THE ORGANISMIC CONCEPTION IN BIOLOGY AS PROPOUNDED BY LUDWIG VON BERTALANFFY

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CHAPTER I

INTRODUCTION

The word 'biology' is used here to mean functional biology as related to botany and zoölogy. Branches of applied biology, such as medicine, are excluded and so is human psychology. Functional biology is essentially concerned with the operation and interaction of structural elements of the living organism from atoms and molecules to organs and whole individuals. The question of how living things function and operate is foremost in the functional biologist's mind. In restricting the meaning of biology essentially to functional biology and thereby not taking into account evolutionary biology, I do not invalidate my analysis and criticism of Bertalanffy's organismic conception, for much of what he claims for his position is meant to apply to functional biology. In fact, Bertalanffy's definition of a living organism is one which does not attempt to capture the historical or evolutionary character of living things.

As I believe my thesis will to some extent show, there is a reciprocal effect between the science of biology and the philosophy of biology in that the problems and methodology (both experimental and theoretical) of the former give support and stimulation to the more general ideas and speculations within the latter, while at the same time the biologist's philosophical beliefs determine to a large extent those biological problems which he believes to be most urgent as well as the kind of analysis and technique which in his view will be most effective for the solution of these problems. If we believe with Popper that the actual procedure of scientific discovery begins with a conjecture, hunch, or intuition, often after only one observation, then it becomes evident just how significant our philosophical beliefs can become in determining the course of scientific development. More important to my essay, however, is the consideration of some of the allegedly unique questions and issues biology and phenomena of life present to philosophy. In this respect most of the questions dealt with which relate to biology in particular as opposed to science in general are connected with the larger question of the autonomy of biology vis-à-vis the physical sciences. In reflecting on some of the outstanding features manifest in living organisms, such as complexity, reproduction, self-nutrition, regulability and spatio-temporal organization, it would almost seem to follow as a matter of course that biology must occupy a unique position in the sciences in some way or other. Among some of the questions which will be discussed in connection with the autonomy of biology will be (1) In what sense can the concepts and laws of biology be reduced to physics and chemistry? (2) Does the organization of living things (e.g. hierarchical organization) dictate limitations in the acquisition of knowledge pertaining to higher levels of organization through experimentation and theorizing in connection with the atoms and molecules at the lowest level of the living system? (3) Is it necessarily the case that because we are in possession of a well-tested physico-chemical theory of what we believe to be the ultimate constituents of a living organism, the properties manifested by a living organism must be explainable in terms of this physico-chemical theory? (4) Does the complexity of living organism somehow require that we forego physico-chemical determination of various biological phenomena and thus as biologists become largely involved with the development of what Bertalanffy calls "system laws" or "laws of organic wholes"? (5) Is the relationship of part to whole fundamentally different in the field of animate objects and in the field of inanimate objects?

As the title of my thesis indicates, I will be largely concerned with the organismic conception in biology according to Ludwig von Bertalanffy. However, since Bertalanffy's position claims to offer a viable alternative to either mechanism or vitalism these latter positions will, of course also require examination. Very briefly, a mechanist is of the opinion that biological phenomena will ultimately be shown to be explainable solely in terms of laws recognized within physico-chemical theory. Vitalists react strongly against such a position, claiming instead that living organisms show by their behavior that they are partly constituted of a non-physical component which directs the outcome of the very complex life processes. The non-physical character of this alleged constituent operative within life phenomena guarantees the autonomy of biology with respect to physico-chemical theory.

In Chapter II I will examine Driesch's neovitalistic theory of entelechy in some detail and show why I think that Driesch's position is untenable and how such a viewpoint is inimical to the expansion and development of biology. In choosing Driesch to exemplify neovitalism I am aware that he is not as contemporary as the mechanists I have chosen for exemplifying the mechanistic viewpoint. However, Driesch undoubtedly is one of the most influential vitalists of this century, and although he did not have the opportunity to consider any of the recent findings in biology I believe that he would have maintained his theory of entelechy had he lived to become acquainted with these developments. For in fact, the phenomena relating to embryology and morphogenesis which led Driesch to develop his vitalistic theory still remains largely unexplained in terms of any scientific theory.

The organismic conception represents a currently popular speculative position in contemporary biology, largely because it claims for biology an independent position with respect to the physical sciences without apparently having to resort to vitalistic forces in order to do so. Some of the better known advocates of this position are W. E. Ritter, E. S. Russell, J. S. Haldane and W. E. Agar. Bertalanffy who is currently professor of theoretical biology at the University of Alberta, represents the most prominent proponent of this position. In developing and justifying his views, Bertalanffy utilizes a vocabulary which to some extent is peculiar to the organismic conception, including such words as "whole," "part," "sum," "summative property," "non-summative property," and "hierarchical order". In explicating Bertalanffy's position it will be necessary to determine as clearly as possible the concepts which, according to Bertalanffy, the above words express. Only then will we be able to assess such statements as:

> A specific gestalt principle immanent in the organism is to be supposed . . .. This conception is not vitalistic, for it does not assume any transendent factors interfering in living nature . . .. But it is organismic, since the organization immanent in living systems is considered to be specific, and so the autonomy of biological systems is contended.[1]
>
> The whole is more than the sum of its parts.[2]
>
> Biological laws are not a mere application of physico-chemical laws . . .. It appears that the level of biological laws is a higher one, as compared to that of the laws of physics.[3]

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[1] Ludwig von Bertalanffy, Problems of Life: An Evaluation of Modern Biological and Scientific Thought (New York: Harper & Brothers, 1960), p. 65.

[2] *Ibid*., p. 147.

[3] *Ibid*., p. 157.

> The organismic method is to find exactly formulated laws for organic systems as a whole.[4]

The essential points which I intend to make with respect to Bertalanffy's organismic conception are:

(1) The meaning Bertalanffy gives to certain key words of the organismic conception (e.g. "whole," "summative property," and "non-summative property") are inadequate and sometimes unintelligible in the context in which he subsequently uses such words. This leads to serious ambiguities regarding the statement of fundamental principles of the organismic conception.

(2) Other views which Bertalanffy expresses in connection with his organismic conception are seriously deficient, not because they are formulated by means of obscure concepts but because Bertalanffy does not provide any clear interpretation for such statements in a biological context.

(3) By introducing certain modification in the formulation of some of the views expressed

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[4] *Ibid.*, p. 171.

(3) by Bertalanffy, I will endeavour to emphasize those features of his position which I believe to be of value to biology in particular and to the natural sciences in general. In this respect I hope to indicate how an overly mechanistic position in biology can stifle biological research and discovery.

(4) Bertalanffy, although he advocates a kind of absolute dichotomy between biological law and physical law, nowhere provides an adequate justification, in non-vitalistic terms, for such a belief.

(5) Bertalanffy is inclined to maintain a belief in what he calls laws of organic wholes even in the face of recalcitrant empirical evidence.

(6) Lastly, in the light of (1) - (4) I conclude that there is strong evidence for the thesis that underpinning Bertalanffy's organismic conception is an implicit belief in vitalistic agencies in the form of organizational forces opperative within life phenomena.

CHAPTER II

VITALISM AND MECHANISM

Before entering upon a critical analysis of the organismic conception of life as presented by Ludwig von Bertalanffy, it is important to acquire a reasonably clear conception of two opposing theories which have been propounded for at least the last two thousand years. I am speaking of course of vitalism and mechanism. Both these views have claimed to give a true account of the nature of life; however it is largely because of a dissatisfaction with these two alternatives that a third position has been developed in the form of an organismic or general system theory. In order to fully appreciate whether or not von Bertalanffy has developed a viable position which indeed is fundamentally different in character from either vitalism or mechanism, I will begin by examining the character of vitalism and mechanism.

VITALISM

One of the earliest and best known accounts of the vitalists' position is that presented by Aristotle. In fact Aristotle laid the foundation, so to speak, from which most of the subsequent vitalistic theories evolved. In considering the question "What is soul?" in Book II, Chapter I, of De Anima, Aristotle, amongst other things, describes the essential nature of a living organism. He distinguishes between matter and form, potentiality and actuality. The term 'substance' has three senses; one with respect to matter (potentiality) another with respect to form (actuality) and finally a combination of actuality and potentiality. Living organisms are considered to be composite substances. As Aristotle says ". . . every body which has life in it is a substance in the sense of composite."[1]

Now Aristotle's concept of this composite substance is such as to preclude any doubt in the reader's mind as to the unity made manifest by the composition of soul and body. Doubting such a unity, according to Aristotle, is like doubting that the wax and the form given to it by the stamp are one. When a body having life

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[1] Aristotle, De Anima, 412a15.

potentially is actualized by the soul the result is an organized whole, a living body, just as the power of sight is essential to an eye being an eye.

A living organism, according to Aristotle, displays all or some of the following powers; self-nutrition, reproduction, sensation, motivity and thinking. The soul is the source of these powers. However, such powers as any particular living thing may have must be present potentially within the body or else their actualization would be impossible. That is ". . . the soul is actuality or formulable essence of something that possesses a potentiality of being besouled."[2]

The soul, in addition to being the source of the powers manifested by a particular living thing, is also the cause of a living body in the sense of final cause. In this respect the soul acts, metaphorically speaking, as an agent of nature, ensuring the natural growth and development of a living organism.

Generally speaking it seems that the following characteristics may be abstracted from Aristotle's account of the soul which wholly or partly have been incorporated (in various forms) into subsequent vitalistic theories. Firstly there is the role of the soul as the formal and

[2] <u>Ibid</u>., 414a25.

final cause of the body; secondly the soul essentially accounts for such unique characteristics of living organisms as nutrition and reproduction, sensation, motivity, thought and purposeful behavior; and finally the importance of appreciating the workings of a part of the composite unity of the soul and the body, in terms of this unity.

A 20th century biologist and philosopher who was much influenced by the theory of Aristotle is the famous neovitalist Hans Driesch. He was led to a vitalistic theory of life after performing many scientific experiments, of which the most famous were those involving the eggs of a sea urchin. One clearly sees the course of his development from the following quotation:

> The experiments of several years upon the power which organisms possess of regulation of form, and continual reflection on the collective results of experiments on the physiology of development, upon which I had been working since 1891, combined with a logical analysis of the concepts of regulation and action, brought about an entire change of my opinions and the gradual elaboration of a complete system of Vitalism.[3]

Driesch's development of his vitalistic theory is fairly sophisticated. His conception of nature involves events which are in a process of what he calls becoming.

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[3] Hans Driesch, <u>The History and Theory of Vitalism</u> (London: MacMillan Company, 1914), p. 177.

This becoming is more or less a kind of dynamic process which may be adequately rationalized only if the events involved in the process are conceived to form some kind of causal chain. Furthermore Driesch introduces two important stipulations, namely:

1. The cause of a particular system must always be of a higher degree of manifoldness (i.e. of a higher degree of complexity).
2. The degree of manifoldness of a natural system cannot increase of itself.

Now both these stipulations are somewhat vague and unsatisfactory insofar as they aim to say anything concrete and definite, although in certain respects they do have an intuitive appeal. However, to continue, Driesch describes four types of becoming of which the fourth involves a system of becoming between two points in time, say t_1 and t_2, in such a manner that the number of different kinds of relations among its elements increases without this increase being attributed to any kind of spatial object. A most important example of such a transformation, so Driesch contends, is that in which a system at t_1 consisting merely of a sum of its parts is transformed to one at t_2 in which it would consist in some sense of a unity or organic whole which could not be accounted for in terms of mechanical predetermination. As an example Driesch considers an aggregate of sixteen dots on a page, with a low degree of manifoldness, being transformed in a configuration of sixteen dots shaped like a fish, without spatial preformation. Driesch calls such a mode of becoming unifying or individualizing causality[4] and claims that it is the prototype of biological becoming.

The question now arises as to how this fourth type of becoming called unifying causality is to be rationalized in terms of Driesch's general theory of becoming? First of all Driesch readily accepts, for example, that biological processes, for instance mitosis (the process of division of a cell), can be described in terms of physico-chemical laws. What Driesch does not accept, however, is that physico-chemical explanations can adequately account for the complex ordering of processes and the particular types of transformations which do in fact occur. In other words, after witnessing all transormation in mitosis one might be able to describe physico-chemically what occurred, but prior to the process prediction of just that series of processes could not be made using physico-chemical laws. Consequently Driesch introduces an entity called an entelechy which exists within living organisms and determines which processes will be realized, out of all the possible processes, in the

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[4] *Ibid*., pp. 189-201.

development of the organism. We can see, then, that the entelechy, as conceived of by Driesch, operates within the limits imposed by the general laws of physics such as that of Conservation of Energy and the other laws of thermodynamics. The entelechy by selectively "suspending" possible change and "relaxing" such suspension acts to direct the complex processes of the organism and in this way determines its final forms.[5]

With respect to the nature of the entelechy, Driesch has little to say. As we have seen, he does believe that the entelechy is limited because ". . . it is on *given*, *preformed*, *material conditions* that the action of the entelechy depends."[6] Furthermore:

> Entelechy is an agent sui generis, non-material and non-spatial, but acting 'into' space, so to speak; an agent however, that belongs to nature in the purely logical sense in which we use this word.[7]

So it is by introducing this non-physical substantial entity called entelechy that Driesch attempts to rationally account for the special nature of biological systems. In particular, he claims that his theory could best explain such phenomena as harmonious-equipotentiality, complex-equipotentiality and human action. An harmonious equipotential system is one whose parts, which mutually interact

[5] *Ibid*., p. 203.

[6] *Ibid*., p. 204

[7] *Ibid*.

to maintain the unity of the system, have the potential to generate other parts of the unity. A complex-equipotential system is one constituted of parts which, under certain conditions, can be made to form a whole system whereas ordinarily they would only form part of the system. Such a system is manifested by a fertilized egg of a sea urchin which, if divided into two parts, under certain conditions, will develop into two separate sea urchins.

