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CHAPTER I

THE PROBLEM OF SCIENTIFIC METHODOLOGY

Experimental biology has such thoroughly convincing justifications, as in medicine, agriculture, and industry, that those men who are the most deeply interested in biology have failed to make a critical analysis of its principles. "Nothing is more striking in this science than the contrast between the brilliant skill, ingenuity, and care bestowed upon observation and experiment, and the almost complete neglect of caution in regard to the definition and use of the concepts in terms of which the results are expressed."(1) It is, nevertheless, this very confused substratum of biology which through Darwinism has laid an exhaustive claim to our whole life. The claim was possible only because of the confusion.

Logically enough, champions of the newer forms of materialism, such as Marxism, pragmatism, logical positivism, have given more thought to the underlying principles of experimental biology than those who claim to pursue knowledge primarily for its own sake, and they have indeed made the greater contributions. The layman is under the impression that these forms of materialism are the only ones favoured by our present knowledge.

More recently there have been most healthy reactions.

(1) G. Woodger, Biological Principles, N.Y., 1929, p.3. Cf. Appendix II for a résumé of this work.
We call attention to W.R. Thompson's Science and Common Sense. (1) He does not show however just where the method of experimental biology fits into the Aristotelian scheme which he describes. The present thesis is an attempt to set forth in Aristotelian terms the kind of knowledge we acquire in biology.

There is a body of principles upon which all modern biologists agree, such as the primacy of experience, the ever provisional character of definitions and of generalisations, the need to test again and again principles which never stand for the ultimate truth. In other words, experimental biology is not merely a quest for conclusions. It must forever question its very principles. However, the questions 'Is the cell the unit of life?' and 'Are all the generalisations provisional?' are of a different order. The latter is taken from a body of principles which we call methodological. The former is chosen from a body of principles arrived at by experimental investigation and reasoning. And even if we held experimental principles on purely historical grounds as statements of past facts which would be plausible for the future, they would nevertheless be of a different order than those of the methodological kind. For the former relate to the very subject matter of experimental biology, whereas the latter bear on the nature of biological knowledge as such.

Let us suppose that these methodological principles are true. Does it follow that they are to be extended to all fields of knowledge? If the biologist is to know just what he knows, he must have some idea on this problem. Either the kind of knowledge he acquires is the very archetype of science, or it is merely a part of a more general scheme to which it has some definite relation.

But it is especially the catholic biologist who should know the nature of the knowledge he acquires through the experimental method. What would he think of the following statement of Claude Bernard: "The requisite for a scientist who undertakes the investigation of natural phenomena is that he reserve for himself a liberty of mind which rests on philosophical doubt. It is not necessary, however, to be a sceptic; it is necessary to believe in science, that is to say, in determinism, in an absolute, necessary relation underlying the phenomena proper to living beings, as well as to all other beings; but it is necessary at the same time to be thoroughly convinced that we have this relation only in a manner more or less approximative, and that the theories which we possess are far from representing unchangeable truths. When we construct a general theory in our sciences, the sole thing of which we are certain, is that all these theories are, absolutely speaking, false. They are only partial and provisional truths, which are necessary for us, as steps on which we rely in order to advance in our investigation; they represent
simply the actual state of our knowledge, and consequently they must be modified with the growth of science, and so much more often as the sciences are less advanced in their evolution." (1)

For the work of Bernard, the eminent Catholic scientist Louis Pasteur had the highest admiration. He considered it the most illuminating, complete, and profound work ever written on the principles of experimentation. He predicted that its influence in the scientific field would be very great. (2) Does the Catholic biologist realize the implications of Bernard's Introduction? Could he extend this method to all fields? If not, why not? Does he realize the consequences of the generalisation of the hypothetico-deductive method? Does he know where his science fits in with the only philosophy recommended by the Church? Is not the logic of the scientist in open conflict with what is today called traditional logic?

The Catholic biologist has another problem in finding someone to answer these questions correctly. What answers would he receive from the teacher of Scholastic philosophy? Is the latter sufficiently acquainted with the method of experimental biology? Would he not for the most part cramp this method in an attempt to save his own which seems to be.


challenged? Would it occur to him that the answers might lie in those parts of traditional logic which have been completely ignored in our schools?

There is one important fact which we should recognize. The scientists themselves, although as a rule inadequately trained in philosophy, have thrown the greater light on the material to be used in determining the nature of their knowledge. Their contribution, however, remains purely instrumental. We shall take Eddington for an example. In his most important work, which is also the best we know on the nature of physical science, he starts off by stating the subject of his work:

"Between physics and philosophy there lies a debatable territory which I shall call scientific epistemology. Epistemology is that branch of philosophy which treats of the nature of knowledge. It will not be denied that a significant part of the whole field of knowledge is that which has come to us by the methods of physical science. This part takes the form of a detailed description of a world — the so-called physical universe. I give the name 'scientific epistemology' to the sub-branch of epistemology which deals with the nature of this part of our knowledge, and therefore indirectly with the nature and status of the physical universe to which it formally relates.

There are two matters of definition which it is desirable to make clear at the outset.

Some writers restrict the term 'knowledge' to things of which we are quite certain; others recognise knowledge of varying degrees of uncertainty. This is one of the common ambiguities of speech as to which no one is entitled to dictate, and an author can only state which usage he has himself chosen to follow. If 'to know' means 'to be certain of', the term is of little use to those who wish to be undogmatic. I therefore prefer the broader meaning; and my own usage will recognise uncertain knowledge."
Anything which would be knowledge if we were assured of its truth, is still accounted as knowledge (uncertain or false knowledge) if we are not assured.

It will not be necessary for us to formulate a general definition of knowledge. Our procedure will be to specify a particular collection of more or less widely accepted knowledge, and then to make an epistemological study of its nature." (1)

But the critical reader encounters in the very beginning of the work a difficulty which will only increase as he reads on. This is so not merely because Eddington does not state what he means by philosophy, but more precisely because he explicitly puts aside a question upon which will depend the very meaning and truth of his far reaching conclusions. He says: "It will not be necessary for us to formulate a general definition of knowledge." But if we are left in confusion about what knowledge is, how can we be clear about scientific knowledge? The avoidance of such fundamental issues leaves the reader in suspense, and increases the already too great confusion on scientific knowledge.

If all knowledge were of the type he analyses in this work, we would consider his general idealist philosophy well-grounded. But as it stands, his conclusion is a 'latius hos'. Physical science as he describes it is dialectical. Dialectics as we understand it is a type of knowledge whose generalisation would amount to idealism and ultimately to dialectical materialism; but the generalisation itself would depend in the end upon the definition of knowledge. While we agree with his analysis of

the nature of physical science, we are in complete disagreement about the principles he should invoke to warrant his generalisation. On the other hand, we can use his analysis to situate this type of knowledge in our general scheme. We can do as much with Claude Bernard’s analysis of the experimental method in biology.

Whence this breach between philosophy and what is now called the sciences? We shall make no attempt to give the underlying causes, but we may indicate some historical facts. The first is the absorption of philosophy of nature by metaphysics. We are not merely referring to Wolff and his scholastic followers, but to earlier scholastics such as Fonseca, Vasquez, Suarez, whose notion of chance, movement, and final cause, made philosophy of nature superfluous as a distinct science.

But why should the absorption of philosophy of nature by metaphysics entail a complete breach between philosophy as we understand it and the sciences? By reducing philosophy of nature to metaphysics, we neglect that movement of concentration proper to philosophy of nature, according to which the experimental sciences are but an extension of philosophy of nature. For as Saint Thomas shows in his commentary on Aristotle’s book De Sensu et Sensato:

"Quia habitus aliquius potentiae distinguuntur specie secundum differentiam ejus quod est per se objectum potentiae, necesse est quod habitus scientiarum, quibus intellectus perficitur, etiam distinguuntur secundum differentiam separationis a materia; et ideo Philosophus in sexto Metaphysicorum distinguuit genera scientiarum secundum diversum modum separationis a materia. Nam ea, quae sunt separata a materia secundum esse et rationem, pertinent ad metaphysicum;"
Hence the order of the various parts of philosophy of nature:

"Naturalis philosophia de naturalibus est; naturalia autem sunt quorum principium est natura; natura autem est principium motus et quies in ea in qua est; de his igitur quae habent in se principium motus, est scientia naturalis... Necssarium fuit quod praecipue in scientia naturali unus liber, in quo tractaretur de his quae consequuntur ens mobile in communi... Hi autem est liber Physicorum... cuius subjectus est ens mobile simpliciter... et ideo statim in principio libri de caelo, qui sequitur ad istum, incipitur a notications corporis. Sequuntur autem ad hunc libros alii libri scientiae naturalis, in quibus tractatur de speciebus mobilium; puta in libro de caelo de mobili secundum motus locales, qui est prima species motus; in libro autem de Generatione, de motu ad formas et primis mobilibus, sicuti essentia, quantum ad transmissiones eorum in communi; quantum vero ad specialibus eorum transmissiones, in libro tertio, de mobilibus mixtis animati, in Anima et consequentibus ad ipsum." (2)

Continuing this movement of concretion in the study of living


Rings, Aristotle proceeded as follows:

"Nam primo quidem consideravit de anima secundum se, quasi in quaedam abstractione. Secundo considerationem facit de his quae sunt animae secundam quamquam conceptionem, sive applicationem ad corpus, sed in generali. Tertio considerationem facit applicando omnia haece ad singulas species animalium et plantarum, determinando quid sit proprium unicumque speciei. Prima igitur consideratione continetur in libro de Anima. Tertia vero consideratione continetur in libris quos scribit de Animalibus et Plantis, media vero consideratione continetur in libris, quos scribit de quidusdam, quae pertinent communiter, vel ad omnia animalia, vel ad plura genera eorum, vel etiam ad omnia viventia."

(1)

The scholastics then have for centuries been living in a universe segregated from the conception of matter. But when we do have such a conception of philosophy of nature, it is because we have a false notion of abstraction both in metaphysics and in philosophy of nature. Hegelian idealism, as a substitute for metaphysics and philosophy of nature, is really the logical outcome of this process, where conception is derived from logical abstraction by means of an abstract dialectical movement. To consider philosophy of nature as a branch of metaphysics, and even in practice to teach metaphysics before philosophy of nature is in fact an idealistic procedure.

Now it is true that some authors have maintained the distinction between philosophy of nature and metaphysics, but of the former they have retained merely the first part made up of generalities on the principles of mobile being. What is treated in Book II of the Physics is generally overlooked, namely the analysis

(1) St. Thom., De Sensu et Sensato, loc. cit.
of the definition of nature; of the sciences subalternated to mathematics; of chance and fortune; and of final cause in nature and in demonstration.

As to Book III of Aristotle's Physics, these same authors make no thorough study of the difficult problem of motion. Instead of leading us on to a closer study of natural beings, our philosophy of nature chokes off if it has to speak of more than generalities.

Let us consider the term dialectics, which is sometimes used as a synonym for logic as such. In fact we have restricted Logic to the Prior and Posterior Analytics. The important section of the Perihermeneias on name and verb has been rather thoroughly neglected. The Dialectics proper of the Topics, and the treatise on Sophistry, have been reduced to a few negligible lines in most text-books. Is it any wonder, then, that we have failed to see that the hypothetico-deductive method applied in modern mathematics and in the experimental sciences is really dialectical; and that as dialectical this method is opposed to the demonstrative method of the Posterior Analytics.

There is a connection between the neglect of concretion in philosophy of nature and the neglect of dialectics in the teaching of logic. For it is as we approach natural beings in what they have from matter, that our discourse becomes of necessity more and more dialectical. Let us consider a concrete case, that of definition in philosophy of nature.
"Aliquando enim datur aliquus definitio, in qua nihil est ex parte corporis, sicut quod ira est appetitus vindictae; aliquando asegnatur aliquus definitio, in qua est aliquis ex parte corporis seu materiei, sicut quod ira est ascensione sanguinis circa cord.

Prima est dialectica, secundum vero est physica, cum ponatur ibi aliquis ex parte materiei; et ideo pertinent ad naturam. Hic enim, scilicet physicus assignat materiem, sicut dicit, quod est ascensione sanguinis circa cord. Alius vero, scilicet dialecticus, ponit speciem et rationem. Hoc enim, scil. appetitus vindictae, est ratio irae.

Quod autem definitio prima sit insufficiens, indicavit Aquin. Nam omnia forma, quae est in materia determinata, nisi in eis definitiones ponatur materia, illa definitio est insufficiens; et haec forma, scil. appetitus vindictae, est forma in materia determinata; unde est non ponatur in eis definitiones materia, constat quod ipsa definitio est insufficiens. Et Idea nacentis est ad definitiones, quod in definitiones ponatur hoc, scilicet forma, esse in anima huicmodi, scil. determinata." (1)

Consequently, definitions by form alone in philosophy of nature are merely dialectical. To be natural, a definition must also comprise the matter, as we have just seen. Now the matter to be defined, and whose definition together with the definition, is not matter considered in its indeterminacy, but rather elements, or organized matter; as is clear from the definition of the soul:

"Anima est actus primus corporis physici organici potentia vitas habentis." (De Anima, II, 1, 12a-30.)

It seems that it is the definition of this matter of natural beings which the evolution of the experimental sciences has proven to be dialectical. Hence in this field of extreme concretion, our definitions remain afloat both as to form and matter.

They do not go beyond a dialectical stage. The modern scientist would say the same thing by claiming that he is free. And so, what Saint Thomas taught on suppositions in astronomy seems to have a much wider application than could be suspected in his times.

Dialectics was very much alive when the whole of philosophy was still in a stage of becoming. The definite form which philosophy took on, by reason of certain fundamentals in metaphysics and in the prior part of philosophy of nature, caused dialectics to be entirely overlooked. John of Saint Thomas himself says:

"In secunda vero parte (artis logicae) agentis de his, quae pertinent ad materiam logicaeam seu ad posterioriasticas resolutions, maxime in demonstrationes, ad quas praecipue ordinatur logica." (1)

"quae enim pertinent ad partem topicum, quae agit de probabilitate, et quae pertinent ad libros enchorthum, qui agent de parte sophistica, constitutur in praecedenti, quae non agent de parte et perfecta resolutions judicii, et idem solus libri Priorum et Posteriorum vocantur analytici ab Aristoteles..." (2)

Yet in his time, the new interest in nature had compelled the scientists to use dialectics, although these scientists were themselves unaware of it. This has been shown by Duhem. But neither did the latter realize how this dialectic bent on saving the appearances of things (ad salvandas apparentias) was really an application of the fundamentals of Topics.


A deeper study of the nature of dialectics is the proper clue to a better understanding of the method of experimental science. The purpose of this thesis is to show that the kind of knowledge acquired in the field of modern biology, by the application of the experimental method, is really dialectical.
CHAPTER II

KANT AND PRIORISM

"But if it were necessary to choose a leader from among the older philosophers, there can be no doubt that our choice would be Kant. We do not accept the Kantian label; but, as a matter of acknowledgement, it is right to say that Kant anticipated to a remarkable extent the ideas to which we are now being impelled by the modern development of physics."

- Eddington. (1)

The methodology of experimental science does recognize a fundamental principle usually associated with Kant: a priori knowledge, which Eddington defines as "knowledge which we have of the physical universe prior to actual observation of it", (2) and which had already lead him to repeat Kant in saying: "We have found that where science has progressed the farthest, the mind has but regained from nature that which the mind has put into nature". (3)

Commenting on the Posterior Analytics of Aristotle (4)

St. Thomas shows the difference between Metaphysics, Logic, and Dialectics:

"Sciendum tamen est quod alia ratione dialectica est de communibus et logica et philosophia prima. Philosophia prima enim est de communibus, quia eius consideratio est circa ipsas res communes, scilicet circa omnia quae in rebus sunt habet negotiari ratio,

(3) Eddington, Space, Time and Gravitation, p. 201.
logica autem est de operationibus rationis;
logica etiam erit de his, quae communia sunt omni-
bus, idest de intentionibus rationis, quae ad omnes
res se habent. Non autem ita, quod logica sit de
ipsum rebus communibus, sicut de subjectis. Con-
siderat enim logica, sicut subjecta, syllogismum,
emunicationum, praedicatum, aut aliquid huiusmodi.
Para autem logicae, quae demonstrativa est, eti-
circa communis, intentiones versatur docendo;
tamen usus demonstrativae scientiae non est in
procedendo ex his communibus intentionibus ad
aliquid ostendendum de rebus, quae sunt subjecta
aliarum scientiarum. Sed hoc dialectica facit, quia
ex communibus intentionibus procedit arguendo
dialecticus ad ea quae sunt aliarum scientiarum,
sive sint propria sive communia, maxime tamen ad
communia. Sicut argumentatur quod odium est in
concupisciibili, in qua est amor, ex hoc quod con-
traria sunt circa idem. Est ergo dialectica de
communibus non solum quia perturbat intentiones,
communes rationis, quod est commun toti logicae,
sed etiam quia circa communia rerum argumentatur.
Quaecumque autem scientia argumentatur circa
communia rerum, operatur quod argumentatur circa
principia communium, quia veritas principiorum
communium est manifesta ex cognitione terminorum
communium, ut entis et non entis, totius et partis,
et similibus."

How to proceed from intentions of the mind to things is
obviously to anticipate reality, and to move from mental constructs
to things. And in this respect Dialectics differs from both Meta-
physics and Demonstrative Logic. In the first Introduction to the
Critique of Pure Reason, Kant seems to identify Metaphysics with
Dialectics. If such were the case, and since the conclusions of
Metaphysics cannot be verified in an experimental manner, Meta-
physics would be merely bad dialectics. Overlooking the fact that
the second intentions of Logic are based on first intentions, with-
out which the former are nonsense, and having given the second
intentions the status of first, he again confuses Logic with
Metaphysics.
Kant declares that if Aristotle's Logic has remained unchanged, it is because the mind is therein unconditioned and independent of objects, proceeding entirely a priori. The sciences will share the certainty and definiteness of Logic in the measure that the mind can extend this a priori procedure to their objects. Kant says:

"Mathematics and physics are two types of theoretical knowledge which must determine a priori their object; the first in an entirely pure fashion, the second at least in part and to the extent that the other sources of knowledge, than the reason, permit it to do so.

Mathematics from the most remote times which the human reason can recall, has followed in the company of the Greeks, the sure way of science. But one should not think that it was just as easy for Mathematics as for Logic in which the reason has to attend only to itself, to find, or better, to open for itself this royal road. I believe, rather, that mathematics was feeling its way for a long time, especially with the Egyptians, and that this change was the effect of a revolution due to one man alone, who conceived the happy idea of a trial after which there was no longer any reason for being deceived as to the sure road to follow; and so the certain way of science was opened and traced for all times and for infinite distances. The history of this intellectual revolution, a great deal more important than the discovery of the route around Cape of Good Hope, - the history of this man who had the good fortune to discover the route, have not come down to us in detail. Nevertheless, the tradition which Diogenes Laertius, naming at the same time the supposed discoverer of the most simple elements of geometric demonstrations which, following the common opinion, have no need of proof, - this tradition proves that the remembrance of the change effected in the first step of the way newly discovered, was due to appear extremely important to the mathematicians, and was saved for that reason from oblivion. The first one who demonstrated the isosceles triangle, (who was Thales, or some other person,) struck a great light; for he found that it was not necessary to attach oneself to what he saw in a figure, or even to any simple concept which he had of the figure, but that
he should engender or construct this figure by means of what he would think on this subject and would represent a priori in his concepts, and that in order to know with certainty a thing a priori, he must attribute to that thing only what would derive necessarily from what he had put therein, in consequence of his concept.

