeur than ever.

While personalities come forth with special distinctness of outline and forcefulness of expression during occasional events of vast import to the race like the present war involving literally the whole civilized portion of the human species, yet I would insist that the difference between the manifestations at such times and at ordinary times is almost entirely one of degree, rather than of essential nature. The attentive observer will not fail to find personalities as here understood always and everywhere, no matter how simple and lowly the lives, and monochrome the external conditions. In little details of intelligent, but still more of reflex, instinctive, and emotional life, all of which compounded together makes what we often call temperament, the keen and sympathetic observer will always see *persons* in the deep sense here indicated. Not the transcendent genuises merely, the Aristotles, the Shakespeares, the Napoleons, have the right to be called personalities, because of the unique powers with which they are endowed; but each and every one of civilization's humblest-ranked myriads, and each and every nature-tutored denizen of the virgin forest, of the untilled plain, and of the unregenerate desert, have the same right-in-kind.

Personality and the "Breath of Life" Viewed in the Light of Physical Chemistry of the Organism

Swinging the discussion back now on the physico-chemical aspect of the organism, I recall first the truth alluded to a little while ago, namely, that it is preëminently the chemical rather the physical attributes of elementary inorganic substances which furnish the distinguishing marks of these substances. Even in the inorganic world we saw that substances are most readily and decisively differentiated from one another by the transformation-products resulting from the reaction of the substances upon one another. "Transformation of energy," using a form of expression favored by the disembodying tendencies in recent chemical theory, is the most distinctive thing about all chemistry, inorganic as well as organic. The oxidation and other chemically reactive changes and products of nickel and iron, we noticed, are the most differentiative things about these metals. Let us push the application of this criterion of difference a little farther in comparing human persons. We give energy-transformation and work performed a leading place here also. And being naturalistically chemical rather than chemically chemical we are forced to touch the "high spots" only at first regardless of what may be in between them. We are free to seize upon the end or completed products of the reactions and transformations. What reaction-products, I ask, of nickel and iron towards any other substance or set of conditions are more unlike than the reaction-products of an efficient Department-of-Justice official, let us say and an efficient food conserving house-keeper, in this time of common national danger? Yet these diverse products may come from not only the same danger stimulus, but likewise from as nearly identical physico-chemical environic stimuli as it is possible to secure. Were official and house-keeper to eat of the same food, drink of the same fluids, breathe of the same air, and be subject to the same temperatures month in and month out the difference in product would not be a whit less.

So stands the case when viewed in its "high places" only. But the high places are as real places as any whatever. No realities, it matters not how obscure or subtle, pertaining to the intermediate places, can make the high places other than what they are. Judging human beings by what they do, by work done through the transformation of the substances and energies which they take from the external world, their personalities are surely not less well-attested than are the individualities of elementary chemical substances.* But it will not do to be satisfied with touching the high places in this rather jaunty fashion. Some attention must be given to

* A rather full discussion of the point here touched may be found in my essay, *The Higher Usefulness of Science,* where I raise and try to answer the query, "What is nature because man is a part of it?" Perhaps a less ambiguous way of asking the question would be, "What must nature be in order that it may produce such an animal as man?"

the subtler aspects of the problem. The little we shall do in this way may be introduced by the query, what reason is there for including in our hypothesis the supposition that it is "some substance in the air, almost certainly oxygen," with which the organism reacts chemically, to produce consciousness and all other phenomena of life? Why single out this substance from the other elementary substances essential to life, as for instance carbon or nitrogen? * My reply begins by recalling the immemorial recognition of the "breath of life" the "life giving air" and so on, of universal experience. It is well to recall likewise such semi-philosophic conceptions as that of the pneuma or "psychical breath of life" of later Greco-Roman philosophy. The inextricable entanglement, historically, of breath and air with spirits is also worth remembering, especially the continuance of this into the modern period of scientific analysis, unmistakeable traces of which are seen in the writings of William Harvey and the foremost physiologists of the era to which he belonged. For example, the *spiritus nitro-aereus* of John Mayow which, we now know, was his term for oxygen as glimpsed first in the history of science, may be mentioned.

More important than any of these reminders from the history of knowledge is that of the familiar fact that the most crucial evidences of truly independent or autonomous life of the individual higher animal are respiratory. That the new born human babe's first breathing-act is its first genuine independent life-act is one of the most commonplace of truths. And recall how the "return of life" as we say of the nearly drowned person, and of one who has "fainted dead away" is marked by the resumption of respiratory activities. Certain reflexes, as those from stimulating the eyelids, and pos-

* The argument in answer to this query should be taken as an extension of, and in important respects a replacement of, that contained in my essay, *Is nature infinite?* [37] wherein I discuss the specificity of individual organisms as indicated by how they use their nutrient substances.

sibly certain heart flutterings, may be more persistent movements than those connected with breathing. But these are less certain signs of individual life. It is only to philosophy of the elementalist sort that the mere twitch of a hand or an eyelid or a trace of heart action would be a satisfactory proof of life. Nor would it be to a philosopher of this school should the "living substance" under observation happen to pertain to a loved relation or friend. *Satisfactory* evidence of life in this case would come only with the nearly simultaneous return of breathing and consciousness. A right interesting section could be written at this point on the importance of nutriment in the ordinary sense, and of drink, as compared with air at the very beginning and ending stages of the individual life. For instance such questions would have to be considered as that of the independence of the new individual for a while at the outset on food-yolk in many animals below the mammals, and on placental connections in mammals; that is on material metabolically elaborated by the older or parent individual. But such a discussion not being indispensable to this sketch, must be foregone. Enough here to emphasize the fact that while it may be entirely justifiable to regard oxygen as a food as some good modern physiologists do the two important facts should never be lost sight of that (1) oxygen (air) is the one and only ever-present and never varying constituent of the dietary. In other words that it is the one constituent which nature supplies as by "free grace" to use a good old theological expression; and that (2) oxygen is the one and only food that needs no digesting and so no digestive organs or tissues set apart for its metabolic elaboration.*

Oxygen is the only food which passes directly as such to

* Were the view held by some physiologists, that the alveolar epithelium of the lungs transmits atmospheric oxygen to the blood by an active process spoken of as a secreting, this statement would need modifying somewhat. However, the view does not seem to be accepted by most authorities.

every part of the organism. In oxygen the organism finds one of its most fundamental food materials for which it does not normally have to go in search or to compete with other organisms. The familiar fact and its significance appear not to have attracted the attention of biologists much. Even L. J. Henderson [30] who has written so illuminatingly on many aspects of organic adaptiveness says nothing definite on this point. These two facts are weighty reasons for my proposal to look upon oxygen as one chemically elementary substance and the organism as another, the reaction between which is basal in the production of consciousness and all life phenomena. Consequently the problem of how, exactly, the organism endowed with full-fledged consciousness reacts toward oxygen is certainly one of the most important of all problems on the purely physico-chemical side of life. And, as said early in this sketch, it is just here that my theory is most avowedly hypothetical. It would be quite out of the question to present in the remaining pages of this book, even had I the requisite knowledge for doing so, all that might profitably be said on the subject. Consequently only two or three of what seem to me the most crucial matters will be mentioned.

In the first place I ask the reader to recall what has been said in various of the preceding chapters which have brought out the indubitable trend of the interpretation of life phenomena according to the principles of physical chemistry, away from the elementalistic conception of the organism. The interpretation of the organic cell as a system of phases in dynamic equilibrium, so strongly set forth by Hopkins and Bayless will be remembered. And this will call to mind the sharp way in which the new conception, with its appeal to the rôle of surface-layers, membranes, and areas of contact between all sorts of constituent substances, sets itself over against such pseudo-objective conceptions as that of biogens, not to mention the horde of out and out subjectivistic "elements" of which pangens and determinants have perhaps had the greatest vogue. The importance of the anti-elementalistic tendency of physical chemistry when it comes to be applied to biological problems is greatly enhanced, it appears to me, by the circumstance that J. Willard Gibbs, who was one of the very first to appreciate in a full scientific sense the importance of massive as contrasted with minute-particle phenomena in inorganic nature, and so was one of the "fathers" of physical-chemistry, made no assumptions about the invisible composition of substances in his treatment of "Heterogeneous Equilibrium" and allied topics. "Certainly," writes Gibbs, "one is building on an insecure foundation who rests his work on hypotheses concerning the constitution of matter." [31] If this is true as touching the relatively simple structures and movements in the lifeless world how much more obviously true is it as touching the living world, and especially such life phenomena as human consciousness!