There are two criticisms which as far as I can see show that vitalism is inadequate as a means of explaining the phenomena of life. The first, which is particularly applicable to theories analogous to Driesch's, can be introduced by asking the question "Why is a living organism the kind of system requiring the special treatment Driesch claims it does?" No doubt Driesch would launch into an explanation regarding his position about "rationalized becoming". I have already indicated that the stipulations he places on this concept are somewhat vague and inadequate largely because of words such as "complexity" or "manifoldness". What criterion do we have for saying that a system at one time has greater manifoldness than the same system at another time? But even granting Driesch's conception of "rationalized becoming" there is even a more serious problem. It will be remembered that for Driesch there were four kinds of becoming; the one we examined was the fourth, called unifying causality. However the first type which has not been described is essentially of the kind found in any physico-chemical explanation in terms of cause and effect wherein there is considered to be some material, spatio-temporal event which is denoted as the cause of some effect. Driesch claims that biological systems manifest the fourth kind of becoming as well as the first. But how can we ever be sure that a phenomenon we experience does not, in fact, manifest the first type of becoming only? Surely not on the basis that we have tried to account for it in terms of the first type without success and therefore invoke the fourth type. However this is all that Driesch has done, it seems to me. And when one thinks seriously about just what is invoked in the form of the fourth type of becoming we begin to see just how inadequate Driesch's explanation is.

This brings me to the second criticism. Quite bluntly one can ask "Just what kind of explanation does the introduction of such entities as entelechy, *élan vital* or soul provide with respect to the phenomenon of life?". And I think the answer is that they provide no kind of explanation at all. If, for example, someone were to say that the static noise on a telephone line was caused by electrical disturbances in the atmosphere and he went on to say that the reason why he knew this was because of the static noise on the telephone line we would consider such an account ridiculous. Similarly if we asked why a caterpillar wrapped itself in a cocoon and were told that it was the ultimate effect of the caterpillar's entelechy, we would find such an explanation unsatisfactory. We would find it unsatisfactory because generally speaking that which is introduced to explain a phenomenon or class of phenomena should have as its effect something other than the phenomenon or class of phenomena it was introduced to explain. If not, the explanation is circular. It is into this category which most vitalistic "explanations" seem to me to fall.

According to Rudolf Carnap[8] (who discussed Driesch's theory of entelechy with him) it was Driesch's habit to claim that his entelechy, although non-physical, was really not different in principle from the physicist's magnetic field which explains why an iron nail is attracted to a magnet. And no one has seen a magnetic field. However this view is erroneous. The concept of magnetic field does a great deal more than just explain the movement of

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[8] Rudolf Carnap, Philosophical Foundations of Physics, ed. by Martin Gardner (New York: Basic Books Inc., 1966), pp. 14-15.

iron nails. In addition there are many other phenomena with which magentic fields are lawfully connected and which can be described in terms of mathematical equations, as for example Maxwell's equations, which describe the transmission of electromagnetic waves in space. It is in this way that the concept of magnetic field ties up meaningfully with other features of the physicist's conceptual scheme which in turn provides an account of the physical world. It is as a result of such interconnections that the concept of magnetic field was able to suggest new and fruitful areas for investigation and experimentation. The concept of entelechy is not like this for it does not allow one to leave the phenomena with which it deals, so to speak, and by merely reflecting on the abstract structure of the concept and its interrelations with the rest of the relevant theory to come to new and fruitful ideas regarding further investigation. It is when we try to reflect upon the concept of entelechy in this way that we realize its empty character.

MECHANISM

One of the earliest proponents of the mechanistic view was Democritus (460-371 B.C.) who speculated that the entire cosmos was constituted of tiny particles called atoms. For Democritus life derives essentially from the interaction of various kinds of atoms among which are the "soul atoms". These "soul atoms" are a special kind of material substance, akin to atoms of fire. When a living organism dies such atoms separate from the body and are scattered in the void. We notice in the theory of Democritus the absence of any tendency to invoke a non-material substance in order to give an account of living things although he does see life as a phenomenon caused by a special kind of material.[9]

The 17th Century witnessed the beginning of modern developments in philosophy, physics, mathematics and physiology. Descartes, himself one of the most important contributers to contemporary thought, envisaged living organisms as operating entirely in accordance with the laws of physics (although he did not subscribe to an atomistic theory). He was familiar with, and obviously very impressed[10] by the work of the English physician Harvey who in 1628 discovered the circulation of the blood. In the Fifth Part of his Discourse on Method Descartes discusses in general terms the physiology of living organisms, especially emphasizing the function

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[9] A. P. Cavendish, "Early Greek Philosophy", in A Critical History of Western Philosophy ed. by D. J. O'Conner (London: Free Press of Glencoe, 1964), pp. 12-13.

[10] Descartes, Discourse on Method, Fifth Part.

and character of blood circulation through veins and arteries. It is here that he also claims that organisms are essentially machines which do not differ in principle from those that could be produced by human industry. Even though Descartes recognizes the very complex nature of living organisms he nevertheless asserts ". . . that if there were any machines which had the organs and appearance of a monkey or of some other unreasoning animal, we would have no way of telling that it was not of the same nature as these animals."[11]*

Descartes' views were important in directing men's attention to the application of the physical sciences to the workings of the joints, muscles, bones and blood circulation of the living organism, which certainly has produced many fruitful results. However there are very few mechanists today who would agree with Descartes' concept of a living organism as something analagous to a clock. I also thing there is a tendency for people to take a dim view of contemporary philosophers and scientists who are mechanists because they believe that such mechanists have done little to

[11] _Ibid_.

*Descartes, in his philosophy, confined his mechanistic views to biology and did not extend it to psychology (the mind, the soul).

improve upon Descartes' position. Consequently I will spend some time in trying to give a reasonable account of contemporary mechanism.

Many people who think of such things as mechanisms or machines have in mind some system consisting of wheels and levers operating or describable wholly in terms of classical mechanics. No doubt Descartes and many of his followers used the words "mechanism" and "machines" in this way when describing living organisms. However it would generally be quite incorrect to impute such a concept to certain modern day scientists and philosophers who claim to be mechanists. As technology has advanced up to the present day there has been a natural tendency to extend the use of the word machine or mechanism to apply to many varied and complex devices which are not wholly describable in terms of classical mechanics. For example, many devices which are electrical or electronic in nature are often termed machines. A good example of this is the modern digital computer such as the I.B.M. 360 System which is capable of fantastically complex functions and which is commonly referred to as a machine in the relevant literature. Furthermore in genetics and biochemistry the word mechanism often is used in such phrases as "The moleculor mechanisms of variation within

D.N.A." and "The mechanism of gene reproduction." Such extended uses of these words should naturally lead us to suspect that anyone labelled a mechanist in the 20th century will undoubtedly consider the living organism other than in terms of milking machines, mechanical clocks or blocks and tackle. In order to realistically appreciate certain claims made by such mechanists as Jacques Loeb, E. B. Wilson and D. E. Wooldridge, I will describe certain biological phenomenon and discoveries which no doubt inspired such views. I do not claim that what I describe are necessarily the specific phenomena which these and other mechanists have in mind when they propound their mechanistic views, but it seems to me that what I will describe can be plausibly considered as representative.

Cell theory, one of the great achievements of modern biology, had its beginning in the work of Schwann who in 1838 claimed:

> We have seen that all organisms are composed of essentially like parts, namely of cells.[12]

Subsequent studies of the cell in the 19th and 20th century have produced some amazing results. Each cell

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[12] G. G. Simpson, C. S. Pittendrigh and L. H. Tiffany, Life: An Introduction to Biology, quoting Schwann (New York: Harcourt, Brace and Company, 1957), p. 39.

has been found to consist of a thin membrane enclosing a fluid of a highly complex chemical structure which can be generally divided into the cytoplasm and nucleus. Most organisms consist of a large number of such cells which ultimately account for the characteristics of the whole organism. Such features as colour of skin, colour of eyes, mental disorders, physical disabilities like diabetes can be traced to the production, or lack of production, by particular kinds of cells of particular substances.[13] It seems reasonable to speculate, then, that within the cell is contained the requisite information for the development and continuation of a particular organism. Within the last fifty years great strides have been made in unraveling the mysteries that are hidden within the cell.

There exist in the cell two classes of chemical substances, namely protein and nucleic acid. One of the most important functions of protein is carried out by the action of protein enzymes, which act as powerful catalysts, enabling the breakdown of food particles at body temperatures. Other protein enzymes enable the rapid combination of structural units of the previous process in order to produce the large and complex

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[13] Isaac Asimov, The Genetic Code (New York: New American Library, 1962), pp. 18-19.

molecules necessary for growth and continuation of the organism. Each protein enzyme is extremely specific with respect to what it causes to be broken down and built up, so that in order for the entire organism to be able to function normally literally thousands of different kinds of protein enzymes are required. The entire class of such reactions among the protoplasmic ingredients constitutes metabolism. It is through metabolism that the organism grows and is sustained in its final form, so that the different proteins found in different organisms determine the various characteristics of different species.[14] In addition, certain living things, notably man, are able to generate antibodies in order to combat various infections and poisons. Again the antibodies are different varieties of proteins which are able to attack specific kinds of viruses, bacteria, and so on. Protein also plays an indispensable role in the structure of connective tissue, fibrous tissue of muscles and membrances of cells.[15] It is no wonder, then, that the name protein was taken from the Greek, meaning "of first importance".

However the second material which is present in the cell, namely nucleic acid, must be considered of even

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[14] D. E. Wooldridge, Mechanical Man: The Physical Basis Of Intelligent Life (New York: McGraw-Hill Book Company, 1968), pp. 13-14.

[15] Simpson, Pittendrigh and Tiffany, Life, p. 110.

greater importance than protein, for it is the nucleic acid which is able to produce protein enzymes. In fact it is generally accepted that nucleic acid is the genetic material in the organism[16] which through its control upon the cell and organism as a whole (essentially through its influence upon protein production) is able to determine the ultimate characteristics of a particular organism. The blueprint for any organism, then, is coded by means of the intricate structure of nucleic acid (which for the majority of organisms is desoxyribose nucleic acid, D.N.A.). The number of different types of D.N.A. molecules is extremely large yet they are constructed out of only five elements, namely carbon, hydrogan, oxygen, nitrogen and phosphorous. Probably one of the most amazing properties of these molecules is that they can reproduce themselves! In this way it is ensured that every cell which originates from parent cells in the process of mitosis always receives its compliment of specific types of D.N.A. molecules and consequently the appropriate "instructions" for further development and maintenance.

From the above very brief account of some of the most fundamental processes which according to present-

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[16] A. R. Peacocke and R. B. Drysdale, The Molecular Basis of Heredity (London: Plenum Press, 1967), p. 7.

day biology occur in the living organism it is clear that nucleic acid is of central importance. What is of further significance with respect to our considerations is that biochemists have been able to synthesize simple forms of nucleic acid out of relatively inert materials. Such artificial forms have actually manufactured correspondingly simple protein molecules.[17] Another example of similar experimentation can be carried out with respect to digestion of food exactly as it occurs in living organisms. For example a mixture of pepsin and hydrochloric acid is able to digest protein just as the process which takes place in the stomach of vertebrates.[18]

There also have been some fascinating investigations and speculations regarding the origin of many of the large molecules which play an essential role within organisms. The view held by certain scientists, notably the Russian biochemist A. I. Oparin[19], is that

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[17] Wooldridge, Mechanical Man, pp. 16-7.

[18] Rainer Schubert-Soldern, Mechanism and Vitalism Philosophical Aspects of Biology (Notre Dame: University of Notre Dame Press, 1962), p. 57.

[19] A. I. Oparin, The Origin of Life, trans. by Sergius Morgulis (New York: Dover Publications Inc., 1953), pp. 61-63, also Chapters 4 and 5.

the atmospheric conditions that surrounded the primordial earth of about two billion years ago was such as to make possible the production of the appropriate materials for the development of living organisms. Experiments have been carried out under conditions presumed to exist in pre-biological times and the results have shown that under such conditions it was indeed possible that various complex molecules could have been produced up to and including the very complex molecules of nucleic acid.[20]

Now whether someone is to be labelled a mechanist will depend entirely upon the way he views the aforementioned biological phenomena, discoveries, and speculations with respect to a living organism. The mechanist Jacques Loeb, who himself carried out numerous experiments relating to fertilization and heredity, would have been confirmed in his belief that ". . . the sum of all life phenomena, can be unequivocally explained in physico-chemical terms."[21] That is, for Loeb life can be adequately conceptualized in terms of a large number of organized physico-chemical processes explainable

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[20] John Keosian, The Origin of Life (New York, 1964), pp. 46-66.

[21] Jacques Loeb, The Mechanistic Conception of Life, ed. by Donald Flemming (Cambridge, Mass.: Harvard University Press, 1964), p. 5.

in terms of physico-chemical laws. Loeb does not emphasize the organization of the physico-chemical processes but in a number of places throughout his book he does implicitly allude to such organization, as when he says such things as:

> We know that growth and development in animals and plants are determined by definite although complicated series of catenary chemical actions . . . .[22]

Loeb lived at a time when vitalists like Driesch were also very active; consequently he was probably most anxious to emphasize that there was nothing aside from physico-chemical processes and their corresponding laws needed to adequately account for life. In the first chapter of his book he explicitly claims that the whole organism is not governed by anything other than the appropriate physico-chemical laws.

> That a part is so constructed that it serves the 'whole' is only an unclear expression for the fact that a species is only able to live - or to use Roux's expression- is only durable, if it is provided with the automatic mechanism of self-preservation and reproduction.[23]

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[22] Ibid., p. 210.

[23] Ibid., p. 26.