"Physics has found more slowly the broad road to science; for it is scarcely more than a century and a half ago that the ingenious attempt of Bacon, Baron of Verulam, in part provoked, (because there was already the trace of it,) and in part stimulated again this discovery, which cannot be explained except by a sudden revolution in the ways of thinking. I wish here to consider Physics only in so far as it is founded on empirical principles.

When Galileo rolled balls down an inclined plane with an acceleration determined and chosen by himself, when Torricelli attributed to the air a weight which he computed as equal to the weight of a known column of water, or when later Stahl transformed metals into lime, and the latter in turn into a metal, by separating and adding certain elements, then there was a new light for all physicists. They understood that reason discovers only what it produces itself according to its own designs; it must take the lead with principles which determine its judgments according to constant laws, and force nature to respond to its questions, instead of leaving itself be conducted by nature as on a string; for otherwise our observations made at random and without any plan traced beforehand would never lead to a necessary law, which the reason nevertheless looks for and demands. The reason must present itself before nature, holding in one hand its principles which alone are able to give the concordant phenomena the authority of law, and in the other hand it must hold the experiment such as it has planned according to the same principles. Reason demands to be informed not as a school boy who binds himself to speak only what pleases the teacher, but as a judge on his bench, who constrains the witnesses to answer the questions put to them. Physics, therefore, is indebted to the happy revolution which has occurred to its method by this simple idea, that it must hunt for (and not imagine) in nature, conformably to the ideas which the reason itself brings to it, what reason ought to learn of nature, of which it can never learn anything simply by itself. It is thus that Physics enters for
the first time upon the sure road of science, after
groping along for so many centuries." (1)

Clearly this procedure falls under the heading of what
Aristotle calls dialectics. But in the latter conception,
knowledge acquired through the application of dialectics remains
either vague when certain, as in the example given by St. Thomas;
or merely probable when attempting precision. It remains a "pis
aller". And while we might call it a powerful substitute for
ignorance, it is by its very nature provisional. It is indeed
knowledge by anticipation; vague knowledge is an anticipation of
precise knowledge, and opinion is an anticipation of certainty;
and verisimilitude of truth. But the latter remains the measure
and the first principle. A hypothesis is good, not because the
mind posits it, but rather because it may be tested by experience,
or because experience confirms it. The a priori of Aristotelian
dialectics is therefore the very contrary of Kant's.

It should be noted however that since Kant did adhere to
Newtonian physics as to something definite whose principles would
remain unchanged, he failed to recognize the ever provisional
character of the kind of knowledge that he fostered. The history
of the experimental sciences shows them to be dialectical in the
Aristotelian sense, rather than in the Kantian.

Nevertheless, the substance of Kant's specific position
has been maintained by contemporary authors both idealists and

(1) E. Kant, Critique of Pure Reason, preface to the second
edition.
materialists, who see in the provisional character of scientific principles a deeper emancipation of man. Vagueness and uncertainty are declared signs of the freedom of our mind, and of the highest form of wisdom. I shall give a few examples of this attitude.

The first I take from F.C.S. Schiller:

"The mental attitude which entertains hypotheses on the other hand, and can take 'fact' as hypothetical and possibly unreal, means an intellectual revolt against mere givens. It has become critical in appearances, and has partially freed itself from the oppression of brute fact. It meets reality with an active response, and does not merely submit to whatever comes along. It feels free to anticipate reality by its guesses, to question it, to experiment, to distrust and doubt appearances, to rearrange the world at least in thought, to play with it, and with itself. For hypothesis is a sort of game with reality, akin to fancy, make-believe, fiction, and poetry. In the hypothetical attitude 'facts' have ceased to be accepted on face value, to be just fact, and become capable of being symbols, whose suggestions are more important than their bare existence. Whatever they may be in reality, they are no longer fixed in the mind, but afloat; not being fixed ideas, they can be moved about and played with. But, like games in general, this play has a serious function. By loosening the connexion between what the real is (or seems) and what we think about it, it enables us to think it other, and better, than it is; and so, guided by our hypotheses and 'ideals' (which are postulates), we can set to work to make it other, and better, than it was." (1)

The second example is taken from Abel Rey who writes on the evolution of thought. Having described the various stages from primitive times, he finally comes to the sixth stage in which

the mind is absolutely free even of first principles.

"The present era announces a new liberation, as profound perhaps as the two previous ones. It aims at these intransigent, these mathematico-physical absolutes. There is no longer a tool that serves the intellect, except the intellect itself in its inventive omnipotence. The universalisation of the hypothetico-deductive method, in its broadest signification, is the logical illustration of it. An order forever temporary and relative. It dilutes accordingly to correspond to the relations discovered by intuitions ever more rich and profound. It renew itself by changing, whenever necessary, even its very foundations. Logic, a collection of rational formulas, appears no longer as an architectural conception constructed once and for all into an unchangeable unity resting on an eternal foundation. Thought must constantly be ready to build on new foundations, or to modify the arrangement of the edifice, and consequently, to complete, to adjust, and to renew its tools. (1)

Tobias Dantzig maintains a similar position:

"The abandonment of the naive realism of the classical period of science entails the abandonment of the absolute; and when I say the abandonment of the absolute, I mean not only space, time, and matter, but absolute certainty and — I say as well say it — absolute truth too. What remains is but a universe of discourse, a playground for the human mind.

That the physical sciences have at all survived this drastic revision is entirely due to the flexible mental apparatus with which the mathematician has supplied them; for mathematics is sublimely indifferent whether the forms in which it deals represent significant statements or are but empty shells which can contain everything or nothing. In some such shell modern physics has found a refuge.

The physical sciences have avoided a catastrophe by a series of sacrifices. They have recognized that the rational permanent frame in which they have sought to confine the universe is an elusive squirming web; they waived the right to deal in significant statements; they have pledged to confine themselves to formal relationships between entities which derive their existence from mathematical equations, and mathematical instruments, graduated and calibrated to conform with these equations. The scientist has resigned his mission to unravel the chaotic universe of man's sense-impressions; he is now engaged in an intellectual game which is being played with hypothetical scales, clocks, and quanta in the shadowland of number, form, and chance. *(1)*

Finally we have the position of two dialectical materialists, both noted biologists; namely, Marcel Prenant and J.B.S. Haldane. Basing biology and the experimental sciences on dialectical materialism, they hold, although for other reasons than claimed by Kant, that the sciences have no absolute principles, that nothing is certain. Prenant says:

"In the fact of universal change resides the dialectical character of marxist materialism. Dialectical materialism, says Lenin, insists on the approximative, relative character of every scientific proposition concerning the structure of matter and its properties; it insists on the absence in nature of any absolute lines of demarcation, on the passage of moving matter from one state to another which appears to us as sometimes incompatible with the first." In 1877 Engels cited from universal dialectics some very diverse examples: the evolution of the nebula of Laplace, that of the surface of the globe, that of living species, the liquefaction of gas, the transformation of energy, and many others. We should add today, among the most characteristic, the evolution of radioactive elements, the transmutation of simple chemical bodies, and perhaps even, to the limits of modern physics, the reciprocal change of energy into matter, and of matter into energy. All these categorical distinctions

vanish one after the other, the general formula of 'rational materialism' simplifies itself more and more, as Engels says, 'the science of nature has actually come to the point where it can no longer escape dialectical synthesis: there exists one matter in movement.' (1)

J. B. S. Haldane differs very little from Prenant, and therefore need not be quoted at any length.

"So far we may say that Marxism anticipates pragmatism, although it differs from pragmatism in almost all other respects, notably in its consistent emphasis on the changing world, and above all in its belief that absolute truth, if never reached, can be continually approached." (2)

Now it is not difficult to see the point which all these authors exploit. For we should admit that if we could not recur to hypothesis and anticipation, our mind would remain bound and enslaved. The mind's power to question is a condition of its freedom from ignorance and uncertainty. (3) And dialectic is by

(1) A. Prenant, Biologie et Marxisme, Paris 1937, p. 77
(3) Note the explanation of this condition by Aristotle: "For those who wish to get clear of difficulties it is advantageous to discuss the difficulties well; for the subsequent free play of thought implies the solution of the previous difficulties, and it is not possible to untie a knot of which one does not know. But the difficulty of our thinking points to a 'knot' in the object; for in so far as our thought is in difficulties, it is in like case with those who are bound; for in either case it is impossible to go forward. Hence one should have surveyed all the difficulties beforehand, both for the purpose we have stated, and because people who inquire without first stating the difficulties are like those who do not know where they have to go; besides, a man does not otherwise know even whether he has at any given time found what he is looking for or not; for the end is not clear, to such a man, while to him who has first discussed the difficulties it is clear. Further, he who has heard all the contending arguments, as if they were the parties to a case, must be in a better position for judging." Aristotle, Metaphysics, Bk. III, c.1, 995a 25–995b 5.
its very nature interrogative, not only of conclusions, but of its very principles. (1) Tentative dialectic is essentially critical; it is a critique based not on scientific knowledge, nor on a clear formulation of principles, but rather on what Descartes called "le bon sens", on that of which everyone is deemed to have enough. (2)

(1) "Scilicet tamen est quod interrogatio aliter est in scientiis demonstrativis et aliter est in dialectica. In dialectica enim non solum interrogatur de conclusione, sed etiam de praemissis; de quibus demonstrator non interrogat, sed ea sumit quasi per se nota, vel per talia principia probata; sed interrogat tantum de conclusione. Sed cum eam demonstraverit, utitur ea, ut propositiones, ad aliam conclusionem demonstrandas." St. Thom. In Post. Anal. I, loc. 21, n.3; cf. also In Post. Anal. I, loc. 5, n.4.

(2) "Dialectic is at the same time a mode of examination as well. For neither is the art of examination an accomplishment of the same kind as geometry, but one which a man may possess, even though he has no knowledge. For it is possible even for one without knowledge to hold an examination of one who is with knowledge, if also the latter grants him points taken not from things that he knows or from the special principles of the subject under discussion, but from all that range of consequences attaching to the subject which a man may indeed know without knowing the theory of the subject, but which if he does not know, he is bound to be ignorant of the theory. So then clearly the art of examining does not consist in knowledge of any definite subject. For this reason, too, it deals with everything; for every 'theory' of anything employs also certain common principles. Hence everybody, including even amateurs, makes use in a way of dialectic and the practice of examining; for all undertake to some extent a rough trial of those who profess to know things. What serves them here is the general principles; for they know these of themselves just as well as the scientist, even if in what they say they seem to the latter to go wildly astray from them. All, then are engaged in refutation; for they take a hand as amateurs in the same task with which dialectic is concerned professionally; and he is a dialectician who examines by the help of a theory of reasoning." Aristotle, De Sophisticis Elenchis, chap. XI, 172a 20-35.
Relative to ignorance, hypothesis and anticipation are a kind of emancipation, contributing thereby to free our mind. But the emancipation through hypothesis and tentative anticipation has no finality in itself. In them the mind remains enchained, unless we claim that at no time do we fall back into this state by refusing to move on toward truth. The end we have in view is something better than hypothesis, though we may never reach it.

However, the authors we have just quoted have obviously rejected any definiteness. The dialectical movement has fallen back upon itself, and the freedom to construct has become a joy based on uncertainty as on its first principle.

There is another point which these authors have exploited to the full. Experimental knowledge, since as such it implies mere givenness, does remain an imperfect kind of knowledge. The very plurality of first principles and of media implies imperfection. This applies both to first principles in the Aristotelian sense, and to what we call today the facts of experience, such as that of the elephant's trunk. It is necessary for us to free ourselves from mere givenness in so far as this is possible. To know why the elephant has a trunk would be better than to know only that he has one. And any tendency toward the why through hypothesis is a healthy movement toward upsetting mere givenness. But the why of the trunk should remain the limit of the hypothesis. But the revolt against all mere givenness is a revolt against the finite mind.
The literature which has grown around the dialectical nature of experimental science is such a fierce and ludicrous exploitation in favour of anti-Aristotelian and anti-Christian dogmas, that our recognition of the simple truths about the experimental sciences, as they are practised today, leaves us uneasy. This is because of the bad company in which we seem to move. Is not the concession of a prioriism a concession to Kant, to dialectical idealism or materialism, to relativism and pragmatism? The situation is the more embarrassing that even among our own Scholastic philosophers Aristotelian dialectic is generally deemed a thing of the past.
CHAPTER III

J. VON UEXKULL AND PHENOMINALISM

1.

An attempt to bring out the implications of Kant's critique for biology was made by an enthusiastic follower, J. Von Uexkull, in THEORETICAL BIOLOGY. (1) In the introduction he says:

"No attempt to discover the reality behind the world of appearance, i.e. by neglecting the subject, has ever come to anything, because the subject plays the decisive role in constructing the world of appearance, and on the far side of that world there is no world at all.

All reality is subjective appearance. This must constitute the great, fundamental admission even of biology. It is utterly vain to go seeking through the world for causes that are independent of the subject; we always come up against objects, which owe their construction to the subject.

When we admit that objects are appearances that owe their construction to a subject, we tread on firm and ancient ground, especially prepared by Kant to bear the edifice of the whole natural science. Kant set the subject, man, over against objects, and discovered the fundamental principles according to which objects are built up by our mind." (2)

While this position is unacceptable in its generality, it actually describes a type of knowledge well recognized long before Kant.

What do we mean by appearance? Appearance is a relative

(1) J. von Uexkull, Theoretical Biology, n.y., 1926.
(2) J. von Uexkull, op. cit., Introduction, p.xv.
notion embracing three terms: that which is said to be the appearance: appearance is predicated of something; and that of which it is said to be the appearance. Thus (a) clouds are said to be (b) an appearance of (c) rain. Hence, that which is called an appearance is considered a sign of something else. More specifically it is an instrumental sign, a sign which makes itself known before it makes known that of which it is a sign.

Now an instrumental sign may have either a contingent or a necessary connexion with that which it signifies. Either connexion may be such on the part of things: our knowledge would be true in both cases if we know them as such. Again, the connexion may seem necessary or contingent: in this case the sign is only an appearance of the signified. In so far as it is only an appearance of the signified. In so far as it is an appearance of truth, an appearance is verisimilitude, or probability.

The following text is from Saint Albert the Great:

"Probabilia autem (ex quibus fit syllogismus dialecticus) sunt verisimilia. Dupliciter autem verisimilia: aut enim in se sunt verisimilia, eo quod ipsa habitudo praedicati ad subjectum verisimili est, eo quod nec praedicatum est in subjecto per se, nec subjectum in praedicato per se, nec utrumque in utroque, nec praedicatum necessarium et essentialium inhaerentiam habet cum subjecto, sed verisimile est in signis non in causis necessariis acceptum. Aut quia necessarium habet inhaerentiam, sed non accipitur nisi per signum: et hoc est probabile secundum modum acceptio, quamvis in se sit necessarium: sicut solem esse majorem terra
(eo quod ubique unius quantitatis apparat)
probabiliter acceptum est. Solem autem esse majoram
terram per quantitatem diametri acceptum est neces-
sariorum et non probabile, secundum quod probabile
et necessarium opponitur. Probabile autem sic
dictum verisimile est, quod per suiipsius veritatis
figuras videtur omnibus aut pluribus aut sapientibus,
et his sapientibus videtur omnibus aut pluribus aut
maxime notis et probabilibus: ita quod sapientibus
et his vel omnibus sapientibus vel pluribus vel
maxime notis vel probabilibus, totum pro uno membro
ponatur.

Signa vero verisimilitudinis, aut occurrunt statim
in superficie et in exterioribus rei quae accipit
-sensitiva potentia comparans sensata ad invicem;
et si tali sunt signa, probabile est quod videtur
omnibus, sicut nives esse albas per hoc quod nix est
parvae partae perspicuoi in parva conuenti, in culis
partibus unique lux diffunditur; hoc enim signum
sensui est medium. Si autem signa indicium facientia
de verisimilitudinis sunt non in superficie, sed
aliquaet profusa, non ad necessaria, sed nec
in superficie extrinsecus manentia: tunc est id
quod videtur pluribus: quia sensui aliquid miscent
rationes, sicut quod stella in candida minoris urae
sit polus, eo quod non apprehenditur eius singularis
actus: hoc enim rationis judicium sensui est perpetuum.
Si autem signum verisimilitudinis profundatur in
essentialium et convertibilium causas quae sunt
convertibilita sicut causae: tunc est quod videtur
sapientibus, sicut est, quod luna movetur in
epicico: quia profundius et altius transit per
umbrae mortae: hoc enim non est causa sed signum."(1)

Obviously, if biology as Uexkull understands it is merely
about signs and appearances, then it is dialectical.

By subject Uexkull means the knower. Therefore to say that
we always come up against objects, which owe their construction
to be knower, is to say that what we consider to be together on
the part of things, is really together because the knower has

put them so. This putting together however is relative to something else, in which what we tentatively put together is really so or not.

Let us consider the intuitive or experimental proposition "snow is white". That snow is white is known through sense experience. This is true, not because we see a necessary connexion between "snow" and "white", but merely because experience presents us with such an object. We did not put the snow and the whiteness together; for that is what is given in sense experience. Now we may go on to study the kind of connexion between these two terms. The senses reveal only a contingent connexion; although we find the two together, this does not mean that they are necessarily together. (1) But how does white belong to snow? Per se, or per accidens? As genus, property, or accident? This is the connexion to be proved. The proof will be either demonstrative or dialectical. Whatever

(1) "Unde constat, quod non est idea propositio per se nota quod intuitiva alio per experientiam sensuum nota, quia quod sensu cognoscitur, non cognoscitur ut propositio, sed ut simplex objectum apprehensum, neque ex sola explicatione terminorum innotescit, sed quia experientia externa attingitur. Et sic nives esse albam, licet in sensu sit per experientiam notum, in intellectu tamen non est propositio nota ex terminis per se connexis, sed potius materia contingentia." John of St. Thomas, Logic, P.11, q. 24, art. 4, (Reiser edition, p.767).
dialectic puts together will be formally put together by the knower, and this tentatively referred to the thing in itself. And this knowledge is phenomenal, in so far as, relative to what it is now referred to, the very starting point was merely appearance or phenomenon.

It should be noted therefore that the experiential proposition "snow is white" may be understood in two ways: either as signifying an experiential truth; or as the sign of some connexion to be further determined. In this last respect it is a phenomenon; it is the appearance of some kind of connexion. The connexion we shall hence forth establish, if it does not reach the true connexion, (that is the one which is on the part of the thing in itself,) will itself retain the state of mere appearance: the knowledge will remain phenomenal and dialectical.