So we are able to requisition one of the admittedly most important advances of modern times in inorganic science as support for the supposition that the air we breathe, and presumably its oxygen, contributes in some direct and fundamental way to the production of consciousness even though this substance, if its "ultimate nature" is what inorganic chemistry and physics have hitherto attributed to it, has little or nothing to suggest that it possesses such a unique latent attribute. The reader should not fail to recall here Hume's recognition of the "secret powers" of substances.

But is it not possible that physico-chemical and physiological knowledge of oxygen and air, the "breath of life," do contain somewhat more to justify the supposition than is usually recognized? In this connection I relate that one of the most mentally adhesive statements I ever heard from a bio-chemist, its adhesiveness depending largely on the fact that the chemist was one of great experience as a laboratory investigator, was to the effect that chemical analyses make known what they find and absolutely no more. In other words such analyses never exclude the possibility of substances other than those found. And this chemist asserted furthermore that all organic analyses leave residues to some extent. No manipulative methods are known, it appears, capable of effecting a really complete analysis of any organic substance. Whether these restrictions on analyses still hold I am not sure, though I have seen or heard nothing which leads me to suppose they do not.

It is this general shadow of manipulative imperfection which overhangs all formal physics and chemistry, coupled with the advances being made from time to time in our knowledge of oxygen and air which has led me to put into my hypothesis a shade of doubt as to whether oxygen is the constituent of the air the reaction of which with the organism produces consciousness. The demonstration of helium and argon, and probably neon, crypton, and xenon in atmospheric air, all within a little more than two decades, has influenced my thinking in the same direction. Besides, the idea, become a commonplace of physics and chemistry in a single night, figuratively speaking, that the "atom is as complex as the solar system" has had its part in shaping my conceptions; as have also such well-credentialed conceptions from the inorganic sciences as that "Uranium II" is "a long-lived element" which is the "parent of the actinium series of elements, but has no genetic connection with the uranium series"; and that "in the lead pleiad there are seven elements having quite different atomic weights."[38]

The extent to which, as exemplified by this case, the inorganic sciences have found themselves driven into the organic realm for terms with which to express their new conceptions must impress every thoughtful person. Earlier, what we might describe as purely contemporaneous physical dynamics had to borrow such terms as energy, power, force, work, from the nomenclature of living beings. Later, with the permeation of all knowledge by the conception of the natural or derivative origin of everything (a genuinely organic conception, notice), has come even for elementary chemical substances, the induction into physics and chemistry of such ideas as genetic relations, parenthood, and length of life. So my suggestion that the air we breathe must be recognized to possess latent attributes which by reacting with the organism produce consciousness, falls into a genetic series in the history of the interpretation of nature.

The very important question, as already indicated, of exactly how atmospheric or molecular oxygen operates in the living being generally and the conscious being particularly, is largely for the future to answer. One should never fail, however, to couple this question with the same question as to the behavior of oxygen, and for that matter of any other chemical substance, in any reaction whatever. Exactly how, for example, does oxygen operate with hydrogen to produce the attribute of refrangibility of water; or with phosphorus to produce the peculiar glow which that substance may exhibit under some conditions?

Concerning the positive knowledge and the views as to details of the action of oxygen in connection with the organism, only a little can be said here though that little may be very important. Looked at from the standpoint of the old, the orthodoxly atomistic chemistry, probably the most anomalous thing about my hypothesis is that the organism conceived as equivalent, chemically speaking, to an elementary substance, is the unquestioned fact that the organism is not only composed of several chemical substances, but that one of these is oxygen itself. Stated baldly, the anomaly is that two chemical substances are supposed to react upon each other, one of which (the organism) is known not only not to be simple, but to contain the other substance. But even the old chemistry with its "compound radicals," of which cyanogen $(CN)_2$ is said to have been the first discovered, and of which the unitedly-acting combinations of carbon and hydrogen as methyl, CH_3, affords some slight support for our conception so far as the mere matter of chemically unitary compoundedness is concerned. In so far, however, as technical chemistry can be drawn upon for supporting our hypothesis, it is the new, or physical chemistry, as has been repeatedly stated, that is our main reliance. Unless I am greatly deceived, the real inwardness of that great movement in inorganic science is against the age-old conception of the ultimate adequacy of atoms to explain inorganic nature, almost as positively as the organismal conception is against the ultimate adequacy of any constituent element whatever, to explain organic nature. The surface energies, for example, developed at contact faces and giving rise to the phenomena of adsorption * appear to be not a whit less real and ultimate energies than are any that can be attributed to atoms and molecules taken as such. And, be it noticed, one of the most distinctive things about these areal and massive energies is that they dominate atomic and molecular energies to a certain extent. This is just what the now universally recognized principle of "mass action" is in so far as such action has been studied enough to make possible its formulation into law; that is enough to learn how it influences velocity and quantity of chemical change. But would any careful physicist or chemist pretend to know to a certainty that such action is restricted to influence of that sort? Surely not. Are we certain for instance that it can not under any

* *Adsorption* is the loading of the surface of a solid body immersed in a solution, with the dissolved substance. Thus it is by adsorption that charcoal takes the coloring matter out of a colored solution. The action results from the facts that there is surface tension at the interfaces between the charcoal and the liquid, and that this tension is lessened by the presence of the dissolved color-substance in the liquid. The substance then moves to the place of lessened tension and concentrates on the surface of the solid.[32] The principle has very wide application in nature, particularly in organic nature, where colloidal substances and water are in contact so extensively.

circumstances influence qualitative as well as quantitative change? Surely we are not. This of course is far from contending that mass action actually does influence qualities. My sole point is that so long as there is lack of certainty that it does not or may not exert such influence any assumption which implies such certainty is unwarranted and unscientific.

Putting together, then, the physically massive conceptions of inorganic chemistry and the organismal conceptions of bio-chemistry what seems to follow touching the chemico-substantive composition of organisms is that a portion of all the substances essential to life, carbon, oxygen and others, have been combined from all eternity (whatever be the meaning of the phrase) in the peculiar way called organic, while other portions have remained in the state called inorganic. This leads me to remark, quite incidentally so far as this discussion is concerned, that according to this view the assumption would be that organisms have always existed, or at least that they have existed as long as "matter" or anything else of which we have any information or clear conception, has existed. The warrantableness of this assumption I am relieved from arguing here from having treated the problem at some length in another place. (*Are we obliged to suppose the spontaneous generation of life ever occurred?*)[33] All that need be said now about the outcome of that discussion is that the warrantableness lies in the absence of any ground for assuming the contrary. I take my position squarely on the direct evidence in the case. All the evidence of that sort we have—and in that discussion I emphasize the fact of its vast quantity—is to the effect that organisms are produced by other organisms known as parents and in no other way.*

* To the stock and rather vapid rejoinder that such a solution of the problem of the origin of life is no solution at all, but only a putting off of the difficulty, the obvious reply from my standpoint is that I am making no pretense of "solving the problem," as "solution" would be meant in the anticipated rejoinder. From my standpoint, however, the everlastingly-from-parents hypothesis would be a solution of the problem if the hypothesis were proved true.