E. B. Wilson, like Loeb, emphasizes the importance of probing the physico-chemical processes as the only means of acquiring an adequate conception of the nature of living organisms. He, however, emphasizes more directly the importance of the organization of physico-chemical processes when they effect certain observable phenomena of life. For example, in his discussion of the role of the cell in heredity he claims:

> Development may be defined as the sum total of the operations by which the germ gives rise to its typical product. Its particular course is determined (given the normal conditions) by the specific organizations of the germ-cells which forms its starting-point. As yet we have no adequate conception of this organization, though we do know that a very important part of it is represented by the nucleus. . . . Nevertheless the only available path lies in the mechanistic assumption that somehow the organization of the germ-cell must be traceable to the physico-chemical properties of its component substances and the specific configurations which they may assume.[24]

Here we see explicitly some key features of the mechanistic position, some of which were alluded to in the case of Loeb. Firstly the development of an organism may be considered

[24] E. B. Wilson, The Cell (New York: MacMillan Company, 1928), p. 1037.

as the aggregate of physico-chemical process within the germ-cell. Secondly, it is significant to note that Wilson believes that the organization of the organism derives from the physico-chemical properties of the substances within the cell. That is, the organization is not imposed by the whole onto its parts but just the reverse.

This latter remark regarding the source of organization of organisms deserves further attention. It amounts to saying that the genetic material within reproductive cells determines what the cell will become. This does not mean, however, that environmental conditions are not of considerable importance. Quite obviously appropriate quantities of such materials as oxygen, carbon dioxide, water, inorgamic salts, hydrogen ions and other organic nutrients are crucial to embyological development. For example it has been demonstrated experimentally that variations of the concentrations of the above mentioned materials can cause quite specific modifications of morphogenesis. However as M. V. Edds points out, although such environmental conditions are essential to the normal development of the embryo, ". . . they permit rather than determine or direct the structural patterns which finally emerge."[25] Edds elaborates on the belief regarding the source of organization of organisms as follows:

> It is like saying that morphogenesis proceeds from the inside out, not from the outside in. It says that the incredibly rich variety of organic forms is a consequence of individual cellular activities, and that the cells, themselves, not the 'organism as a whole', direct and specify those activities. It says that whatever one's view of the ultimate source of order in living nature, the ordering proceeds upward through the cells into the organism, not downward from the organism into the cells.[26]

A third feature of the contemporary mechanistic view is expressed by Dean E. Wooldridge - after reviewing essentially the same kinds of development in biology and related fields as we have alluded to in this chapter - when he says:

> In the late 1960's we seem justified in the broadest possible application of what may be called the central thesis of physical biology; that a single body of natural laws operating on a single set of material particles completely accounts for the origin and properties of living organisms as well as nonliving aggregations of matter and

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[25] M. V. Edds, "Animal Morphogenesis" In <u>This is Life</u>, ed. by Willis H. Johnson and William C. Steere (New York: Holt, Rinehart and Winston, 1962), p. 276.

[26] <u>Ibid</u>.,, p. 278.

> man-made structures.[27]

For Wooldridge such laws would be physico-chemical laws and would, as a result, lead to the conclusion that ". . . biology is indeed a branch of physical science . . .".[28]

My purpose in the latter part of this chapter has been to present what I consider to be representive views of the mechanistic position. Any criticism of this position will come later. To summarize, then, we see that the mechanist's thesis involves three aspects: (1) that life is solely constituted of physico-chemical processes; (2) that the overall organization manifest in living things is derived from these unit processes in complex interaction; (3) that the whole organism can be adequately described solely in terms of the laws of physics and chemistry. Many contemporary developments in biology and related fields appear to give a great deal of force to this thesis. However it is also generally recognized that these are a very large number of biological phenomena which defy complete physico-chemical analysis, for instance, photosynthesis, heredity, mitosis,

[27] Wooldridge, Mechanical Man, p. 167.

[28] *Ibid*., p. 168.

and so on, which are central to vital processes of organisms. In such a situation it is not surprising, then, to find someone like Ludwig von Bertalanffy presenting what he claims to be a third alternative, in the form of the organismic conception of life, to which we will now turn.

CHAPTER III

BERTALANFFY'S ORGANISMIC CONCEPTION

I will introduce Bertalanffy's organismic conception as he conceives it in connection with embryology and then proceed to expand upon his position by introducing more general considerations. The way in which Bertalanffy develops his position is by considering what he claims to be three fundamental kinds of theory regarding embryology, namely Weismann's preformistic theory, Driesch's entelechy theory and Goldschmidt's physico-chemical theory. By examining these three theories Bertalanffy emphasizes important aspects of biological phenomena which he contends can only be properly dealt with in terms of an organismic conception.

Weismann's preformistic theory conceives of the fertilized egg an containing pre-existing <u>anlagen</u> (very tiny elementary developmental machines for building the individual organs). These <u>anlagen</u>, according to the theory, become distributed within different cells as cell division progresses, resulting in different cells possessing a unique type of mechanism which subsequently determines characteristic organs and tissue. Bertalanffy points out, however, that this theory was soon refuted by Driesch's experimentation wherein a fertilized egg of a sea urchin was divided into two parts each of which was allowed to develop independently. Contrary to Weismann's theory, in both cases a whole organism developed. Bertalanffy emphasizes that in general cell action under experimental conditions can be made to manifest characteristics which largely exceeds its behavior under normal conditions and that consequently it is wrong to consider cell development to be determined by fixed developmental action. At very early stages of development the cell represents, with some restrictions, what Bertalanffy calls an ". . . 'equipotential system', i.e. every part can produce all and the same, namely, a complete organism."[1] Furthermore he emphasizes that the potencies of the cells are determined by their position in the overall developmental system and that as development progresses the cells progressively become differentiated towards a certain developmental fate. Bertalanffy then concludes that ". . . development is not the action of independent dispositions or developmental machines, it is governed by the whole."[2]

I will not consider Bertalanffy's rejection of Driesch's vitalistic theory of development on empirical grounds as it introduces little of interest with respect to his position.

In examining Goldschmidt's theory of development Bertalanffy is essentially considering whether or not a sophisticated type of physico-chemical theory of biological phenomena is tenable. Bertalanffy describes the main features of Goldschmidt's theory as follows:

> According to it, development is essentially based upon chemical actions of a catalytic nature which are inaugurated by the genes and lead to a differentiation of the egg-cytoplasm and later on of the cell regions in the embryo. In turn, the differentiated sorts of protoplasm are localized in a

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[1] Ludwig von Bertalanffy, <u>Problems of Life: An Evaluation of Modern Biological and Scientific Thought</u> (New York: Harper and Brothers, 1960), p. 57.

[2] <u>Ibid</u>., p. 59.

> definite pattern, due to physico-chemical equilibria, and thus organ-forming areas are established by the way of 'chemodifferentiation'. . . . With the progress of development ever new genes react with the chemical differentiations already present, and so a relatively small number of gene-catalyst and organ forming materials is able to set going a vast number of reactions and morphogenic processes. The basic principle is that of harmonized reaction velocities: in every cell and every embryonic area many different reactions are going on simultaneously; gene actions and their quantitative harmonization decide which of these processes shall take the lead and thus determine the developmental fate of any given part.[3]

Since the time Goldschmidt presented this theory in 1927 there has been considerable research and experimentation which has revealed the chemical structure of genetic material as well as how protein enzymes interact with certain chemicals within their environments and thereby control cell differentiation. Furthermore there has been encouraging research regarding genetic interaction with various chemical environments to show how chemicals can act directly upon mucleic-acid molecules and thereby cause them to produce specific effects. The latter phenomenon is called gene switching.

The point is that recent research essentially confirms much of Goldschmidt's conception of development-

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[3] *Ibid*., pp. 59-60.

in fact Bertalanffy says this. Furthermore I would suggest that Goldschmidt's theory might very well be considered as representative of the kind of paradigm within which many non-organismic* biologists today operate so that the main differences between Bertalanffy's position and that of other biologists should manifest themselves at this point.

As we might expect, Bertalanffy rejects as inadequate the above described physico-chemical account of development in favour of his holistic or organismic conception. A summary of the main features of this holistic view are:

> The conception of the system as a whole as opposed to the analytical and summative points of view; the dynamic conception as opposed to the static and machine-theoretical conceptions; the considerations of the organism as a primary activity as opposed to the conception of its primary reactivity.[4]

The term 'system' or 'whole' is defined by Bertalanffy as ". . . a complex of elements in mutual interaction."[5] The analytical and summative approach to which Bertalanffy

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* Organismic in Bertalanffy's sense.

[4] Ibid., pp. 18-19.

[5] Ibid., p. 11.

refers above essentially involves analysis of the parts of some system without regard to their interrelation within the system and then the explanation of the system as a mere aggregate or summation of these analysed parts. According to Bertalanffy this kind of approach has characterized early biological theory, which ultimately proved to be unsatisfactory because it ignored the complex interaction within the organism. Of course, Bertalanffy is right here - even the operation of a mechanical clock cannot be analysed in terms of Newtonian mechanics if the relationship among its parts are not taken into account. It is also clear that Goldschmidt's physico-chemical theory of the unitary action of the embryo provides an analysis of the complex interaction of chemical constituents within the developmental system.

The static and the machine-theoretical conception considers the organism primarily in terms of static spatial structures. Weismann's theory of development might be thought to epitomize such an approach. Bertalanffy emphasizes, however, that the regulative property of the organism and its sub-systems cannot be adequately accommodated by such an approach. For example, a static machine structure does not undergo a continual breaking down and reconstruction of parts. In short, a living system does not only display a distinctive spatial structure, at some point in time, but more important the processes which occur within the system manifest a dynamic order. This is especially clear when one considers the course of development of the embryo.

I think that Bertalanffy's emphasis on the activity of the organism as opposed to the reactivity of the organism is sound. The living system is not generally passive reacting only to external stimuli but rather manifests continual activity in the form of metabolism, activity of the nervous system, general behavior, and so on.

I have given a brief account of what Bertalanffy calls the "leading principles" of his organismic conception, which seem to me to represent views that would generally be accepted as reasonable guidelines to anyone pursuing biological research or attempting to develop a sound biological theory. At the same time, however, it also appears that these principles are not contravened by Goldschmidt's theory or by one which is analogous to Goldschmidt's theory. What further reasons, then, does Bertalanffy have for finding Goldschmidt's theory unacceptable?

The first criticism Bertalanffy introduces with respect to Goldschmidt's theory is that even though such a theory extricates a large number of chemical factors which undoubtedly occur in development it does not account for the immanent organization within the developmental system. As far as I can see, however, Goldschmidt's theory not only extricates the various chemical actions which occur in development but also aims at describing the dynamic, regulative character of the developmental system in terms of the complex interaction of the various chemical processes. If such a theory is not going to be considered to be sensitive to the complex interactions of the developmental constituents what theory is? The point is that Bertalanffy finds a theory like the one propounded by Goldschmidt unacceptable, not because it does not capture the organizational character of the developmental system of an organism (which we previously saw represented the strong points of Bertalanffy's approach to biological phenomena), but in fact because it is a physico-chemical theory. Bertalanffy's attitude, in this respect, becomes manifest when he says:

> If we call the egg a 'polyphasic colloidal system' we have said very little; a reaction between chemically defined 'gene hormones' and 'organ forming substances' would produce only chemically defined compounds, not organized formations as they are produced in the course of development.[6]

This comment introduces the question of reduction of biology to physics and chemistry which will be discussed later; however, there are a couple of observation which can be made at this point. What Bertalanffy says here is true but not particularly significant. Obviously from statements couched in chemically defined terminology we can only derive statements of a similar character. However this does not mean that there can be no relationships established between physico-chemical concepts and biological concepts. To interpret the previous quotation in this way would be to beg the question of reduction.

Another feature of the previous quotation which is of interest is that Bertalanffy appears to consider biological organization as being of a somewhat special and unique character. This feature of Bertalanffy's organismic conception will be subsequently examined

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[6] *Ibid.*, p. 62.

to determine whether or not certain specific claims he makes regarding biological organization can be reasonably maintained. Bertalanffy's observation in this respect, however, will certainly have to involve more than pointing out that "organisms are organized".

Another point which Bertalanffy introduces as a criticism of physico-chemical theories is that there are a number of mysterious processes in development which appear to be independent of physico-chemical differentiations. He then proceeds to describe in general terms such phenomena. However, the fact that there are biological phenomena which have not been accounted for in physico-chemical terms is by no means surprising or informative. Certainly this fact alone is not going to convince anyone that a physico-chemical explanation might not be found. Bertalanffy, in introducing what he seems to consider as an anomalous phenomenon within the physico-chemical paradigm, describes certain "mysterious" features of the morphogenesis of a mushroom, concluding with the following remark:

> Morphogenetic movements appear to be an integrated action of the whole; with respect to gastrulation,

> it was spoken of 'amoeboid movements', not of the individual cells but of the embryo as a whole.[7]

In the context in which Bertalanffy makes this remark, it is not clear whether he is merely exhorting the reader to take into account the mushroom as a whole when trying to analyse and explain the course of its morphogenesis or whether he considers his remark to be some kind of explanation. The point is, however, that all he specifically says about a whole or system is that it is ". . . a complex of elements in mutual interaction." Such a vague concept might be meaningfully and usefully employed in prescribing very general guidelines to be followed in the course of biological research and theorizing but it is virtually useless in any specific context where someone requires advice on how to deal with some specific biological phenomenon. And of course such a remark about some allegedly anomalous biological phenomena is certainly not going to indicate to a non-organismic biologist any viable alternative to the physico-chemical approach.

Now Bertalanffy's introduction and use of the word 'whole' is one of the distinguishing features of his organismic conception. However what this word

[7] *Ibid.*, p. 63.

connotes, according to Bertalanffy's definition, is by no means sufficiently unique to distinguish the organismic view from any but the crudest kind of biological theory, i.e. one which Bertalanffy characterizes as an analytic summative, machine theoretical or reaction theory. More important, however, is that the concept of a whole, because of its vagueness, can be used in explanations which often differ from a non-organismic explanation of the same phenomenon by the absence of the periodic occurrence of the word 'whole'. Such occurrences give the impression that Bertalanffy's account is different when in effect there is no significant difference at all. Furthermore, Bertalanffy can generously sprinkle his biological descriptions and explanations with the word 'whole' without danger of error because the concept is so vague that it can always be appropriately interpreted to fit the context. Examples of Bertalanffy's use of the concept of a whole are:

> The _whole_ organism is produced by the _whole_ genome. . . . So the genome is not a sum or mosaic of independent and self-acting _anlagen_ but a system that, as a whole, produces the organism, the development of which, however, is altered according

> to changes in parts of this system, the genes.[8]

Or with respect to the nervous system of insects:

> Therefore, it [co-ordination of leg movement] cannot depend on a fixed control mechanism, but must depend on the conditions present in the periphery and central nervous system as a whole.[9]

And again with respect to the permeability of the cell membrane:

> The correct interpretation is probably a system theory of permeability: The orderly and regulated transfer of substances as it is found in the living and metabolizing cell seems to be governed by the ensemble of factors present in the context of the organism as a whole.[10]

My main point in drawing attention to the above quotations is that they take on a rather trivial character when one seriously considers the meaning of 'whole', according to Bertalanffy's definition.