The argument might be pressed by stating that snow itself is phenomenal. Let us consider a definition: "snow is water vapour in the air crystalized into geometrical forms". Now what is water? What is air? What is it to become a crystal? Further definitions will lead us back to similar questions, and other experiences. But this does not make the water we drink phenomenal, and our senses do reveal its difference from beer. Nevertheless any definition we shall give will lead us into dialectical discourse. Relative to this discourse, the snow or the water are phenomenal. What is signified by the name snow
may be known well enough to be distinguished from bricks, and to be shevelled, but it is not necessarily known well enough to serve as a proper term in a demonstrative discourse. (1)

2.

In the preface to the same book Uexkull states:

"Natural science falls into two parts, doctrine and research. The doctrine consists of dogmatic assertions, which contain a definite statement concerning nature. The form these assertions take often suggests that they are based on the authority of nature herself.

This is a mistake, for nature imparts no doctrines; she merely exhibits changes in her phenomena. We may so employ these changes that they appear as answers to our questions. If we are to get a right understanding of the position of science vis-à-vis of nature, we must transform each of the statements into a question, and account to ourselves for the changes in natural phenomena which men of science have used as evidence for their answer." (2)

(1) "Dicendum quod nomen dicitur ab aliquo imponi dupliciter: aut ex parte imponentis nomen, aut ex parte rei cui imponitur. Ex parte autem rei nomen dicitur ab illo imponi per quod completur ratio rei quam nomen significat: et haec est differentia specifica rei; et hoc est quod principaliter significatur per nomen. Sed quia differentiae essentiales sunt nobis ignotae, quandoque utimur accidentibus vel effectibus loco earum, ut VIII Metaph (VII, comm. 10) dicitur; et secundum hoc nominamus rem; et sic illud quod loco differentiales essentiales assumit, est a quo imponitur nomen ex parte imponentis, sicut lapis imponitur ex effectu, qui est laedere pedem; et hoc non oportet principaliter significatum per nomen, sed illud loco cuius hoc imponitur. Similiter dico, quod nomen verbi a verberatione vel a boatu dicitur ex parte imponentis, non ex parte rei." St. Thomas, De Veritate, q. 4, a.1, ad 3.

(2) von Uexkull, op. cit., preface, p ix.
The author presumably means that the propositions of biology are dialectical, and never adequate expressions of nature. But a dialectical proposition is interrogative, not absolutely, but of the probable. (1)

"The difference between a problem and a proposition is a difference in the turn of the phrase. For if it be put in this way, 'An animal that walks on two feet' is the definition of man, is it not?' or 'Animal' is the genus of man, Is it not?' the result is a proposition: but if thus, 'Is 'An animal that walks on two feet' a definition of man or no?' (or 'Is 'animal' has genus or no?') the result is a problem. Similarly too in other cases. Naturally, then, problems and propositions are equal in number: for out of every proposition you will make a problem if you change the turn of the phrase."

(Aristotle, Topics, I, c.4, 101b 35-40.)

A dialectical proposition however may take on the appearance of a definite assertion, in so far as it is assumed or posited. But this assumption or position has not made it true. (2)

(1) Aristotle, Topiea, I, c.10; Albert the Great, op. cit., Lib.I, Trac.III, c.1, where he says: "sed dialectica proposition est interrogatio consensus in probabili, nec consensus requiritur si probari non debeatur; manifeste autem falsum probari non potest, et manifeste verum non indiget probari, sed ad alterius aliquia assamitur probationem.

In diffiniendo ergo propositionem dialecticam secundum potissimum status dicius, quod propositione dialectica est interrogatio probabili, ita quod probabilis sit genitivi causae, hoc est, interrogatio de probabili, quod est materia propositionis dialecticae. In probabili enim (quia ponitur in judicio cuius cui proposition, utrum sic videatur vel non) oportet quaeque respondere judicium et consensus, ante habere procedere possit opposens. Sic ergo dialectica propositione interrogatio est probabilis. Et hac rationem aetiam Boetius in diffinirone syllogismo dicit, quod est oratio in qua quiusdam positio et concessio, responsiones ad propositiones syllogismi dicit, quod est oratio in qua quibusdam positio et concessio, responsiones ad propositiones syllogismi dialectici. Guis causa est, quod probable de se non habet sufficientem causas consequentia vel inferentiae, et causas inferentiae sufficientes acceptat a concessione respondentia. Haec igitur est tota diffinitione propositionis dialecticae."

(2) Aris., Topics, I, c.14.
We assume them for the purpose of reasoning from them and testing them.

In so far as biology starts from phenomena of the kind described above, we must bear in mind that while our statements may appear highly plausible and confirmed by all the material at our disposal for the present time, they nevertheless remain open to question and are problematic.

And it is true that dialectical propositions and problems somehow contain the answer, as in the examples given from Aristotle. For the questions, what is man? or What is a cow? are not dialectical, though the answer may remain dialectical.

Uexkull rightly continues:

"Investigation cannot proceed otherwise than by making a supposition (hypothesis) in its question, a supposition in which the answer (thesis) is already implicit. The ultimate recognition of the answer and the setting up of a doctrine follow as soon as the investigator has discovered in Nature what he considers a sufficient number of phenomena that he can interpret as positive or negative on the lines of this hypothesis.

The sole authority for a doctrine is not Nature, but the investigator, who has himself answered his own question. (1)

While the thesis or position adds something to the dialectical proposition or problem, (2) it does take the nega-

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(1) von Uexkull, op. cit., preface p.ix.

(2) "Positio autem adjuncta est problemati, et est quasi accidentale principium syllogismi dialectici. Problema enim est principium quod est circa conclusiones dialecticas, de quo neutro modo opinamus. Positio autem est de quo opinamus contraria..." (Albert the Great, Top. I, Tract. III, c.3).
tive or the affirmative of what is contained in the question, though not as a definite assertion. The position or thesis itself remains open to question.

It does not follow however that the investigator is the sole authority for a doctrine. For while the dialectical proposition and doctrine, considered as such, are constructs of the knower without an adequate counterpart in nature, nature remains the measure to which the biologist attempts to conform himself. For instance, if experience favours one part of the proposition without making the proposition true, nature has something to do with the answer. That is to be answered in the affirmative or in the negative depends upon the question of the investigator; but that the answer is disjunctively affirmative or negative depends not on the question but on nature.

Uexkull goes on to say:

"A man may have assimilated the conclusions of natural science in the form of doctrine, and may know how to employ them in speculation, according to the rules of logic; but he still knows nothing whatsoever concerning Nature - or at any rate, infinitely less than does any peasant or gardener who is in daily intercourse with her." (1)

This statement brings out what we have already said about experience and phenomena. The peasant does not cultivate phenomenal carrots. But in a way the biologist does. We eat the former,

(1) von Uexkull, op. cit. preface p ix.
not the latter. What the author says about doctrine is true of dialectical doctrine. The rules of logic would then mean those of formal logic and of that part of material logic which we call dialectic, or dialectica docens. (1) But it seems exaggerated to say that he then still knows nothing concerning nature. We would say that he still has no science of nature.

"Peasants and gardeners, however, are not students of nature, unless they happen to have acquired the art of interrogation. This art forms the gateway to all knowledge in natural science. In biology it is associated with quite especial difficulties, and so it should occupy the central position in the whole doctrine." (2)

(1) "Licet autem dicatur, quod Philosophia est scientia, non autem dialectica et sophistica, non tamen per hoc removetur quin dialectica et sophistica sint scientiae. Dialectica enim potest considerari secundum quod est docens, et secundum quod est utens. Secundum quidem quod est docens, habet considerationes de ipsis intentionibus, instituenas modum quo per eam procedi possit ad conclusiones in singulis scientiis probabilitar ostendendas; et hoc demonstrativa facit, et secundum hoc est scientia. Utens vero est secundum quod modo adjungit utitur ad conclusiones aliquid probabiliter in singulis scientiis; et sic recedit a modo scientiae. Et simul iter dicendum est de sophistica; quia prout est docens tradit per necessarias et demonstrativas rationes modum arguendi apparetur. Secundum vero quod est utens deficit a processus verae argumentationis.

(2) "Et in partes logicae quae dicitur demonstrativa, solus doctrina pertinet ad logicam, usus vero ad philosophiam et ad alias particulares scientiae quae sunt de rebus naturalibus. Et hoc idem, quia usus demonstrativae consistit in utendo principiis rerum, de quibus sit demonstratio, quae ad scientias reales pertinet, non utendo intentionibus logicis. Et sic apparent, quod quaedam partes logicae habent ipsum scientiam et doctrinam et usum, sicut dialectica tentativa et sophistica; quaedam autem doctrinam et non usum, sicut demonstrativa." (St. Thomas, In Meta. A. 4, Lib. IV, lect. 4, n. 576-577.)

(2) von Uexkull, op. cit., preface p. x.
The art of interrogation is again dialectic. (1) But the art the author here refers to is more specifically the art of interrogating nature, and nature is then considered in a way as the one who knows, her answer being what we can collect from experience. Not that all questioning of nature is as such dialectical, but it certainly is when the problems are of the kind mentioned above.

"In the present book I have endeavoured to frame the theoretical considerations concerning biology, in such a way that there can no longer be any doubt that, in their very nature, biological doctrines always remain unsolved problems." (2)

We must agree with the author that biological doctrines starting from phenomena, already mixtures of reason and sense, and therefore impure in a fashion, can never go beyond the problematic state. All their conclusions are dialectical propositions and therefore problems.

"In nature everything is certain; in science everything is problematic. Science can fulfil its purpose only if it be built up like a scaffolding against the wall of a house. Its purpose is to ensure the workman a firm support everywhere, so that he may get to any point without losing a general survey of the whole. Accordingly, it is of the first importance that the structure of the scaffolding be built in such a way as to afford this comprehensive view; and it must never be forgotten that the scaffolding does not itself pertain to nature, but is always something extraneous." (3)

What Uexkull here means by nature and certainty is not clear. We hold a degree of objective uncertainty in nature,

(1) Aristotle, De Sophisticis Elenchis, chap. 11, 172a 10,172b 1.
(2) von Uexkull, op. cit., preface, p.x.
(3) von Uexkull, op. et loco cit.
as we shall explain later. Nature itself has its problems.

What the author calls science is really dialectics of nature.

And this does remain problematical. And of these two the opposition holds. Dialectics of nature is like a scaffolding against a house. And although the scaffolding is not the house, the former has some relation to the house. The shape of the scaffolding gives some knowledge of the shape of the house. The purpose of the scaffolding is not the mere extrinsic imitation of the house. It is built to reach the house the better.

The comparison might lead to confusion, for by means of a scaffolding we actually reach the house. But dialectics does not actually reach nature.

To say that dialectics is extraneous to nature is to speak formally for:

"Dialecticus procedit ad ea consideranda ex intentionibus rationis, quae sunt extranea a natura rerum. Et ideo dicitur quod dialectica est tentativa, quia tentura proprium est ex principiis extraneis procedere." (1)

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(1) "Differunt autem ab invicem. Philosophus quidem a dialectico secundum potestates. Nam majoris virtutis est consideratio philosophi quam consideratio dialectici. Philosophus enim de praedictis communibus procedit demonstrativa. Et ideo eius est habere scientiam de praedictis, et est cognoscitivus eorum per certitudinem. Nam certa cognitio sive scientia est effectus demonstrationis. Dialecticus autem circa omnia praedicta procedit ex probabilitibus; unde non facit scientiam, sed quandam opinionem. Et hoc ideo est, quia ens est duplex: ens aet. rationis et ens naturae. Ens autem rationis dicitur propriis de illius intentionibus, quae ratio adinvent in rebus consideratis; sicut intetio generis, specie et similibus, quae guidem non inveniuntur in rerum natura, sed considerationem rationis.
As examples of such intentions of reason St. Thomas had just mentioned the intention of genus and the intention of species, as distinct from natural genus and species. The logical notions are in themselves clear enough and definite. We need these intentions for classification in biology, not only for the classification of the various kinds of plants and animals, but no less for their parts. But the classification has nothing definite. While it has some foundation in nature known through experience, we would not dare of substituting our classification by means of logical intentions for that of nature itself. The very use of these intentions is governed by principles which are ever provisional. Weckull himself shows that genus and species as used by biology are what we call logical intentions. (1) Singer's pages on the same subject although confused, bring out the same point. (2)

consequentur. Et huiusmodi, scilicet iones rationis, est proprie subjectum logicae. Huimodi autem intentiones intelligibles, entibus naturae sequiparatur, ea quod omnia entia naturae sub considerationes rationis cadunt. Et ideo subjectum logicae ad omnia se extendit, de quibus ens naturae praedicatur. Vnde concludit, quod subjectum logicae sequiparatur subjecto philosophiae, quod est ens naturae. Philosophus igitur ex principiis ipsius procedit ad probandum ea quae sunt consideranda circa huiusmodi communia accidentia entis. Dialectics autem procedit ad ea consideranda ex intentionibus rationis, quae sunt extranea a natura rerum. Et ideo dicitur, quod dialectica est tentativa, quia tentare prorsum est ex principiis extraneis procedere. (3) (St. Thomas, In Meta. Aris., Lib. IV, lec. 4, n. 574.)

Uexkull goes on to say that "from time to time it will always be necessary to renew the scaffolding". (1) The experimental sciences are forever in search of first principles, of primary elements. This research makes them definitely dialectical. (2) What are the primary elements of the physical world? What are the primary elements of living things? The experimental science of a given period has a provisional answer to these questions. But it does remain provisional. The discovery of more fundamental elements change the whole system.

We shall not venture to say what the present biologists consider to be the lowest forms of living beings, or the first elements of living beings as living beings. One thing is certain however. None would venture to say that such or such is definitely the lowest, or is definitely first. But so far as such and such are considered first elements, we have the attribute of first principle added to what we actually do not know to be first on the part of things. The result of this combination is in this respect a construct of the mind.

The rest of Uexkull's THEORETICAL BIOLOGY is an attempt to renew the scaffolding of biology which should take account of "conformity with plan" as the basis of life. It is beyond the scope of this dissertation to analyse this concept.

(1) Aristotle, Topics, I, c.2, 101a 35.
CHAPTER IV

MARXISM AND PRAGMATISM

The philosophies of Marxism and pragmatism have in common an instrumentalist conception of knowledge. In their opinion, human knowledge, but for its greater complexity, is fundamentally the same as that which we attribute to brute animals: "leo non delectatur in aspectu cervi vel caprae... sed delectatur in spe habendi cibum". (1) Their knowledge is essentially ordered to something else.

We should admit, however, that if all our knowledge were of a dialectical nature, that since dialectic, whether doxens or utens, remains merely useful and instrumental, (2) the position common to marxists and pragmatists would follow logically, unless vagueness and uncertainty were ends in themselves.

But what is that to which this kind of knowledge can nevertheless be ordered successfully? Although dialectical knowledge of nature is either vague or merely probable, the distance between knowledge and nature can be breached by action. Prenant quotes the second thesis of Marx on Feuerbach:

"The question of knowing whether human thought can attain objective truth is not a theoretical question, but a practical one. It is in praxis

(1) Aristotle, Ethica, III, c. xii, 1116a 15; St. Thomas, loc. 19.
(2) Aristotle, Topics, I, c.2."
that man must demonstrate the truth, that is to say, the reality, the power, the precision of his thought.\(^{(1)}\)

There is then no precision in thought itself; it will be called precise and true in fashion, only if its leads to practical results. We should recognize that however vague physical and biological doctrine may be, they do permit us to make highly efficient machines and to cure diseases. Even a bad shot can hit a target, provided the target is big enough. And if the target is small, buckshot will do the trick. For the purpose of action such seems to be the relation between dialectical knowledge and nature.

Neither Dewey nor the marxists attribute the vagueness and uncertainty of knowledge to some fundamental inadequacy of our mind to grasp all that is really there. Such a conception, they say, would suppose an inherent immutable order in nature which we strive to grasp ever more adequately; or it would suppose reason to seek necessity, and universality, without having at the same time the powers to accomplish this ideal. This would lead to perpetual frustration. Dewey says:

"The doctrine that nature is inherently rational was a costly one. It entailed the idea that reason in man is an outside spectator of a rationality already complete in itself. It deprived reason in man of an active and creative office; its business was sim-

\(^{(1)}\) Textes Choisis sur Marxisme, Québec, 1940, p.7.
ply to copy, to re-present symbolically, to view a given rational structure. Ability to make a transcript of this structure in mathematical formulae gives great delight to those who have the required ability. But it does nothing; it makes no difference in nature. In effect, it limits thought in man to retraversing in cognition a pattern fixed and complete in itself. The doctrine was both an effect of the traditional separation between knowledge and action and a factor in perpetuating it. It relegated practical making and doing to a secondary and relatively irrational realm.

Its paralyzing effect on human action is seen in the past it played in the eighteenth and nineteenth centuries in the theory of "natural laws" in human affairs, in social matters. These natural laws were supposed to be inherently fixed; a science of social phenomena and relations was equivalent to discovery of them. Once discovered, nothing remained for man but to conform to them; they were to rule his conduct as physical laws govern physical phenomena. They were the sole standard of conduct in economic affairs; the laws of economics are the "natural" laws of all political action; other so-called laws are artificial, man-made contrivances in contrast with the normative regulations of nature itself.

Laissez-faire was the logical conclusion. For organized society to attempt to regulate the course of economic affairs, to bring them into service of humanly conceived ends, was a harmful interference."(1)

In this conception, theoretical uncertainty loses its very meaning:

"It is a strict truism that no one would care about any exclusively theoretical uncertainty of certainty. For by definition in being exclusively theoretical it is one which makes no difference." (2)

And again we should admit that this would be the case if all knowledge were dialectical.

It will not be necessary to analyze in detail Prenant's efforts to show that Marxism is not only in line with current conceptions of the nature of experimental biology, but that it is confirmed by them. His position, like that of pragmatism, follows simply enough from the generalization of dialectic and the recognition of its instrumental nature.
SECOND PART
SECOND PART

CHAPTER V

THE EXPERIMENTAL METHOD ACCORDING TO CLAUDE BERNARD

In the first part of this dissertation we have shown that there is some relation between what is now being taught on the nature of the experimental sciences and that branch of rational philosophy called dialectic. We shall now turn to a classic in the field of experimental biology, written by Claude Bernard (1813-1878). (2)

Sergean in his Discours du Centenaire, commemorating the birth of Bernard, declared that the Introduction à l'Étude de la Médecine Expérimentale was for the XIX Century, what Descartes' Discours de la Methode was for the XVII and XVIII Centuries. (1)

The background of this work is worth remarking. Claude Bernard was most familiar with the traditions of the laboratory. It was in contact with facts and their manipulation that his ideas on the experimental method developed. As a professor at the Collège de France, and later at the Sorbonne, he introduced his course to physiology by a number of lectures on the theory of the experimental method. Constrained by sickness to leave Paris, he found time to put his previous ideas in a distinct work, Introduction à l'Étude de la Medicine Expérimentale, which he

(1) Cf. Moniteur Universal, Nov. 1866.
(2) Cf. Appendix I, for the history of the scientific method.
published in 1865. (2) His exposition of the experimental method is all the more weighty because Bernard ranks among the greatest physiologists of all time. He not only made many important discoveries on the functioning of the liver, the pancreas, and the vaso-motor nerves, but also checked these discoveries so carefully that they seem to be as acceptable today, as the day they were made.

