HANS DRIESCH AND VITALISM: A REINTERPRETATION

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HANS DRIESCH AND VITALISM: A REINTERPRETATION

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ABSTRACT

Traditionally, the achievements of Hans Driesch (1867-1941) in the field of experimental embryology have been dissociated from his philosophy of vitalism. His vitalism has been judged to have been a result of his inability to explain certain experimental results in conventional mechanistic terms. Further, the fact that Driesch eventually left his career in biology to become a professor of philosophy has been taken as proof of the assumption that a philosophy of vitalism is incompatible with the practice of experimental science. The present study shows, however, that Driesch's theory of vitalism was not only compatible with his experimental work but was actually an important contributing factor to that work in that it provided him with a unique set of questions to be answered experimentally. Moreover, his vitalism is shown to have had an influence on his experimental approach from a very early point in his career. The notion that Driesch was suddenly converted to vitalism only after his most important experimental work was done is shown to have no basis in the historical record and several of his publications are cited to demonstrate the continuity of his vitalistic approach in science. The study concludes that even though certain metaphysical views such as vitalism may no longer be fashionable in science, these views have, in the past, contributed to the advancement of scientific knowledge by suggesting heuristic avenues of research and novel interpretations of experimental results.

CONTENTS

INTRODUCTION

The aim of this study is to follow the development of a theory of vitalism found in the work of the German experimental embryologist Hans Driesch (1867-1941). Although Driesch is well known as one of the founders and most brilliant practitioners of the "Developmental Mechanics" [Entwicklungsmechanik] approach to embryology, his theory of vitalism has been largely ignored. This ignorance is unfortunate since, as I will show, a better understanding of Driesch's experimental work can be had in light of his philosophical position. While science is not philosophy, it nonetheless rests on metaphysical foundations which are not themselves subject to empirical verification. A wide range of metaphysical beliefs is consistent with a wide range of good science. Further, historically speaking, the metaphysic has a heuristic role in the production of scientific theory even though a given scientific theory may be compatible with differing metaphysical views.[1]

What effect does this state of affairs have on the practice of history of science? The historian, I would argue, has a tendency to reconstruct science ahistorically; that is, according to present metaphysical views. Thus, scientists who held what are now unfashionable views (e.g. vitalism) have their work historically reconstructed in such a way as to minimise the role of their questionable metaphysics. This is particularly true in the case of Driesch. It is assumed that his vitalism can have had no heuristic value in the production of his excellent experimental research, that it was at best a redundant whim or at worst an actual hindrance to his development as a scientist and resulted in his eventual abandonment of science. I intend to show that this attidude is historically and philosophically false. Vitalism was for Driesch an integral part of his scientific outlook and played a role in determining the direction of his research. Further, as I will show, he never abandoned science.

Thus, whether or not one feels that vitalism is a satisfying philosophy of the organic world, it is valid to include an analysis of this philosophy as part of an analysis of the development of a programme of experimental work. It is my view that Driesch's experimental work and his philosophical position are, in fact, closely inter-related and an examination of his publications over close to a twenty-year period provides evidence for this position. Before proceeding to this examination, however, a brief review of the historiography appropriate to this study will be given.

Historiography in nineteenth and early twentieth century biology has tended to focus on two principal areas of investigation; that of evolution and that of medical science. The literature on Darwin alone is staggering, not to mention all the related areas ranging from geology and paleontology to heredity and Social Darwinism. In medical science, topics such as the bacteriological revolution, cell theory and the development of new medical specialisations which reflected both the growth in biological knowledge and the belief in its applicability to medicine have received considerable attention.[2] A central thesis, though not unchallenged, which has developed in both major areas is that of the gradual triumph of a materialist, mechanist philosophy in biology, particularly in Germany.[3]

The centrality of this thesis has perhaps been the cause of the virtual ignorance until quite recently of certain topics. One of these is the influence of Naturphilosophie on German biology. Further, even among those who have considered the impact of Naturphilosophie, it has been a virtually unquestioned premise that the concept of nature of Friedrich Schelling was the philosophical basis from which biological principles were derived by scientists associated with the Naturphilosophie movement.

A revisionist view with profound implications for the present study has been suggested by Timothy Lenoir in his book, The Strategy of Life.[4] Lenoir argues, against the standard view, that the Naturphilosophie of Schelling and Hegel was not influential in determining the programme of research of the vast majority of biologists in Germany in the late eighteenth and early nineteenth century. Rather, the philosophy of biology which *did* inform the work of scientists from Johann Friedrich Blumenbach (1752-1840) and Georg Reinhold Treviranus (1776-1837) to Johannes Mueller (1801-1858) and Theodor Schwann (1810-1882) was that of Kant. This claim is based on the identification of three separate traditions in Naturphilosophie, which are, in terms of their chronological development, the transcendental, the speculative or romantic, and the metaphysical.[5] I will first discuss the latter traditions briefly and then deal with the former tradition in more detail since it is the one Lenoir identifies as influencing the work of most German biologists.

The speculative or romantic tradition is associated with Schelling, whose approach is to construct the whole material system of nature from a single original unity often characterised as "world-soul" [Weltseele]. In biology, Schelling's idea of world soul was the basis for the concept of archetypes [Urtyp] from which all individual forms were constructed. The archetype itself was non-material, an Idea in the Platonic sense. The organisms present in nature were concrete realisations of the Idea, or to be more precise, realisations of some aspect of the Idea. The derivation of a physical form existing in time and space from an unchanging Idea or timeless potential was possible because of the "fundamental unity of matter, process, and spirit."[6] Further, the spirit or Idea necessarily existed prior to any of its realisations as physical forms. Schelling believed that since human beings were also a part of the material system of nature, they could understand nature intuitively, through introspection.

Closely related to the speculative tradition is the metaphysical tradition associated with Hegel, who objected to what he saw as the irrational and mystical elements of the former school and its attempt to use formal principles to deduce the material world from the self-activity of the Ego.[7] Although Hegel's view of nature was in basic agreement with that of Schelling, he did not believe that formal principles could be used for what was, according to Schelling, an essentially intuitive process. In other words, the disagreement did not lie in ascribing to mind or spirit a place prior to physical existence, but rather in assuming that the intuitive method was an appropriate means for finding out about the material world. The Hegelian school saw scientific principles and natural phenomena as tied through immanent connections and necessarily following from one another. These principles were not, however, immediately accessible to the mind, but could be deduced with the aid of empirical study in dialectic fashion. The task of natural science in this approach was thus, to reflect the logical structure immanently present in the material world. While Hegel could claim to have re-introduced the need for empirically based natural science as the method with which to deduce facts about the organic world, he had not gone any further than Schelling in explaining the basic problem of the nature of the relationship between Idea and organism.