We can now state briefly as much more of the bio-chemical aspect of the problem as seems indispensable to our present argument. A few remarks on what the physiology of our day often calls tissue respiration will compass what is in mind. The key fact in this is of two-fold character: (1) The tissues of the organism, not its blood or any other fluids, contain the substance which is in the strictest sense living. (2) This substance is called living because chemical changes of a very distinctive sort are going on in it. These changes are of a fundamentally double nature as regards atmospheric or molecular oxygen; namely, combinative and incorporative change, and separative and expulsive change. The last-mentioned, the separative and expulsive change, is known as oxidation and manifests itself to ordinary experience in the discharge of oxygen combined with carbon as carbon dioxide, and in the setting free of energy in the form of muscular and other work, and of heat. The first-mentioned, or incorporative change, consists in taking in and storing up oxygen, "somehow," as the more carefully worded physiologies put it. This statement may be taken as a very brief natural history description of the most fundamental steps in what formal physiology calls metabolism with its two aspects, the constructive, or anabolic, and the destructive, or katabolic. Probably no one will question that this conception of the foundations of the life process for nearly, if not quite, all animal life is that held by the best physiologists since the time of C. Bernard at least. No physiologist whom I have consulted has stated the nature of the process more definitely than has Sir Michael Foster. "The Respiration," he writes, "of the muscle then does not consist in throwing into the blood oxidizable substances, there to be oxidized into carbonic acid and other matters; but it does consist in the assumption and storing up of oxygen somehow or other in its substance, in the building up by help of that oxygen of explosive decomposable substances, and in the carrying out of decompositions whereby carbonic acid and other matters are discharged first into the substance of the muscle and subsequently into the blood." [34] And he points out in other connections that what is true of muscle in this regard is essentially true of all other tissue systems. In another still more recent text book we read: "Nothing definite is known, however, as to the nature of the probable combinations formed by oxygen with the different materials for building up muscles and other tissues, or of the intermediate anabolic and katabolic forms through which it passes in combining with carbon into carbonic acid." [35] And this author then expresses what are, apparently, his own views, by quoting from Foster as follows: "The whole mystery of life lies hidden in the story of that progress [that of construction and destruction in the tissues] and for the present we must be content with simply knowing the beginning and the end."

The kernel of my suggestion so far as metabolism is concerned, is that the anabolic, or the assimilative, the truly synthetic aspect of the complete operation, is the continual renewal, or keeping up of the oxygen constituent of the organism which comes to it by heredity, that is which has always been in the "line of descent." It is the maintenance of what might be spoken of as the original oxygen constituent of the organism. There would always then be operating in the organism oxygen of two sources, that from the one source designated, employing our well-established evolutional terminology, phylogenic or hereditary oxygen; and the other ontogenic or individual oxygen. In general the same kind of reasoning would hold for the other chemical simples, carbon, nitrogen, and so on; but these are in quite a different status from oxygen owing to the fact that they are not normally taken by the animal organism in the pure or uncombined state, but only in some other organic combination, as food in the ordinary sense.

Metabolically expressed, then, we may say in short that the warrantableness for considering the individual organism as a chemical element, is the fact that it maintains its identity as regards all its elementary constituents except one, oxygen, be wrenching these, so to speak, from other organic compounds (by digesting these) and then by synthesizing the elements into its own particular substance. Another way of expressing the same conception is to say that the organism is an element, chemically speaking, because it reacts directly in a chemical sense with another element.

Did this chapter pretend to be anything more than a sketch of a theory of consciousness a considerable discussion of the "activation" of oxygen would naturally come in somewhere, perhaps at this point. The essense of activation is the fact that when oxygen passes into the organism by the respiratory process it is somehow changed into a condition which enables it to oxidize living tissue-substances as it can not to any degree, seemingly, when brought into contact with the same substances outside the organism. This discussion would involve the various theories which have been put forward to account for this phenomenon, as those which make use of the principle of enzymes, of peroxides or of some other. All that our aims here require us to notice is that nothing conclusive as touching the nature of activation would come from the discussion. How unsatisfactory a state this whole subject is in may be seen from the following words of a foremost American biochemist: "It has been a popular practice to appeal to hypothetical enzymes to explain some of the obscure chemical transformations in the organism. Thus we have been wandering through the mazes of the oxidases, oxygenases, peroxidases, reductases, catalases and other products of perplexing nomenclature in the hope of escaping the uncertainties of intermediary metabolism."[36]

Summed-up Statement of Justification of the Hypothesis

The final gathering-up-and-putting-together may now be made of all that has been said about the physico-chemical aspect of the organism on the one hand, and about its psychical aspect on the other. That is to say, we are now ready to epitomize the results of our examination of the ancient and honorable but withal unsolved problem of how Body and Soul go together. As regards "body" or "the physical" we have been led to the physico-chemical conception of the organism as a well-nigh inconceivably complex mass of substances, mostly in the colloidal state, operating as a system of phases in dynamic or constantly changing equilibrium. As regards "soul" or "the psychical," we have found also a series of phases of activities, namely the phases of intellect and reason, those of instinct, those of feeling and emotion, those of the will, those of the tropisms and the "simple reflexes," and finally those of simple protoplasmic response. According to my hypothesis, the phases of the bio-chemico-physical sort and the phases of the psychical sort have common ground in the organism as a whole, the phases of intellect and reason corresponding to the cerebro-spinal nervous system; the phase of instinct corresponding probably to the autonomic nervous system; the phases of feeling and emotion corresponding mainly to the glandular and visceral systems; those of the will to the body-muscular system; those of the tropisms and simple reflexes to the receptor-conductor-effector systems; and finally those of simple protoplasmic response to the fundamental protoplasmic mechanism of response, whatever its structure.

According to the scheme presented in the sketch and summed up here, just as physical functioning and physical form reach back to the very dawning of animal life, both in the individual and in the race or type, so consciousness with its nether limits in what, following the terminology of embryology (see section on the pro-morphology of the egg-cell, Chap. 8), might be called pro-consciousness, is an attribute of all animal organisms. As comparative anatomy and physiology have made us familiar with the physical aspect of the animal organism existing as the fully realized or developed adult at one end of the ontogenic series, and as the unrealized adult or germ at the other end of the same series, exactly so its psychology gradually familiarizing us with the realized, or adult mind at one end of the ontogenic series, and as the unrealized or germinal mind at the other end of the same series. When we affirm that the completed individual organism is latent in the germ, we must understand that the psychical aspect no less than the physical aspect is so latent. With very little doubt, it seems to me, the real meaning of the so-called sub-conscious, and of psycho-analysis as a method of investigating it, is that the ontogenic stages of the psychic life of the human organism are being discovered and that a method of investigating these stages is being worked out. Freud and his followers have been and still are somewhat in the dark, I think, as to just what they are doing, albeit their discoveries and methods are of the utmost importance.

REFERENCE INDEX

1. Wheeler, W. M. 519
3. Sedgwick & Taylor 262-3
4. Royce, J. 2
5. Royce, J. 3
6. James, Wm. I-341
7. Wheeler, W. M. 521
8. Whitman, C. O. 310
9. James, Wm. II-451
10. Cannon, W. B. 280
11. Cannon, W. B........... 282
12. James, Wm. II-450
13. James, Wm. II-451
14. Hopkins, F. G. 217
15. Hopkins, F. G. 220
16. Hopkins, F. G. 220
17. Sellars, R. W........... 75
18. Descartes, René..Pt. 1, Sec. 8
19. Montague, W. P. 120
20. Hume, David...Sec. IV, Pt. 1
21. Ritter, W. E. (1918[2]) Essay 2
22. Huxley, T. H. 98
23. Huxley, T. H. 96
24. Huxley, T. H. 100
25. Huxley, T. H. 94
26. Dewey, John 9
27. Darwin, Chas. ....
28. Crile, G. W. ....
29. Morgan, C. Lloyd 256
30. Henderson, L. J. ....
31. Gibbs, J. W. ....Pref. x
32. Lewis, Wm. C. McC...II, 303
33. Ritter, Wm. E. (1918[2]) Essay 1
34. Foster, M. 469
35. Luciani, L. ....I, 395
36. Mendel, L. B. 21
37. Ritter, Wm. E. (1918[2])
38. Wendt, G. L. 442
39. Stout, G. F............ 680

POSTSCRIPT

THE argument in favor of the organismal way of viewing living nature has now run what appears to me its natural course, to its inevitable end. Yet I cannot bring myself to write "Finis" without making a few remarks which though connected vitally with the argument, do not seem an essential part of it.