The Introduction is divided into three parts. The first part is an analysis of the general characteristics of the experimental method. It exposes the relation between fact and idea, between experimentation and the mental attitude of the experimentalist. This first part may be termed 'the logic of the experimental method'. "His work (Bernard's) says Lalande, has become known as the classical exposition of the experimental method not only in the natural sciences, but also in physical chemistry. This is due to the clearness with which he has put in relief the a priori ideas, and the spirit of invention, along with confidence in biological determinism, fidelity to facts, openness of mind even in respect to the new formulations for which one is enthusiastic; finally, the regard for the technique of verification, the importance of the counter-proof (for example the use of control animals)." (2)

In the second and third parts of the Introduction, Bernard considers the actual employment of the experimental method in

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(2)Lalande, Les Théories de l'Induction et de l'Expérimentation, (Boivin et Cie), Paris, 1929, p.204.
physiology and medicine. The experimental method is fundamen-
tally the same in studying living or non-living subjects.
However, the study of living organisms is more difficult due
to the interiority of life-phenomena, the complexity of the
phenomena, and the easy destruction of the subject. Bernard
therefore considers the various problems involved in experici-
mentation. From Parts II and III his philosophy of nature can
be gathered. In Part III Bernard describes some of his experiments,
and points out the difficulties of experimental medicine.

He does not treat directly the relation of biology to
philosophy strictly understood, but remains in the field of
method. It is a method, however, which is presented in such a
way as to challenge philosophy proper. He defines such concepts
as he thinks necessary to explain the nature of the experimental
method. His exposition of life phenomena, of the kinds of reason-
ing, of determinism, bring him necessarily into philosophical
controversy. Bernard at times speaks harshly of metaphysics and
of religion. Consequently his Introduction is thought to condemn
all science not fundamentally positivistic. Morelle says: "We
would have one think metaphysically in order to initiate movement,
but live and act physically; and to fashion science is to live
and to act". (1) Bernard disclaims that experimental medicine,
meaning also its methodology, belongs to any philosophical system.
He warns the experimentalist to cultivate the philosophical
spirit, but to avoid philosophical systems. (2)

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(1) C. Bernard, L'Introduction etc., (Première Partie), ed.
Delagrave, Paris, 1937; p.15.

(2) C. Bernard, L'Introduction etc., Ballière et Fils, Paris,
1865; cf. pp.359-362.
In the Introduction, Bernard makes a clear, concise, and brilliant analysis of the experimental method the more interesting in that he claims for it freedom from all philosophical systems and controversy. He contends that "experimental medicine, as well as all the experimental sciences, does not feel the need of attaching itself to any philosophical system. The role of the physiologist, as that of every scientist, is to seek the truth for itself, without wishing to use it for the verification of such or such system of philosophy. When the scientist pursuing scientific investigation takes as a basis any philosophical system, he wanders into regions far from reality, or the system gives to his mind a kind of false assurance and an inflexibility which is not in accord with the liberty and flexibility which the experimenter must always retain in his researches. It is necessary therefore to avoid with care any semblance of a system, and the reason (I give) is that systems are not in nature, but only in the minds of men. Positivism, which in the name of science rejects all philosophical systems, has as they, the falsity of being a system. For, to find the truth, it suffices that a scientist put himself before nature, and that he search it by following the experimental method aided by investigatory talents more or less perfect. I think that, in this case, the best philosophical system is to have no system." (1)

We may call attention to two reasons justifying this attitude. The scientist must be free to formulate those hypotheses

(1) Bernard, Introduction etc., Paris, 1865, pp. 386-387)
which will best explain the phenomena he deals with. No experimental method can be good unless it assures this freedom. As we shall see later, Bernard claims for hypothesis that which St. Thomas has claimed for the astronomical hypothesis of his time.

"Excedendum quod ad aliquam rem dupliciter inducitur ratio. Uno modo ad probandum sufficienter aliquam radices: sic ut in scientia naturali inducitur ratio sufficiens ad probandum quod motus coeli semper sit uniformis velocitatis. Alio modo inducitur ratio, quae non sufficienter probet radices, sed quae radici jam positis ostendat consequenter effectus: sic ut in astrologi positur ratio excentricorum et epicyclorum ex hoc quod, has positiones facta, possunt salvari apparentia sensibilia circa motus coelestes; non tamen ratio haece est sufficienter probans, quia etiam forte alia positione facta salvari possent." (1)

If we considered these hypotheses true, we would indeed be bound, and the progress of experimental science would be hampered. In this field, experience alone should be our first and last guide, and whatever we conceive must save the phenomena as we know them. To give definite value to some hypothesis, however plausible it may seem due to present evidence, is to deny experience. Claim to freedom in this field is therefore well-founded and indispensable.

Secondly we can well understand Bernard's suspicion of philosophical systems. If a philosophy hampers the freedom of experimental science, if it would impose definite principles of such a nature that they deny the freedom of hypothesis, then surely it is a bad philosophy as far as the scientist is concerned. And if such is nature of philosophical systems as such, then we

(1) St. Thomas, Summa Theologica, I, q.32, a.1, ad 2.
should have nothing to do with them.

The INTRODUCTION is largely a study of the problem of experimentation in physiology, pathology, and therapeutics; and it also describes certain experiments made by Bernard. Nevertheless, he gives in the first part the pure theory of the experimental method. The method as such is applicable, he says, to the physical sciences, and to the various biological sciences. "The naturalist who observes animals to learn their mode of life and habits, the physiologist and the doctor who wish to study the inner functioning of living bodies, the physician and the chemist who determine the phenomena of lifeless matter; all are in the same class — they have before them manifestations which they are able to interpret only by the aid of an experimental criterion, the sole study of which occupies us here." (Introduction p. 57) In many places Bernard uses the term 'physiology' and 'biological sciences' interchangeably. (Cf. pp. 27, 30, 55, 102, 115, 118, 127, etc.) Naturally as a physiologist he speaks usually of the method in relation to physiology. But he does not consider physiology as belonging to any but the biological sciences. Hence there is no mistake in choosing the INTRODUCTION to give us the exposition of the experimental method in biology.

In fact, Bernard says that the method is the same in the physical sciences as well as in the biological sciences. Unfortunately, this might well be contrary to the freedom which he claims for the experimental sciences. Why must the method be the
same? Should the method of mathematical physics be the same as that of experimental psychology? We can see no reason for accepting this a priori restriction. But more of this later.

Passing to a detailed consideration of the experimental method according to Claude Bernard, we shall first present his doctrine on observation and experimentation.

I. Observation and Experimentation.

Bernard disagrees with a common explanation that observation is the witnessing of phenomena which nature ordinarily offers, whereas experimentation is the witnessing of phenomena created or determined by the experimenter. In the words of Cuvier: "The observer listens to nature; the experimenter questions it and forces nature to reveal itself." But does this mean that the observer is always passive, and that the experimenter is alone active?

Bernard cleverly distinguishes between the art of investigation and the art of reasoning. In the first place, that observation is characterized by nature being active and the mind being passive is not exactly true. For example, if an epidemic occurs then a doctor has the opportunity to observe the causes and the progress of it. But suppose that another epidemic arises in a different locality, and the same doctor takes to observation. Again nature is revealing herself, but is the doctor simply passive? No, because the observation is of a different kind.
"This second observation, made in view of a preconceived idea on the nature and the cause of the disease, is one which must be called a provoked or active observation." Hence the oversimplification of holding that in observation nature is active and the experimenter is passive. In the second place, experimentation is not simply the provocation and manipulation of phenomena. A physiologist who is performing experiments on the facial nerve to learn its distribution could meet with an accidental case of gun-wound in which he need only observe to continue his experiment. He would not, then, be actively working on nature to produce or to control phenomena. Yet he would be experimenting. So neither is the above definition of experimentation satisfactory.

In the true explanation of observation and experience following distinctions are made. "Speaking in the concrete, one says that he makes an observation or experiences when he gives himself to the investigation and research of facts from which the mind can draw instruction. Speaking in the abstract, one says that he depends upon observation or learns from experience when he means that observation is the basis, 'le point d'appui' for the mind which reasons, and experience is the basis of the mind which concludes, or better, that it is the fruit of correct reasoning applied to the interpretation of facts. From this it follows that one is able to learn from experience without making experiments, by the sole fact that one reasons according to well established facts, just as one is able to experiment and to observe without
acquiring experience if one limits himself to the mere witnessing of facts.

"Observation therefore is that which shows the facts; and experience is that which instructs on the facts and gives the experience a relation to a thing. But as this instruction is impossible except by comparison and judgement, that is to say, by following a reasoning process, it results that man alone is capable of having an experience and of perfecting himself by experience." (Introduction p.22)

Bernard is careful to distinguish two kinds of instruction which are derived from experience: one that is empirical, the other that is experimental. That of the empirical kind is obtained in a practical way from each thing, and it is accompanied by a vague, experimental reasoning. It is this vague but spontaneous mental activity which the experimental method perfects by making it clear, off-hand, and consciously directed toward an end. The experience in a science is thus always acquired by virtue of a precise reasoning established on an idea which the observation has engendered, and which the experience controls. (Introduction p.23)

Bernard expresses many times, in simple language, the formula of the experimental method. The experimental method does no other thing than pass a judgement on the surrounding facts, by the aid of a criterion which is another fact used to control the judgement,
and to give the experience. Experience according to Bernard is of the greatest importance; it is the unique source of human knowledge. For the mind itself has only a feeling (sentiment) of a necessary relation between things. It can not know the exact form of this relation without experience. (Intro. p. 24)

Keeping in mind the very important distinction between the art of investigation and the art of experimental reasoning we should note:

1) The art of investigation is the corner-stone of the experimental sciences, because it supplies the facts upon which the experimental reasoning rests.

2) Investigation may be considered as a work of observation or of experimentation. In observation, the observer studies phenomena which he can not manipulate, but simply take from nature. In experimentation the experimenter is able to prepare and modify the subject being studied. This difference in the non-control and control of phenomena in investigation is the foundation for distinguishing a science of observation from a science of experimentation; for example,
astronomy from physiology. (1)

3) Pure observation of itself never suffices for

(1) We should take note, however, of a very important bit of doctrine coming from Eddington: "In a recent book, SCIENCE AND HUMAN EXPERIENCE, Professor Dingle draws what I think is a quite unwarranted distinction between the macroscopic and the microscopic entities in the physical world. According to him the latter are unverifiable by hypotheses, existences, or events whose unobservability is part of their essential nature (p. 47). He is contrasting them with ordinary 'observing' bodies and events, and he wishes to convey that electrons and protons have an essentially different status in our knowledge from the more ancient denizens of the physical world, such as sticks and stones and stars. I feel sure that this distinction is untenable.

And electron is no more (and no less) hypothetical than a star. Nowadays we can count the electrons one by in a Geiger counter, as we count the stars one by one on a photographic plate. In what sense can an electron be called more unobservable than a star? I am not sure whether I ought to say that I have seen an electron, but I have the same doubt as to whether I have seen a disc of light surrounded by diffraction rings, which has not the least resemblance to what a star is supposed to be; but I give the name star to the object which some hundred years ago started the chain of causation which has resulted in this curious light-pattern. Similarly, I have seen a wavy trail not in the least resembling what an electron is supposed to be; but I give the name electron to the object which has caused this trail to appear. How can it possibly be maintained that I am making a hypothesis in one case and not in the other?

I do not think that either the star of the electron should be called a hypothetical entity. We make no hypothesis by merely giving a name to that which is the origin of certain impressions which reach our senses. But it is difficult to separate the name from the hypothetical images that are commonly associated with it. No doubt hypothetical properties and characteristics have often been attributed to electrons, and some of these have turned out to be erroneous. But I rather think that the same thing has sometimes happened to the stars,"

constructing a science, since it is simple testification of facts. For scientific knowledge there is always required experimental reasoning. (Introd. p.23)

4) In investigation the observer and the experimenter have a common end; namely, to get the facts. Thus an experiment is nothing but a provoked observation. The facts in both cases are the principal interest. But in the experimental method the facts are accompanied by experimental observation and reasoning. Ordinarily the experimenter will be investigating in order to control and verify the experimental idea. The experiment is another observation provoked for the purpose of a control. (Introd. p.36.)

5) The technique of investigation, including observation and experimentation, is specialized in each science. Bernard in the second and third parts of the INTRODUCTION discusses thoroughly the problems of the investigator in the biological sciences, and especially in physiology.

Passing to the consideration of the experimental reasoning in science, we must note its key position in the experimental
method. We have already noted the precise meaning of observation and of experience in this method. In observation, the facts are the basis of an a priori idea, or hypothesis; in experience (which is usually founded in an experiment), the facts are the basis for a judgement as to the value of the idea. It is the process of reasoning (of experimental and not of empirical reasoning) which carries the mind from the idea to the judgement, and scientific knowledge. (In the next section experimental reasoning will be analyzed.)

Hence at bottom all sciences, (we exclude the mathematical sciences), reason the same way, that is, they have the same philosophic method. In the sciences of observation, man observes (experimentally) and reasons (experimentally), but he does not experiment. In the sciences of experimentation, again man observes and reasons; but more, he acts upon the materials before him. In experimenting he becomes an inventor of phenomena which give the facts. In a sense then, the experimental sciences are active rather than passive. But in every science, the mind must be active through reasoning experimentally.

Turning to experimentation, we should remark the two important operations. The first consists in premeditating and realizing the conditions of the experiment; the second consists in witnessing the results of the experiment. It is impossible to carry out an experiment without a preconceived idea; to institute an experiment, we have said, is to pose a question; one never conceives a question without the idea which solicits the response.
I consider it, therefore, as an absolute principle, that the experiment must always be instituted in view of a preconceived idea, it being of little import whether this idea is more or less vague, more or less well defined. As for the witnessing of the results of the experiment, which is itself only a provoked observation, I equally hold the principle, that it must be made then as in every other observation, that is to say, without a preconceived idea." (Intro. p.432) By demanding that the experimenter always observe carefully the actual facts, and that he take the facts as they turn out to be, Bernard answers the charge that to start an experiment with an a priori idea is to discredit the results at the very start. As he carefully notes, one must distinguish between the invention of an experiment with the witnessing of the actual results.

"The perfect scientist is the one who embraces at the same time experimental theory and practice. 1) He witnesses a fact. 2) The fact gives rise to an idea in the mind. 3) In view of this idea, he reasons, institutes an experiment, imagines and realizes the material conditions of it. 4) From this experiment there arise new phenomena which he must observe, and so on. The mind of the scientist is in a way always placed between two observations: one which serves for the point of departure for reasoning, and the other which serves him as the conclusion... The observer and the experimenter correspond therefore to different phases of the experimental research. The observer does not reason, he witnesses; the experimenter on the contrary reasons and relies on the acquired
facts to imagine and to provoke through his reasoning some other facts. But, if one in theory and abstractly can distinguish the observer from the experimenter, it seems impossible to do so in practise, since we see that the same investigator is alternately observer and experimenter. (Introd. pp. 43-45.)

**ART OF INVESTIGATION**

Aim: To get a supply of facts. Aim: To judge the *a priori* idea, coming from fact, by another fact.

1) **Concrete observation:** To witness accurately and without prejudice the facts found in nature.

In the sense that the investigator must take the facts as *given by nature,* he is passive. The sciences of observation are the result.

2) **Experimentation:** To manipulate nature and to find the facts by experiment. The investigator is active and provokes nature to reveal itself. The result is the sciences of experimentation.

**ART OF EXPERIMENTAL REASONING**

1) **Observation:** To form an idea from a fact in or out of an experiment. An *a priori* idea is necessary to initiate and to control the experiment.

2) **Experience:** The conscious and careful observation of a fact which confirms the truth of the preconceived idea. This experience may come with or without experimentation.

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**Science:** An experience acquired by virtue of a precise reasoning established on an idea, which the observation has engendered, and which the experiment controls.
2. **Experimental Reasoning**

a. The *A Priori* Idea

We have said that the fact gives rise to an idea in the mind, which idea is the same as a question demanding a response. That response must be found in the facts which some well planned and executed experiment will reveal. In the end the experiment will control the idea. It is important to make precise this pre-conceived, or *a priori* idea.

1) **Origin of the Idea.** This idea is not something innate, as for example the *a priori* forms of Kant. The idea must come from the fact. The mind may at the time be making a simple observation, or a provoked observation. Sometimes the idea is formed from a theory, which in turn is taken from fact. (p. 57.)

The mind forms the idea from feeling ('sentiment') or intuition; that is, when the mind is faced with facts it naturally tends to form an idea that things are so and so, or that this effect is due to that cause. (pp. 57, 61.) But as coming from an individual mind, the preconceived idea has its peculiarities; it is a *quid proprium*. Minds differ as to their power to find ideas in facts that are obscure. (p. 61.)

2) **Nature of the Idea.** Considered negatively:

a) As coming from fact it is not a mere figment of the imagination. b) As in the experimental method, it
is not something totally improbable. The scientist usually starts with the most likely explanation of phenomena. c) Finally, the idea is not of such a form that it is unverifiable in experimentation. An a priori idea as unverifiable is of no scientific value. (p. 58.)

Considered positively, the idea is not what the scholastics call a concept, but rather a proposition. This seems to be evident from the words of Bernard: 'The experimenter poses his idea as a question, as an interpretation anticipated from nature, and more or less probable. From it the experimenter deduces logically certain consequences which he checks each instant with reality by means of the experiment... The experimental idea is therefore also an a priori idea, but it is an idea which presents itself under the form of an hypothesis whose consequences must be submitted to the criterion of experimentation in order to judge its value.' (p. 49.) It is clear then that Bernard means by the a priori idea a dialectical proposition or a 'problem' which may be formed into a 'position' or thesis. This thesis is used in a deduction which with its consequences forms an hypothesis, the latter in turn being a dialectical proposition or interrogation to be tested. This is why he uses idea and hypothesis interchangeably: 'Feeling ('sentiment') engenders the idea, or the experimental
hypothesis, that is to say, the anticipated interpretation of the phenomena of nature." (p. 57.)

3) **Necessity of the A Priori Idea.** We have already remarked that the idea is necessary in the experimental method. No experiment, if it is to be worth anything, can be instituted without the idea. The experiment receives its whole direction from the idea. (Cf. p. 55.) Hence the experimenter must get an idea somehow if he lacks one. Let him even observe or experiment until he gets the first idea how so ever imperfect it may be. (p. 37.)