The third and oldest tradition is that of transcendental Naturphilosophie associated with Kant which tried to determine "the *a priori* conditions for the possibility of experience, which is to provide the source from which the general laws of nature are to be deduced."[8] Thus, Kant's view of natural science must always be understood in light of his transcendental principle. Further, it is important to understand how, after coming to the conclusion in The Critique of Pure Reason that all natural events can be explained in terms of mechanical causality, Kant is able in the Critique of Judgement to reconcile the adoption of a teleological approach to organisms with the mechanical approach used for the rest of nature.[9]

In The Critique of Pure Reason Kant attempted to determine how physical laws could be presupposed since Hume had shown that they were not given in experience and could not be proved by pure logic. He began by making certain assumptions about the way we think, namely that the representations making up our consciousness stand in a possible, continuous connection and thus belong to the unity of our ego consciousness [Ichbewusstseins]. In other words, we arrange and order everything intuitively within our consciousness but the interconnections we make are not given to us in experience, rather they are only possible because our egos understand themselves as unities. Thus, these interconnections must be presupposed *a priori* and it then follows that one of these interconnections may be the connection of representations of events by the principle of causality. Now, one could conceive of other ways of connecting events, which would be contrary to, or "antinomial" to use Kant's own term,[10] the principle of causality, but only the latter principle is confirmed perceptually. The causal principle is both the condition for the possibility of unity of consciousness and for the fact that experience of objects is in any way possible. Having laid the groundwork by presenting us with the necessary *a priori* framework which makes possible any experience at all, Kant then argues that the same categories which make up this framework (i.e., causality, reciprocity, etc.) are present in [Newtonian] physics. Formally speaking, then, Newtonian physics is the only justifiable way of viewing the world. Kant did go beyond this position because in later works like the Metaphysical Foundations of Natural Science he deduced *a priori* much of the *content* as well as the form of Newtonian physics.[11]

In the Critique of Judgement Kant was interested in showing by what means we perceive organisms as being alive and in what way we can understand the principle of causality in those entities we describe as living. His answer to these questions required the introduction of the concept of teleological judgement because our concept of a living organism requires that we see the existence of the various parts as dependent on the whole. The result of this conceptualisation, as Clark Zumbach notes in his recent study of Kant's view of biological methodology, is, "No concept of a whole which forms its parts emerges from the mechanical conception of nature. It follows that biology cannot be reduced to physics, not because biological wholes literally *cause* their parts (for a biological whole is the effect of its parts, and an effect cannot be temporally prior to its cause), but rather because we *explain* the presence and arrangement of their parts in a way which makes sense of the formative powers of a living thing."[12] As I noted in the preceding paragraph, Kant had already argued that the causal principle was part of the necessary *a priori* framework which makes possible any experience at all. Now he seemed to be arguing that mechanical causality was insufficient as an explanation of living forms. Kant recognised the seeming antinomy between the contradictory claims that on the one hand, "All production of material things is possible according to merely mechanical laws," while on the other hand, "Some production of material things is not possible according to merely mechanical laws."[13] He argued, however, that there was only the "appearance of an antinomy between the maxims of the proper physical (mechanical) and the teleological (technical) methods of explanation"[14] because each proposition belongs to a different principle of judgement. Thus, Kant's justification for the use of teleological method in biology rests on the distinction which he makes specifying teleology as a *regulative* rather than *constitutive* principle.

It is important to understand the distinction Kant makes between regulative and constitutive principles of judgement. Kant defines judgement as "the faculty of thinking the particular as contained under the universal."[15] By this he means that one uses judgement to relate the particular to the universal in a law-like fashion. There are two ways judgement can function, defined by Kant as *determinant* and *reflective*. The former subsumes the particular under it in the case where the universal is given *a priori* while the latter must derive the universal from the particular where the universal is left undetermined.[16] When applied to the natural world, determinant judgement is constitutive, i.e., objective, but reflective judgement is only regulative, i.e., subjective. In order to clearly understand the distinction between judgements which are constitutive of experience and those which are not, it is necessary to refer back to the Critique of Pure Reason where Kant described the categories which make up the framework that makes experience possible. The categories provide formal universal rules by which appearances can be synthesised and are thus, by definition, constitutive of experience since they provide the *form*, though not the content, of objective experience.[17]

On the other hand, as Kant notes in the Critique of Judgement, there are "so many modifications of the universal transcendental natural concepts left undetermined by the laws given, *a priori*, by the pure understanding--because these only concern the possibility of a nature in general (as an object of sense)--that there must be laws for these [forms] also."[18] This is where reflective judgement is necessary in Kant's view because the second set of laws are empirical and as such "must be considered in accordance with such a unity as they would have if an understanding (although not our understanding) had furnished them to our cognitive faculties, so as to make possible a system of experience according to particular laws of nature."[19] Kant is making a crucial distinction here; he is arguing that we need not assume that the understanding derived from reflective judgement is *actual*. Rather, the laws we determine using reflective judgement are laws which apply only to our own understanding, not to nature itself. Kant then makes the argument that the "purposiveness of nature" is a concept which derives from reflective judgement and further, that it is quite different from the concept of purposiveness in human affairs although it can be thought of as analogous to the latter.[20] Kant's claim is that organic nature is purposive insofar as *human knowledge* is concerned.[21] In other words, according to Kant, we cannot but view nature as purposive.