There are places in Bertalanffy's writings where he appears to recognize the fact that there is nothing uniquely biological about the words 'whole' or 'holistic' although strangely enough at other times his

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[8] *Ibid*., p. 75.

[9] *Ibid*., p. 116.

[10] *Ibid*., p. 156.

use of these terms appears to indicate otherwise. However, ignoring this latter tendency for the time being, we should not expect to find a theory utilizing the concept of a whole as being particularly unique for this reason alone; there are systems constituted of interrelated parts and analysed as such in physics, chemistry and modern technology. It is Bertalanffy's contention, however, that there are principles operative within living systems which extend beyond those found in non-living systems. He claims:

> . . . a specific gestalt principle immanent in the organism is to be supposed . . .. This conception is not vitalistic, for it does not assume any transcendent factors interfering in living nature, but on the contrary excludes such factors. But it is organismic since the organization immanent to living systems is considered to be specific; and so the autonomy of biological systems is contended.[11]

From this quotation, it is quite clear that Bertalanffy believes that the autonomy of biological systems derives essentially from the unique character of the organization within such systems. At this point my main purpose is to describe the two distinguishing features which Bertalanffy claims living systems to

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[11] Ibid., p. 65.

possess. In the subsequent chapter I will examine whether or not these two properties of organisms warrant certain conclusions made by Bertalanffy regarding the fundamental character of his organismic conception and biology in general.

The kind of organization found in living systems is described by Bertalanffy as hierarchical order, the principles of which were introduced and developed by J. H. Woodger[12] in his book Biological Principles. Although Bertalanffy does not present in schematic form the general concept of hierarchical organization, it seems to me that for our purposes it is advantageous to consider it in this form. Assume a system S constituted at the highest level, say X, of major constituents A,B, C,D. Assume further that these constituents are made up of elements (a', a'', a''', . . .), (b', b'', b''' . . .), and so on, forming the level Y. Obviously in an actual case we would have more than two levels but for the sake of simplicity we will assume that the a's, b's, c's and d's cannot be further analysed. Now the types of relations existing within system S will

[12] J. H. Woodger Biological Principles: A Critical Study (London: Routledge and Kegan Paul Ltdl, 1967), pp. 311-315.

depend upon the level one is considering. At the X level the relationships among relata is denoted by Rx. At the Y level there will be relationships among the a's denoted by Ray, among the b's by Rby, the c's by Rcy and the d's by Rdy. Furthermore it is stipulated that there are certain special relationships interconnecting some of the a's with some of the b's, some of the b's with some of the d's, and so on. As a consequence we have the constituents at the X level manifesting relationships of the kind Rx between each other.

Considering constituent A at the X level we can see that the properties of A will be determined by the elements (a, a', a'', . . .) which make up A, as well as the organizing relationships Rax which obtain between the various elements. The properties of A, when analysed within the system will be found to depend upon A's interrelationships with B, C, etc., so that any analysis of A in isolation from the context of system S would not reflect its behavior within such a context, unless effective methods were found for providing an artificial context. Bertalanffy, as well as Woodger, contend that if one analyses a particular part of a system, say A, down to the Y level, and determines the isolated properties of the a's which constitute A, then one will learn nothing of the X level, for this level, in effect, will cease to exist. The X level can only be properly investigated and understood if the elements A, B, C and D are analysed at this level rather than carrying the analysis to the next level. Thus any analysis of system S, which is hierarchically ordered, is going to involve more than just an analysis of the individual constituents within the system but a determination of the relevant and regular relationships between constituents at each level of organization.

Bertalanffy applies the above schemata to biological examples such as division hierarchy, spatial hierarchy, genetic hierarchy, hierarchy of processes and hierarchical segregation, and thereby exemplifies in a biological context what was generally described in the preceeding paragraph. In doing this Bertalanffy introduces some further considerations regarding hierarchical organization. The first point is that the interaction among the parts of organic systems is more pronounced, generally, as one progresses from the lower forms of animal life to the higher forms. Bertalanffy describes this phenomenon as progressive integration and emphasizes that because of this, experimentation and analyses at the higher levels must be especially sensitive to these interactions. As well as progressive integration in the higher animals there is progressive differentiation in the sense that the functions performed by various sub-systems can only be performed by those systems. Such specialization of parts are paid for in terms of loss of regulability of the organism, so that if one part of the organism is seriously affected or destroyed the organism may be entirely destroyed. Consequently, in a highly developed hierarchy one finds a rank ordering of parts with certain components playing a central role in the development and maintenance of the organism.

The second feature of organic systems, which Bertalanffy emphasizes, is that they are open systems as opposed to closed systems. Very generally, in physics a system is a bounded region in space wherein are analysed the transfer of mass and energy. All regions external to the system are described as the surroundings of the system. A closed system is one which is considered to have a constant mass, and only energy is assumed to cross the boundary. On the other hand, an open system is one in which there is not only energy transfer across the boundary but also mass flow either into or out of the system.[13]

Although non-living systems are not, in principle, closed systems, they can be analysed as such. It was essentially as a result of the study of closed systems that the various laws of classical thermodynamics evolved. Bertalanffy points out that concentrated theoretical work on open systems was motivated by developments in two areas, namely (1) biophysics of the living organism and (2) the continuous reaction-systems of industrial chemistry, which were found to be generally superior to batch processes.

Bertalanffy emphasizes that the various properties of open systems provide a basis for quantitative analysis of various phenomena of life[†] which previously had perplexed men like Driesch and resulted in the development of vitalistic theories. In particular, an open system under certain conditions is

[13] Edward F. Obert, <u>Elements of Thermodynamics and Heat Transfer</u> (New York: McGraw-Hill Book Company, Inc. 1949), pp. 14-15.

[†] In Chapter IV I will critically examine Bertalanffy's actual application of the concept of open systems to living organisms.

capable of reaching a steady state by which is meant ". . . the state of the fluid found at any point within the control volume* is the same at all times."[14] This steady state of the system, in virtue of not being in a condition of equilibium, is capable of doing work. Obviously if anyone were to consider the living organism as a closed system, then indeed the second law of thermodynamics would seem to be contradicted. However, to conceive of the organism in this way is incorrect, for the organism must be analysed as an open system. Another characteristic of open systems which is particularly illuminating when related to living organisms is that of equifinality. Bertalanffy describes equifinality as ". . . the tendency towards a characteristic final state from different initial states and in different ways, based upon dynamic interaction in an open system attaining a steady state."[15] Closed systems do not display this property of equifinality, for their final state is unequivocally determined by their initial

[14] George N. Hatsopoulos and Joseph H. Keenan, Principles of General Thermodynamics (New York: John Wiley & Sons, Inc., 1965), p. 113.

* 'Control volume' is an alternative term for 'open system'.

[15] Ludwig von Bertalanffy, General System Theory: Foundations Development, Applications (New York: George Braziller, 1968), p. 47.

conditions. This property of equifinality was manifested to Driesch in embryology when he divided or combined the fertilized eggs of a sea urchin in various ways and observed the evolution of a completely developed organism. For Driesch, this constituted one of his most important arguments for vitalism, but according to Bertalanffy it is merely a characteristic property of open systems. Further questions which appear to be amenable to quantitative analysis in terms of open systems are those related to regulation. It is Bertalanffy's contention that such biological phenomena as ". . . constancy of a characteristic pattern of composition with continual change of building materials, independence of and persistence in changing supply, at different absolute sizes, restoration following catabolism, normal or enhanced by simulation - are consequences of general properties of open systems."[16]

I have spent some time describing these two concepts of hierarchical order and open system because it seems to me that many of Bertalanffy's claims, whether acceptable or highly questionable, derive from the importance he attributes to these two concepts. For Bertalanffy "hierarchical order" and the organism as an "open system" are the two fundamental principles on

[16] Bertalanffy, Problems of Life, p. 133.

which a viable biological theory will develop. In fact Bertalanffy defines a living organism as:

> A hierarchical order of open systems which maintains itself in the exchange of components by virtue of its system conditions.[17]

Personally I believe that there will be some very fruitful results which will derive from the further development of a theory of open systems and its application to biology. In fact, there is a good possibility that the development of quantitative relationships for open systems - for example a mathematical relationship in thermodynamics which would define the steady state - might even extend some of the fundamental concepts of physics and chemistry. However, this possibility does not appear to me to justify certain claims which Bertalanffy often makes regarding the character of his organismic conception and the consequent division which he sometimes alleges to exist between physics and chemistry on the one hand and biology on the other.

Bertalanffy believes, for example, that:

> The biological order is specific and surpasses the laws applying in the inanimate world, but we can progressively approach it with continued research. It

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[17] *Ibid*., p. 129.

> calls for investigation at all levels: at the level of physico-chemical units, processes, and systems: at the biological level of the cell and the multicellular organism; at the level of supra-individual units of life. At each of these levels we see new properties and new laws.[18]

What we will have to examine closely is whether or not the hierarchical organization of a living system necessarily involves the appearance of significant and autonomous properties and laws at different levels. Does this kind of organization pose radically new problems regarding the specific arrangement of parts and processes? Bertalanffy asserts that because of the organization of a living system ". . . even a knowledge of all the chemical compounds that build a cell would not explain the phenomenon of life."[19] There may certainly be some truth in this latter remark but obviously certain qualifications will have to be made - qualifications which Bertalanffy fails to make. Sometimes when Bertalanffy says things like ". . . biological laws are not a mere application of physico-chemical laws, but we have here a realm of specific laws."[20] he appears to hold such a belief because of

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[18] Ibid., p. 20.

[19] Ibid., p. 12.

[20] Ibid., p. 157.

the rather different characteristics manifested by open systems. The impression one gets from Bertalanffy's writing is that open systems are inherently biological while closed systems are inherently physico-chemical.[21] I think there is an important conclusion which Bertalanffy draws from the applicability of open systems to biology but I do not believe that it implies any significant division between biological analysis and physico-chemical analysis.

The considerations which I have introduced in the previous two paragraphs and elsewhere in this chapter indicate some of the questions which I will take up in Chapters IV and V.

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[21] See Bertalanffy's General System Theory, p. 39, p. 156, and Problems of Life, pp. 157-158.

CHAPTER IV

THE QUESTION OF REDUCTION

The point at which the difference between Bertalanffy and a mechanist like Wooldridge becomes most obvious is over the question of the reduction of biology to physics and chemistry. Whereas Bertalanffy generally appears to be of the opinion that such a reduction is at present far from attainable, we find Wooldridge speaking as if it were practically an unquestionable fact. What is quite surprising is that a dispute of this character can rise to a level of such seriousness without certain necessary fundamental distinctions being made. If such clarifications were made concerning the formal and non-formal conditions of reduction in general and of biology to physics and chemistry in particular then the disputants might come to some sort of agreement or might at least realize that the problem has no clear-cut answer. At any rate, what follows will be an attempt at a philosophical elucidation of some features of reduction and their bearing on certain issues raised in Chapters II and III.

In order to determine the formal requirements for the reduction of one theory to another consider two theories T_1 and T_2, such that T_1 is the theory to be reduced and T_2 the reducing theory. T_1 and T_2 are constituted of laws L_1 and L_2 respectively. Let C_1 be one of the systems which T_1 explains and let $S_1{}^t$ be the state of this system at any time t*. Similar stipulations are to hold for T_2 with the appropriate change in subscripts. Lastly '$D_1{}^t$' will be used to denote 'C_1' and '$S_1{}^t$' and '$D_2{}^t$' will be used to denote 'C_2' and '$S_2{}^t$'. Knowing the laws pertaining to a particular system as well as a description of the system and its state at some particular time, then one can

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*C_1 is described by a series of statements. A state of a system is described by a complete set of relevant variables for the system. As time changes the variables change according to certain process laws. A process for a particular system is merely a certain class of states in temporal order.

calculate a future state of the system at some other time. In symbols this may be described as follows:

(a) $(L_1 \& D_1^0) \rightarrow D_1^t$ $(L_2 \& D_2^0) \rightarrow D_2^t$

In order for a reduction to be effected between T_1 and T_2, a connection must be established between the two theories. The nature of this connection will presently be discussed but it must conform to the following conditions:

1. The connection must enable every D_1^t to be co-ordinated to one and only one D_2^t (in symbols $D_1 \xrightarrow{t} D_2^t$). That is every description of a system and its state at a time t according to theory T_1 must be co-ordinated with one and only one description of a system and its state at the same time according to theory T_2. Note that it does not matter whether or not every D_2^t is co-ordinated to a D_1^t but every D_1^t must be co-ordinated to one and only one D_2^t.
2. The connection must result in a D_2^t being co-ordinated either to one and only one D_1^t or to no D_1^t.

3. Let D_1^0, D_1^t, D_2^0 and D_2^t satisfy the conditions as described in (a) above. Then for every D_1^t:

   (b) If $D_1 \xrightarrow{0} D_2^0$ then $D_1 \xrightarrow{t} D_2^t$

   In words, (b) expresses the condition that if D_1^0 is co-ordinated with D_2^0 and L_1 carries D_1^0 to D_1^t then L_2 carries the co-ordinated system from D_2^0 to D_2^t.[1]

When a theory T_1 has been reduced to a theory T_2, theory T_1 can be dispensed with. Bergman shows this in the following way. Consider, D_1^0 as given and then carry out the following calculations. (1) Find the D_2^0 which is co-ordinated to D_1^0 in virtue of the connection. (2) Knowing D_2^0 in conjunction with L_2, calculate D_2^t. (3) Again in virtue of the connection determine the D_1^t that is co-ordinated with D_2^t.[2] In other words knowing D_1^0, D_1^t can be predicted using only L_2 and the connection. Thus any phenomena in C_1 which is explained or predicted by means of L_1 can be explained or predicted by L_2. This, in effect, is the crux of reduction, the important result being that the

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[1] Gustav Bergman, <u>Philosophy of Science</u> (Madison: The University of Wisconsin Press, 1958), p. 165.