4) **Testing the Idea.** While the idea, or hypothesis, initiates the experiment, it in turn must be submitted to what the experiment reveals. As the facts are observed, simply or in a provoked observation, the hypothesis is verified, changed, or entirely rejected. (pp. 41, 49.) The hypothesis is said to be between the facts. Later we shall see that the attitude of the mind toward the idea, even as verified, remains one of doubt. (p. 70.) Hence it is never more than an instrument.

b. Reasoning.

This preconceived idea, as we noted, is used by reason, or in the process of reasoning. According to Bernard: "The forms
of reasoning are two: 1) The **investigative**, or interrogative, which is used by a person who does not know, but wishes to learn. 2) The **demonstrative**, or affirmative, which is used by the person who knows or thinks he knows, and who wishes to instruct others." (p. 77.) Bernard then notes that according to some philosophers the investigative is called inductive reasoning; and the demonstrative, deductive. He notes that others hold for two scientific methods: the inductive, which is proper to the experimental sciences; and the deductive which is proper to the mathematical sciences.

With this latter conception of scientific methods Bernard disagrees. All sciences, both mathematical and physical, equally use induction and deduction. Therefore they cannot be strictly divided as sciences of deduction as opposed to sciences of induction. "The mathematician and the naturalist do not differ in their search for principles. The one, as the other, inducts, makes hypotheses and experiments; that is to say, strives to verify the exactitude of his ideas. But when the mathematician and the naturalist have secured their principles they are then completely different. In fact, as I have said already in another place, the principle of the mathematician becomes absolute, because he does not apply it to objective reality such as it is, but to the relations of things considered in extremely simple conditions, which the mathematician chooses and creates in some sort in his mind. But, thus having the certitude that no other conditions than those determined can enter into the reasoning, the principle is absolute, conscious, adequate for the mind, and the logical
deduction is equally absolute and certain; there is no longer need for experimental verification, because the logic suffices.

"The situation of the naturalist is different; the general proposition at which he arrives, or the principle on which he supports his reasoning, remains relative and provisional because it represents complex relations about which he can never be certain that he is able to know all. Hence his principle is uncertain, since it is unconscious, and inadequate to the mind; hence the deduction, howsoever logical, remains doubtful always, and it is necessary then to invoke experimentation in order to control the conclusion of the deductive reasoning. This difference between the mathematician and the naturalist is capital in view of the content of their principle and of the conclusions that they draw; but the mechanism of the deductive reasoning is exactly the same for both of them. Both start from a proposition; only the mathematician says: This point of departure given, such a particular case necessarily results. The naturalist says: If this point of departure were correct, such a particular case would result from it as a consequence." (pp. 51-52.)

Bernard here seems to oppose demonstrative reasoning to a hypothetical proposition. Most modern logicians commit the error. The reason presumably is that all conditional propositions virtually include a consequence, as in the statement: "If this point of departure were correct, such a particular case would result from it as a consequence." But for hypothetical reasoning we must then add: "But such is the consequence. Therefore the
point of departure is correct." However, that such a particular case is the fact, is precisely what we must be established before we have hypothetical reasoning.

Again, the statement "If this point of departure were correct, such a particular case would result from it as a consequence" might be true or probable. It would therefore be wrong to oppose certain reasoning to hypothetical reasoning, for the latter as a form of reasoning prescinds from the matter. The division of reasoning into certain and probable or dialectical is taken from the matter.

Neither is dialectical reasoning interrogative except in the sense that its propositions are interrogative. Dialectic is interrogative not only of the conclusions but of the principles as well.

Notwithstanding these errors concerning logic, we do grasp what Bernard means to say. We may state fairly that he intends to oppose what we call scientific reasoning to dialectical reasoning.

According to Bernard, then, the following important of doctrine may be stated relative to scientific knowledge in general.

1) Sciences are not strictly distinguishable as inductive or deductive; because all sciences use both inductive and deductive reasoning. Yet if a person wishes to use inductive and deductive to
signify the difference in the principles used by mathematics and the experimental sciences, and that the a priori idea (principle) in the experimental sciences must be considered uncertain and requiring verification, then this distinction between inductive and deductive science is admissible. The forms of reasoning are divided as follows:

I Division:

i) Investigative Reasoning: Used by oneself in order to learn.

ii) Demonstrative Reasoning: Used by one to instruct others.

II Division:

i) Reasoning in the Experimental Sciences:

a) Investigative (inductive) used to find principles.

b) Demonstrative (deductive) used to draw conclusions from the given principles.

E.3. In the experimental sciences the principles and conclusions remain uncertain.

ii) Reasoning in Mathematics:

a) Investigative (inductive) used to find principles, which are certain.

b) Demonstrative (deductive) used to draw certain and necessary truths.

2) All principles come ultimately from induction, that is, from the investigation of something inside or outside ourselves. (p. 80.)
3) Given principles, the mind always deduces or reasons in the same way. It is only the subject treated that constitutes 'varieties' of reasoning. (pp. 78-79.)

4) As we shall see later, in all reasoning, purely mathematical or experimental, true knowledge would be impossible without the principle of determinism. (p. 79.)

In regard to reasoning in the experimental sciences, for example in experimental biology, the doctrine may be restated succinctly:

1) The *a priori* ideas (principles) are taken from facts directly or indirectly by the mind. This entails inductive reasoning.

2) Deductive reasoning consists in drawing consequences from the *a priori* idea, and thereby constituting a hypothesis.

3) Because the reasoning is uncertain, (as based on uncertain, unconscious principles,) it must be submitted to experimental verification. Thus the experiment controls the idea. In the measure that the scientist assembles the facts, his principles become more and more general and more certain; thus he acquires the certitude which he deduces. In the experimental sciences, the certitude is *always* provisional because the scientific mind is limited to
certain facts and conditions. (pp. 24-25.)

Finally, there is need of the counter-proof in the experimental method. The hypothesis even as verified is so uncertain and provisional, that the verification must be rechecked. This counter-proof is not a simple comparison of phenomena. It is the application of the principle: 'Sublata causa, tollitur effectus.' The experimenter must prove that in the absence of the cause, which he thinks he has discovered, the effect (phenomenon) does not occur. In the experimental sciences deception is so easy, that this counter-proof is essential to the method. To doubt the verified consequence of a hypothesis is to carry philosophical doubt to the proper extreme. (pp. 97-100.)

When Bernard uses the term 'cause', obviously it is not to be understood formally, but rather as the term of a relation, which term is itself but a sign.

c. Induction.

Bernard speaks of the induction of principles both for the experimental sciences and for mathematics. Before examining the nature of this induction used, we should note the three uses of the word 'induction'. Taken in the first sense, induction signifies the abstraction of an universal concept from a singular object. All our human intellectual knowledge depends upon this type of induction for its concepts, since man begins life in pure potency to knowledge. The second type of induction signifies the knowledge of a per se nota proposition; it is knowledge of
a principle which presupposes sensible experience, but which in itself is immediately intelligible. Induction in the third sense signifies an argument, and it is as such that induction is "A progression from singulars sufficiently enumerated, to an universal". For example, to experience several times that fire is hot, and then to conclude that all fire is hot, is to make an induction. And induction is said to be ascending when it is ordained to the discovery of universal truths or generalisations. These truths are universal only to the extent that singulars have been examined and found to prove that the predicate belongs to the singulars which the universal designated. The conclusion of such an induction is only probable. (1)

Descending induction proceeds from the universal to the singulars; it is used to demonstrate the falsity of the universal, as an universal, in some particular instance. But supposing the truth of the universal found by ascending induction, the descending induction can serve to show the correspondence of the universal to the singulars contained under it. In both ascending and descending induction, the argument lacks a perfect mean. (2)


(2) "Inductio autem non assurit medium unius extrema ad probandum extrema uniri inter se, sed probat extremum seu predicatum aliquod convenire alicui subiecto communi, quia convenit singularibus, aut singularitatis convenire, quia convenit communi." (John of St. Thomas, op. et loco cit.)
What is the kind of induction used in the experimental method?
The method uses ascending and descending inductive reasoning.
Bernard, however, confuses the various kinds of induction, and
descending induction with deduction.

1) The experimental method uses induction also in the
first sense, that is, the abstractive process whereby
the mind has concepts (universals) which represent
objects. And Bernard is correct when he says that
all sciences use induction, if he means induction
in this sense.

2) Does the experimental method use induction to find
per se nota principles? We say that it never does,
but Bernard says that the mind in knowing the
exterior world has an intuition of the principle of
determinism. If this were true, and if the basis of
the experimental method is the principle of
determinism, then we could say that the method uses
induction in this second sense. But aside from this
one principle, all others in experimental science,
according to Bernard, are only provisional, relative
true, etc. Hence we may say that Bernard would induct
one per se nota principle, and only one. Yet even
the principles of mathematics are absolutely true, or
self-evident because of any induction. "The principle
of the mathematician becomes absolute, because he does
not apply it to objective reality, such as it is, but
to the relations of things considered in extremely
simple conditions, which the mathematician chooses
and creates in some sort in his mind." (Cf. p. 61.)
We would hardly agree with Bernard that such prin-
ciples are absolutely true.

3) Induction in the third sense, as a progression from
particulars to the general, and vice versa, is the
induction which gives only probability, and charac-
terizes the experimental sciences. As ascending,
this induction is the basis of generalization. It
is a distinct type of reasoning, which has only
probability. Bernard certainly claims it for the
experimental method, although he does not seem to
distinguish it from induction of the first kind.

"The situation of the naturalist is
different; the general proposition
at which he arrives, or the principle
on which he supports his reasoning,
remains relative and provisional,
because it represents complex relations
about which he can never be certain
that he is able to know all." (Cf. p. 62.)

Ascending induction is not used by all sciences.
There are such sciences, as philosophy and mathema-
tics, which have no need of ascending induction,
since they are dealing with pure intelligibles. The
per se nota principles of philosophy come through
induction in the second sense.

But it is descending induction that Bernard especially
confuses, by failing to distinguish it from deduction.
"Given principles, the mind always deduces or reasons in the same way. It is only the subject treated that constitutes 'varieties' of reasoning."

(Cf. p. 63.) This statement, of course, is far from true. This is evident from an analysis of the examples which Bernard gives of deductive reasoning, - examples which happen to be descending inductions.

We can now turn to an analysis of these examples, having clarified the nature of the induction of which Bernard speaks.

a) "The urine of carnivores is acid; But the rabbits before me have acid urine; Therefore they are carnivores, that is, fasting."

(Note: Bernard calls this an inductive reasoning, only in the sense that the principle (major) is probable, and not absolutely certain. He does not use inductive as opposed to deductive. (Cf. p. 64, No.1.)

b) "White chyle is due to the emulsion of fat; But in the rabbit the chyle forms in the region where the pancreatic sugar enters the intestine; Therefore pancreatic sugar emulsifies fat, and forms white chyle." (Introd. p. 270.)

In the analysis of the first example of experimental reasoning, we can easily find the ascending and descending inductions. The major is a generalization which has been made by experimentation on a number of animals given flesh as food. By testing the urine of these separate animals, the scientist has discovered that all of them have a urine that is acid. He has proceeded to make the generalization; all carnivores have acid urine. Bernard
has accepted this scientific generalization, which is clearly the result of an ascending induction. The minor of the reasoning is an instance put under this generalization. Then follows the conclusion which is only probable. It says no more than: this rabbit with acid urine is probably a carnivore. The acidity of urine is not an infallible sign that an animal is a carnivore, because all carnivores were identified with this phenomenon, simply by making a generalization from several particulars. And so the generalization holds certainly for only those particular animals already examined. There remains the experimental work to prove that the rabbits studied are carnivores. Bernard was able to prove by experiment that these rabbits when fasting from the herbs they ordinarily have for food, begin to digest themselves; that is, become carnivores. Hence Bernard has made a descending induction, and has mistaken it for a true syllogism. This mistake can be quite easily made.(1)

These notions on induction in no way contradict what we have said about Bernard's use of hypothesis. The conclusion of his descending induction remains a hypothesis which must be verified. The scientific generalization made by ascending induction serves as a position or thesis; but it is still a dialectical proposition whose truth is not definitely known.

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(1) Note: Inductive reasoning and the syllogism are two distinct species of argumentation and they are not reducible one to the other. The mean of the syllogism is a true universal; whereas the mean of the inductive argument is a collective universal, that is, it stands for a number of singulars investigated. Hence these two should not be confused even though they may appear alike in external form.
Without delaying on the various loose acceptations of the term hypothesis we wish to point out one acceptance worthy of note. Hypothesis is even identified with a state of mind. In discussing the various opinions on the nature of hypothesis Schiller says:

"This (the problem) will naturally start with a consideration of hypothesis in its most comprehensive sense, as a mental attitude of distinct activity. For it is here that the primary difference lies between enunciating a fact and entertaining a hypothesis. In the former alone is a claim made to propound a truth that is a reflection of reality; in the latter, no such direct relation to reality is alleged. For hypothesis is a sort of game with reality, akin to fancy, make-believe, fiction and poetry. Yet on the otherhand this hypothetical attitude mediates between thought and action, and helps to break down the superficial distinction between the theoretic and the practical. It drives the scientist out of the purely receptive attitude, and makes him a doer." (1)

We have already analyzed carefully the nature of Bernard's hypothesis, but it is also an hypothesis in the broad sense of being that which the scientist uses to penetrate the obscurity that surrounds the most obvious phenomena of nature. When Schiller says that the hypothesis is used to anticipate reality by a guess, to question it, to experiment, to distrust and doubt appearances, to rearrange the world at least in thought, he is expressing almost exactly

what Bernard considers to be the purpose of his hypothesis. Bernard, however, puts greater stress on verification than does Schiller. We shall therefore pass on to the consideration of truth and verification in the experimental method.

d. Truth in Experimental Reasoning.

The experimenter proceeds from partial truths to more general ones, but without ever daring to pretend that he holds the absolute truth. (p. 49.) What is the result of this a priori and provisional reasoning? It is to know neither the beginning, nor the end, but only the middle, (that is, the phenomena,) of things. But the fact that the scientist is never in a position to know more than the middle is no deep concern to him. If he were ever to have absolute truth on any point, he would know all truth about the external world. There would be no experimental science. (pp. 38, 49.)

Bernard here supposes on the part of nature a determinism so complete that the possession of absolute truth on one single point, would imply knowledge of the whole, each part of the universe being therefore a reflection of the whole. We shall see later on how this follows from his false interpretation of the principle of determinism.

"When we fashion a general theory in our sciences, the sole thing of which we are certain, is that all these theories are, absolutely speaking, false. They are merely partial and provisional truths which are necessary as steps upon which we rest in our advancing investigation; they represent
only the actual state of our knowledge, and consequently they must be modified with the growth of science, and so much the more often as the sciences are less advanced in their evolution." (p. 63.)

"The theories that represent the ensemble of our scientific ideas are without doubt indispensable in order to represent science. They must serve as the origin of new ideas of investigation. But these theories and these ideas not being unchangeably true, one must be ready always to abandon, to modify, to change them when they no longer represent reality. In a word, it is necessary to modify a theory in order to adapt it to nature, and not modify nature to adapt it to a theory." (p. 70.)

That all general theories are absolutely speaking false is not to be taken strictly. They would be false if they were held to be true. But when hypothesis or suppositions are held for the sake of saving appearances, when we know them to be neither true nor false, but provisionally plausible and sufficient, then our attitude is true. As St. Thomas says:

"Dicitendum quod ad aliquam rem dupliciter inducitur ratio. Uno modo ad probandum sufficienter aliquam radicem: sicut in scientia naturali inducitur ratio sufficiens ad probandum quod mutus coeli semper sit uniformis velocitatis. Alio modo inducitur ratio quae non sufficienter probet radicem, sed quae radici jam positae ostendat congruentes consequentias effectus: sicut in astrologi, ponitur ratio excentricorum et epicyclorum ex hoc quod, hac positions facta, possunt salvari apparentia sensibilia circa motus coelestes; non tamen ratio haece est sufficienter probans, quia etiam forte alia positions facta salvari possent." (St. Thom., Summa, I, q. 32, a. 1, ad. 2)

Furthermore we must restrict Bernard's statement that all theories are false, because of the nature of the experience which verifies hypotheses. What is the meaning of experimental verification? Let us note that in rigid demonstration there is no need
of experimental verification. The conclusion is true necessarily
by reason of the premises. In the experimental reasoning the
conclusion is only probable, as coming from inducted generaliza-
tion, or pure supposition. Hence it is not surprising to find the
experimenter looking for a way to check again his reasoning. It
is in need of rectification, which rectification he must find in
experimentation.

What is the significance of a conclusion being supported
by new experimental facts? Evidently, the new facts can not make
the hypothetical reasoning necessarily true. Bernard is confusing
when he says that the scientist acquires the certitude he deduces.
(See page 64.) He is not speaking strictly of certitude, because
he considers his certitudes as provisional. At most, the experi-
mental facts can support the probability, and may reveal new
helpful facts. On the new facts discovered, the experimenter may
form another hypothesis. Schiller very clearly explains the
meaning of verification.

"For verification neither is, nor can it ever
become, proof absolute. It is committed by its
method to the formal flaw of Affirming the
Consequent: when it argues that a hypothesis
is true because the facts that follow from it
are observed, this lacks cogency because these
same facts, together with others still unknown,
might follow still better from another hypothesis
not yet formulated. And we cannot lay it down
that a hypothesis shall only be accounted true
when it alone can account for the facts... All
that can be said, therefore for a hypothesis which
successfully holds the field is that, though it
is not absolutely proved and cannot claim absolute
truth, it can be accepted as true provisionally and
doneo corrigatur. And is not this after all, the
most sensible thing to do? It is to hold it as
good as true, and to give it all the practical
privileges of truth, and this is all the assurance we practically need, without encouraging a groundless scepticism which appeals to an abstract possibility there is nothing to support. Only we reserve our right to exchange our hypothesis for a better, should one become available, and are perhaps a shade more likely to look out for it. But what harm is there in that?" (1)

Yet there is, in a very restricted sense, truth in the experimental method. The work of verifying the hypothesis may lead to the discovery of new facts. The discovered facts can certainly be called true. But the truth of these facts is not the same as the truth of a rigid syllogism, or the probability of a dialectical reasoning. And it is only by extrinsic denomination that the truth of the facts can be said of the hypothesis. This is the meaning of the statement, that the hypothesis is verified.

3. Principles of Science

There is more implied in the experimental method than intuition of an a priori idea which is used by reason and submitted to experiment. Bernard, in spite of his claim that he avoids all philosophical systems, states the broad principles of science upon which the experimental method rests.