Using primary sources, Lenoir is able to show a direct connection between Blumenbach and Kant, including the fact that they refer to each other's work.[22] Further, he demonstrates that the approaches of several other biologists he considers were influenced by this Kantian philosophy of biology. He uses the term "teleomechanism" to characterise the central feature of the approach which these biologists developed and identifies successive stages of the programme in terms of their additional theoretical components.[23] In assessing the relationship between the teleomechanists, the other schools of Naturphilosophie and the mechanists, Lenoir stresses how different their approaches to biology were as a result of their different views of nature. In the view of biologists in the Schelling-Hegel tradition, nature was fundamentally organised and further, the highest form of organisation was that of self-consciousness.[24] Thus, in this view the "purposiveness of nature", as Kant called it, was not merely a regulative but rather a constitutive concept. On the other hand, the mechanist view of nature, which also lacked the Kantian idea of regulative concepts, denied purposiveness altogether.

One can summarise the three views of nature as follows. Nature can be viewed as either purposive or not purposive. If it is viewed as purposive, then that concept can be thought of either as constitutive, i.e., given in nature, or as regulative, i.e., having to do with how human beings are able to view nature. Further, among those who viewed purpose as constitutive, methodological differences existed as to how best to understand nature, i.e., by observing it (Hegel), or by introspection (Schelling).

What effect did these different conceptions of nature have on their proponents actual empirical approaches to biology? Despite starting from a virtually antithetical philosophical basis, the mechanist approach had much in common with that of the Schelling-Hegel tradition. For different reasons, both schools believed that the formation of organic structures could be explained by the aggregation of some sort of fundamental units. In the mechanist case, the fundamental units were molecules and standard chemical processes were all that was needed to effect the aggregation of these molecules in order to form organs and ultimately organisms. In the case of the Schelling-Hegel group, the fundamental units were spherelets and geometrical transformations of these resulted in the development of different organic shapes. Contrary to both the reductionism of the mechanists and the mathematical mysticism of the idealists, the teleomechanists stressed the importance of processes and function in the development of organic forms. For both mechanists and the vitalists of the Schelling-Hegel variety then, structure or form was prior to function while for the teleomechanists the processes of organisation within an organism were prior to its structure. Thus, contrary to the ideas of historians who emphasise the conflict of mechanism versus vitalism, Lenoir suggests that the central issue in biology at this time was the problem of the primacy of structure versus that of process or, in other words, the problem of causality as defined by Kant.[25] This new approach allows one to re-examine both the views of individual scientists and many important scientific debates.

For example, Theodor Schwann, one of the founders of cell theory in the late 1830's has been described by Everett Mendelsohn as a mechanist who desired "to banish vital forces from the organism and reduce vital functions to recognizable physical causes."[26] Mendelsohn, one of today's leading historians of biology, takes the standard view when he argues that Schwann tried to explain cell formation and growth as a process of crystallisation which would be consistent with a mechanist approach of reducing organic phenomena to purely physical explanation.[27] Lenoir, however, contends that Schwann was engaged in a debate within the teleomechanist camp and used crystallisation only in a metaphorical sense, noting that crystallisation proceeded by apposition while cell formation proceeded by intussusception which was unknown in chemical processes.[28] Further, Schwann's criticism of some of his predecessors, notably von Baer or Mueller, was not that their explanations were teleological as such but rather that they had gone beyond Kant by saying that the whole was ontologically prior to and determinative of its parts. Schwann's position was more conservative in that he wanted to exclude the notion of a directive agent but he maintained a notion of teleology as a regulative concept.[29]

Lenoir's aim in presenting a detailed analysis of Schwann's theory and cell theory in general is not merely to show that the teleomechanist approach was consistent with current observations of cell structure but rather to suggest that within the teleomechanical framework cell theory became more fecund and was used to make interesting predictions. Thus, the teleomechanists were setting the pace for this research, so to speak. Although they later rebelled against their teacher, the mechanists Hermann Helmholtz, Emil DuBois-Reymond, Ernst Bruecke, and Matthias Schleiden were all students of Johannes Mueller, as was Schwann. The very research which eventually led them away from the teleomechanist view was initially set for them within that framework.[30] Thus, from a heuristic standpoint, teleomechanism was a successful scientific framework.

Lenoir makes use of terms introduced by Imre Lakatos to describe his interpretation of scientific methodology.[31] Lakatos' Methodology of Scientific Research Programmes provides Lenoir with what he calls "convenient terminology and a useful framework for interpreting the materials of the present study."[32] Since Lenoir's approach to historical explanation relies on an understanding of Lakatos' methodology, a brief description of this methodology is necessary. Lenoir explains that, " a research programme is not a scientific theory.It is a set of guidelines for the development of specific theories. These guidelines consist of (a) a "hardcore" of fundamental assumptions which set forth the central principles of an approach to nature, and (b) a set of heuristic guidelines for developing specific models that simulate reality."[33] The guidelines are called in Lakatos' terminology the "positive heuristic". In

Lenoir's study, as I have already indicated, teleomechanism is the hard core of the research programme under consideration and it is derived from an acceptance of Kant's philosophy of biology. The positive heuristic of a programme can be refuted by empirical evidence or simply dropped in favour of another positive heuristic which seems more powerful (that is, has excess empirical content) but the hardcore cannot be empirically refuted nor can it be abandoned without negating the whole programme.[34] Applying this model to Schwann's cell theory, Lenoir is arguing that Schwann retained the hard core of teleomechanism but replaced the positive heuristic of Mueller and von Baer, which required that cell formation be directed, literally speaking, by the whole organism, with his own, which posited the cell as the basic biological unit capable of independent growth and reproduction based largely on his empirical research on cell formation as well as recent advances in organic chemistry.

Lakatos' methodology is particularly apt as a framework within which to examine biology in the nineteenth century because it allows a metaphysical position (Kant's philosophy of biology) to be considered as an internal rather than external factor in science since it is the hard core of the teleomechanist research programme. Thus, a metaphysical theory becomes an essential component in the process of scientific research rather than something totally extraneous. It is a framework which I too will adopt since Driesch can be shown to have started from the same teleomechanical hard core as the biologists in Lenoir's study. In fact, Lenoir himself points out that while teleomechanism was defeated in the 1850's and 1860's, "In the last decade of the nineteenth century it resurfaced once again, being resurrected by the 'neo-vitalists', Driesch and Hertwig.[35]

I will not, however, adopt all of Lenoir's terminology because of a conflict in usage between him and Driesch. In his book The History and Theory of Vitalism, Driesch anticipates some of the observations which Lenoir makes about the varieties of Naturphilosophie; i.e., he identifies similar schools and he associates individuals and schools in the same way as Lenoir was later to do.[36] Driesch uses the term "teleological mechanism" to describe Schelling's doctrine and "vitalism" to describe Kant's.[37] Although there are some minor substantive differences between Driesch and Lenoir in their views of Kantian philosophy, the difference in terminology is, for the most part, merely verbal. Since I will be referring extensively to works by Driesch, I will retain his terminology throughout.