These remarks concern the general effect of the organismal standpoint on those who may grasp it firmly and adopt it unreservedly. Since, as pointed out in the "Historic Background" with which this book opens, the standpoint has been recognized by biologists with varying degrees of fullness from the time of Aristotle at least, there can be no doubt that the human mind is naturally attuned, as one might say, to this general type of response to organic phenomena. It seems therefore fitting that a presentation like that which I have made should be accompanied by a few words on the probable influence of a wide prevalence of the organismal view. The pertinent question will be asked, how could it have come to pass that if the standpoint has been so long in the world it should have missed full recognition and have failed to exert its due influence? The reply is obvious to an attentive reader of this book: At no time until the present in the long historical growth of knowledge of the living world has information been sufficient to make possible a rounded-out statement of the conception. To illustrate, it is only in the very last years that enough has been known of the physical chemistry of the cell to engender such an interpretation of this exceedingly important biological entity as that which biochemists are just now reaching. Yet this interpretation is indispensable to anything even approaching a full development of the organismal view.

But nothing stands out more boldly from the pages of this book than the insufficiency even yet, of actual knowledge for making the standpoint complete. If therefore, I append to my presentation a brief reference to the larger effect the view has had on myself, and on this basis forecast what the effect would be on thinking people generally were they to make it their own, such a forecast will surely be in harmony with the larger purpose of the book, even though the anticipatory remarks have no place in the presentation itself.

The long and laborious gathering and arranging of facts, and weighing of natural evidence and formal arguments which has constituted the development of the standpoint in my own mind, has compelled me to re-examine and re-assess the whole frame and fabric of my spiritual life. Nothing, so far as I can tell, has escaped. Not my scientific knowledge alone—my professional stock-in-trade—but all my ideas and beliefs touching religion, art, society, politics, industry, personal relations, and private living, have come in for their share of scrutiny and renovation.

An exceedingly brief "synoptic" classification and characterization * of the entire range of these effects can be given in the terms of formal science and philosophy.

As to classification, the effects fall into a two-fold grouping. One of the groups appertains to the great province of the nature of knowledge; the other to the equally great province of the nature of morals.

The characterization of effects on the nature of knowledge which seems to me most inclusive and most practically significant, may be stated thus: By the validation of objective knowledge, largely through the principle of what I have called standardization of reality, but partly through

* See my essay, *The Place of Description, Definition and Classification* (Ritter, 1918).

the organismal hypothesis of consciousness, such knowledge is elevated to the rank of strict equality with "pure thought," often so-called; that is, with subjective, or intuitive knowledge. In this way mathematico-mechanistic science is deprived of the regal place it has claimed for itself since the era of Descartes and Leibnitz, and is brought to the plane of absolute equality as to importance and dignity, with sense-experiential science. By thus adjusting the claims of these two great realisms of science, an attitude toward the infinite totality of nature, and a methodology for interpreting it, which have hitherto borne the stamp of subjection and inferiority assume their rightful places in the great hierarchy of philosophical science. This leveling-down of mathematical mechanics and the deductive method and leveling-up of observational knowledge and the inductive method, implies the complete overthrow of psycho-physical dualism in psychology, and the rescue of personality from bondage to a theoretically infinite monotony of "Matter and Energy."

The characterization of the effects of the organismal view on morals centers around the perception that in the establishment of human personality the persons are organically interdependent upon one another; that is, interdependent through their "attributes of relation," this resulting in the incorporation of men into a pluralistic universe far more real and vital than philosophic pluralism has hitherto been in position to grasp. Through a type of human conduct guided by knowledge of these principles of personality and the interdependence of personalities, and through supplementing mathematico-mechanistic methods of study by a rigid application of observational and statistical methods, a genuine science of morals, both theoretical and practical, is made attainable.

That my enterprise of developing the organismal view is only part and parcel of the general current of interpretation of living nature which has flowed through the centuries seems clear even from my meager acquaintance with the history of philosophic thought. Thus we read in Windelband (*A History of Philosophy*, Eng. by Tufts,): "For the decisive factor in the philosophical movement of the nineteenth century is doubtless the question as to the degree of importance which the natural-science conception of phenomena may claim for our view of the world and life as a whole." (624). Then after speaking of the sharp antithesis between the *Weltanschauung* elaborated by the "Highly strained idealism of the German Philosophy" of the early nineteenth century, and the "*materialistic Weltanschauung*" of the later decades of the same century, the author writes: "If we are to bring out from the philosophical literature of this century and emphasize those movements in which the above characteristic antithesis has found its most important manifestation, we have to do primarily with the question, in what sense the psychical life can be subjected to the natural-science mode of cognition." (p. 625).

That Part II of this book of mine, especially Chaps. 20 to 24, go a long way toward answering the cardinal question formulated by Windelband appears to me certain. And, I may add, it also seems quite clear to me that the gigantic struggle at arms which that philosopher's nation has now brought upon the world, is one of the strongest proofs that philosophic thought and, following this, social and political leadership in Germany have failed miserably to discover the *Via Media* between the *Weltanschauung* of the "highly strained idealism of the German Philosophy" and the *materialistic Weltanschauung* which has finally reached its natural climax in militaristic brutism, and is almost certainly (Sept., 1918) approaching its overthrow.

Nothing could more fittingly end this book, devoted as it is to demonstrating the operative nature of organic unity in one of its great segments, than a reference to the fact that the philosophy of life now determining German morals, and which has drawn its inspiration largely from the hypothesis of natural selection, has failed—pathetically beyond the power of words to express if done unintentionally; and criminally in equal measure if done intentionally—to understand the real meaning of Darwin's teaching as a whole.

Certain it is that had the German philosophers of *Machtpolitik* recognized the place of unqualified supremacy ascribed by Darwin to the mental and moral endowments of man, it would have been impossible for them to make the dogma of survival of the fittest serve their ends in any such way as they have made it, and done so honestly. Attentive reading of *The Descent of Man* makes it perfectly plain that Darwin simply accepted all the higher human attributes—moral, esthetic, and religious, no less than those of the intellect—as fundamental data in his reasoning about man's evolution. His sole effort as touching these was merely to see in how far they could be regarded either as helped forward in their development by natural selection, or at least as not inconsistent with it. Apparently it never even occurred to him to regard his hypothesis as supreme-over-all, so that all attributes whatever, the noblest ones of man with the rest, must either be forced into conformity with it, or their reality and power virtually denied. "I fully subscribe to the judgment," runs the opening sentence of the chapter on "The Moral Sense," etc., "of those writers who maintain that, of all the differences between man and the lower animals, the moral sense or conscience is by far the most important." And, especially significant at this time, Darwin quotes with obvious approval, an apostrophe to Duty by Kant, in which this "Wondrous thought" is represented as "holding up its naked law" in the soul, and demanding reverence. Darwin's entire discussion in this part of the *Descent* makes it clear that what he had in mind was to discover as far as possible the germs of "conscience," of "feeling of right and wrong," of an "inward monitor," of "sympathy," of "parental and filial affection," of "social affection," of the "instinct of self-sacrifice" and so on, in the lower animals so as to have a starting point for these attributes as they occur in civilized man. It was not at all his purpose to show, as the German perversion of the struggle-and-survival hypothesis holds, that the evolution of man has consisted largely in a farther differentiation and intensification of the dominantly brute attributes, with an infusion as a kind of by-product from the struggle for existence, of certain "humanistic sentimentalities," which in reality are signs of weakness and must be suppressed.*

And this perversion by German science and philosophy of Darwin's teaching is rooted very deep in German culture and character. The straightforward, common-sense descriptions and inductions of the practical-minded, country-dwelling, country-loving, unacademic English naturalist were altogether too simple and unsophisticated to satisfy a *Kultur* permeated through and through with the "highly strained idealism" of Kant, Fichte, Hegel, and Schopenhauer. The two worst errors committed by Darwin were his over-emphasis on the natural selection hypothesis, and his propounding of the gemmule-pangenesis hypothesis; and it is highly characteristic that it was in just these two "strained" speculations that German biology and practical philosophy should have taken up Darwinism the most ardently and overworked it the most absurdly and disastrously.