"The human mind has passed successively through three stages; namely those of sentiment, reason, experience. First, sentiment imposing itself on the mind created the truths of faith, that is to say, theology. Reason, or philosophy, coming next as the

(1) Schiller, loc. cit., p. 424.
teacher, produced the scholastic. Thirdly, experience, that is to say the study of natural phenomena, taught man that the truths of the exterior world are not formulated from the start by feeling, or by reason. These two are only our indispensable guides; because to find truths, it is necessary to descend into the objective reality of things; in which these truths hide with their phenomenal form. *(p. 50.)* It is the excellence of the experimental method to embrace and to perfect the previous two stages of knowledge. Experience and experiment as in the experimental method constitute a new science for the human mind. In so describing the advance of knowledge Bernard subscribes to the 'law of three stages' taught by the positivist, August Comte. *(1)* It is the third stage that Bernard analyzes and exposes according to his peculiar theory.

Secondly, Bernard explains ultimately how the experimental method attains relative truths about real external objects. In doing so he borrows an important principle from Descartes. "Just as in the human body there are two orders of functions, those that are conscious and the others that are not conscious, so in the mind there are two orders of truth or of notions, those that are conscious, interior or subjective, and the others that are unconscious, exterior or objective. The subjective truth are those that flow from principles of which the mind has consciousness, and which bring with themselves the sentiment of an absolute and necessary evidence. In fact, the great truths are in

*(1) Cf. Turner, History of Philosophy, Boston, 1903, pp.608-609, for brief description of the 'law of three stages'.*
reality only a sentiment of the mind; that is what Descartes understood by his famous maxim.

"We have said, on the other hand, that man could never know the first causes or the essence of things. Consequently truth never appears to the mind under the form of an absolute and necessary relation or connection. Moreover, this connection cannot be absolute except in so far as the conditions of it are simple and subjective, that is to say that the mind has the realization of knowing all. Mathematics represents the connection of things in conditions of ideal simplicity. The result is that these principles or connections, once found, are accepted by the mind as absolute truth, that is to say, independent of reality. One understands consequently that all the logical deductions of mathematical reasoning are as certain as their principles, and they have no need of being verified by experience. Otherwise, one would be putting the senses above the reason. It would be absurd to try to prove what is absolutely true for the mind, or what is not able to be conceived otherwise.

"But when, in place of studying subjective connections of which the mind creates the conditions, man wishes to know the objective relations of nature which he has not created, the interior and conscious criterion fails him. He is always conscious, without doubt, that in the objective or exterior world, truth is also constituted by necessary connections, but that the knowledge of the conditions of these connections escape him. It would, in fact, be necessary for him to create these conditions in order to
possess an absolute knowledge and conception of them." (pp. 51-52.)

Descartes in his DISCOURS DE LA METHODE took a position of universal doubt, with the intention of systematically reconstructing all human knowledge after the ideal rigor of mathematics. It is the three fundamental rules of the cartesian method which Bernard especially borrows and modifies for explaining the basis of the experimental method. Descartes' point of departure was a clear idea defined as: "Quae cum clara sit, ab omnibus aliis ita sejuncta est et praecisa, ut nihil plane aliud, quam quod clarum est, in se continent." He insisted upon rigorous deduction from these first intuitive and consequently certain ideas. He considered deduction a successive intuition. Thirdly, the universal criterion of true knowledge was the intuitive evidence of the clear and distinct ideas. Descartes in LA METHODE proposed to put in evidence the veritable richness of the soul; he would open to each one the means of finding in oneself, and without borrowing from another, all the science that is necessary for him in the conduct of his life. It is not hard to detect the influence of cartesian philosophy on Bernard's doctrine. He emphasizes the importance of the mind, of its richness in the experimental method. He speaks of feeling and intuition of ideas (a priori and otherwise); of mathematical principles which are absolute, conscious, adequate, and therefore intuitively certain; of other ideas that lack interiority and consciousness, that is really to say, clearness; of rigorous deduction in mathematics; and even of, (as we shall now see,) philosophic doubt. However Bernard does not ever
turn from his primary consideration: to expose experimental reasoning and the experimental method, which give science.

Finally there are two observations to make. First, the reality of the external world is not questioned by Bernard. It is clear to the mind. Nor is the necessary principle of determinism, which is fundamental in Bernard’s science, ever defended. In his simplicity he states: "Man is always conscious, without doubt, that is: the objective or exterior world truth is equally constituted by necessary connections, but the knowledge of the conditions of these connections escape him." (p. 52.) Secondly, the mathematical sciences as interior and perfectly conscious are absolutely true and certain. Bernard is moreover careful to note that as soon as mathematical principles are applied to nature, then experimental verification is required. (pp. 52-54.) It is because the criterion of reasoning is no longer purely interior and conscious. Hence the truth of the deductions is no longer certain and absolute.

This shows clearly enough that he refutes his contention that the experimental method, as he conceives it, is independent of any philosophical system. By this we do not mean that he is wrong in his effort to state the necessary conditions of experimental science, conditions which lie beyond the reach of the method itself. But what he says here shows that he does adhere to philosophical systems, to a mixture of Descartes, Kant and Compte. This was entirely superfluous.
We agree to what he says on the certainty of mathematics, not on the precise cause of it. It is indeed the science most proportionate to our mind. It needs no verification in experience. But what is important in Bernard's continual recurrence to mathematics, is the necessity of making clear the nature of experimental science by comparing it with some ideal science of which the former falls short. It is in fact true that dialectic presupposes formal logic and demonstrative logic.

4. Determinism and Philosophical Doubt

It is necessary to clarify these apparently contradictory principles; namely, that the scientist is certain of determinism, but that he is also certain that his position is always one of philosophic doubt.

a. Determinism.

We have seen that Bernard claims man is certainly conscious that in the experior world truth is likewise constituted by necessary connections, that is determined. Without this principle there could be no science. "The absolute principle of the experimental sciences is a necessary and conscious determinism in the conditions of the phenomena. It is of such a sort that a natural phenomenon, whatever it is, being given, the experimenter can never admit that there is a variation in the expression of this phenomenon, unless at the same time there be the intervention of new conditions in its manifestation; moreover,
he has an a priori certitude that these variations are determined by rigorous and mathematical connections. Experience simply shows us the form of the phenomena; but the connection of the phenomenon to a determined cause is necessary and independent of experience, and it is necessarily mathematical and absolute. We thus see that the principle of the criterion of the experimental sciences is in reality identical with that of the mathematical sciences, since in each of them this principle is expressed by a necessary and absolute relation of things. Only, in the experimental sciences these connections are surrounded by numerous, complex, and infinitely varied phenomena, which hide the connections from our view. By the aid of experience we analyze, we dissociate the phenomena, in order to reduce them to relations and conditions more and more simple. We so wish to seize the form of scientific truth, that is to say, to find the law which should give us the key to all the variations of the phenomena. This experimental analysis is the only means that we have for searching out the truths in the experimental sciences; and the absolute determinism of the phenomena, of which we have an a priori consciousness, is the sole criterion or the sole principle which directs and supports us. In spite of our efforts, we are still very far from this absolute truth; and it is probable, especially in the biological sciences, that we shall never see it in its nudity.\(^4\) (94–95.) Science therefore has for its end, the banishment of indeterminism, or in other words, to find proximate causes.
It is clear that some kind of determinism is necessary for the scientist working with natural beings. He has to take for granted that the same effects have the same causes, and that once he has determined the conditions of a phenomenon, he may cause it to occur again. If there were absolute indeterminism in nature, then they would be simply prime matter, which by itself is inexistence, as well as unintelligible. In fact, there is no absolute indetermination in nature, for natural beings are determined by their form, and act according to this form. As constituted by matter and form, they act orderly. No one knows this better than the scientist.

However, when Bernard claims that "the experimenter has an a priori certainty that the variations of phenomena are determined by rigorous mathematical relationships," and that one should extend this determinism to all observable phenomena, then one can ask from what source does this certainty spring? And if as he says "the doubter is the true scientist" whence comes this evidence for the universal application of absolute determinism to all phenomena of nature? The true scientist is said to be conscious of it a priori. It must be admitted moreover that if experimental science is an absolute science, it must be equally true that its principles and the material basis of these principles are absolute. But how do we know that experimental science is an absolute science? This is a problem which one should not presume to resolve in an a priori manner. The
true scientist searches for the absolute where he can find it. He need have no doubt as to the existence of science, but he must ask himself if the matter he is studying conforms itself to the demands of an absolute science.

Supposing that there is not absolute determinism in nature, then determinism can be adopted as a methodological principle. In practice the scientist will hold that the events in his scientific world are knowable only in so far as they are determined. The future will be adequately knowable only in so far as it is already determined in the present. Accordingly he will consider all indetermination as provisional, and will strive to resolve it as much as possible. Determinism will thus be a limit toward which his doctrines will move. But he can never hope to arrive at absolute determinism. Neither will the experimental scientist care to, for then his scientific work would come to an end. Bernard seems to grasp the methodological character of his principle of determinism when he says:

"Certainly we shall never arrive at the absolute determinism of everything; man would no longer be able to exist. There shall always be, therefore, indeterminism in all sciences, and in medicine more than in the other sciences. But the intellectual conquest of man consists in diminishing and pushing back indeterminism, in proportion that by the help of the experimental method it gains ground on determinism. That alone must satisfy man's ambition, because it is by this method that man can and will extend more and more his power over nature." (p.246)

b. Philosophical Doubt.

Because the scientist has the principle of determinism, he can not be a sceptic; he must believe in science. The sceptic
believes in no science whatsoever. On the other hand, the
scientist knows that his a priori idea is not absolutely
certain, and that his theory is never more than an approxi-
mation. In fact, all theories are absolutely speaking false.
Consequently, he must retain always a mental attitude of
philosophical doubt.

"The great experimental principle, therefore, is doubt,
the philosophic doubt which leaves the mind its freedom and
initiative, and from which come those qualities the most precious
in an investigator in physiology and in medicine. One should
believe in our observations and our theories, but with the
right of verifying them experimentally. If one believes too
much, the mind is tied and restricted by the consequences of
his own reasoning; he has his liberty of action no longer, and
lacks consequently the initiative which one possesses who
knows how to free himself from blind faith in theories, which
is in reality scientific superstition." (Introd. p.66.)

Certain consequences follow from this mental attitude of
philosophic doubt. First the scientist gives to the experimental
method an independent character. It is never that he thinks is
true, or what another scientist thinks is the correct theory,
it is what the experimental method proves. And so, all due
credit to great scientists past or present, authority must yield
to the method. Secondly, if philosophical doubt leaves the mind
free, it also leaves it in a continual state of change, of
movement. Science is no longer something fixed, certain, and
final. This is what Bernard holds to be true. "In the experimental sciences the truths being only relative, science is able to advance only by revolution and absorption of the older truths under a new scientific form." (Introduction, p. 72.)

Understanding the difference between demonstrative science and dialectic, Bernard's doctrine on the place of doubt in the experimental method is easily admitted. Using dialectical reasoning, experimental biology cannot give a certainty which will rest the mind. Hence it is, that the scientist no sooner formulates his theories than he begins to doubt their worth, in view of the new facts continually presenting themselves. This doubt is an important part of the method, because it is a necessary condition for movement on to better theories. While liberty, independence, and initiative of mind are attributed by Bernard to this principle of doubt, they actually derive from the dialectical character of the method itself, and the doubt is but a declaration of one's right to such dispositions of mind.
CHAPTER VI

ON DIALECTICAL DEFINITIONS IN EXPERIMENTAL BIOLOGY

We have so far shown by a comparison of modern and traditional texts that the method of experimental biology is dialectical. But this comparison bore only on propositions and discourse. It now remains for us to show that the very definitions and the terms used to signify these definitions are dialectical. If we do not continue in this chapter to compare our position with texts from modern writers on the subject, it is because the latter have not treated this subject in explicit enough fashion.

1. On the Nature of Dialectical Definition.

We here take definition as "oratio naturam rei exponens;" and which is divided into the nominal definition and into the real definition. "The real (quid rei) definition is divided into essential, descriptive, and causal.

1) The essential or quidditaptive definition is speech explaining something through its parts or essential predicates, as: 'Man is a rational animal.' Moreover, since the physical parts in each thing are considered as matter and form; and the metaphysical parts as genus and difference, the quidditative definition is twofold; namely,

a) The real physical definition which is composed of matter and form; and

b) The real metaphysical definition which is constituted by the genus and difference.

Although in a physical definition matter holds the place of the genus, and the form
of the difference, it so happens that every definition is composed of genus and
difference, or something in the place of
genus and difference.

2) The descriptive definition is that which is com-
posed of a thing's accidents, either proper or
common, as when one says: 'Man is an animal with
the power of laughing,' or 'Man is a biped animal.'

3) The causal definition is that which is presented
by way of extrinsic causes; these extrinsic
causes are two; namely,

a) Efficient, and
b) Final, as is taught in Physics II.

These causes are extrinsic because they do not
constitute nature, but are outside (extra) it
(for matter and form are the intrinsic causes).
When one defines the human soul: 'That which is
a form created by God for beatitude', 'by God'
expresses the efficient cause, and 'for beatitude'
is the end (the final cause)." (1)

The descriptive definition by common accidents is diale-
tical. And it is in turn of two kinds according as the common
accidents are real, or intentions of reason. An example of the
first is "biped featherless animal" for man; of the second the
logical definition of substance: "that which is not predicated
of a subject and of which all other things are predicated."

These two definitions have in common that they are
extrinsic and common, and opposed to all other kinds as to
proper definitions. The proper definition makes a thing
known as to its intrinsic principles, such as "rational animal",
or "tool-making animal" (for "tool-making" supposes reason);

or "what is per se and in no other." (1)

In the dialectical definition the nature is signified by something extrinsic and common. Nothing of the nature itself is revealed, as when we define substance through predication, and when we say common, we do not mean that it is also applicable to non-substance, but rather that it gives us only an abstract, general, vague notion of substance. The same is to be said of definitions of natural things through form alone, such as the definition "ira est appetitus vindictae" referred to in the first chapter. This is clear from St. Albert the Great:

"Physicus autem et dialecticus diffinient differenter usum quodque istorum quae diximus esse animae opera et passiones. Si enim quaerimus quid est IRA, intendantem de diffinitione quaeror, dicent dialecticus quid est appetitus contrarii doloris, aut aliquid huicmodi diffinient per intentiones comunes formales, quae non sunt vera

(1) "et quia possest alicui videri, quod ex quo Philosophus ponit omnes modos, quibus dicitur substantia, quod hoc sufficeret ad scienam quid est substantia; ideo subjungit dicens, quod nunc dictum est quid sit substantia "solum typo", id est dictum est solum in universali, quod substantia est illud, quod non dicitur de subjecto, sed de quo dicuntur alia; sed oportet non solum ita cognoscere substantiam et alias res, scilicet per definitionem universalem et logicae, hoc enim non est sufficiens ad cognoscendum naturam rei, quia hoc ipseum quod assignatur pro definitione tali, est manifestum. Non enim huicmodi definitiones tanguntur principia rei, ex quibus cognitio rei dependet; sed tangitur aliqua communis conditio rei per quam talis notificatio datur." (St. Thomas, In Met. Arist. Comm., Lib.VII, lec. 2, n.1230.)
causa rei propria, sed intentiones communae
inventae in pluribus et nulli propriae: et ideo
diffinit per formas quae forma de se communis
est, et non appropriatur ad esse rei nisi per
propriae materias uniuscuiusque rei. Physicus
autem dicet quod ira ascensus vel ascensus et
calefactio sanguinis circa cor, tangens propriam
causam efficientem quae est ascensus et calefactio
sanguinis, et propriam materiam quae est sanguis
cordis bulliens, et subjectum quod est cor. Et
horum quidem alius reddi materiam propria
scilicet physicus; alius autem reddi speciem et
intentionem formae simplicem et communem quae est
rei ratio communis. Nisi enim considerat rationem
sive intentionem communem rei, eo quod non descen
dit ad propria: ille autem considerat principia
realia quae sunt esse rei. Necessarium autem est
quod ista realia principia sint in materia huinu-
modi quae determinata et propria est, si erunt et
habent esse in natura. Si autem domus ratio
differens est assignata et considerata a dialec-
tico et mechanico qui naturalen imitatur, si autem
aria imitatur naturam. Dicet enim dialectico quod
domus est operamentum sive receptaculum prohibens
aeris corruptiones, quae sunt excellentia frigoris
ex excellentia caloris, et corruptiones imbrum et
caemutum. Mechanicus autem dicet quod domus est ex
lapidibus et lignis sic et sic compaginatis vel
ligatis. Alius vero ab utroque iustorum est, qui in
his materialibus determinatis praebeat in dificatione
sua speciem quae est, intentio domus propter
finem qui dictus est, hoc est, ut prohibeat corrupti-
tiones. (St. Albert, Lib. I, De Anima, Tract. I,
cap. 7.)

But a definition by physical parts may itself be dialec-
tical as in the case of "biped featherless animal." This is not
known as what is absolutely proper to man, it merely allows us
to distinguish man from other objects of our present experience.
It is possibly common to other things not yet known. But even if
it were not, so long as we do not see some connexion between
"biped featherless animal," and some principle intrinsic to the
very nature of man, the definition remains dialectical, as was
the case in the logical definition of substance. It nevertheless
is a definition, since what is common is possibly considered as proper.

2. Dialectical Terms.

The definition "rational animal" explains the term "man". But the definition "featherless biped animal" does not explain the term "man" in the same way. In the latter case, "man" stands for the definition, and cannot signify the nature absolutely, for the definition does not make it known absolutely. In other words, the proper definition explains the nature signified by the term "man" whereas when "man" is known only as "featherless biped animal", the term "man" stands only as a symbol for the definition. Or better, since the dialectical definition does not express the nature in itself, but only extrinsically and tentatively, the term cannot stand for the nature; it can signify only the unity which the mind has tentatively formed about something still inadequately known.

Another illustration may be taken from the ambiguous term "prudence". It is often said that "man is the most prudent of animals". Obviously "prudent" is not taken in its proper meaning explained by the definition "recta ratio agibilium". But there is a similitude of prudence in the higher brutes. "Prudence" may then be taken to stand both for what is prudent proper and for its similitude. But 'what is proper and its similitude' does not constitute a nature, that is, an "unum per se", but rather something which is one according to reason. The same holds for the term intelligence as understood in experimental psychology, as when we say "the intelligence of apes".

It follows that whenever a proposition contains such a term, the proposition itself will be dialectical.

3. Examples of Dialectical Terms and Definition
Taken from Experimental Biology.

The first term that we shall examine is that of 'protoplasm'. It is a fundamental term in biology and no one will dispute its importance to that science. It is our purpose to prove that this term is dialectical. Protoplasm is 'defined':

1) As a physical structure: "Protoplasm in the living state has the properties of a liquid system, containing particles of solids and droplets of immiscible liquids in a freely movable state. To the ultra-microscope it presents the characteristics of a colloidal system." (1)

2) As to chemical nature: "By analysis a great variety of organic compounds have been obtained from protoplasm. But it is obvious that it can not be decided in this way whether they were combined together as a 'giant molecule' in the chemical sense. The usual component elements of organic compounds are present, together with salts, and as much as 85 to 90 percent of water, etc." (Bayliss p.16)

3) "Protoplasm forms 'organs' for particular purposes; these organs appear and disappear, according to need. By fixing reagents, structures of various kinds, networks, alveoli, and so forth can be produced, but these structures have no resemblance to the living condition." (Bayliss, p.26.)