Nevertheless, Lenoir's decision to adopt new terminology points, I believe to a larger problem faced by historians of science. It is the problem of a built-in bias against vitalism and for mechanism which is perpetuated by the use of different definitional criteria for each term. How have historians traditionally defined mechanism? Surely not in the Cartesian sense of "machine-like", since by this definition even Newton's physics, in which action at a distance is postulated, is not purely mechanistic. Lenoir, like most historians, seems to use the term "mechanism" more or less interchangeably with the term "reductionism". For example, in justifying his adoption of the new term "teleomechanist" to describe the biologists of his study, Lenoir states: "Teleological forms of explanation in biology offer a middle ground between vitalism and reductionism."[38] Elsewhere he refers to the "mechanistic-reductionist approach" and also simply the "mechanistic approach".[39] Thus, while Lenoir is very careful to distinguish between "teleological" and "vitalistic" forms of explanation and even among different varieties of "vitalism", he makes no attempt to distinguish "mechanist" from "reductionist" and occasionally even conflates these terms. Lenoir does not make use of the term "physicalism", but this too is yet another variation on the mechanist-reductionist theme.

What becomes apparent, in Lenoir's book and in several other studies which I will discuss in more detail later, is that "mechanism" is a weakly defined term while "vitalism" is a strongly defined term. Semantically speaking, both "mechanism" and "vitalism" can be defined in either a strong or a weak sense. A strong definition of mechanism could be one which requires that any process or thing so defined be "machine-like" in a Cartesian sense. A strong definition of vitalism could require that some non-material ontologically directive force be postulated. A weak definition of mechanism might be one which states that organic processes are reducible to laws of physics and chemistry, without necessarily stating what those laws are or even if they are thought of as being entirely "knowable" in the first place. Similarly, a weak definition of vitalism could state that organic processes cannot be reduced to physical or chemical laws but must be explained from a teleological perspective. Such a definition would be virtually identical with Lenoir's definition of teleomechanism. The above definitions are not the only possible ones but they are adequate for the purpose of making the following observation.

A weak definition of either term is easier to defend precisely because it is "weak"; i.e., because the claims one makes by saying organic phenomena are or are not reducible are qualitatively less content-rich than the claims one would need to make by saying either that such phenomena are "machine-like" in a Cartesian sense or that they are the result of non-material forces. Further, in terms of developing scientific research programmes based on mechanistic or vitalistic philosophical foundations, it is hard to see how a strong definition of either mechanism or vitalism would contribute anything more to the programme in terms of determining the hard core. In fact, since the hard core is by definition a set of "fundamental assumptions", it should not be over-determined but rather should consist of an absolute minimum of content needed to specify one programme as distinct from any other. From this perspective it can be argued that weak definitions, as a sort of "lowest common denominator", are the only sort which can apply insofar as scientific research programmes are concerned. Whether or not one believes in non-material forces or Cartesian machines does not affect the sort of approach one takes in science because both these beliefs go beyond what is needed for the hard core of a research programme. What does affect one's *scientific* approach is one's position on the issue of reduction of organic phenomena.

In historical practice, I would argue that by conventionally employing the term "mechanism" in its weak sense while at the same time using "vitalism" in its strong sense it becomes natural to view mechanism as somehow more *scientific* and free of metaphysical content while seeing vitalism as *extra-scientific* and having metaphysical content. In fact, both views have metaphysical content and both can give rise to heuristic programmes of scientific research. Indeed, Lenoir's book can be seen as an attempt to rehabilitate vitalism as an approach to biological investigation and it is only indicative of the refusal of most modern analysts to consider vitalism in this light that Lenoir felt it necessary to dissociate what I would call "scientific vitalism" from "metaphysical vitalism" by renaming the former "teleomechanism". Although I sympathise with Lenoir's reasons for coining a new term, I prefer to retain the term "vitalism", with the caveat that it be understood in the weak sense. As approaches to biological investigation both vitalism and mechanism offer valid criticisms of each other, but neither can claim that the other does not produce "good science". My study will show, by taking a look at the work of one scientist, that a vitalist approach led to an increase in scientific knowledge and the opening up of new areas of investigation which might otherwise have remained closed.

Most of Driesch's early writings have not been translated into English. All the translations in the text are my own unless otherwise indicated.

CHAPTER I

DRIESCH AND DEVELOPMENTAL MECHANICS: FROM PHYLOGENY TO ONTOGENY

In making the argument that Driesch's vitalism was an integral part of a scientific research programme, it is necessary to demonstrate a few basic historical facts. First, given that Driesch was associated with the Developmental Mechanics [Enwicklungsmechanik] programme (founded by Wilhelm Roux) throughout his entire career as an experimental embryologist, it must be shown that his vitalism was compatible with that programme; second, that from an early point in his experimental career, vitalism was part of the hard core of his approach to embryology, providing him with a unique set of questions; and third, that this vitalistic element of Driesch's theoretical framework was consistent throughout his career, while at the same time allowing for important differences between earlier and later formulations of his position.

Developmental Mechanics was a programme devoted to the investigation of the individual development of organisms through the use of experimental techniques. In order to fully appreciate how fundamentally the direction of embryology was changed by this approach, it is necessary to understand the basic philosophical disagreement which was responsible for the change in the first place.