[2] <u>Ibid</u>., p. 166.

reduced theory is in a sense expendable.

Let us now consider the nature of the connection which must obtain between theories T_1 and T_2 so that reduction may be realized. As far as I can see, there are essentially two ways of making the connection between T_1 and T_2, namely (1) by definitional connection and (2) by what Pap calls composition laws[3] or Bergman calls cross-sectional laws. In the case of definitional connection the primitive terms of T_1 which do not occur in T_2 are defined in terms of T_2. This connection could be between a primitive of T_1 and some construct of the primitives of T_2. Once these definitional connections are established the laws of T_1 are deducible from the laws of T_2 in conjunction with the definitions. The second kind of connection is factual or material and is empirically confirmable. In this case primitives of T_1 are co-ordinated with primitives or constructs of T_2 on the basis of the belief that the state of affairs signified by primitives of T_1 is, as a matter of fact, signified by certain primitives or constructs of T_2. This latter type of connection, then, amounts to a

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[3] Arthur Pap, An Introduction to the Philosophy of Science (New York: The Free Press of Glencoe, 1962), p. 363.

scientific hypothesis. Clearly the terms involved in such a connection must be such that there exist experimentally independent ways of their instantiation with respect to some state of affairs.

Let us consider a very simplified version of the reduction of one theory to another, namely the incorporation of classical thermodynamics into the kinetic theory of matter. For the sake of brevity and simplification, we shall consider only one small part of this reduction, namely the derivability of the Boyle-Charles' law from the laws of mechanics and certain assumptions regarding the molecular nature of a gas. The assumptions we make regarding the gas, for the purposes of this derivation, will be as follows:

1. The gas is constituted of spherical molecules, which are perfectly elastic.
2. The interaction between the molecules is negligible except upon impact.
3. The molecules move in uniform linear motion between impacts.
4. The gas is homogeneous; that is, any small element of volume within the container will have the same number of molecules.

Consider an area A of the side of a vessel which contains such an ideal gas. This area will be hit by molecules which then bounce back towards the center of the gas. Let v_1 be a velocity component perpendicular to A. Let a unit volume adjacent to A contain n_1 molecules with velocity v_1. Finally let the average time for these n_1 molecules to strike the wall be t. Then the number of molecules striking A in time t will be

$$(1) \quad \frac{n_1}{2} \, X A v_1 t$$

As each molecule strikes the wall there will be a change in momentum of $2mv_1$, so that the total rate of change of momentum of the molecules striking the wall is

$$(2) \quad \frac{\frac{n_1}{2} \times A t v_1 x \, (2mv_1)}{t}$$

As a result the pressure will be

$$(3) \quad P_1 = \frac{\frac{n_1 A t v_1}{2} \times (2mv_1)}{At}$$

$$(4) \quad \text{or } P_1 = n_1 m v_1{}^2$$

Considering a different velocity component v_2 we would similarly find that the molecules with this velocity component would exert a pressure of $n_2mv_2{}^2$ against A. Similarly for velocity components v_3, v_4 . . . . Therefore the total pressure due to all the molecules is

$$(5) \quad P = n_1mv_1{}^2 + n_2mv_2{}^2 + n_3mv_3{}^2 + \dots$$

taking into account all velocity components.

If the direction normal to the wall is chosen as the x-direction then we define the average of the squares of the velocities in this direction as

$$(6) \quad \overline{v_x{}^2} = \frac{n_1v_1{}^2 + n_2v_2{}^2 + n_3v_3{}^2 \dots}{n}$$

where n is the total number of molecules per unit volume. Substituting (6) into (5) gives

$$(7) \quad P = nm\,\overline{v_x{}^2}$$

Now all directions of motion are equally probable; therefore

$$(8) \quad \overline{v_x{}^2} = \overline{v_y{}^2} = \overline{v_z{}^2}$$

Thus the average of the square of the velocity is

$$(9) \quad \overline{v^2} = \overline{v_x{}^2} + \overline{v_y{}^2} + \overline{v_2{}^2} = \overline{3v_x{}^2}$$

From (9) and (7) we get by substitution

$$P = \frac{nm\overline{v^2}}{3}$$

If there is one mole of gas in the vessel of volume V, then n = N/V, where N is Avagadro's number (6.025×10^{23}).

$$\therefore \quad P = 1/3 \; N/V \; m\overline{v^2}$$
$$\text{or } PV = 2/3 \; N \times 1/2 \; m\overline{v^2}$$

Now the following connection is postulated between the temperature and the kinetic energy of the ideal gas, namely:

$$1/2 \; m\overline{v^2} = 3/2 \; kT$$

In other words, the temperature is proportional to the average kinetic energy of the gas. k represents Boltzmann's constant.

$$\therefore \quad PV = NkT \quad \text{(T in degrees Kelvin)}$$

Thus we have derived the Boyle-Charles' law using:

1. The kinetic theory of gases.
2. A postulate connecting temperature with the kinetic energy of molecules.
3. Certain assumptions about the molecular nature of the gas under analysis.[4]

The classical formulation of the Boyle-Charles' law (PV = nRT; P - pressure, T - temperature, n - number of moles of gas, and R - universal gas constant) enables the state of an ideal gas at t_1 to be predicted from an initial state at t_o, if the final values of two of the three variables are known. I will now briefly indicate how this same type of prediction can be carried out using only the kinetic theory of matter and the connection between temperature and average kinetic energy. A thermodynamic specification of the state of one mole of an ideal gas at t_o would be given in terms of P_o, V_o and T_o. With respect to the kinetic theory of matter this same system would be described in terms of P_o, V_o and

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[4] I combined two sources in order to develop this derivation, namely:
John M. Irvine, The Basis of Modern Physics (Edinburgh: Oliver & Boyd, 1967), pp. 14-17.

F. W. Sears and M. W. Semansky, University Physics: Mechanics, Heat, and Sound (Reading, Mass.: Addison-Wesley Publishing Company, Inc., 1957), pp. 318-321.

$\frac{1}{2}m\overline{v_o^2}$; the latter quantity being obtained from the relationship $\frac{1}{2}m\overline{v_o^2} = 3/2kT$. The relationship $PV = 2/3N \times \frac{1}{2}m\overline{v^2}$ (which was developed using only the kinetic theory of matter) can now be applied to the system at t_o (characterized by P_o, V_o and $\frac{1}{2}m\overline{v_o^2}$) to predict the state of the system at t_1, if P_1 and V_1 are given. Once $\frac{1}{2}m\overline{v1^2}$ is calculated, we can then calculate T_1 from the connection $\frac{1}{2}m\overline{v_1^2} = 3/2kT_1$. We see, then, that the state of the system at t_1, can be calculated from the given information P_o, V_o, T_o, P_1,V_1 using only the kinetic theory of matter and the connection, so that in this sense we have no need to resort to the Boyle-Charles' law.

From the previous considerations of reduction, we can abstract two formal requirements which must be met if reduction is to be successfully carried out, namely:

1. A connection or connections must be established between the primitives of the reduced theory and the reducing theory.
2. All relationships which are derivable within the reduced theory must be derivable using the statements of

the reducing theory in conjunction
with the connections established
in (1).[5]

With these particular requirements in mind we are now in a position to consider the explicit and implicit claims made by Bertalanffy and by non-organismic biologists.

The first point that must be taken into account is really quite obvious but important. If we are trying to decide whether one realm of scientific knowledge is reducible to another, both disciplines must be sufficiently developed so that they have evolved into viable hypothetico-deductive systems in which the primitives and corresponding concepts of both are clearly apparent. When such is the case, the connections to be established between the primitives of both theories will be more readily suggested. In addition to the primitives there are the axioms of both systems, the types of inference and the corresponding theorems. Knowing these features of the theory to be reduced is important because it gives us an idea of the scope that will be required

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[5] These two requirements are emphasized by Nagel, The Structure of Science, pp. 353-354, and Pap, An Introduction to Philosophy of Science, p. 363.

of the reducing theory and because it allows us to decide whether or not reduction has been effected after an actual attempt has been made. Also when two theories are formalized, it may be that a certain approach towards realizing the reduction is suggested as a result of isomorphism between the two theories. Clearly the formalization of both disciplines seems essential to deciding unequivocally whether or not reduction is possible. That such formalization occurs within the realm of physico-chemical theories is confirmed by reflecting upon such systems as Newtonian mechanics, Old Quantum Theory as developed by Bohr or the Modern Quantum Mechanics of de Broglie, Schroedinger, Heisenberg, Pauli and Dirac.[6] In the study of biology the situation is certainly not analagous as the following remark by Waddington indicates:

> Biology in general has been very weak in the development of theories of wide application. . . . in recent years there has been a movement, led by Woodger (1937), to develop a theoretical biology in terms of logical analysis; but on the whole the problems which can be dealt with in this way are simple ones, such as the relations among entities which form part of a hierarchical

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[6] For a readable account of the latter two and the postulates required for their development see Irvine, The Basis of Modern Physics, Chapters 5 & 6.

> system based on various kinds of one-to-two relationships; and these are not problems crucial to our understanding of biological processes. What we should, I think, like to possess is a body of theory comparable to the major physical theories, such as thermodynamics, general relativity, wave mechanics.[7]

It seems, then, that the question of reduction of biology to physics and chemistry will not be achieved until a sufficiently formalized general theory of biology has been developed. Of course, this does not mean that great efforts will not be made at the present time to show that such concepts as the cell can or cannot be adequately captured in physico-chemical terms. Some of the work of James D. Watson appears to be directed towards just such a goal by showing that the various properties of a cell can be fully accounted for in physico-chemical terms.[8] However, even given the success of such an enterprise it will still leave considerable room for doubt as to whether biology in general is reducible to physico-chemical theory.

[7] C. H. Waddington, New Patterns in Genetics and Development (New York: Columbia University Press, 1966), p. 45.

[8] James D. Watson, Molecular Biology of the Gene (New York: W. A. Benjamin Inc., 1965), Chapters 2-11.

The second point I want to discuss, which is closely related to the first, involves what might be called the "time element". This is another obvious though little considered factor, when the question of reduction is bandied about. However, it is undoubtedly important. For example, present-day thermodynamics, although since 1860 reducible to statistical mechanics is certainly not reducible to the mechanics of the mid-eighteenth century. And so the time element is equally important when considering the reduction of biology to physics and chemistry. As we have seen Bertalanffy makes claims like:

> Even a knowledge of all the chemical compounds that build a cell would not explain the phenomena of life.[9]

or

> Thus the last alternative leads to the conclusion that embryonic development presents problems that are beyond a mere application of _gestalt_ principles known in inanimate nature. Rather a specific _gestalt_ principle immanent in the organism is to be supposed . . .. This conception is not vitalistic, for it does not assume any transcendent factors interfering in living nature, but on the contrary excludes such factors. But it

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[9] Bertalanffy, _Problems of Life_, p. 13.

> is organismic, since the organization immanent to living systems is considered to be specific, and so the autonomy of biological systems is contended.[10]

Such remarks by Bertalanffy tend to suggest that the organismic conception will always occupy an independent position with respect to the physico-chemical paradigm. If this is the case, then I think there would be considerable doubt as to whether such an extreme viewpoint could be justified. The following is a list, which is by no means exhaustive, of four positions which may be held regarding the relationship of biology to physics and chemistry, namely:

1. Biology is reducible to present-day physico-chemical theory.
2. Present-day physico-chemical theory does not allow for reduction; however, all we need is advances within this theory.
3. Reduction to a physico-chemical theory with present-day concepts is not possible but the development of a physico-chemical theory with

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[10] *Ibid.*, p. 65.

additional fundamental concepts will result in the reduction of biology.

4. Biology is not reducible to a physico-chemical theory, however, the introduction of vitalistic forces or substances is not required.[11]

The difference between position 2 and 3 essentially derives from the fact that someone who believes in 2 would maintain that all that is required for reduction of biology to current physics and chemistry is a more complete working out of the implications of current physico-chemical theory. Position 2 does not require the introduction of any logically independent primitive concepts to those which already exist within the current physico-chemical paradigm. On the other hand anyone advocating postion 3 would contend that the primitive concepts of current physico-chemical theory will have to be supplemented before reduction of biology would be realized. Furthermore position 3 obviously does not allow for someone to introduce an additional concept or concepts which denote vitalistic forces, as for example

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[11] John G. Kemeny, A Philosopher Looks at Science (New York: D. Van Nostrand Company, Inc., 1959), pp. 210-211.

Driesch's entelechy. Any move in this direction, by a vitalist like Driesch, would be immediately rejected by a mechanist. However there might be cases where someone claims to be introducing allegedly primitive physico-chemical concepts which implicitly possess a vitalistic component. Such a move, though more difficult to detect, would similarly be rejected as vitalistic. A belief of this latter kind usually is reflected in the fact that whereas a mechanist will claim that certain concepts are necessarily different in some respect or other from certain other concepts, a vitalist in addition, maintains that there is a necessary difference between the objects or phenomena these concepts denote. Because the vitalist believes in a necessary difference between animate and inanimate things in the world, he concludes that such a difference must necessarily be reflected in the concepts used to explain animate and inanimate phenomena if such explanation is to be satisfactory.