My examination of the germplasm-determinant theory of Weismann in Part I of this book has revealed something of the scope and nature which the gemmule fallacy was destined to assume when it fell subject to German speculation. The more subtle and far-reaching and humanly practical consequences of the adoption and elaboration of the struggle-and-

* The effort which Dr. George Nasmyth has made in his book *Social Progress and the Darwinian Theory* to set right Darwin's position in this matter, ought to bear fruit after a while.

survival hypothesis by German speculation has not yet been subjected to thorough-going biological criticism, though several moves in this direction have been made.

Even the realism of recent German political and economic theory and practice is a "highly strained" speculative realism. This philosophical monstrosity is largely attributable, demonstrably so I believe, to a cultural and governmental system in which the principle of universal organic personality is grossly violated. And what a price in misery and blood and treasure the whole world, but old Europe particularly, is paying for a consummation which a truer philosophy of life would have foreseen and forestalled!

Can the leaders of German *Kultur* be convinced of the fundamental fallacy of their theory of human and national life, only by discovering that their military establishment, built up through many decades of patient, costly organization and discipline, but under guidance of a philosophy of mechanism and brutism, is yet incapable of overpowering a military establishment, a large portion of which may be improvised in the course of a few months, if such improvision be under guidance of a philosophy of personality and humanism?

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GLOSSARY

Acrogemaly. A disease characterized by hypertrophy of the terminal parts of the body, as of the face and extremities; an outgrowth involving bony and soft parts.

Adrenals. A pair of small glands situated in front of the kidneys. They are glands of internal secretion, their secretion exercising in particular a regulating effect on the nerves of the heart and blood vessels.

Adrenin. The "active principle" in the secretion of the adrenal glands.

Alcyonaria, include many corals and other coelenterate animals with eight mesenterics and eight tentacles.

Alveolar. In anatomy, a numerously-pocketed, or sacculated, structure, typified by the terminal cavities of the lungs, but occurring in various tissues; believed to constitute also one kind of protoplasmic structure.

Amœba. A unicellular animal, a genus of rhizopodous Protozoa.

Amphibian. An animal living both in water and on land. Properly a class of vertebrates whose young are typically aquatic and respire by gills; examples, frogs, toads and salamanders.

Amphioxus, literally pointed, or sharp at both ends. The current name for one of the very simplest and lowest vertebrate animals occurring in the sand and mud of the seashore in many parts of the world.

Anabolic, the chemical up-building of the living body; constructive metabolism.

Antibodies. "The products of a reaction of the body towards a natural or artificial introduction into it of certain foreign substances, bacteria and their poisons, vegetable poisons of other kinds, and various albuminoids." The name antibodies has reference to the antagonism these products have for the introduced substances.

Antigens. Substances the reaction of which with the living body produce antibodies.

Ascidians. Marine animals having a gelatinous or leathery envelope containing cellulose. In the larval stage a notochord or forerunner of the vertebral column is present. Some free living species retain the notochord all through their lives.

Axon. The long, slender, sparcely-branched, fibrillar process of a ganglion cell; contrasted with the shorter, more branched, more irregular *dendron*.

Axostyle. A slender, flexible rod of organic substance forming a supporting axis for the body in some Protozoa.

Biogen. Literally life producer. Imaginary ultimate units of life. Such special significance as the "biogen theory" has over other theories which make imaginary vital or physiological units a goal of the ultimate explanation of life, is found in the fact that the biogen theory aims to be more definitely chemical than the others. The German physiologist Max Verworm has elaborated this speculation more fully than has any one else.

Biophor. Literally life carrier. Biophors, the imaginary ultimate vital units of the Weismannian system of speculative biology, differ from biogens in the fact that Weismann, not being a chemist or even a physiologist, but a zoologist interested in reproduction and heredity, rather than in function generally, did not undertake to put his speculation on a chemical basis.

Bioplasm. Formative living matter; not differing in any but a speculative way from protoplasm.

Biotic. Pertaining to living beings.

Blastogenesis. Reproduction by budding, as used in this book; in general, propagation from an undifferentiated germinal mass.

Blastomeres. The first segments or cells formed by the division of the ovum.

Blastozooids. The united individuals produced by budding, and constituting the colony, or cormus in the compound ascidians.

Blastula. The stage of development of the embryo from the ovum in many animals, in which the organism consists of a hollow sphere the wall of which is composed of a single layer of cells.

Bryozoa. Literally "moss animals," from the resemblance, fancied more than real, of some of the species to mosses; also called Polyzoa. Marine animals occurring abundantly on all shores. Most of the species propagate by budding as well as by eggs and sperm, the bud-produced individuals remaining attached to one another to form colonies, as in many hydroids and ascidians. Each individual consists of a body proper bearing a circle of tentacles, and an enveloping case often calcareous, into which the body may be quickly and completely retracted.

Calymma. The much-vacuolated portion of the body of radiolaria, situated outside the central capsule, the vacuoles containing fluid impregnated with gas. The main office of the structure seems to be in connection with the flotation of the animals.

Cambium. The layer in woody plants between the outer dead layer, or bark, and the inner dead mass, or wood proper, from which new tissue is formed; the true growing part of plants which live several years and attain a large size.

Cartesian Philosophy. The mode of viewing man and nature inaugurated in modern times by Rene Descartes. The most distinctive thing about it is the sharpness with which the dualism, or antithesis, between mind and matter stands out in it. Its great practical importance for the present era lies in its genetic relationship to psycho-physical parallelism in psychology, and to all forms of idealism in philosophy.

Centrolecithal Eggs. Eggs in which the protoplasmic portion and nucleus constitute a surface layer, the inner mass being chiefly yolk, that is food material for the future embryo. The eggs of most insects and crustaceans are of this type.

Cephalize. The tendency among animals for a head to become differentiated from the rest of the body.

Chemical Messengers. Substances produced by the organism, either in special glands, the glands of internal secretion, or in general tissues, and carried by the blood and lymph over the body generally, to influence the growth or functioning of other tissues. *Hormones* is another name given to these substances.

Chemotropic. Pertaining to the reaction of organisms to chemical stimuli.

Chromatin. The finely granular substance most distinctive of the cell-nucleus. Its name comes from the readiness with which it is colored by many dye-stuffs.

Chromatophores. Pigment-bearing sacs, often single cells, in plants and animals. It is by means of these that the rapid color changes in the skin of many animals are accomplished.

Chromidosomes. One of the many names given to minute specially stainable bodies in the cytoplasm of many cells.

Chromosomes. The more or less definite bodies of the cell-nucleus into which the chromatin granules are grouped. Their constancy of structure and relation to hereditary characters have given them great prominence in much of recent biological theory.

Chromophil. Any body in the living organism that has an avidity for staining reagents.

Coccus. In the classification of bacteria according to their shapes, those which are spherical are called *cocci*.

Coelenterata. A group of animals that have a digestive cavity, but nothing corresponding to the abdominal cavity; also called radiata from the radial arrangement of the body.

Cortical. Pertaining to the cortex, or outer layer of an organ, as of the brain, bark of the tree, &c.

Cretin. An individual affected with cretinous disease, a disease characterized by certain bodily deformities and mental impairments. The malady frequently accompanies goitre, and is now considered due to deficiency in secretion of the thyroid gland.

Crustacea. A class of invertebrate animals belonging to the great phylum arthropoda, briefly characterized by their exoskeleton and paired jointed appendages.

Cytology. Science of the cell.

Cytoplasm. Substance of the cell-body, as opposed to the cell-nucleus.

Determinants. That particular class of imaginary ultimate vital units by which the development of hereditary attributes is determined. They were invented by Weismann, and were conceived to constitute the germ-plasm, and to be located primarily in the chromosomes of the egg and sperm. In later speculation determin*er* is used more frequently than determin*ant*—on some account that is not clear.