Seven years after the first edition of Darwin's On the Origin of Species appeared and one year before Driesch was born, Ernst Haeckel's General Morphology of Organisms was published. Two years later, Haeckel produced another work with the rather pompous title of The Natural Story of Creation.[1] In the preface to the latter book, Haeckel proclaimed, "Evolution is henceforth the magic word by which we shall solve all the riddles that surround us, or at least be set on the road to their solution."[2] Although Haeckel may have been more confident than most in his belief in the power of evolution theory to answer all the important questions of biology, his words accurately reflect an intellectual mood which existed not only in biology but in a much wider range of both scientific and humanistic pursuits. That mood can best be described as "historical", in opposition to the earlier "rational" mood of the Enlightenment.[3] This does not mean that until the nineteenth century no historical approaches in biology were recognised; rather, one might argue as Ernst Cassirer does, that it was only in the nineteenth century that the historical approach, once seen as having only marginal value, came to be viewed as the only way to discover the "laws of *real* nature".[4]

In the midst of the intellectual excitement over what was seen as the imminent, if not actual discovery of these "laws of *real* nature", few seemed concerned about whether or not the laws of real nature were really laws. Consider, for example, Haeckel's so-called biogenetic law. Stephen Jay Gould remarks, "Thus his phrase "biogenetic law" is often misunderstood, or at least not granted the force that Haeckel intended; for, under his definition it is *the* law of the history of evolution."[5] Gould rightly points out that Haeckel did not merely see ontogeny as the short and rapid recapitulation of phylogeny, but further, that he viewed phylogenesis as the *mechanical cause* of ontogenesis. ["Die Phylogenese ist die mechanische Ursache der Ontogenese."][6] In other words, this view represents a conflation of the concepts of historical and immediate causation with emphasis on the former. While Haeckel represents the extreme end of this sort of approach, the tendency is nonetheless representative of a wide range of his contemporaries including Darwin.

In this approach, the existence of different organic forms is explained by the hypothesis of descent from common ancestors; i.e., evolution. Whether the mechanism is natural selection of chance variations (Darwin) or inheritance of acquired characteristics (Lamarck) or some combination of the above, historical determinism is a common feature of all strands of evolution theory at this time. The forms which one encounters in the real world are determined by history, meaning singular historical events. "Rational" laws such as Newton's laws of motion are insufficient for explaining these real forms. Thus, the cause (in the sense of an efficient cause) of organic form is an historical cause. It may be stated in a rather simplistic formulation such as Haeckel's biogenetic law or it may be given a more sophisticated treatment as in the case of August Weismann's detailed studies of the origin of markings in caterpillars.[7] The common thread remains clear -- the historical approach is necessary in order to account for organic form. Further, questions of being cannot be separated from questions of becoming, or, to use caterpillars as the example, the development of stripes or spots in the individual can only be explained with reference to the historical development of such traits in the species.

Driesch is usually portrayed, as for example by Frederick Churchill,[8] as being blind to this historical approach. It is claimed that his lack of appreciation of the force of Darwin's arguments led him to deny the importance of phylogeny to ontogeny. Cassirer argues that Driesch saw history in biology as a "perverted tendency" which he vigorously opposed.[9] Cassirer quotes from an early work of Driesch's the view that, "it can be a matter of complete indifference to us that just such and such forms have been realised on this earth and have succeeded one another in time -- wholly indifferent, that is, to general, to theoretical natural science, which finds the idea of history, associated as it is with particular times and places, alien."[10] Taken out of context, as this quote is, it does seem to support the argument that Driesch denied the importance of history in science. However, if one turns to Driesch's original sentence, it is possible to see his statement in a different light. He says, "The core of the theory of descent is the capability of change [Umwandlungsfahigkeit], not the historical factor as is always falsely maintained. It can be a matter of complete indifference to us -- assuming that the theory is correct -- that just such and such forms have been realised..."[11] In other words, Driesch is willing to accept historical change but this in turn must be subsumed by mechanical explanation. Immediate causes are to be the object of investigation.

In the first place, Driesch's position is based, not on a denial of evolution, but on the assumption that the theory is indeed correct. Moreover, he identifies two components, the "capability of change" and the "historical factor", of which only the first can be made into a lawlike principle within the scope of what he calls "theoretical natural science". The historical factor, what in Darwinian theory would be called natural selection, is only an epiphenomenon, in the sense of a limiting circumstance, not an agent as the word "selection" tends to imply. Driesch had a great deal of respect for Darwin and admired his work, but he had little use for what he called "dogmatic Darwinism" which he believed tried to make a universal law out of what was in his view a unique historical process; i.e., phylogeny.[12] In a discussion of the relation between history and "biological systematics", Driesch argues, "As, in fact, it is most probably by history, by descent, that organic systematics is brought about, it of course most probably will happen some day that the analysis of the causal factors concerned in the history will serve to discover the principle of systematics also."[13] In other words, the importance of studying the historical record of organic forms is not to be denied, but the purpose of such a study is to determine universal laws which apply to biological systems regardless of the historical context. Phylogeny as such is historically unique and can never assume the status of a universal law or principle.

Driesch was certainly not alone in his basic belief in the necessity of the older Rational approach in embryology. The whole Developmental Mechanics [Entwicklungsmechanik] programme, as conceived by its founder Wilhelm Roux and as practiced by the new generation of experimental embryologists in Germany and the United States, can be said to represent a return to the rationalism of Kant insofar as it was mainly concerned with the immediate causal problem in biology. The notion of phylogeny as the mechanical cause of ontogeny was totally unacceptable from a Kantian point of view. Cassirer notes, "Here we are dealing not with the study of merely factual questions, for indeed the empirical validity of Darwin's theory seemed to Roux as firmly established as it did to Haeckel, but rather with the *epistemological* question of what is meant by the idea of cause and under what conditions the concept may be employed in biology."[14] Driesch may have been less enthusiastic about jumping on the Darwinian bandwagon, but he too accepted the empirical evidence for evolution even though he was more circumspect about explanations of the mechanism of evolution. In this he was certainly in good company since, as Scott Gilbert notes, "neither Morgan nor Wilson felt that natural selection was an adequate explanation for the origins of developmental phenomena."[15]

If one compares the new rationalists who were part of Developmental Mechanics with pre-Darwinian rationalists like Karl Ernst von Baer, the most important difference between them regarding the question of evolution is the fact that the later biologists were able to subsume an idea of evolution within their framework while the earlier group had seen their approach as excluding that theory. The enormous impact of Darwin could not be ignored by these new rationalists even if they objected to the historical approach per se. Unlike their earlier counterparts, however, they realised that the *fact* of evolution need not be accompanied by a deterministic historical explanation. These rationalists would probably have agreed with von Baer's point that the similarities one sees in early embryonic stages of groups of animals *need* not have an historical origin, but considering geological and paleontological evidence of change as well, they were also more likely to accept the evolutionary *interpretation* of such similarities. The new rationalists were not bound by their commitment to a particular epistemological notion of cause to deny the possibility of evolution. Evolution was allowable in the sense of a unique historical series of events, but those events -- phylogeny -- had no causal connection to, or influence on individual development -- ontogeny. Universally applicable laws, like those of physics and chemistry, would explain individual development while changes in phylogeny could be viewed as simply the result of law bound variations occurring over time. History itself could *determine* nothing.