Now Bertalanffy appears to oscillate between positions 3 and 4,* while on the other hand it would seem that contemporary mechanists are inclined towards

*In considering Bertalanffy's position in this context I intend to interpret it as belonging to either position 3 or 4, for in fact some of the claims he makes <u>suggests</u> this, although not unequivocally. In the final chapter I will point out how a belief in vitalistic forces definitely appears to underpin Bertalanffy's organismic conception.

position 1, 2 or 3. Mechanists like Loeb or Wooldridge can probably be classified under 1 while Wilson would fit into the second position and Edds into the third. The clash between Edds and Bertalanffy appears to originate in their beliefs regarding the kinds of new concepts that might have to be introduced into the physico-chemical paradigm so that a unified body of scientific theory can result. I make this last remark presuming, of course, that Bertalanffy holds to the third position. And this is possible, for although I have indicated otherwise by drawing attention to certain statements that Bertalanffy has made, it seems that at other times he is inclined towards the third position as, for example, when he says:

> The evolution of physics tends towards an ever more comprehensive unification which, though by no means finished, in principle lets us expect that the entire physical world can be constructed from a few ultimate elements and basic laws . . .. On the other hand, it is possible, and even demonstrated to some extent, that the very inclusion of biological problems and fields leads to an expansion of the system of concepts and laws of physics.[12]

[12] Bertalanffy, Problems of Life, p. 157.

How position 1 can be justified at the present time is somewhat difficult to understand in the light of what we have said about the requirements for successful reduction. However, there seems little doubt, for example, that Wooldridge subscribes to 1. Later I will suggest why Wooldridge, as well as others like him, come to hold such a view and why I think their justification of this position is mistaken. Although position 2 is not as extreme as 1, it involves the belief that the present fundamental conceptual scheme of physics and chemistry is essentially all that is required to deal with biological phenomena. Whether or not this is the case cannot really be decided by citing numerous phenomena, both in the biological and the inanimate world, which do not appear to be amenable to current physico-chemical theory. I would suggest, however, that the history of science certainly indicates that this position will probably turn out to be false.

Personally I am inclined towards position 3. As we saw in Chapter II, when Descartes spoke of understanding living organisms in terms of the physics of his day, what he had in mind, essentially, was a reduction of biology to mechanics. It was mechanics, at Descartes' time, which constituted the respectable body of physical theory. As was also indicated in Chapter II, there has been considerable expansion since that time in the concepts and scope of the theory of physics. This is especially evident when we think of the incorporation of electromagnetic theory as developed by Ohm, Kirchoff, Lenz, Faraday and Maxwell. Now electromagnetic theory was found not to be reducible to Newtonian mechanics although scientists had tried initially to effect such a reduction. Failure of this attempt did not result in electromagnetic theory developing into a body of knowledge called something other than physics but rather the theory was incorporated into physics, thus extending the concepts and scope of the physics of that time. If such a tendency, as just described, is maintained, then there is a liklihood that any well-developed theoretical system which is found to be essentially autonomous with respect to existing physico-chemical theory will merely be incorporated into it as constituting a further extension of such physico-chemical theory. Consequently, it would appear that as a matter of course position 3 will be realized as opposed to position 4 and that if a thesis of Bertalanffy's organismic conception is position 4, then this conception is untenable.

A third feature of reduction which appears to be overlooked, both by Bertalanffy and by various non-organismic biologists, when considering the relationship of biology to physico-chemical theory, is that reduction involves the logical deduction of certain statements from certain other statements. Such an oversight results in the question of reduction being discussed in terms of the ultimate irreducibility or reducibility of one set of properties to another set of properties. However the question of reduction cannot be settled by considering the various properties of different things as though objects possessed certain essential and absolute features. Such an approach tends to generate groundless speculation as to the ultimate nature of various classes of objects, which cannot result in any conclusive proof or disproof regarding the reduction of one theory to another. The main difficulty which is at the bottom of such disputes over properties is that properties themselves are discussed in isolation from any theory which might serve in the explicit analysis of the meaning of the terms referring to such properties. Consequently, the interesting implications deducible from statements about certain properties as well as the interrelationships or lack of interrelationships between certain properties is difficult if not impossible to determine.[13] The question of whether properties of one type reduce to properties of another type can only be dealt with by an analysis of the theories which interpret these properties; for it depends on the interpretation what the results of the reduction will be. As we have seen, this problem is essentially either of a logical or an empirical character.

It is this idea of reduction in terms of the derivation of one kind of property from another which to some extent leads men like Loeb and Wooldridge to be overly optimistic about the incorporation of biology into contemporary physico-chemical theory. Often this tendency manifests itself in the form of the presentation of numerous experimental results which are supposed to indicate the reducibility of a certain class of properties to a certain other class of properties. The point, is, however, that the properties in question usually have not been fully described in terms of any

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[13] Nagel, The Structure of Science, pp. 364-366.

theoretical system, so that although certain experiments might be highly suggestive they do not indicate that reduction has been realized.

The final point which I want to discuss briefly can best be introduced by considering an important feature of the derivability of the Boyle-Charles' law from the kinetic theory of matter and the hypothesis connecting temperature with the kinetic energy of molecules. In classical thermodynamics the Boyle-Charles' law had only a limited application in predicting the interrelationships between P, V and T of actual gases. The result was that the above relationship only holds for an ideal gas. This came about as a matter of definition, for an ideal gas, by definition satisfies the following equations:

$$PV = nR\theta$$

$$(\partial\psi/\partial P)_\theta = 0^{*}$$

Whether an actual gas can be treated as an ideal gas or not depends on the amount of error one is prepared to accept. For example, below pressures of two

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[*] P - pressure, V - volume, n - number of moles, R - universal gas constant, θ - temperature, $(\partial\psi/\partial P)_\theta$ - rate of change of internal energy of gas with respect to pressure, temperature held constant.

atmospheres the ideal-gas equation can be applied without introducing error of more than a few per cent.[14]

Now the various assumptions we introduced regarding the molecules which make up a gas, when we carried out the derivation of the Boyle-Charles' law, was to make the gas under consideration conform to an ideal gas. These assumptions were special cases of the more general assumptions of statistical mechanics. The interesting question is whether or not another equation of state is derivable from the kinetic theory of gases utilizing the more general assumptions of this theory. For example, the molecules could be considered to interact with one another not only upon impact but, in addition, cohesive forces would be taken into account. In fact such a deduction was carried out which produced the famous van der Waals equation of state, which is still in use today, namely:

$$(P + a/v^2)\ (v - b) = RT^*$$

This equation produces fairly accurate results in the liquid region, the vapour region and near and above the critical point, whereas the Boyle-Charles' equation of

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[14] Mark W. Zemansky, Heat and Thermodynamics (New York: McGraw-Hill Book Company, Inc., 1957), p. 120.

* a & b are constants and v represents molar or specific volume.

state only holds at low pressures in the vapor and gas regions.[15]

The important conclusion to be drawn from the above considerations is that reduction is more than merely an empty exercise in scientific and mathematical ingenuity. Reduction can provide some extremely valuable suggestions and stimulation towards further developments in the reduced theory as well as improve upon the accuracy and scope of existing relationships. Furthermore, of course, the reduction of one body of scientific knowledge to another results in an overall unification of the reduced theory and the reducing theory thereby displaying functional relationships among hitherto unrelated facts. One advantageous effect of establishing such relationships is that physical constants, for example, can be shown to be based on various experimental evidence. When the experiments in question are carried out the constant can be determined in a number of independent ways; the agreement of the results tends to display the coherence of the theory as a whole and also its effectiveness when describing and predicting phenomena in the empirical world. As an example of this,

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[15] Ibid., p. 207.

the constant called Avagadro's number can be calculated from diverse experimental evidence within thermal phenomena, from Brownian movements and from crystal structure, all of which produce results in close agreement.[16]

Now existence of the heuristic advantages which have been described as a result of the interrelationships established between the parameters of classical thermodynamics and the kinetic theory of matter do not necessarily justify us in assuming that in every case of reduction such advantages will accrue. It might be, for instance, that a particular theory at a particular stage of its development is, in principle, reducible to another theory, but that the reduced theory just happens to be grappling with problems which the reducing theory not only is unable to provide with any positive solutions but that the reducing theory also tends to employ techniques and concepts which are inimical to the attempts of the reduced theory at solving these problems. For instance there is no reason why a theory T_2, to which another theory T_1 has been formally reduced should be a superior theory for directing scientific research

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[16] Nagel, The Structure of Science, p. 361.

towards some of the unsolved problems of T_1. The point is, however, that once people realize that T_1 has been reduced to T_2 or even that reduction might be carried out within the near future, they seem to be so taken by this fact that they automatically assume the superiority of the reducing theory. Such an impression is conveyed by Wooldridge when he says:

> We have arrived at the idea of the reducibility of biology to physics, not by abstract philosophizing, but by the most common kind of scientific reasoning - it is the most obvious and least complicated explanation that fits all the facts.[17]*

In the light of Bertalanffy's organismic conception as described in Chapter III it seems evident that he believes that there are problems facing the organismic biologist which are not amenable to solution by applying the techniques and concepts of current physico-chemical theory, that such a physico-chemical paradigm is largely unsuited to the task of providing viable solutions. As we have seen, these problems appear to center round the complexity, form, regulability and spatio-temporal organization of the living organism. Now

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[17] Wooldridge, Mechanical Man, p. 168.

* My Underlining

few people will deny that there are problems of real difficulty in these areas. Furthermore, in the present stage of our knowledge it is impossible to decide whether reduction of biology to physics and chemistry can be achieved. Nevertheless this does not prevent serious consideration of what the unsolved problems in biology require in the way of concepts and techniques and whether or not physico-chemical theory might conceivably be able to satisfy such requirements. It is Bertalanffy's contention that:

> There is a kind of complementarity between analytical and global treatment of biological systems. We can either pick out individual processes in the organism and analyse them in terms of physico-chemistry - then the whole, because of its enormous complexity, will escape us; or we can state global laws for the biological system as a whole - but then we have to forsake the physico-chemical determination of the individual processes.[18]

What we are about to subject to critical examination in our final chapter is Bertalanffy's justification of his thesis that the whole organism is only amenable to an analysis which is somehow autonomous vis-à-vis the physico-chemical analysis of individual processes and parts.

[18] Bertalanffy, <u>Problems of Life</u>, p. 155.

CHAPTER V

ORGANIC WHOLES

As we saw in Chapter III, Bertalanffy conceives of a living organism as ". . . a hierarchical order of open systems which maintains itself in the exchange of components by virtue of its system conditions."[1] The first question with which we must deal is what can be reasonably said to follow from the sole fact that an organic system or whole is hierarchically organized. A conclusion which Bertalanffy draws is:

> Every system in the hierarchical order, from the ultimate physical units to the atoms, molecules, cells and organisms, exhibits new properties and modes of action that cannot be understood by a mere summation of the properties and modes of action of the subordinate systems.[2]

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[1] Bertalanffy, Problems of Life, p. 129.

[2] *Ibid.*, p. 147.

Now immediately following this claim Bertalanffy provides the reader with an example to the effect that when sodium and chlorine combine the properties of sodium chloride are different from those of the component elements. Bertalanffy contends that, similarly, the properties of the living cell differ from the component proteins. However such examples are not particularly germane to the question of the relationship of the properties of a hierarchically organized system to those of its parts, except in a misleading way. No one is going to deny that certain properties and relationships at one level of a hierarchically organized system may not be manifested at a lower level. Thus, for example, a wolf can hunt sheep although no particular part of a wolf can be said to hunt sheep. The really important issue is in what sense the properties and modes of action at one level are autonomous with respect to the properties and modes of action of the constituents at a lower level.*

Bertalanffy's views with respect to this latter question are not easy to determine. In the above

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*Lower and higher levels are to be understood in connection with hierarchical order as discussed in Chapter III.

quotation he says that there are properties of the whole system which are not a result of "a mere summation of the properties and modes of action" of the constituents. How properties or modes of action can be added up is obviously not clear. A song, for example, which results from a series of notes being played on an instrument might be said to be more than a mere summation of the individual notes. But how can we decide on this matter, unless we clearly understand what it is to sum individual tones.[3] Of course, we can interpret the noun "sum" to mean the unordered class of individual tones, but although such an interpretation makes our initial claim true it does not make it particularly significant. This example points to the importance of possessing a clear understanding of summativity and non-summativity which is at once valid and significant within the various contexts in which these properties may be predicated.

Bertalanffy addresses himself to this problem by asking ". . . what the supposed 'non-summativity' of

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[3] A thorough paper relating to parts and wholes is one by Ernst Nagel, "Wholes, Sums, and Organic Unities," Philosophical Studies, III (February, 1952), pp. 17-32.

higher levels with respect to the lower ones really means . . .".[4] He then says:

> The answer is simple. The properties and modes of action of higher levels are not explicable by the summation of their components taken in isolation. If, however, we know the ensemble of the components and the relations existing between them, then the higher levels are derivable from the components.[5]

I find this answer unsatisfactory for a number of reasons. Firstly, it seems to presume that the concept of "summation", in this context, is sufficiently clear to explain "non-summation" which I have already indicated is not the case. The second difficulty, which will be shown to relate to the first, can best be displayed through the results of an analysis of the last sentence in the above quotation. Here Bertalanffy appears to be saying that if we know which components constitute a particular level of a system and the relationships existing between these components, then higher level properties are derivable from this lower level. Let us see how this belief fares when applied to an actual case. Suppose that a property of a particular clock is that the hour hand moves a certain distance along the

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[4] Bertalanffy, Problems of Life, p. 148.

[5] Ibid., p. 148.

circumference of the face of the clock in one hour. Further suppose that we know the shape, weight and so on of all the parts (which I presume Bertalanffy would call the "ensemble of the components") as well as the relationships existing between the parts. With this information can we derive a statement regarding the movement of the clocks hour hand? Obviously not, for aside from the information already given, we need to know a physical theory like Newtonian mechanics which will enable us to carry out the various calculations and make the appropriate derivation. We immediately see the importance of a theory in determining which properties at one level in a system are explainable in terms of the properties of a lower level.