Diatoms. An immense group of aquatic, unicellular algæ especially characterized by their firm, box-like, regularly-shaped, chitinous shell.

Dinoflagellates. Literally organisms which are two-lashed, owing to the two flagella possessed by most of the species. A group of aquatic unicellular organisms almost as numerous as the diatoms. The photo-synthesizing power of living substance possessed by these two groups, and their enormous abundance at and near the surface of the bodies of water in which they live, make them fundamentally important for all the life of the waters of the earth.

Distal. A common anatomical term signifying away from a given point, usually some definite feature, as the attachment of a muscle, taken as a point of reference.

Dorso-Ventral. A term much used in the anatomy of the higher animals to signify a direction from-back-to-belly of a creature.

Duodenum. The first portion of the small intestine between the stomach and the jejunum.

Dynamic Center. A phrase used rather frequently in recent biology, especially in the biology of the cell, to express the conception that certain structures, as the centrosome, are in some way not clearly specifiable the "seat" of various vital activities. The phrase has some such implication for general physiology as "nerve center" had and with many still has, for nerve physiology.

Ecological. Pertaining to Ecology, the science of organisms in relation to their natural environments. This old but newly appreciated and named branch of the science of living nature, may properly be regarded as the natural history of plants and animals modified to meet the modern demands of comprehensiveness and exactness in dealing with a great province of natural phenomena.

Ectoplasm. The outermost, somewhat denser layer of protoplasm in many cells, especially in many unicellular animals. Opposed to *endoplasm*, the inner, more fluid mass. The ectoplasm in protozoans corresponds to the skin of higher animals. The presence of a more or less sharply set-off outer layer or membrane or skin in all organisms whatever is coming to be recognized as having a more fundamental physiological meaning than that of a protection for the delicate parts underneath, now that so much is being learned by physical chemistry about surface phenomena.

Energy. Work and capacity to do work. It is important to note that work and capacity to work necessarily imply some *object*, organic or inorganic, to do the work, and hence that when the energy of a horse, or of a stream of water, is spoken of the word energy has a very different meaning from what it has in such a phrase as the "energy conception" of nature or of the organism, the implication in these cases usually being that energy is the real essence of nature and of the organism, the shape and other so-called static attributes which all bodies present, being only incidental and mere appearances.

Enzyme. A chemical substance produced by an organism, plant or animal, to the end of bringing about chemical transformation in other substances, but without itself being transformed. The ptyalin of saliva by which starch is changed into sugar, is typical. Enzymes play a very great part, especially in digestion and nutrition, in the physiological processes of all organisms.

Epigenesis. That theory of development of the individual organism which holds the organs and parts to be actually new productions, and not merely enlargements or actualizations of what already existed, this latter conception of development constituting the theory of *preformation*. Although these opposing theories were debated with fury, almost, some years ago, little is heard about them now though none of the particular problems around which the discussions centered can be said to have been solved.

Epimorphosis. The mode of regeneration of organisms in which a multiplication of cells on the surface of injury is first produced, then from this "embryonal tissue," the new organ or part is formed; contrasting with *morphalaxis,* a mode of new formation which consists in a direct transformation of an already existing part into the new part.

Factor (in Genetics). A hypothetical unit of structure or of chemical composition, contained in the germ-cell, which in some way is held to condition the development of a particular character in the adult, or of a complex of characters which are transmitted in constant association with one another. Factors are believed to interact with one another in development, and at times to be so "linked" that they are only partially independent in transmission.

Flagellum. A lash-like appendage or large cilium serving as an organ of locomotion in some Protozoa and some bacteria.

Foraminifera. A class of rhizopodous marine Protozoans, usually having a porous shell.

Formative Stuffs. Hypothetical substances which are supposed to be formed in one part of an organism and transported to another part, there to produce, or to influence the production of new organs. For example, several botanists have supposed that the flower substance of some plants is actually produced in the leaves.

Gamete. A reproductive cell which unites with another reproductive cell to form a zygote.

Gastrula. That stage of embryonic development in many animals which consists of two germ-layers inclosing a central cavity. It is produced from the blastula (which see) by the in-sinking of one-half of this into the other.

Gemmiparous. Producing gemmæ, or buds (reproducing by budding), applicable to both plants and many animals.

Gemmule. In the original and proper sense a small aggregation of cells set apart in the tissues of some plants and animals, notably in many sponges, for the purpose of reproduction. In origin and structure gemmules are more like buds than eggs, though the end served is very similar to that served by seeds. In a secondary and wholly hypothetical sense, gemmules are imaginary, minute bodies given off by all the tissue cells of an organism and assembled in the germ cells, there to cause the development of the next generation. This taking of a very concrete name from botany and zoology, and using it in a wholly imaginary way to explain hereditary development was due originally to Charles Darwin, but with more or less unimportant variations of meaning has since been resorted to by many of the best known biologists.

This example indicates the great importance for biology, especially for the biology of reproduction and development, of distinguishing between the same terms used in a strictly objective and descriptive sense on the one hand, and in hypothetical, or purely imaginary sense on the other.

Gene. A term much used in present-day genetical science, but apparently not differing in any significant particular from *factor* (which see).

Genetic. Pertaining to genetics,

evolutionary science dealing with natural propagation and development, the interest centering at present in that portion of development which is hereditary and involves sex cells.

Germ-plasm. Actually all the protoplasm of the germ-cells which participates in development; theoretically merely the small portion of the germ-cells supposed to be "hereditary substance."

Gonad. A mass of undifferentiated, generative tissue from which the male and female reproductive glands originate.

Gonophore. The ultimate generative zooid of a hydrozoan, giving origin directly to the generative elements.

Hectocotylized. Applied to the remarkably altered condition assumed by one of the arms of the male cephalopod to make it an organ for impregnating the female.

Heliotropic. Responding to the stimulus of sunlight.

Heteromorphosis. A kind of regeneration in which the part produced is different from that which was lost, as, for example, when an antenna-like structure grows in the place of an eyestalk, in some crustaceans when the eye stalk is cut off.

Histogenesis. The process of tissue genesis, or production, from undifferentiated cell masses, in plants and animals.

Histology. The science of tissues, plant or animal; microscopical anatomy.

Hormone. Literally something which excites or stirs up. Originally and strictly applied to those internal secretions (which see), the office of which is to incite the parts on which they act to greater activity. But internal secretions are also known now which retard or inhibit the action of the part they affect; and to these it has been proposed to apply the term *chalone,* that which slackens. But some physiologists use hormones as synonymous with "internal secretions."

Homonymous. As used in this book, an anatomical term referring to the different members of a series which differ more or less, but still all have the same general name. Thus all the pairs of appendages of a lobster are homonymous, or ambulatory appendages originally, although used for a variety of purposes now.

Hydranth. One of the bud-produced polyps of a hydroid colony.

Idioplasm. Literally plasm which is very specially one's own. First used to designate the hypothetical part of the germ-cells which is supposed to be alone responsible for hereditary transmission. *Idioplasm* may be regarded as the historical antecedent of *germ-plasm* (which see).

Interstitial. Pertaining to or situated in an intervening space; a term much used in anatomy to signify within an organ.

Internal Secretion. The term has long been used in the physiology of the higher animals, in contradistinction to "external secretion," to designate the products of glands, like the thyroid, which discharge their products into the blood or lymph, instead of upon the surface of the body or into the digestive or some other cavity of the body. The existence of internal secretions was known long before anything was known about their use; hence this non-committal name, so far as function is concerned. The recent discovery of their office has suggested the name *hormone* (which see) for them, and has revealed their great importance not only for physiology, but for philosophical biology.

INVOLUTION. Literally inrolling, or inwrapping. In descriptive biology used to signify the return of an organ to its original or normal condition after some violent or pronounced deformation of it. Sometimes, but apparently unjustifiably, used as a synonym of degeneration. Since the doctrine of evolution has become prominent in biology, a process the opposite of evolution has been thought by some to be necessary, and to this *involution* has been applied.