Ultimately, evolution became almost a non-issue for scientists working in the Developmental Mechanics tradition, at least until the early twentieth century. This was not because Darwin's theory of evolution was not, in itself, one of the great topics in biology, but rather because, from a rationalist point of view, it was useless to argue about a theory which was not testable. Whatever one's feelings about evolution might be, they need not affect the sort of rigorous experimental approach that was to be used in embryology. Interestingly though, the major thrust of twentieth century evolution theory came from within Developmental Mechanics. It was only when a particular area of research being worked on by a group of these embryologists led to the formulation of a rational gene theory of inheritance that evolution theory reasserted itself in embryology.[16] This time, however, the theory focused on the rational element which could be given a lawlike formulation (Driesch's "capability of change" -- mutation and inheritance) not on the historical element ("historical factor" -- natural selection) which by virtue of its uniqueness could not be given the status of a universal scientific law, at least according to a rationalist understanding of the idea of cause.

My point in discussing the relation between embryology and evolution theory is not to defend Driesch's position on evolution, but rather to show that his critique of that theory was a direct result of his rationalism and Kantian view of causality. Further, this rational approach was shared by all the embryologists associated with the Developmental Mechanics tradition. Driesch's participation in Developmental Mechanics should then be seen, at least in part, as a logical consequence of his rationalism. Driesch's vitalism must also be seen in a similar relation to his rationalism. What this means, historically speaking, is that although vitalism and rationalism are not necessarily logically related, they are logically consistent. In fact, from a Drieschian point of view, the mechanism of most of the other major figures in Developmental Mechanics can be seen as a sort of internal inconsistency within a broader rationalist framework. In order to understand how such a viewpoint is possible, it is necessary to return for a moment to Kant's original argument about causality within the realm of living organisms.

It is crucial to remember that Kant did not conceive of teleological explanations in biology as a sort of replacement for the technical shortcomings of the biologists of his day. In other words, one did not ascribe to teleology anything which was beyond current understanding from a purely mechanical point of view. Thus, for example, to explain fermentation as a uniquely "vital" process as was often done in the first half of the nineteenth century could not be considered as a correct application of the Kantian principle of teleology. Just because fermentation was always associated with the presence of live organisms, that did not prove it was beyond the realm of simple physico-chemical explanation, but only that it was beyond the technical capability of scientists at the time to discover exactly what that explanation was. Many critics, both in the nineteenth century and today, seem to assume that all vitalist explanations take this form; that is, that they begin where current scientific knowledge leaves off.[17] Eventually, according to this line of reasoning, everything to do with living systems will reveal itself amenable to familiar physico-chemical explanation. Thus, the problem is identified as essentially empirical and all vitalistic or teleological explanations must then, by definition, fall outside the scope of science. This formulation contains a logical flaw, however, in that it assumes what it sets out to prove; i.e., that biology can ultimately be reduced to physics and chemistry.

For Kant, however, the problem of explanation in biology resides at the more fundamental level of epistemology. One cannot lump together a set of physico-chemical processes and derive something called "life". Intimate, even total knowledge of all these processes cannot explain any living organism because the very concept of an organism requires us to think in terms of a unity and it is only because we are capable of perceiving in such terms that we can then relate individual processes to a functional whole. Because each "whole", be it a single cell or a redwood tree, does indeed "function", the idea of purposiveness is indispensible to the concept of a living organism. This is not the same as saying that organisms have a purpose in any anthropomorphic sense. Kant emphasises that the laws derived from our teleological judgement apply to our own understanding rather than to nature itself.[18] Nevertheless, since we can only perceive organic nature in terms of unities, we are epistemologically unable to rely solely on a mechanical framework in biology. Teleological explanations are thus *absolutely necessary* regardless of the level of knowledge we may possess about individual processes within an organism.

The historical question which immediately arises is to what extent most biologists understood or were even aware of this Kantian rationale for teleology in biology. Lenoir points out that although famous scientists like Helmholtz and DuBois-Reymond saw themselves as intellectual heirs to Kant, they "characteristically emphasized those positivistic features of Kant's epistemological writings consistent with a stringent reductionism."[19] Although works such as Critique of Pure Reason and The Metaphysical Foundations of Natural Science were often cited by these mechanists, no mention was ever made of the Critique of Judgement anywhere in their writings.[20] Similarly, Mocek argues that the "materialistic Kant" was the philosophical basis for what he calls Roux's "mechanical materialism".[21] Thus, one can see that an interesting situation had arisen within biology. Not only was there a division among scientists based on "historical" versus "rational" approaches to causality, but within the rational camp there was a further division which seemed to be based on whether or not one had read the Critique of Judgement.

At this point it may seem as though I am arguing that all rationalists were Kantian, in one sense or another. This was certainly not the case, especially in the United States where the Developmental Mechanics programme found its greatest support outside of Germany. Many of the American scientists associated with the programme, such as Edmund Beecher Wilson (1856-

1939) and Thomas Hunt Morgan (1866-1945) not only had no formal background in philosophy as such, but also tended to define the problems of embryology from a methodological rather than epistemological standpoint.[22] Within Germany, there was opposition even to the more materialistic Kantian based rationalism of someone like DuBois-Reymond from a group of scientific materialists who objected to the Kantian distinction between phenomena and noumena.[23] Kant had claimed that "in the empirical employment of our understanding things will be known only *as they appear*."[24] In other words, Kant had claimed that our understanding of things might not, in some ultimate sense, be true but that we could never know this because we have, by definition, no access to the noumenal world. To these materialists, whom Frederick Gregory has termed "naive realists", Kantian epistemology was threatening precisely because it maintained this phenomenal-noumenal distinction.[25] They wanted to eliminate philosophy from science altogether and thought this could be done merely by asserting that the phenomenal world was the *only* world. They failed to realise -- and for this the are rightly called "naive" -- that their position was no less philosophical than Kant's since it was based on an equally untestable epistemological foundation. What is important, from the point of view of the historical task of determining the basis upon which the Developmental Mechanics programme was founded, is that Kantian rationalism was, in fact, a crucial element in the creation of that programme.