In speaking of summative properties, Bertalanffy cites, as an example, the molecular weight of a compound, claiming that it is simply the arithmetical sum of the weights of the constituent atoms. But without recourse to chemical theory how would one know the functional relationship between molecular weight and atomic weights? That it happens to be merely the arithmetical sum of the atomic weights is an empirical generalization, which, as modern physics has shown, is not strictly correct. It might be suggested that by "sum" Bertalanffy only means arithmetical sum. The point is, however, that if he does, he does not say so. Furthermore his use of the word 'sum' in a large number of different contexts reflects that this is not the case. For example:

> It appeared to be the goal of biological research to resolve the complex entities and processes that confront us in living nature into elementary units - to analyse them - in order to explain them by means of the juxtaposition or summation of these elementary units and processes.[6]

Here we find 'summation' and 'juxtaposition' being used as though they are interchangeable with respect to elementary units and processes. One is tempted to interpret "sum" in the sense of "unordered class" in this context, but then, of course, it becomes a little more difficult to find summative properties under such an interpretation. "Being a pile" might be a candidate of such a property; however, such a classification would not really serve Bertalanffy's purpose.

The mistake which Bertalanffy has made is to try to make sense of summativity and non-summativity

[6] *Ibid.*, p. 10

without appreciating their connection with other factors such as scientific theories and some particular class of properties. Unless Bertalanffy believes in some sort of metaphysical theory regarding the ultimate reality of the world, of which he is not telling us, little or no sense can be made of speaking of properties as summative or non-summative in an absolute way. Alternatively, Bertalanffy could specify a certain relative sense of summativity and non-summativity. Towards this end I suggest the following definition of non-summative properties.

> A class of properties P_1 at a level L_1 in a hierarchically ordered system is non-summative relative to a theory T, a class of constituents C at a level L_e, and a class of properties P_0 of these constituents, if the properties P_1 cannot be deduced by means of theory T in conjunction with a characterization of the class of constituents C with

respect to properties P_o.[7]

We are now in a position to appreciate that just because a system is hierarchically ordered it does not necessarily follow that any level within the hierarchy will exhibit "new properties and modes of action" which are not derivable from lower levels. Consequently there is no _a priori_ justification for Woodger's and Bertalanffy's belief that if an investigator analyses, say, properties at a level X, in a hierarchically ordered system, down to the constituents and their relationships at a lower level, say Y, he cannot learn anything of the properties at the X level. By the same token, however, if a non-organismic biologist conceives of the lowest level of organization of an organism solely in physico-chemical terms, it certainly does not follow that higher level properties will ultimately be explainable in terms of this lowest level. Edds, for example, displays a tendency towards thinking this way

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[7] Carl G. Hempel and Paul Oppenheim, "The Logic of Explanation," in _Readings in Philosophy of Science_, ed. by Herbert Feigl and May Brodbeck (New York: Appleton-Centry-Crofts, Inc., 1953), p. 336. and Paul Henle, "The Status of Emergence," _Journal of Philosophy_, XXXIX (August, 1942), pp. 490-491. In formulating this definition of non-summative property, I have applied the definition of emergent property presented by Hempel, Oppenheim and Henle.

when he says:

> It is often remarked that the properties of table salt inhere neither in sodium nor in chlorine but in their combination. It is further said that no amount of information about sodium or chlorine alone would ever permit one to predict the properties of sodium chloride. Is this not an overly pessimistic view? The "new" properties which emerge when any subunits combine must be based on properties within the subunits. They cannot have come from nothing.[8]

There is no necessity connecting the properties of table salt, for example, with those of its constituent elements. The fact that certain properties of sodium chloride are traceable to certain properties of sodium and chlorine, by virtue of a certain physico-chemical theory, is entirely contingent on the properties in question and the physico-chemical theory developed.

Now that I have provided a reasonably clear meaning of non-summative property, I want to connect this idea with certain modified features of Bertalanffy's organismic conception and thus emphasize those points in Bertalanffy's position which I believe to be of value to biology generally. The modifications I make, as in the case of summative and non-summative properties, are generally for the purpose of rendering certain words and the statements

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[8] Edds, "Animal Morphogenesis," in <u>This is Life</u>, p. 283.

in which they occur reasonable intelligible. Following this discussion I shall analyse certain key statements pertaining to Bertalanffy's position, an analysis which, I think will demonstrate an unsatisfactory feature of Bertalanffy's organismic conception. Because this feature appears to be central to Bertalanffy's organismic conception, its rejection will essentially involve the rejection of his position.

My first step in assessing the positive features of Bertalanffy's position is to replace the variables in the previous definiton of non-summativity with constants so that we have some idea which properties and in what respect they are non-summative. One process or characteristic P_1 could denote would be the transmission of genetic characteristics. Theory T can denote current physico-chemical theory which describes the laws and properties of molecular and atomic entities. L denotes the lowest hierarchical level within the organism. Now, as a matter of fact, there are a number of processes which occur within biological genetics which have not been explained in physico-chemical terms, so that such characteristics are non-summative relative to T. This fact alone, however, is by no means surprising to anyone working in the field, especially to those aiming at a full account of genetic phenomena in physicochemical terms. We can easily imagine other biological processes falling under P_1, processes within the general areas of embryology, metabolism and morphogenesis. Thus we can appreciate that within the realm of biological phenomena there are properties or processes manifest which are non-summative according to the previous definition.

According to Bertalanffy, the main purpose of the organismic method ". . . is to find exactly formulated laws for organic systems as a whole."[9] If we interpret the word "whole" within this quotation as "system of interacting parts" then the statement is not particularly informative. As I indicated in Chapter III, Bertalanffy's use of the word "whole" is unsatisfactory in the light of the meaning he explicitly gives to it. At the moment, however, I will interpret "whole" as not merely a system of interacting parts but a system of interacting parts which manifests non-summative properties. The natural question to ask is what kind of system-laws does Bertalanffy have in mind? One such law is the principle of allometry. Bertalanffy claims

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[9] Bertalanffy, Problems of Life, p. 171.

that many phenomena of metabolism, morphogenesis, physiology, etc., are describable in terms of the following differential equation:

$$\frac{dq_1}{dt} \cdot \frac{1}{q_1} : \frac{dq_2}{dt} \cdot \frac{1}{q_2} = \alpha$$ [10]

Bertalanffy contends that this equation represents the simplest possible law of relative growth taken in its broadest terms, i.e. the increase of one variable q_1 with respect to another variable q_2. For example q_1 could denote the basal metabolic rate of a dog, while q_2 denotes body weight; α is a constant. There are also the so-called Bertalanffy growth equations which are derived from consideration of the organism as an open system. One such growth equation is expressed as follows:

$$w = \left\{ \eta/\kappa - \left(\eta/\kappa - w_0^{(1-\alpha)} \right) e^{-(1-\alpha)\kappa t} \right\}^{1/1-\alpha}$$ [11]

where w is weight at time t, w_0 is initial weight at t_1, η and κ are constants of anabolism and catabolism respectively, and α is the allometric constant. It is relationships of this general character which Bertalanffy has in mind when he speaks of the organismic method.

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[10] Bertalanffy, General Systems Theory, pp. 64-65

[11] Ibid., p. 174.

Due to Bertalanffy's emphasis of the importance of establishing empirical relationships amongst parameters of a biological order which denote properties or processes which are non-summative with respect to current physico-chemical theory of the molecular and atomic entities, I think we can begin to appreciate an important and valuable feature of Bertalanffy's position. Obviously there is no a priori reason why such relationships cannot be established and why they cannot be functionally exact and empirically testable. Certainly, a biological mechanist who rejected such an approach as non-scientific because it seemed to function outside the physico-chemical paradigm would be unjustifiably restrictive in his effort to understand biological phenomena. For example Mendel's research and the resulting gene theory of heredity established in 1865 would not be considered as a physico-chemical theory, yet this does not result in Mendel's first and second law being ineffective in providing an explanatory framework as well as directing further research. The cell theory, first put forth by Schleiden and Schwann in 1839, stated that all larger plants and animals are constructed of fundamental units called cells which display certain characteristics which are empirically ascertainable. This theory, although not physico-chemical, also provides a valuable conceptual framework for further research and experimentation. These historical facts point to the advantage which can be gained from the independent development of a particular discipline like biology with respect to what might be considered the more inclusive body of physico-chemical theory, even if in the long run the former may ultimately be shown to be an extension of the theory of the latter.

Furthermore, Bertalanffy in trying to effectively apply and develop his organismic method, has come to conceive of the organism as an open system. In Chapter III, I pointed out that the properties of open systems are significantly different from those of closed systems. For example the entropy of a closed system always increases towards a maximum whereas the steady state of an open system is not so characterized. Prigogine, who has done considerable work towards developing a thermodynamic criterion to define the steady state, has suggested the following relationship under certain restricted conditions.

$$dS = d_eS + d_iS$$

The equation known as Prigogine's Theorem says that the change in entropy of an open system is equal to the entropy change d_iS due to irreversible processes in the system, which according to the second law of thermodynamics will always be positive plus d_eS, the entropy change by import of materials into the system, which of course may be positive or negative. Now, as a matter of fact, the laws of classical thermodynamics were defined in terms of closed systems. This has lead Prigogine to claim:

> Classical thermodynamics has been concerned mainly with the study of closed systems. One striking achievement of recent developments has been to withdraw this limitation so as to generalize the methods of thermodynamics to open systems which are of a great importance for biological thermodynamics as well as for many other fields such as meteorology and geology.[12]

Thus by attempting to apply the concept of open system to life phenomena, Bertalanffy can quite rightly

[12] I. Prigogine, Introduction to Thermodynamics of Irreversible Processes (New York: John Wiley & Sons, 1967), p. 3.

emphasize against mechanistic biologists the fact that not only should there be no reason why a biologist should not develop exact mathematical relationships between parameters of a biological order, but that such an approach might further result in the creation of concepts which will require the expansion of existing principles within the physico-chemical paradigm. By not uncritically working within the physico-chemical paradigm biologists may find that they are better able to make significant scientific advancement with respect to understanding empirical phenomena of a biological character and also at the same time that they can actively participate in the development of more general physico-chemical principles. In respect of this latter point, Bertalanffy says:

> The author considers it as one of his most important achievements to have given from the biological side an impetus to this development [theory of open systems] in physics.[13]

Having presented those features of Bertalanffy's position which I consider to be of value to biology, I now want to point out and analyse those features which make Bertalanffy's position unsatisfactory.

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[13] Bertalanffy, Problems of Life, p. 126.

In describing Bertalanffy's organismic conception in Chapter III I drew attention to the fact that Bertalanffy's use of the word "whole" seems to reflect his belief that there is something more to a whole than merely a system of interacting parts. Of course it is difficult to determine what this something more is, but there seems little doubt that it is closely connected with non-summative properties (to be understood now and subsequently in the vague way in which Bertalanffy uses this term - a way which suggests that some properties are absolutely non-summative). What Bertalanffy appears to want to convey is that only wholes manifest non-summative properties and that these properties or processes cannot be understood unless the whole in which they are manifest is looked at *qua* whole. Part of the meaning of "whole" for Bertalanffy is a system which displays non-summative properties. Furthermore Bertalanffy firmly believes that there *must be* non-summative properties manifest in biological systems and hence that such systems can only be understood as wholes. If they are not analysed as wholes then any reasonable understanding of the non-summative properties will escape the investigator. Since the non-summative properties of a living organism constitute the important *life* phenomena, these phenomena will escape investigation. Non-organismic biologists are doomed to failure when it comes to understanding biological phenomena because they take the "analytic approach" which of necessity prevents them from every "really" understanding the vital processes which constitute life.

The main point I have tried to convey in the previous paragraph is that underlying Bertalanffy's vague and suspect use of certain key words of his organismic conception is an unproved belief that there is an almost necessary dichotomy between the biological realm and the realm of inanimate nature. This feature of Bertalanffy's position is manifest in the following quotation:

> The problem of life is that of organization. As long as we single out individual phenomena we do not discover any fundamental difference between the living and the non-living.* Certainly organic molecules are more complicated than inorganic ones; but they are not distinguishable from dead compounds by fundamental differences.* . . . A fundamentally new problem is presented, however, in the singular and specific arrangement of parts and processes that we meet within living systems. Even a knowledge of all the chemical compounds that

> build a cell would not explain the phenomena of life.[14]

We see that Bertalanffy is definitely predisposed towards a belief that there is a fundamental difference between animate phenomena and inanimate phenomena. Consequently when he speaks of the inadequacy of a knowledge of all the chemical compounds which build a cell for an understanding of life phenomena, he can obviously leave out such detail as which physico-chemical theory and at what time because no physico-chemical theory of any kind at any time will be adequate. That Bertalanffy does, in fact, believe this and that he does not justify such a belief will be made clearer in the subsequent discussion.

With respect to the purpose of biology, Bertalanffy says:

> Biology has the task of establishing the system-laws or laws of organization at all levels of the living world. These appear to transcend the laws of inanimate nature in two ways:
>
> 1. In the organic realm there exist

[14] *Ibid.*, pp. 12-13

* My underlining.
Oddly enough, Bertalanffy rightly rejects the apparent complicated character of life phenomena as justifying a fundamental difference between animate and inanimate nature. I say oddly, because as we shall subsequently see he attempts to justify the transcendent character of biological law because biological phenomena are so complicated.

> higher levels of order and organization, compared to those in the inorganic. Already in the configuration of macromolecular organic substances, and even more in the field of elementary biological units such as viruses and genes, we are presented with problems that go far beyond the structural laws of inorganic compounds.
>
> 2. The processes in the living are so complicated that with laws concerning organic systems as a whole we cannot take into account the individual physico-chemical reactions, but must use units and parameters of a biological order.[15]

Here we find Bertalanffy exhorting biologists to develop system-laws in biology or laws pertaining to biological wholes. For the purposes of the discussion we might consider the principle of allometry as an example which enables us to understand what laws of organic wholes are. Bertalanffy does not offer a definition. According to him these biological "laws" transcend the laws of inanimate nature, or at least appear to. Immediately one wonders what Bertalanffy means by saying that certain laws "transcend" certain other laws. "Transcend" appears to convey the impression that there is some kind of ultimate division between the two sets of laws; a division which definitely derives from more than the mere contingent fact that one set of laws, let us say,

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[15] *Ibid*., pp. 153-154.

is not explainable by means of the other set of the pair. What could be the justification for such a dichotomy? According to the first reason there are "higher levels of order and organization" in the organic realm as compared to the inorganic, and so the laws pertaining to the inorganic constituents cannot explain these higher levels. Appearances to the contrary, this reason only justifies a very weak claim regarding the division between conceptual schemes at different levels of organization. A system may be hierarchically organized but this fact alone does not justify any claim about whether or not an account can be given of properties and process at one level in terms of a theory regarding the elementary physical particles at the lowest level. As a matter of fact, however, there undoubtedly will be properties manifested at various levels of the hierarchy which are not accountable for in terms of a theory about the elementary particles at the lowest level. But this contingency surely is not going to justify anyone saying that the laws governing these higher processes (if they can be established) transcend (in the strong sense previously alluded to) the laws of the elementary particles at the lowest level.