Protoplasm is "in a colloidal state". Here we remark that the physiologist is borrowing a term from chemistry. Colloids have been studied by chemistry and found to be: "A system of more than

one phase separated by surfaces, interfaces, of contact." However, the substances studied as colloids have been mostly inorganic, non-living. Even though we granted that the nature of a colloid was absolutely determined, we could not grant that living protoplasm was the same (identical) with a colloid. They could be identified only in some remote genus of common properties. It is to be noted that in the first definition, no one essential property is designated to make protoplasm a specific and determined colloid. (It is not necessary that we prove here that the term "colloid" is also dialectical.)

Being unable to get a proper definition of protoplasm, the physiologist multiplies his experiences and definitions. He gives protoplasm many natures. Its chemical nature is to be "a composition of many elements, water, proteins, lipoids, etc." Such a definition is purely descriptive, and of the vaguest kind. To find certain elements in protoplasm by way of chemical analysis, is certainly informative. But it does not explain the nature of living protoplasm. As for the so-called essential structures found in protoplasm, Ba/iss warns that they are due to the technique of the fixing and staining. Hence one must not be deceived in thinking that they characterize protoplasm in the living state.

From this brief analysis we must conclude that protoplasm is a dialectical term. By multiplying observations on various organisms, by experimentation, by borrowing scientific formulations from other sciences, the physiologist forms an ena rationis,
a logical entity, which is extraneous to any living organism. Some of the terms in the many definitions of protoplasm may, indeed, be proper terms, but nothing is certain about them. The dialectical character of the term protoplasm is at least vaguely realized by Woodger when he says:

"This concept (of protoplasm) plays an important part in biological discussions, and biologists certainly speak as if there were in nature a stuff to which the term protoplasm is given. But when we come to look into the question there does not seem to be any justification for this belief. By this I mean we do not find any such stuff in nature in the sense in which we find water in nature." (1)

The simplest biological part, which can be sometimes a complete organism, is the cell. This concept is biology's own. The generalization that all living things are composed of cells is probably the greatest generalization ever made in biology.

"The theory of the universality of cells was a very great generalization. It would have been a most important milestone in biological progress, even if it had been in itself less fundamental. For zoology had had no great generalizations before the cell theory was brought forth. There was nothing in the field of biology to compare with Newton's laws of motion in physics. Biologists would have been pardoned for despairing of ever bringing the whole living world under one point of view in some specific regard. The cell theory therefore was a great unifying influence. Contemplation of it must have prepared Biologists for other great generalizations... It is not improbable that the idea of evolution was adopted the more readily because the cell theory had shown that basic unity was as much to be expected among living things as in physics and chemistry." (2)

(2) Schull, Principles of Animal Biology, N.Y., 1929, p. 28.
What is a cell? First let us note that the definition of the cell has changed continually since its discovery one hundred years ago. Parts once considered essential have been discarded, until the simple definition remains: "A cell is a nucleated mass of protoplasm". But in the typical cell many parts are recognized; namely, the nucleus, cytoplasm, various bodies, and a cell wall. These parts, however, are apt to deceive one into making false conclusions.

"Although protoplasm shows so little structure in the living state, it might be thought that, by using fixing and staining reagents, more could be made out. Investigations... have shown on the contrary that the structures obtained in this way are produced by the reagents used, and that quite different appearances are found in the same kind of cells according to the fixing substance used." (1)

Considering that the cell is defined in many ways, and is ultimately reduced to protoplasm of one kind or another, we must conclude that the cell as a generalization is a dialectical entity. While there are cells in the body, the biologist certainly does not know or give the essential definition of a cell. "Cell" is another dialectical term in biological science. Hence scientific data employing the term should be accepted with reservations.

A quotation from Wilson will show how justified an attitude of reservation is.

"Even today we can not frame an adequate brief definition of the cell; but fortunately such a definition is unnecessary. In practice we need

(1) Bayliss, op. cit., p.12.
no more than the simple formula... a mass of protoplasm containing a nucleus... This definition must not be taken in too formal or narrow a sense. Like most other definitions in natural science, it must be allowed a certain flexibility, but in respect to essential accuracy the old definition remains today unshaken by the advances of half a century." (1)

(Note: It is hardly necessary to remark that the 'old definition' is not essentially accurate in the sense of being essentially true. It is simply a dialectical definition which even today has practical value.)

Let us now pass on to consider the organ as studied in biology. We shall especially scrutinize the concept 'function' which to the physiologist is far more important than 'structure'. Moreover we need to investigate whether the peculiar function of an organ is the same as its proper operation. In the first place, the structural definition of organs, and in fact of other parts of the organism, is by no means satisfactory to the physiologist. The structural definition is formed usually by selecting a type, e.g., the typical human heart, studying its physical characteristics, and including the important ones in a descriptive formula. The physiologist is interested in functions; and for him the study of functions experimentally is the crowning achievement of biology. (2)

Some of the definitions of the heart are as follows:

1) "The heart is the central pumping station of the circulatory system." (Hegner)

2) "The heart is a hollow muscular organ, situated in the chest. It is about the size of the closed fist, shaped like a cone..."

(2) C. Bernard, op. cit., p. 252.
The main substance of the heart is muscle; the tissue is called myocardium..." (Kimber and Gray)

3) "The heart is an organ which by its active muscular contraction, decreases periodically in its volume, and thus drives out the blood which has run into it during the time in which it was relaxed." (Bayliss, p. 675.)

The first definition is figurative, and could easily be applied to the pumping station in the water system of a city. The second definition is the long descriptive definition which is so frequently formulated when the mind can not find the proximate genus and specific difference of an object. Others of this type are more technical in their language; e.g., the heart is an organ of cross-striped muscular tissue. The third utility of these descriptive definitions is not questioned. But in regard to them the criticism of Woodger is not amiss:

"The visual shape of the heart is a synthesis of many visual appearances of the heart as it appears after picking. If microscopic sections are studied we have something more abstract still... Now consider the heart as it is in the living body during a concrete duration. Its characteristics are only exhibited over a period of one whole 'repeat' of its series of rhythmically repeated changes. But even this is not strictly true because the heart does not beat uniformly."(1)

The third definition, however, is not purely structural. As giving a function, an operation, it presents a deeper problem for us. Are they essential definitions because they give proper operations?

"Si aliquis bene definit eiusmodique animalis partem, non potest eam bene definire nisi per

(1) Woodger, op. cit., p. 328.
Is the proper operation of the heart known by the biologist? Is it to move the blood through the vessels? Is it periodic muscular contractility?

The movement of blood is an effect of the contraction of the heart. But the movement of blood is by no means the proper activity of the heart. It cannot be considered as coming from the heart as such; that is, this movement can not be predicated of the heart secundum quod ipsum. (Cf. p. 50-51.) Blood is partly moved by other muscular contractions; as for example, by the muscles of the legs in the course of walking. In some of the lower animals, e.g., in the amphioxus there is movement of blood without any heart. The blood is forced through the circulatory system by rhythmical contractions of the aorta. (2) Neither can we claim that rhythmical contraction (beating) of the heart is its proper activity. Other organs and tissues show the same property.

"There are certain important characteristics of muscular structure which are particularly well shown by the heart muscle, while there are others which have, as yet, been investigated in the case of this organ only, although they have in all probability, a general application. It may be noticed that certain of these were at one time supposed to be peculiar to heart muscle, although later work showed them to be also present in nerve and in voluntary muscle." (Bayliss, p. 453.)

"Many organs consisting of smooth muscle, and some with cross-striated muscle, such as the heart, exhibit, even when isolated from the influence of the nerve centers, a continued series of periodic contractions and relaxations." (Bayliss, p. 461.)

(1) St. Thom., In Meta, Aristotelis, Lib. VII, lec. 10, 1485.
(2) Wagner, op. cit., p. 398.
Such evidence as this warns us not to think that we can easily and certainly pick out an operation that is proper to an organ. To find many common properties between organs and tissues is not difficult. And so, although the scientific knowledge of the heart is vast, no scientific induction has given more than a probability as to the real definition of the heart as a part of the body. That the heart has its proper act, and activity, as an organ serving the nutritive faculty is certain. This proper activity is remotely caused by the soul, which gives esse to parts as well as to the whole.

"Anima est forma substantialiae corporis dans esse et specieae toti et partibus, et totum ex partibus constitutum est animae simpliciter." (1)

There are some organs, such as the eye, which the biologist seems to define essentially. Do not the philosopher and the biologist both say that the eye is the organ of vision, that seeing is its proper activity? Let us examine the use of the term 'vision'. For the biologist, the eye is a material organ composed of special material parts, viz., the cornea, lens, refracting media, retina, and so on. Because of its material and physical (as embracing the laws of optics) construction, the eye reproduces an image of an exterior object. Production of an image, which makes the eye comparable to a camera, is what is meant by vision.

In this way, the eye is the organ of vision.

"It is obvious that a mere sensibility to light and shade is of comparatively little value, until a mechanism is developed by which images of external objects are formed on a sensitive

(1) St. Thom., In Ques. de Anima, art. 10, ad 16, (495a).
surface composed of a multitude of elements connected with separate nerve fibres. By this means a picture is, so to speak, conveyed to the brain. (Hayliss, p. 516.)

For the philosopher the eye is a material organ serving the faculty of vision. Vision is an immanent operation, and not to be confused with an organ functioning according to the laws of optics and chemistry. The reproduction of an image on the retina is not vision, even though the image may always accompany an act of vision. As a vital operation, vision does not have as its end the production of an image, but rather the union of the faculty intentionally with an object.

"Visio est actus vitalis, ergo elici debet a potentia vitalis et a concursu animae, nec sufficit media virtute diffusa elici in aere, quia sic primo inciperet visio, ubi non est principium vitae, scil. extra ipsum videntem, et sic illa visio non esset actio vitalis...." (1)

"Licet cognitio sensus externi sit actio immanent, non tanen de necessitate est productio nec resipit terminus ut immutatum a se, sed ut intentionaliter at objective unitum, virtualiter tanen potest haveris viae productionis; sicut cognitio in quantum dictio product verbum, et amor in quantum spiratio impulsum, et senescet externa productum reperantiones seu magiam, non intras se, sed in sensoribus externis. Ex se tanen actus immanens non est actio per modum motus et viae tendentis ad ulteriores terminum, sed per modum ultimum actualitatis." (2)

In this case, we have examples of a dialectical term and of a dialectical definition. Vision as used by the philosopher and by the biologist is not really the same vision. It is the

(1) John of S.Thom., Nat. Phil. IV, q.5, a.2, p.141 (Reiser).
(2) John of S.Thom., Logie Part II, q.22, a.4, p.1714 (Reiser).
same vision only as belonging to some remote genus, in which the mind selects common accidents in order to define vision. It is the vision in the remote genus which is predicated identically of the biologists doctrine of vision and what vision really is. Hence vision, as used in biology, is a dialectical entity and is open to continual change. The change (movement) is apparent in the biologist's technique. He has already discovered many 'truths' about the eye, but there is much more to be learned. He knows the structure of the eye, but such problems as the movement of the cones, and the visual purple are far from being solved. There are the current theories, but these must be studied and verified further. In this way, the function of vision, which is the proper function of the eye, will become continually better understood. But the biologist, as much, will never give us a real definition of vision no matter how great discoveries he makes about the eye in his laboratory.

Let us summarize what we have learned about the method of biology as it studies organs. The physiologist studies organs from the point of view of function. One of more functions may be considered to characterize an organ. In no case does the physiologist do more than describe the functions which he studies. He is continually checking, improving, and even discarding the current doctrines. Hence there is continual movement or change. This change affects the very definitions themselves, which we are sometimes tempted to consider, as proper, and unchanging definitions. There is nothing improper in these changes because the method being used is dialectical. And as such, it is
to be carefully distinguished from that type of reasoning which is more than a method. Moreover, we must be cautious in accepting experimental terms as identical with those which we use according to their strict philosophical sense. This is the wise course, even though it is sometimes very difficult to show that the term as used by the scientist and by the philosopher is not the same.

In conclusion, there is a very important observation to make here. In claiming that biological doctrine is fundamentally dialectical, we do not claim that it is so much a product of the mind that it has no relation with the real. If we say that cells, and organs, are dialectical entities do we mean that no such things as cells and organs really exist in nature? No such interpretation is to be put on our doctrine. Cells, or those entities which we call cells, can be seen by the eye, or at least with a microscope; so too can homogeneous tissues and distinct organs. These certainly can be known to exist really, and can be designated by terms. But the biologist does more than merely assent to their existence. He is intent upon analysing them as biological parts of an organism. One by one he will study them, and tend toward knowledge of their 'nature' by observation and experiment. The cell, to take but one example, will be studied from all possible angles: as a chemical compound, as a physical structure, as a body subject to the laws of energetics, electricity, etc. Its various functions and relations to other cells will be determined. And so, from experimental work, the cell will be known better and its definition will approach more and more to
a proper definition. This cell will hardly be the cell known by
the ordinary person who knows no biology. On the other hand, the
biologist will admit that he doesn't know a great deal about the
cell. He will nevertheless not remain content with the little
knowledge he has. Rather he will continue his experiments and
improve his knowledge of the cell. So in fact, the cell of the
biologist is a dialectical entity; it is neither nothing nor
something absolutely determined. As a dialectical entity it is
open to change. And this is true of the other entities entering
formally into biological science.
APPENDIX I

History of Scientific Method in Biology.

As a background for the experimental method of Claude Bernard, we have presented in this appendix a concise history of the scientific method in biology. The great, if not rapid, advance, of biological doctrine through more than two thousand years will be evident. However, our principal aim is to search for the nature and perfection of the method being used in the various periods under review.

Yet such a proposal presents at the very outset a difficulty. What is the scientific method in biology? Is not the determination of this method the aim of this whole work? This difficulty can be resolved satisfactorily for the present by having a general notion of the scientific method as used today. We shall keep in mind, as Singer says, that there are three distinct steps in the art of scientific discovery. There is, first, the choosing of facts; secondly, the formation of a hypothesis that links the facts together; and thirdly, the verification of the hypothesis experimentally. These three operations, more or less present, more or less distinctly recognized, are considered essential to the scientific method in biology, as well as in the other experimental sciences. (1) It is not necessary now to clarify the notion of hypothesis, or the exact relation between facts, hypothesis, and verification.

What we need to remember is that in the scientific method there is an interplay of facts and supposition. It is these two factors that we should observe and relate in the course of our history.

A. Ancient Science.

The biological sciences, among the Greeks, were first studied in connection with medicine. Hippocrates (500 B.C.), called the father of medicine, and other ancients have left us records which instruct us on the nature of early medical science. "If we were to examine these early medical works, we should find that whole departments of knowledge, which are now considered necessary for a doctor are entirely absent from them. Thus, for instance, they betray little or no anatomical, physiological, or chemical knowledge. The doctor of those times had no instruments for examining patients, such as listening tubes, thermometers, or magnifying glasses. He had only his own senses to guide him and he had very little record of what those who had gone before had seen of disease. On the other hand, his senses were well trained and he observed carefully and well, and put down what he saw with a wonderful eye for what was essential."(1)

There was, therefore, in early science a great reliance on careful observance of the plain facts. While not without his theory as to the cause of disease, Hippocrates was contending against superstition which attributed sickness to the gods or

(1) C. Singer, op. cit., p.2.
to the demons. There was an effort to teach people that
diseases were due to natural causes. Philosophical doctrine
was not without its influence. The ancients supposed that
all matter was composed of four elements: earth, air, fire,
and water. Following this idea, Hippocrates supposed that all
human bodies are composed of four humors: blood, yellow bile,
black bile, and phlegm. These four humors had a special rela-
tionship to the four elements. Health depended on the correct
proportion between these humors, and various disproportions of
them accounted for various diseases. In spite of the crudeness
of this explanation there is an advance over the belief that
diseases were caused by the gods.

"The method of the Hippocratic writers is that known today,
as inductive. These men remained for the most part patient
observers of fact, sceptical of the marvellous and unverifiable,
hesitating to theorize beyond the data, yet eager always to
generalize from actual experience; calm, faithful effective
servants of the sick. There is almost no type of mental activity
known to us that was not exhibited by the Greeks and cannot be
paralleled from their writings; but careful and constant return
to verification from experience, expressed in a record of actual
observation, the habitual method adopted in modern scientific
departments, is rare among them except in these early medical
authors." (1)

During the development of the Greek medical school, centered
around Hippocrates and his writings, medicine in the western

(1) C. Singer, Greek Biology and Greek Medicine, Oxford, 1922,
p.91.
Greek world was being influenced by the philosophy of Empedocles of Agrigentum (430 B.C.). He spoke of the blood as the seat of the 'innate heat', which was identified with the human soul. His teaching led to a belief in the heart as the center of the vascular system and the chief organ of the 'pneuma' which was distributed by the blood vessels. The pneuma was equivalent to both soul and life, but it was sometimes identified with air and breath. Some of these terms were used in medicine for two thousand years.

Medicine, of course, advanced as more attention was given to anatomy, physiology and the kindred sciences. Especially famous is the Alexandrian school (300 B.C.) to which credit is given for organizing medical teaching. Finally there is the great figure of Galen (130–200 A.D.). The works of Galen alone form about half of the mass of surviving Greek medical writings, and occupy, in the standard edition, twenty-two thick, closely printed volumes. These cover every department of medicine, anatomy, physiology, pathology, medical theory, therapeutics, as well as clinical medicine and surgery.

The general standpoint of the Galenic writings is not unlike that of the Hippocratic writings, but the noble vision of the lofty-minded, pure-souled physician has passed away. Galen was an ingenious physiologist, a born experimenter and an expert anatomist. He possessed a good knowledge of the human skeleton and an accurate acquaintance with the internal
parts so far as this can be derived from dissecting animals. He was equipped with all the learning of the schools of Pergamon, Smyrna, and Alexandria; he had practised in Rome. It is to his credit that he repeatedly acknowledged his debt to Hippocratic writings. Galen is criticised for putting teleological explanations in many of his writings. Singer says that Galen did not hesitate to intrude his religious beliefs into his scientific works.

Turning to Aristotle (384–322 B.C.), we have under study one who is probably the greatest philosopher and biologist of all times. His biology appears in continuity with his philosophy. This has already been spoken of when we discussed the movement of concretion in the study of living things. (See page 9.) Aristotle's chief biological works are his Parva Naturalia, Historia Animalium, De Partibus Animalium, De Motu et De Incessu Animalium, and De Generatione Animalium. Only those who carefully scrutinize these various works can realize the great work done by him. They contain a prodigious number of first-hand observations. It has always been hard to understand how one investigator could collect all the facts that he did. It is claimed that Alexander the Great had thousands of men in every part of the then known world assisting him in composing his Historia Animalium. The latter work proves that he was at his best in the department of natural history. He also wrote on the organs and parts of the body, that is anatomy and physiology, but with less accuracy. Aristotle lacking the instruments used today in
science is considered more of an observer than an experimenter. However, he was familiar with the scientific method of his day, and certainly experimented according to the fashion of his times.