JELLY-FISH. In the interest of discriminative knowledge, the habit, rather common among people who have the opportunity to see the transparent, somewhat gelatinous-appearing animals of the ocean, of calling them all "jelly-fishes" should be abandoned. The name should be restricted to the regularly disc- or dome-shaped, tentaculated animals belonging to the coelenterate phylum, thus enlarging the bounds of definite, popular zoological information, by recognizing that marine animals of several large and very distinct classes have this general consistency and appearance.

KARYOPLASM. A cytological name referring to the substance or plasm distinctive of the cell-nucleus.

KARYOSOME. A small, discrete, rather constant body which stains readily, contained in the cell-nucleus; frequently synonymous with nucleolus.

KATABOLISM. The down-breaking, or descensive phase of metabolism; the opposite of *anabolism* (which see).

KINETO-NUCLEUS. One of the nuclei in the two-nuclear protozoa supposed to be concerned in some special way with the movement of the flagella or cilia of these animals.

LAMELLAE, singular *lamella*. A term much used in anatomy to designate the thin plates, scales, etc., that are so numerous and varied in form and size in nearly all organisms.

LARVA. Properly applied only to stages in the lives of individual animals which pass into succeeding stages through a deep-sealed metamorphosis, as for example the grub or maggot of a fly, and its transformation into the adult. Larval stages and profound metamorphoses are very common and widespread in the animal kingdom.

LIMULUS. The technical genus name for the horse-shoe crab, an animal of special interest to general zoology in several ways.

MACRONUCLEUS. In the infusoria, a group of protozoans, there is one large nucleus and one or several much smaller nuclei. The first is called, from its relatively large size, the *macro*-nucleus; the others *micro*-nuclei. From the behavior of the two kinds of nuclei at conjugation and division, the micronuclei are known to be intimately connected with these processes, while the macronucleus seems to be more concerned with the nutritive functions of the animal.

MANUBRIUM. In morphology a part or organ which resembles a handle; specially the clapper-like, or handle-like portion of a medusa which is found within the "bell." The animal's mouth is at the end of the manubrium, and most of its digestive cavity within the stalk of the manubrium.

MATRIX. In biology the ground substance in which cells are embedded in some tissues, and which is produced as a secretion by the cells. It is one kind of intercellular substance. The opalescent, almost homogeneous chief mass of ordinary cartilage is a typical matrix.

Melanin. A rather general term in biology, especially in zoology, applied to dark brown to black pigments.

Merotomy. The automatic cutting off of parts or segments in living organisms.

Mesenchyme. Undifferentiated mesoderm that produces connective tissues, some muscles, and certain other structures in the animal body.

Metabolic. Pertaining to metabolism, the process of chemical building up and breaking down in the living organism.

Metameric. Pertaining to the longitudinal series of parts or joints into which the bodies of many higher animals, such as earthworms, lobsters and fishes, are divided.

Metaplastic. Pertaining to *metaplasm*—applied to changes which cells sometimes undergo from one plasmic type to another; also applied to certain supposedly lifeless inclusions in the protoplasm of cells.

Metazoa. Multicellular animals.

Micronucleus. See macronucleus.

Microphyle. In botany and zoology the aperture in the coats of the ovule and ovum through which the male fertilizing cell penetrates.

Monera. Hypothetical simple structureless masses of protoplasm (without any nucleus). Assumed by Haeckel as the lowest members of the evolutionary series. Advance of knowledge has found no evidence of such organisms.

Morphallaxis. A kind of regeneration in which part of an organism transforms directly into a new and different part.

Morphological. Pertaining to *morphology,* the science of form and structure.

Morula. A stage in the embryonic development of many animals, in which the ovum has completely segmented, but the segmentation cavity has not yet been formed.

Myoneme. A thread-like contractile structure in the cytoplasm of certain higher protozoa.

Nematophore. A body of defense and offense developed in certain hydroids, consisting of a chitinous receptacle in which thread-cells are immersed; the nettling organs on the tentacles of large jelly-fishes.

Neural. Pertaining to nerves.

Neuroblasts. Undeveloped nerve cells.

Nucleo-plasm. Nuclear substance, including the different nuclear ingredients.

Nucleo-protein. One of the compounds of nucleins and paranucleins.

Œdema. Dropsy, a vasomotor neurosis characterized by non-inflammatory swellings on various parts of the body.

Ontogeny. The development of an individual organism from germ to completed or adult stage.

Organelle. A little organ, and *organoid,* organ-like, are terms applied to the organs of unicellular plants and animals, not so much because of their small size and indefiniteness of form and structure as on account of the theory that a true organ must be composed of cells, and cannot be a part of a cell. These terms are among the sequelæ of the cell-theory.

Orienting. Finding or fixing the positions or directions.

Pangen, and Pangenesis. These

terms, basal in Darwin's famous hypothesis of heredity, mean all-generator and all-generative only in the sense that all parts of the body of the organism give off *gemmules* (which see), which assemble in the germ-cells to enable these to *re*produce the organism. Thus the *pan,* or *all*-generative power was conceived as having its original "seat" in the organism all-in-all. In other words Darwin's speculation was almost diametrically opposed to the transformation it has undergone latterly, especially in the prolific mind of Weismann, the germ cells alone, or rather the germ-plasm being the all-generator, according to these speculations.

Parathyroids. Small glands lying near the thyroid but not functionally connected with the latter.

Parthenogenesis. Reproduction by means of unfertilized eggs.

Pellicula. The cuticle or outermost body membrane in some unicellular and other low organisms.

Phase. This old and familiar word has taken on new and greater importance, both scientific and philosophic, with the recent advance of knowledge in the region of over-lap between physical and chemical phenomena, this advance making what is generally called physical chemistry. A *phase* in pure physics, as it may be called, has reference to the position of the particles of a body when the particles are undergoing change. For example, corresponding particles in two succeeding waves of water or air are in the same phase. In physical chemistry *phase* has reference not to position but to state or condition of the constituent particles of a heterogeneous, or unlike system. Thus, a combination of liquid water (in common language just water) and solid water, or ice, is a two-phase system of water. Philosophically viewed, the great significance of phases is that the positions and states of the particles are possible, even conceivable, only in relation to the larger, containing part or whole. Something of the bearing of this on the theory of *pluralism* (which see), when this theory is approached from the strictly objective side, will be easily seen.

Phlogiston. An imaginary substance formerly supposed to exist in all combustible bodies, and to be the cause of fire and flame. For nearly a century before the discovery of oxydation as the true cause of fire, by Lavoisier, the phlogistic theory dominated much of chemical science. The chief interest in the theory now is in its relation to the observational and logical processes involved in interpreting the generative processes of nature everywhere. The phlogistic theory may be taken as a type of elementalistic causal explanation of natural production.

Phylogenic. Pertaining to phylogeny, the development of the race; concerning ancestral organisms, real and hypothetical.

Pituitary Gland. A gland of internal secretion, situated at the base of the brain, and connected in the embryo with the roof of the mouth.

Pluralism, philosophical (so used in this book). The conception that in its deepest nature the universe is multiform and complex; the opposite of *Monism,* the conception that some single Essence or Substance, more or less known or unknown, is the foundation of all things.

PLUTEUS. Name given the characteristic process-bearing larva of sea-urchins and their near relatives. These larvæ are of considerable general interest because of the extensive use made of sea-urchin eggs in experimental embryology, the eggs being easily obtained and easily kept in the laboratory.

PROTEINS. Nitrogenous substances found in the bodies of plants and animals. These substances are usually considered to be the most fundamental, from the chemical standpoint, in organic beings.

PROTISTA. A group name intended to include all unicellular organisms; i.e., both *protophyta*, one-celled plants, and *protozoa*, or one-celled animals.

PSEUDOPODIA. Literally false feet. They are temporary protrusions of the protoplasm of some protozoa, especially of the rhizopodous class, typified by the amœba, the name having reference to the locomotor office of the processes. But their food-taking and digesting office should be noted also.