All one can say is that there are processes occurring at higher levels which as a matter of fact are not explainable in terms of the lowest level, but this situation can simply be altered by altering the theory at the lowest level. Newtonian theory of elementary particles does not provide a satisfactory account of the specific heat of solids (within a certain range of temperature)but it would sound strange to say that the laws governing specific heats of solids transcend Newtonian theory of elementary particles. Currently, of course, it happens that quantum mechanics does provide an account of specific heats of solids within a very wide termperature range.[16]

The second point Bertalanffy alludes to is that the processes within the living are so complicated, from a physico-chemical point of view, that the laws of organic wholes cannot take into account individual physico-chemical processes. Again this fact (if it is a fact) does not justify the use of 'transcendent' to describe the relationship between laws of wholes and inorganic laws. Furthermore, I am a little baffled about Bertalanffy's use of 'complicated' in this context. The life processes of which he speaks

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[16] Irvine, <u>The Basis of Modern Physics</u>, p. 18 and pp. 78-79.

presumably have not been satisfactorily accounted for in physico-chemical terms. How then do we know that a physico-chemical account of such processes will be so complicated that we will have to content ourselves with developing laws of organic wholes as a matter of long term policy? Of course we would probably opt for this latter expediency if we were convinced that the laws of organic wholes transcended inorganic laws in some strong sense. But of course this is what is in question here. I cannot see any significant justification for the belief that a physico-chemical account of biological processes will be so complicated that we should simply forego trying to develop such an explanation. In his book Molecular Biology of the Gene James D. Watson provides a brilliant account of many features of heredity in terms of physico-chemical theory. Amazingly enough, some of the most important features of this account are surprisingly simple. For example, the genetic information contained in the chromosomes of the cell can be described in terms of the sequence of base pairs of only four different bases (adenine, thymine, guanine, and cytosine) along the very large D.N.A. molecule which represents the chromosome. In fact, although four bases are involved in this coding, these bases always pair off in the same way, namely adenine with thymine and guanine with cytosine so that actually all genetic information is coded in the various configurations along the D.N.A which these pairs form.[17] Clearly the processes within the living may look complicated from a physico-chemical point of view but this impression may quite simply be false. As I have already mentioned, of course, there is no reason in principle why exact mathematical relationships cannot be established between parameters of a biological order, relationships of which there is no account in physico-chemical terms. Certainly such regularities, if they are discovered, should not be ignored. However we find Bertalanffy claiming that we should, as biologists, be concerned <u>mainly</u> if not wholly with establishing laws of organic wholes; that these are <u>really</u> the only laws which can adequately explain biological phenomena.

This brings us to the question of how successful Bertalanffy has been in establishing laws for organic wholes. Consider the principle of allometry, previously

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[17] Watson, <u>Molecular Biology of the Gene</u>, pp. 285-295.

discussed, which according to Bertalanffy is expressed by the relationship:

$$dy/dt \,.\, 1/y : dx/dt \,.\, 1/x = \alpha \quad (1)$$

where α is a constant. In discussing this law Bertalanffy's approach is highly suspect in one crucial respect. He commences by recording (1), as I have done, but then without interpreting x and y and stating the value of α under such interpretations, he speaks of (1) as a systems law or a law of organic wholes. It is important to realize that (1) as it stands is merely a mathematical equation and that this alone is no justification for claiming that (1) expresses some law, although it might. Now when it comes to interpreting (1) there appears to be considerable variation in according to ". . . (1) the organism or tissue in question; (2) physiological conditions; and (3) experimental factors."[18] In the discussion which follows this Bertalanffy does not provide any clear-cut boundary conditions under which the so-called principle of allometry can be interpreted so as to truly display a reasonable clear-cut functional relationship between

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[18] Bertalanffy, General Systems Theory, p. 165.

actual biological parameters. Consequently one is led to the conclusion that there is a serious question as to whether (1) has any significant empirical interpretation. In fact, Bertalanffy admits to the inadequacy of the allometric equation with respect to correctly relating certain physiological processes or properties. He continues, however, to believe that (1) expresses a law of biology. Consider the following remark:

> We have to agree that the allometric equation is, at <u>best</u>*, a simplified approximation. Nevertheless, it is more than a convenient way of plotting data. Notwithstanding its simplified character and mathematical shortcomings, the principle of allometry is an expression of the interdependence, organization and hormonization of physiological processes.[19]

But (1) expresses no principle or law** until it is appropriately interpreted and experimentally confirmed either directly or indirectly. As a consequence it is not an "expression of the interdependence, organization and harmonization of physiological processes" but merely the expression of a mathematical equation. Bertalanffy

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[19] <u>Ibid</u>., p. 171.

* My underlining.

** Bertalanffy uses principle interchangeably with law.

however, doggedly holds to the empirically unjustified belief that there just must be a principle of allometry which is badly expressed by (1) ("mathematical shortcomings") and which if it were found would accurately express (exactly what we do not know without an interpretation; but Bertalanffy seems to think it makes sense to speak in this way) the organization immanent in physiological processes. Bertalanffy, as a matter of fact, never clearly explains exactly what, in the realm of biological phenomena, constitutes this elusive organization of physiological process except in terms of uninterpreted mathematical equations. As this example of the principle of allometry displays, he appears to tenaciously hold to his belief in such organizational forces even in the face of recalcitrant empirical findings.

Lastly I want to consider Bertalanffy's characterization of the living organism as an open system. In this respect he claims:

> We find systems which by their very nature and definition are not closed systems. Every living organism is essentially an open system.[20]

Of course, we know why Bertalanffy claims that living organism are by definition open systems - he has defined

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[20] *Ibid*., p. 39.

them that way. Furthermore, if by saying that a living organism is essentially or by its very nature an open system Bertalanffy means that there is an exchange of materials between the living organism and its environment then we would agree. However we might point out that strictly speaking there are very few, if any, closed systems even in the inanimate world, so that it would not be wrong to say that there are many systems other than living organisms which by their very nature (meant as suggested above) are open systems. What appears to be more to the point in the case of calling some region of space-time a closed or open system is whether or not it is analysed as a closed or open system. Most inanimate systems are analysed as closed systems and as such are called closed systems. If a living organism or some process within a living organism is analysed as an open system then, of course, it is called an open system.

What does it mean to analyse some region of space-time as a system? Bertalanffy's approach is to conceive of a system as constituted of a finite number of interacting elements. He then denotes some measure of elements $p_i (i = 1, 2, \ldots, n)$ by Q_i. The system in question is then defined mathematically by the following set of simultaneous differential equations:

$$
\begin{aligned}
dQ_1/dt &= f_1(Q_1, Q_2, \ldots, Q_n) \\
dQ_2/dt &= f_2(Q_1, Q_2, \ldots, Q_n) \\
&\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\
dQ_n/dt &= f_n(Q_1, Q_2, \ldots, Q_n)
\end{aligned}
$$
[21]

Following this definition Bertalanffy carries out further mathematical analysis, making various assumptions as he proceeds, and thereby generates numerous mathematical relationships which describe the effects, upon the hypothetical system, under such assumptions. One such assumption is that Q_i, the measure of the i-th element in the system, denotes the chemical concentration of that element and further that the system is open and receives some quantity of the i-th element at some particular rate. He then proceeds to carry the analyses through and provides a mathematical characterization of equifinality. Similarly, Bertalanffy's principle of allometry and growth equations are the result of abstract systems analysis. A result of this kind of analysis is

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[21] *Ibid.*, p. 56.

that there appears to be a kind of isomosphism between derived equations and biological phenomena. This leads Bertalanffy to believe that some very fruitful consequences might be achieved by conceiving of an organism as a system, in particular an open system. However, to say that every organism is an open system ". . . is a very general statement, which must first be supplemented, in every single case, by a whole series of special statements if it is to be testable in experience."[22] These special statements are required to ensure a clear-cut interpretation as well as to indicate the boundary conditions within which such an interpretation is valid. At this point Bertalanffy's work appears to be somewhat weak in that he does not seem to be able to provide successful interpretations for the equations he has developed in abstract. I have already pointed out difficulties associated with the principle of allometry which, in fact, will carry over into the Bertalanffy growth equations in that, among other things, the dubious constant α, occurring within the allometric equation, crops up in the growth equations.

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[22] Felix Mainx, Foundations of Biology, Vol. I, No. 9 of International Encyclopedia of Unified Science, ed. Otto Neurath (2 vols; Toronto: The University of Toronto Press, 1955), p. 24.

Bertalanffy all too often is inclined towards not taking seriously problems associated with interpretation and boundary conditions. For example just before describing the derivation of the growth equations he says:

> Here, too, I am not primarily concerned with details or even the merits and shortcomings of the model; I rather wish to use it to make clear some principles in quantitative metabolism research.[23]

Clearly, however, Bertalanffy is going to have to be concerned with the merits and shortcomings of the model in actual biological contexts or else his goal to arrive at quantitative metabolic principles will not be achieved.

Someone might be inclined to say at this point that although Bertalanffy may not, as yet, be successful in his attempt to interpret biological processes as open systems, the fact that he is directing his efforts in this direction more or less disproves my previous criticisms regarding his conception of wholes and non-summative properties. However, this is not the case. Bertalanffy's application of systems concepts and relationships to biological contexts always involves

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[22] Bertalanffy, General Systems Theory, p. 171.

such features of metabolism, morphogenesis, etc., which are to him non-summative in the absolute sense previously noted. That is, there are certain features of biological phenomena which, according to Bertalanffy, manifest organizational forces which for some reason are not traceable to a physico-chemical source. That Bertalanffy believes that these organizational forces conform, let us say, to the thermodynamic principles of open systems does not take away the metaphysical overtones which are reflected in Bertalanffy's talk of these forces. We can remember here that Driesch, who was a little more direct in this matter, claimed that his entelechy *conformed to the principles of classical thermodynamics*.

We can now, I think, clearly perceive the relationship of Bertalanffy's organismic conception to vitalism on the one hand and mechanism on the other. Bertalanffy's position definitely appears to incorporate an implicit belief in a vitalistic component within phenomena of life in the form of organizational forces. For as we have seen Bertalanffy's main contention is that animate phenomena are fundamentally different from inanimate phenomena; that within living organisms is operative a specific gestalt principle or organizational force. This feature of living things, according to Bertalanffy, result in the manifestation of non-summative properties which cannot (almost as a matter of necessity, but this is not explicitly stated) be traced to the properties of the atomic and molecular entities at the lowest level, regardless of what physico-chemical theory we may have respecting such entities. This absolute dichotomy, which Bertalanffy contends, exists between certain biological properties and properties of the physico-chemical constituents must derive from the character of the organizational force or forces he believes to be operative within living organisms. His tendency to maintain a belief in such forces even in the face of recalcitrant empirical findings and his inclination to not seriously concern himself with empirical verification, regarding various mathematical relationships he develops, strongly suggest that the organizational force or forces in which he believes are non-empirical. In fact Bertalanffy remarks, in the course of the development of abstract system equations, that such an analysis can produce laws of nature which are independent of experience - i.e. some _laws of nature_ can be known _a priori_.[24] From such

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[24] Bertalanffy, _General Systems Theory_, pp. 62-63.

evidence I conclude that Bertalanffy believes in non-empirical entities regarding which he can acquire knowledge by some kind of non-empirical mode of perception and this this is the character of the organizational forces he believes are immanent in phenomena of life.

To criticize Bertalanffy's conception of non-summative properties, however, is not to deny that various concepts of a specifically biological nature may be used to formulate certain biological theories which may be of great value in the acquisition of empirical knowledge within the realm of biology. In fact, there is no reason why certain theoretical constructs worked out in a biological context may not be <u>added to</u> the physico-chemical paradigm resulting in the expansion of the fundamental conceptual scheme of that paradigm. Few mechanists in biology would deny what I have said. I would guess that there is not a biologist alive who has not used some feature of either the gene theory of heredity or the cell theory with the utmost confidence even though these two theories speak of entities which have not been fully characterized in physico-chemical terms. However, at the same time, a mechanist like Wilson or Loeb, for example, firmly believes that all phenomena of life <u>will</u> ultimately be accountable for in physico-chemical terms. The belief implicit in the mechanists' position is that the physico-chemical paradigm, at any point in history, represents the most reliable source of knowledge we have regarding the empirical world. I am inclined to agree with this; however, I also believe that in order for the physico-chemical paradigm to maintain its eminent status there must be scientists working in the field of biology who are willing to modify and extend the conceptual scheme of physics and chemistry if certain biological phenomena require such modifications. Certainly, if such new theoretical developments are realized they will either be shown to be implied by existing physico-chemical theory or become incorporated as additions to the physico-chemical theory. Bertalanffy's implicit vitalistic beliefs are inimical to such developments, for they result in the belief that biological laws transcend the laws and theory of physics and chemistry. This has the very serious effect of conveying the idea that physico-chemical theory <u>will always be</u> inadequate to the task of explaining biological phenomena. Such an idea, I think, will stifle man's acquisition of knowledge of biological phenomena and hence reduce considerably the value and verisimilitude[25] of his physico-chemical theory.

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[25] Karl R. Popper uses this word to mean the degree of truth likeness or truth content of a theory. See <u>Conjectures and Refutations: The Growth of Scientific Knowledge</u> (New York: Harper & Row, Publishers, 1968), pp. 233-34.

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