Turning now to Driesch's personal development as a scientist, it becomes apparent that a rational approach to biology was for him a very early development indeed. Well before he even began his university career, a number of circumstances contributed both to his choice of zoology as an area of specialisation and to his rejection of the then popular historical approach within the field. By his own account,[26] Driesch's childhood was almost idyllic in spite of the death of his father when he was not quite two years old. His parents had been married for fourteen years before Driesch was born and his mother had developed an interest in natural history during that time. By the time Driesch was born, she had already built up in their Hamburg home a private "zoological garden". At first the collection, while extensive, consisted solely of numerous exotic songbirds, but by the time Driesch was nine years old he had added a terrarium for reptiles and amphibians and then an aquarium for fish and another for salamanders. Eventually, the little "zoo" also included a monkey, a squirrel, six parrots as well as alligators, chameleons, lizards and snakes. Driesch was especially proud of his complete collection of German amphibians.

Added to this early practical introduction to natural history was an almost as early introduction to the more theoretical side of zoology, provided by the opening of the Kosmos Institut when Driesch was fourteen. This private institute offered lectures on current topics in many areas of science and Gymnasium students were encouraged to attend. It was here that Driesch first became acquainted with the Kant-Laplace nebular hypothesis, the latest geological theories and, most importantly, was introduced to Haeckel's interpretation of Darwin's theory of evolution. The fourteen year old was so greatly impressed that not only did he become a believer in evolution (and in the Haeckelian version, no less), but also he made up his mind to study zoology with Haeckel. Although Driesch's introduction to biology was obviously in the direction of the then popular historical approach, he soon questioned the basic premises of that approach. The reason for this lay in his formal educational background. As a Gymnasium student, Driesch did not receive more than the most basic introduction to biology. Most of his knowledge of biological topics came from his personal study and therefore there were large gaps, for example in the areas of physiology and embryology, about which he was totally unaware. He did, however, receive a good grounding in both mathematics (his favorite subject at school) and physics. Indeed, he claims that when he later began to read about the "proofs" for phylogenetic trees offered by Haeckel and others, he failed to follow the reasoning for these since they bore no resemblance to his aprioristic, deductive notion of a "proof" which he had gleaned from mathematics.[27]

After completing his Abitur and winning first prize for the highest overall standing, Driesch decided to begin his university career not at Jena where Haeckel taught, but at Freiburg where August Weismann worked at that time [1886]. His reason for making this choice is revealing in both a personal and philosophical sense. Since, as I have mentioned, he couldn't follow the reasoning behind Haeckel's "proofs", he came to the conclusion that he was not yet "truly capable"[28] of understanding Haeckelian analysis and needed more background before beginning to study with the great man himself. On a philosophical level, one can see that Driesch, perhaps because of his strong mathematical background, had already developed a rational notion of causality which did not allow him to see a "proof" in Haeckel's historically deterministic explanations. On a personal level though, one can see that Driesch was still open to the possibility that such explanations might be plausible, if only he could better understand the reasoning behind them.

After two semesters in Freiburg, Driesch went on to Jena to realise his dream of studying with Haeckel and, perhaps not surprisingly considering his expectations, he was disappointed to find that Haeckel's lecture course on general zoology was "utterly boring".[29] Fortunately, he was more impressed by other courses he would later take with Haeckel, particularly one on the zoology of coelenterates (hydroids, jellyfish, etc.) and he eventually wrote his dissertation on hydroids.[30] Before this, however, he took time from his studies in biology to spend a semester at Munich in 1888 in order to do lab work in both physics and chemistry since he already felt at this time that zoology would have to become more physiological in nature. There were, however, no courses along these lines offered at Jena. It was at this time that he met both Gustav Wolff and Theodor Boveri. Driesch's dissertation was written under very minimal supervision from Haeckel. He had collected several specimens of the two types of freshwater hydroids (*Sertularia* and *Campanularia*) which were the subject of his study while on summer vacation. In the next few months he wrote out the whole thesis and presented it to Haeckel, who until then had no idea that Driesch had even started work on his dissertation. Haeckel told him he would look over the thesis and get back to him in a few days to discuss revisions. As it turned out, however, Haeckel approved the thesis without revision after only two days.[31]

The significance of this situation from an historical perspective is twofold. Even at this very early stage of his career, Driesch had already moved towards a more experimental approach in biology and he also revealed an implicit preference for rational as opposed to historical forms of explanation. Even without going into an exhaustive analysis of the dissertation, both these points are easily demonstrated. In the first place, although Driesch's method was strictly speaking descriptive rather than experimental, he was looking at the actual process of development in living organisms rather than comparing static "stages of development" in dead specimens of different organisms. The emphasis on *process*, on how a given organism actually got from point A to point B, was already apparent and would soon lead to more probing questions on the development of form.

The object of Driesch's study was to analyse the different methods of colony formation in the hydroids already mentioned. He compared the various relations of primary and secondary budding angles in different species and then based his phylogenetic tree on these results. It is obvious, even from a cursory reading of the thesis, that Driesch was most interested in analysing development of form in the individual, not in terms of some hypothetical phylogenetic past, but rather in terms of the actual final form which the "individual" takes. The very notion of what constitutes an individual was crucial because Driesch had to have an "individual" [konstituierende Einheit -- Person] as the basis for his analysis of the relations of these individuals in colony formation. One of his main concerns was how individual components functioned relative to the interests of the colony and how the definition of individual boundaries could affect one's view of that relationship.[32] Since different individuals in a colony might perform different functions, such as feeding or reproduction, and might therefore have differing forms suitable to those functions, the development of form in the individual could only be understood with reference to its function in the colony. In other words, as the word "tectonic" in the title of the thesis suggests, structural features could only be understood as a function of the whole. In the whole thesis, only the last two pages (out of 36) actually deal with phylogeny -- the main part is purely descriptive -- and in this section Driesch explains that he has "tried to provide a real basis for this purely abstract discussion of similarities."[33] In other words, he had first determined what he called a "law of growth for hydroid colonies"[34] and on that basis tried to construct a phylogenetic tree. In no sense does he ever reason in the opposite direction; that is, from a given phylogeny to determine laws of growth.