PTYALIN. The unorganized ferment, or enzyme of saliva, chiefly instrumental in the conversion of starch into sugar.

RADIOLARIA. One of the main subdivisions of the protozoa, especially characterized by their generally spherical outline, and radiating structures, some soft and extensile, others stiff and permanent. The radiolaria are almost all marine.

REGULATION. Much used in studies in the regeneration of organisms, to express the power many plants and animals have of undergoing structural or functional readjustments in order to retain, or to regain, their typical form; a significant adaptation of a general term to a technical end.

RETICULAR. Net-like, a term much used in anatomy, as many portions in both plants and animals of many grades, present this type of structure, though the netting never has the regularity of manufactured netting.

RHIZOPODA. The great subdivision of the protozoa especially characterized by sending out pseudopodia (which see). Amœba is usually mentioned as the type of this subdivision, but the larger number, probably, of rhizopods possess shells of one sort and another, while amœba is entirely naked during all its active life.

SARCODE. Literally like flesh. The name originally applied to what, under microscopic examination, seemed to be the fundamental living-substance of animals. Later discovered to correspond to what was known as protoplasm in the cells of plants.

SARCODYCTIUM. A protoplasmic network of the surface of the calymma of a radiolarian.

SELF-DIFFERENTIATION, SELF-REGULATION, &c. It is not without philosophical significance that the term *self* has forced its way into technical biology, something as it has into technical philosophy. In biology the term is particularly common in connection with developmental phenomena and has reference to operations which depend primarily on the organism itself, and can be referred to "external factors" only remotely and in a round-about way.

SERICTERIES. Glands by which silk and silk-like substances are secreted in many insects.

SOMA, SOMATIC. The body and pertaining to the body. Much used in later discussions of heredity in a strongly hypothetical sense, to indicate the complete independence, so far as development is concerned, of the body from the germ. The antithesis is often made stronger by speaking of the substance of the body and the substance of the germ, using the terms *somaticplasm* and *germplasm.* From the philosophical standpoint it is instructive to compare the theoretically complete separation of body and germ in modern genetics, with the theoretically complete separation of body and soul in philosophy and psychology.

Specificity. The state of being specific, that is, of being manifested as phenomena distinguishable from all other phenomena. The group of terms kindred to *specific* and *species,* long important in systematic and taxonomic biology, are becoming increasingly so with the advance of knowledge, especially in the domains of the chemistry of different kinds of organisms, and of comparative behavior and psychology.

Sporazoa. One of the main subdivisions of the protozoa a leading characteristic of which is indicated by the name, that characteristic being the commonness with which propagation occurs in the group by means of spores produced within the body of the animal. By far the greater number of the species of the group are parasitic, many of them disease producing.

Sporulation. The process of converting into spores, as in the sporazoa, in some other animals, and in many plants. Spores differ from eggs, on the one hand, and seeds on the other, only in the fact that spores are not sex cells, that is, do not need to unite with other cells in order to develop, as is the case with most eggs and seeds.

Spontaneous Generation. The difference between "spontaneous" in this phrase and in the phrase "spontaneous action," as of an animal, should not be missed. In the latter connection the word has nearly if not quite its original meaning, that is, of one's own accord, or initiative; acting by and through one's self alone. The adjective pronouns *meā, tuā* and *suā* are said to have been used always with *sponte* in good Latin prose. Strictly, then, if life really originated from something which was not living, that is by a "fortuitous" concourse or interaction among chemical elements of different sort, *spontaneous* would not be the proper term to describe the operation, simply because it would have involved fundamentally several selves, even if the different elements could each be called a self. It would not have been an operation identifiable by *my, your,* or *his* or *its,* but by *their.* Plurality rather than singularity of action would be the essence of the conception.

Stereotropic. Reacting to stimuli of contact with solid objects.

Stolon. A prolongation of the body of some plants and animals that gives rise to new individuals by budding.

Sudoriparous. Sweat-producing.

Symbiotic. Pertaining to a state of living together of two dissimilar organisms to the advantage of both.

Synaptic. In cytology pertaining to *synapsis,* the conjugation of chromosomes in sex cells preceding the reduction divisions connected with the maturation of germ cells.

Syncytium. As used in this book, a cytological term applied to a protoplasmic mass containing many nuclei, but not set off into distinct cells. The entire embryo is of this character in some animals. In fact a few embryologists have contended that during the embryonal stages of most, if not all animals, the cells are connected by protoplasmic strands and bridges, making them syncytia. The undoubted wide prevalence of syncytial structure among animals especially, has been used as an argument against the cell-theory.

Synthesis. From the organismal standpoint not many terms used in biology are more important than this. The etymological meaning, placed or put together, expresses only a part of the togetherness of an organism; the part, namely, which pertains to the assimilative activity performed by the organism on its nutritive substances. This process may be regarded as a synthesizing one in nearly the literal sense (though even here the process is more one of self-activity and less one of external agency than seems to be implied in the original word). But it is when we come to consider the *original* nature and power of the organism by virtue of which it assimilates food, that the inadequacy of *synthesis,* except in a much modified sense, comes to light, for the organism's ability to assimilate, that is to put or place together, its nutritive substances is wholly dependent, so far as we have evidence, on the fact of its being already and originally a *together* entity. An organism is able to put together, or synthesize, its food just because it itself *is* together, or synthesized. A synthesized state is a prior condition to synthesizing. To be an *organism* at all is to be synthesized.

Systematic. Pertaining to a *system;* literally a standing or being together. It is unfortunate that "systematic" has come to be restricted in its application in recent biology to the formal classification of plant and animal species. As a matter of fact a necessary consequence of the unity of all phenomena of the living world is that all these phenomena "stand" in some natural and ascertainable relation with all other phenomena, so that all biological knowledge whatever must of necessity be *systematic* if it really corresponds to nature.

Taxonomy. Mode of arrangement, the branch of biology which deals with the classification of the species of plants and animals.

Test. As used in zoology and botany, an external covering or tunic, usually nearly lifeless, tough and resistant. Its office is mostly protective.

Thymus. A gland of internal secretion found in the neck region in all vertebrates, and connected originally with the gill system.

Thyroid. One of the most important glands of internal secretion, located, as is the thymus, in the neck region, but connected embryonically with the pharynx rather than with the gills proper.

Trochophore. A larval stage in the lives of many marine worms and molluscs, characterized by being well organized for swimming by means of cilia variously disposed on the surface of the body.

Trypsin. One of the chief "active principles," or enzymes of pancreatic juice. It splits proteids into simpler compounds. It is produced by some plants as well as many other animals than man and vertebrates.

TUNIC. In botany and zoology, any well differentiated membranous covering of an organ or an entire organism; much the same as a *test*.

TUNICATE. Name of a group of marine animals, most sharply characterized by the cellulose-containing tunic, or test, which envelopes the body; by the peculiar basket-like respiratory system; and by the notochord or precursor of the vertebral column, possessed by all the species in the embryonal life, and by a few during the whole life, frequently used synonymously with *Ascidian*, which see.

VAGAL. Pertaining to the vagus nerves, one of the tenth pair of cranial nerves in all true vertebrates.

VASO-CONSTRICTOR. Applied to the nerves which cause contraction of the walls of blood vessels.

VASO-DILATOR. Applied to nerves which cause, or more exactly, permit a widening of the blood vessels by diminishing the tonus of the muscles of the vessel walls. Since the smaller blood vessels are all supplied with both constrictor and dilator nerves the constant balancing between these antagonistic influences, both kinds of impulse being in response to the general needs of the organism, this scheme illustrates well a principle of equilibration widely operative in the animal kingdom.

VISCERAL. A term used in zoology to indicate not only the totality of internal organs, but also the side of the animal on which these are situated.

ZYGOTE. A body formed by the conjugation of two reproductive cells, called gametes. Gametes and zygotes may be either unicellular organisms, or the reproductive cells of multicellular organisms.

ZYMOGEN. The enzyme-producing substance in the secretory cells of glands the secretions of which contain enzymes.

INDEX