In the beginning of De Partibus Animalium Aristotle gives what he calls the canons to be followed in the intelligent study of biological problems. They fall in line with what has been stated before in his Physics. He advocates that one treat general properties of body and soul first in the study of living things. In this way repetition of common facts can be avoided as one treats each animal or function separately. Afterwards, separate species and their peculiar properties can be studied. (1)

With the nature and the number of animal parts described in the Historia Animalium, Aristotle next inquires what are the causes which have in each case determined the composition of each part. (2) Which are the principal causes for the natural scientist to determine? He teaches that there are two; namely, final cause, and efficient cause. In citing these two causes Aristotle differed from other Ancients who made material cause the principal one of study. For Aristotle matter is disposed in this or that manner in a thing, because of the final cause.

"The causes concerned in the generation of the works of nature are, as we see, more than one. There is final cause, and there is motor cause... Plainly that cause is first which we call the final one. For this is the reason, and reason forms the starting point alike in the works of art and in the works of nature... For if a house


or other such final object is to be realized, it is necessary that such and such material shall exist... First this, and then that, is produced... until the end, and the final result is reached... As with this production of art, so also is it with the productions of nature." (1)

The following passage on the heart gives us an idea of Aristotle's method.

"As has been said already, the blood-vessels run all through the other viscera, whereas none passes through the heart; which clearly shows that the heart forms part of the blood-vessels and is their source. Which is reasonable enough; since the center of the heart is a body of dense and hollow structure, and this is full of blood; dense to guard the source of heat; and the store of blood is obviously there because that is the starting point of the blood vessels. In none other of the viscera and in no other part of the body is there blood and yet no blood-vessels; everywhere except in the heart the blood is contained in blood-vessels. And this too is reasonable, as the blood is conveyed by the channels away from the heart into the blood-vessels, whereas none is thus conveyed into the heart from elsewhere; the heart is itself the source and spring of the blood, and the first receptacle of it. All this, however is more clearly brought out in Dissections and Formative Processes, where it is shown that the heart is the first of all the parts to be formed and has blood in it straightway. Further all motions of sensation, including those produced by what is pleasant and painful, undoubtedly begin in the heart and have their final ending there. This is in accord with reason; since, wherever possible there must be one source only; and the best situation for that is the center, because there is only one center, and the center is equally (or nearly equally) accessible from every direction...

"In embryos, as soon as they are formed, the heart can be seen moving before any of the other parts, just like a living creature; and it does this in virtue of its being the source of their nature in all blooded animals. Another piece of evidence to support this is that all blooded creatures have a

(1) Aristotle, op. cit., Bk. I, 1, 639b, 11; 639b, 27-35."
heart: why? Because they are bound to have a source for their blood. All blooded creatures, it is true, have a liver too; but no one would care to maintain that the liver is the source either of the blood or of the whole body, because it is nowhere near the place of primary and governance, and, also, in the most highly finished animals it has something to counter-balance it, as it were, viz. the spleen." (1)

It is not the place here to explain or to justify the close relation which Aristotle established between his philosophy and his biology. Evidently his method was not scientific in the modern sense of that word. It is not a method which would appeal to present day scientists. For, Aristotle in his work asked questions very foreign to the interest of the modern biologist who is seldom interested in the ultimate nature and origin of life. "The business of the modern biologist is mainly with vital phenomena as he encounters them and he is not concerned with the deeper philosophical problems. The man of science considers a part of the Universe where the philosopher makes it his business to regard the whole. With Aristotle this modern scientific process of taking a part of the sensible Universe, such as a particular group of animals or the particular action of a particular organ, and—considering it in and by and for itself without reference to other things, had not fully emerged. Philosophy and science are still inextricably linked and there is no clear demarcation between them." (2)

3. Foundation Period of Modern Biology.

After Galen (130-200) there was no period of great biological

(2) C. Singer, Greek Biology and Greek Medicine, Oxford, 1922, p.53.
activity until the sixteenth century. The teachings of
Hippocrates and Galen were accepted almost blindly until
the time of Harvey (1578–1651). The decline of science in
this interim is attributed by Singer to the mental attitude
of the ruling class in the Roman Empire. The Romans being
practical minded did not consider the theoretical investigation
of nature important. In the period of scholasticism, philosophy
and theology absorbed the interest of the learned. Yet we must
mention the names of Roger Bacon and of Albert the Great. Bacon
already had a grasp of the experimental method, but he did not
work in the field of biology. Albert the Great is famous for
his works on animals and plants. While he is criticised by Singer
for following Aristotle too slavishly, he is nevertheless
recognised as remarkable for his period, because he personally
made careful observations. And too, he at times corrects the
statements of Aristotle. It is seldom that Saint Albert receives
from historians of science the full credit that is due him.

William Harvey may be considered the precursor of modern
biological science. He is famous for his work on the circulation
of the blood in the human. The thoroughness of his work is
attested by such a fact, that he examined the heart action of
over forty other animal species, besides that of man. In the
use and study of specimens, and in the employment of dissection,
Harvey is not unique. He was but following the method of such
famous men as Veraslius, Mustachi, Fallopio, Fabricius of Aquae-
pendente, and others. Yet Harvey seems to have grasped the impor-
tance of experiment. However simple these experiments were, they confirmed his theories on circulation. They were actively performed for the sake of verification, even though the exact significance of verification may not have been understood. Moreover, in arguing for continuous circulation Harvey employed simple mathematics. Consider, he says, the capacity of the heart. Suppose that the ventricle holds but two ounces. If the pulse beats seventy-two times in a minute, then in one hour the left ventricle will force into the aorta no less than $72 \times 60 \times 2 = 8,640$ ounces; or $540$ pounds of blood, which is three times the weight of man. Where can all this blood come from? Where can it go? There must be a circulation of blood. In reasoning thus, Harvey is probably one of the first to use mathematics in biology. Its use presages the use of other sciences in studying biological phenomena. For at times, some departments of biology have been practically reduced to mechanics, physics, or chemistry.

To Harvey, then, belongs the credit of giving in physical terms, an adequate explanation of a bodily process. "His work is not only the starting point of modern physiology, but it is also the first milestone on the road to the modern rationalization of biological thought." (1) With this rationalization such concepts as 'innate heat', 'animal spirits', 'pneumatic force' were gradually abandoned. In their place were substituted simpler concepts of the new biology, of physics and of chemistry. As part of this movement Harvey belongs with the moderns. As still remaining conservative, adhering to Aristotle and to Galenic teaching,

Harvey belongs to the old order. For this reason, he is something of a precursor of modern experimental biology.

In the foundation period, changes also occurred in regard to scientific facts, which we have said are an important element of the scientific method. If we except Aristotle, who is said to have had thousands of other men helping him to investigate nature, biologists of the early centuries were as much in need of facts, as of theories to explain facts. Now facts became so numerous as to overwhelm the investigators. For example, the scientific work of Thomas Morelet (1590) shows a naturalist unable to handle the wealth of material collected from many countries.

His description of grasshoppers and locusts reveals his confusion:

"Some are green, some black, some blue. Some fly with one pair of wings, others with more; those that have no wings they leap, those that cannot either fly or leap, they walk; some have longer shanks, some shorter. Some there are that sing, others are silent. And there are many kinds of them in nature, so their names were almost infinite, which through neglect of Naturalists are grown out of use." (1)

Briefly, the causes of the new wealth in facts are as follows. First, there was the humanistic movement which was reviving the learning of antiquity. There were the scientific voyages which brought information from all parts of the world. And too, there was the new interest of men in the natural things

(1) C. Singer, op. cit., p.173.
that surrounded them. Secondly, not only were a multitude of facts discovered, but they were also made the common property of biologists. In addition to the new art of printing, there were other very important channels of communication. There were the newly formed academies, the collections, the museums, and the scientific journals. These were powerful organizing forces for biological study and progress. Thirdly, there was the invention of the greatest of biological instruments, namely, the microscope. By itself, it opened up a world of mysteries to the biologist, which will never be completely understood scientifically. While the first microscopists had no effective followers until the nineteenth century, there works were not without influence. "The general tone of the biological writings that followed them is very different from that which precedes them. Variety and complexity now begin to overawe the naturalist. Amidst the multiplicity of phenomena, order must be sought if knowledge is not to lose itself in detail. So it is that in the age that follows, the importance of classification becomes greatly emphasized." (1)

We must not think that facts alone can constitute science. Bacon himself in advocating the search and categorizing of facts seemed to think that facts might be passed through a sort of logical mill. The truth is that the experimenter must meet facts with the activity of the mind; a judicious choice of facts must be made in the scientific method. Very often biology has failed

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(1) C. Singer, op. cit., p. 171.
to supply this necessary mental activity, and in these periods biology has remained almost purely descriptive. For example, the use of the comparative method demanded little more than a recording of observed characteristics in plants and animals. The method, however, did turn out to be of value chiefly because it prepared the way for the theory of evolution. We must inquire, therefore, what change, if any, occurred in the experimental method by way of a change in the part played by the mind in this method.

We find a distinct change in the part played by the mind. We saw that Hippocrates used the inductive method in his profession; that is, he generalized the discoveries that he made by simple observation. Certain physical signs meant a certain type of sickness, which would end in such and such way.

In later centuries, we have the generalizations of Galen, Veresalius, and Harvey. They were often enough confirmed by simple observation or experiment. But now we begin to see the mind, in the presence of such a chaotic mass of material, applying a prorvisorional principle of order. This is to say that the biologist, without realizing it began to use supposition, or hypothesis. For example, one of the greatest problems facing the biologist of the sixteenth century was how to order the many specimens of plant and animal life. A start was made in the solution of this problem, by Obel, Casalpini, and Bauhin who chose, each for himself, a principle of classification. Obel used the leaf as the
basis of grouping; Casulpi, the flowers and fruit; Bauhin, the twofold distinction of genus and species. Later, Jung, Ray, Tournefort, Linnaeus, and Cuvier perfected a system which has a place for every known animal and plant. Hence we have evidence of a method in which facts are manipulated by the mind working according to provisional principles, or hypotheses. In this method the problem is not solved for all time, for, any system of classification is open to changes, as we well know, from the theories of evolution, embryology, and heredity. But the method does give the feeling that something is being accomplished.

To trace the history of biological method from the rise of classificatory systems to the present day problems can not be undertaken here. Yet there are some other facts of history which bring out the importance of the question of verification. The rise of the comparative method, the growing interest in palaeontology, the study of the distribution of living things in space and time, all prepared the way for the problem of evolution. From certain facts, the biologist has supposed that living things have evolved genetically one from the other. This is the hypothesis of evolution. In this hypothesis the resemblances rather than the differences (as in classification) are considered the more important. Having posed evolution as though it were a fact, the biologist must then find other facts, or formulate other hypotheses to explain the cause of this evolution. Thus the theories of Buffon, of Lamarck, Darwin, and many others entered into biological history. Whatever the one-time greatness of these
theories as to the causes of evolution, the theories themselves were never strictly verifiable by experimentation. For evolution is a problem of race development, in the usually accepted meaning of the term. The lack of the verifiability seems to explain why men like Woodger consider the problem of race development "of little importance for scientific biology." (1) On the other hand, if evolution is studied as a living problem, these solution may be hidden in the problem of heredity, then it is certainly one for modern experimental biology. Then the demand for verification can also be met. Failing to meet this demand, a hypothesis must be rejected as scientifically useless; it must be considered sterile for science. To what extent modern biology verifies is studied in the thesis.

B. Contemporary biology

In contemporary biology the problem of evolution no longer holds the place of honor. There are a handful of problems, considered key problems, which open the door to the investigation of many associated problems. These key problems are as follows:

1) The Cell and the Organism.

This problem is the first carry-over from the foundation period of modern biology. It is concerned with the constitution of living matter, the construction of plant and animal organisms, the nature of growth and repair.

2) The Problem of Vital Activity.

What are the simple operations of living organisms? Does chemistry, physics, the laws of energetics explain them fully? This is the problem in which biology easily becomes confused with philosophy. Other problems are those of muscular, nervous, etc. activity.

3) The Problem of Biogenesis.

It includes not only the question of the origin of life, but also the questions of infection, disease, and immunity. These are the subject matter of bacteriology.

4) The Problem of Individual Development.

What are the steps by which an individual develops? This is a problem that received the attention of such men as Fabricius and Harvey. Hence it is an old problem. The study of development is now known as embryology.

5) Mechanism of Heredity and Variation.

At present this problem is in the position of favor. It includes the special problems of the germ plasm, sex, the Mendelian laws of inheritance, and mutation. Theories of heredity are being offered to explain evolution.

It would be wrong to think that these problems are lending
themselves to quite easy solution, because of the great advance of biology. In the opinion of Alexis Carrel those who are studying life phenomena are as men lost in a jungle, in a forest where innumerable trees change continuously in place and form. Moreover, these students feel themselves buried under a mass of facts which they can describe, but which they are not able to define by algebraic formulas. This is disappointing to some scientists who would like to do in biology what is done in those sciences treating inert matter. These latter sciences, namely, astronomy, mechanics, and physics, have as their basic concepts which are expressed in precise mathematical language. Their discoveries can be summed up in a mathematical formula. But this is not true to the same extent of biology. (1)

Indeed biology has, in studying its subject, turned to other sciences for help. This is especially noticeable in physiology, in which biologists have used concepts of physics, chemistry, and mechanics to carry on their investigations and to express the results. Liebig used chemistry to explore vital activity; Karl Ludwig relied on physics and chemistry; others, such as Maxwell, employed the laws of energetics. But whether the partitioning of biology among other sciences is a good thing is disputed. There are such well-known biologists as Haldane and Carrel, and writers on the theory and history of biology, such as Woodger and Singer, who demand that biology be autonomous, and free to formulate its own basic concepts.

According to Carrel the real nature of living man is little known today because the biologist has not been studying man in his entirety, in his living part, and in his own relations with the exterior world.

Such reflections will disillusion us that all is well in theoretical biology, and that we are going to find the analysis of the methodology of biology an easy one. There remains before us the problem of the nature of the scientific method in biology. Only through an analysis of this method can we know the nature of biological knowledge.
# Historical Outline of Biological Method

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APPENDIX II

Charles Woodger's 'Biological Principles'.

Charles Woodger has written a remarkable book BIOLOGICAL PRINCIPLES. He attempts a thorough discussion of our problem. In his general introduction he exposes the lack of interest in the critical aspects of biology. Next he covers those sciences which he thinks are involved in the problem. In varying degree they are metaphysics, logic, psychology, and epistemology. He questions the value of the utilitarian aim in science.

In the first part of BIOLOGICAL PRINCIPLES Woodger works out his own epistemology, (partly borrowed from Professor Whitehead), in refutation of the phenomenalism of Mach, Pearson, and others. Then follows an analysis of the metaphysical concepts of substance and cause. The logical aspects of the problem are presented under the heading: Demands, Postulates, and Subjective Factors in Knowledge. It is regrettable that Woodger ignores the help which traditional Philosophy could give him on these problems of metaphysics and of logic. Having chosen his principles, Woodger in the second part of his book studies "what appear to be the most general and most deep-seated difficulties and peculiarities of biological knowledge." He examines these important biological antitheses as to their logical procedure and their ontological assumptions. He hopes to show that these antitheses are largely due to the confusion regarding fundamental principles. Until there is agreement on the principles, no solution of the problems
will be possible.

In analyzing the antitheses Woodger makes a very valuable contribution to the methodology of biology. A thorough analysis of mechanism, as opposed to vitalism, is made. The mechanistic theory of life whether identified 1) with strict laws of mechanics, 2) with physico-chemical laws, or 3) with a machine organization, is inadequate for biology. Woodger of course readily acknowledges the progress that biology had made by using the mechanistic point of view. But he contends that it has its limitations. Hence he demands that biology use its own methodological principles, rather than borrow old ones from physics. The new principles are based on organization, and on function.

"If the concept of organization is of such importance as it appears to be, it is something of a scandal that biologists have not yet been un to take it seriously, but should have to confess that we have no adequate conception of it. The first duty of the biologist would seem to be to try and make clear this important concept." (1)

Organization, on the other hand, does not lead the biologist necessarily to admit vitalism. Whatever the truth of vitalism, the doctrine is of no methodological use to biology.

"To invoke entelechies as agents at the point where the materialistic mechanism gets into difficulties has not proved to be of any value from the methodological standpoint because it is too successful, to general and gives us no light upon the particular case. Entelechies are in just the same position as energy when the latter is regarded as 'the cause of changes in matter', and such an appeal to imperceptibles is contrary to the present tendencies of scientific thought." (2)

(1) Woodger, op. cit., p.291.
(2) Woodger, op. cit., p.266.
In the chapter on biological explanation, the author shows how the procedure in biology may be realistic. The method is to study organisms in their parts and according to various relationships of parts. The notion of function is reduced chiefly to "all the processes ordinarily said to be going on in an organ." The problem of preformation and epigenesis, which is another antithesis, is skillfully simplified by distinguishing individual from race (evolutionary) development. Woodger holds that if organization is the important concept in biology, then the development of the individual is epigenetic.

"My way of expressing the facts of development in terms of the cell-type of organization as being spatially repeated in division brings out clearly this epigenetic character, because we see at once that the very first act of cleavage raises the level of organization." (1)

Woodger not only discusses the hypothetical character of race evolution, but also rejects it as a problem for the scientific method. In his chapter on teleology and causation, Woodger lacking a true understanding of final cause fails to give the excellent analysis that one expects of him. He easily shows that biologists speak of purpose under one term or another, but the exact relation of final cause to the experimental method in biology is not clearly given.

In his conclusion Woodger discusses the future of biology. He calls for a proper development of the three important aspects of any science: namely, the investigatory, the speculative, and

(1) Woodger, op. cit., p. 353.
the critical aspects. He regards the neglect of the critical aspect a real hindrance to the more efficient and rapid progress of biology.

In another work entitled *The Axiomatic Method in Biology*, Woodger applies the mathematical method to biology with the purpose of converting it into an exact science. He is one of the first to make this application, and he humbly calls it an experiment. (1)

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PROPOSITIONES III. COLUMNA

1. Incarnalitas est radix cognationis
2. Cognitio secundum se objective est
3. Cognitionis sensitivae objectum est contingens, non autem reduplicative
4. Cognitio in brutis est anomalia quaedam
5. Secundus usus logicae potest inducere scientiam.
6. Causa specialissimae logicae non potest indicere scientiam
7. Fortuna et casus sunt causae diversae
8. Causa indeterminata est causa irrationalis.
9. Casus et fortuna dicuntur ex parte effectus
10. Natura est ratio indicata rebus ad arte divina ut possint agere propter finem.
11. Natura non dicitur de substantiis separatis
12. In naturalibus causa finalis inditae necessitatem quatenus principium ratiocinat in

mism, non autem quatenus executionis.