Turning now to some of Driesch's early theoretical writings, one finds a great deal of evidence to support the claim that vitalism was part of the hard core of Driesch's research programme even at this stage. In 1890, only a year after the publication of his dissertation, Driesch wrote a short paper called "The System of Biology" in which he criticised the separation of the study of biology in German universities into two distinct areas, each taught in a separate faculty.[35] This paper is interesting because in it Driesch makes a connection between institutional isolation and the inability to properly articulate heuristic scientific questions. Briefly stated, his argument is that medical faculties emphasise questions of physiology as these relate to the practice of medicine while philosophical faculties where zoology and botany are taught focus more on morphology. Although there is overlap, especially between medicine and zoology, the methodological approach of the former is experimental while that of the latter is historical-comparative. Ultimately, Driesch argues, from the point of view of theoretical biology neither discipline looks at the total picture because each is ignoring a vital aspect. An organism has to be studied from both physiological and morphological perspectives, that is, as a totality. Driesch concludes that the only researchers who are actually subjecting the organism to this sort of investigation are those identified with Developmental Mechanics.[36]

It is interesting here to note that many of the best researchers in the German scientific community from around 1870 onward, including many of the most important people associated with the Developmental Mechanics programme, were not associated with the traditional university faculties or departments but rather worked within semi-autonomous institutes [Staatsanstalten].[37] These were not dependant either legally or financially on the traditional corporate structures of the university faculties, but were directly responsible to the state, hence their virtual autonomy in setting the direction of their research.[38] The number of institutes which arose from medical faculties alone between 1860 and 1914 was 173.[39]

Returning to Driesch's paper, the second part of it is devoted to an elaboration of the Developmental Mechanics programme. From the outset, Driesch argues that a teleological element is built in to the theory of the development of form. He says, "The theory of morphogenesis [Formbildung] appropriately begins with an investigation of the question of which basic means are available to create an organism from the germ. The expression 'means' is teleological since every means presumes a goal..."[40] He clarifies that the concept of a goal is to be understood in a purely "descriptive-teleological"[41]sense. In other words, like Kant his understanding of teleology has no anthropomorphic connotations but is merely a result of the fact that the concept of an organism requires us to think in terms of a unity. Kant himself made this clear when he said, "According to the constitution of the human understanding, no other than designedly working causes can be assumed for the possibility of organised beings in nature; and the mere mechanism of nature cannot be adequate to the explanation of these its products."[42]

Driesch maintains that "new research on morphogenetic regulation in higher animals shows that these are not just aggregates of cells but something which is in the deepest sense a unity, a true 'individual'..."[43] He then introduces concepts, which will be further discussed below, such as "potency", "harmonious-equipotential system" and "formative stimuli", all of which were to play an important role in his seminal work, Analytical Theory of Organic Development,[44] published four years later. Significantly, he also mentions the concept of entelechy, which has been seen as arising only much later in his development, that is, after he had made his "sudden switch" to vitalism. Driesch says, "The essential feature of life is the typical[45] order of specific differentiated forms [Ungleichfoermigkeiten]; there is absolutely no sense in which one could talk about such and such an amount of 'eagle' or 'lion' or 'earthworm'. This fact alone indicates that the concept of a 'life substance' should be rejected; rather, the concept of 'entelechy' should take its place..."[46]

What is the point which Driesch is trying to emphasise in contrasting the two concepts of "life-substance" and "entelechy"? Historians often tend to conflate these terms when they try to define vitalism but Driesch obviously must have seen the distinction as being very important since he rejected the former concept while at the same time seeing in the latter a possible basis for explanation of morphogenesis. "Life-substance" is a notion which, in historical terms, is properly associated with theories of vitalism of the Schelling variety.[47] Entelechy, on the other hand, is a specifically teleological concept which refers to the principle by which the parts are constructed in reference to the whole. (The word itself is derived from the Greek, "en telos" or "that which contains the end in itself".) It may be conceived in terms of a "life force" as many of the earlier group of Kantian biologists like Johannes Mueller did,[48] but it may also be thought of without reference to force or energy at all, as Driesch was later to do when he fully developed his own conception of how the teleological principle might work without violating the laws of physics and chemistry. At this point, however, Driesch's concept of entelechy was not very well defined. He was fairly confident only of the fact that some teleological principle was required to explain the development of form in ontogeny.

At this point it would be well to briefly review the general trends in embryological research at the end of the nineteenth century. We have already seen that the rationalist supporters of Developmental Mechanics objected to the notion of cause as defined by the evolutionists, and felt that experimental research was needed to determine "the causes underlying development, that is, the 'true causes', the individual formative powers and combinations of these powers."[49] Many of those in Developmental Mechanics therefore assumed that the answer to the problem of development would be found in purely physiological research using an analytical experimental method.

Thus, experimental manipulation of living organisms became the fashion as researchers tried to find out what the processes of development were and how they were controlled and related to each other. Experiments such as those performed by Roux where organisms were altered in the early stages of development were done in order to determine precisely what effect the loss of certain cells would have on subsequent development. The fundamental assumption that specific cells performed specific functions and further, that the loss of these cells would result in incomplete development, was based on the mechanist view that since structure determined function, a loss of structure must be accompanied by a loss of function. Scientists such as Gustav Wolff, who did experiments on lens regeneration in marine snails,[50] represented another major trend in Developmental Mechanics which was based on the teleological understanding of development. Wolff's experiment showed that the cells from which a new lens was formed were of a different origin than the embryonic cells from which the original lens had arisen, suggesting that the organism could arrive at the same final structure by different methods under different circumstances. Both Roux and Wolff would have defined the process of cell differentiation as the major problem of developmental biology, but the former tended to think in terms of the accumulation of structures and accompanying processes whereas the latter thought in terms of the final differentiated product as the goal and the individual processes as merely the various means which could be used to attain it.

Driesch clearly belonged to the latter group as his first experiments show. These were carried out in 1891 and concerned the development of normal plutei from half-blastomeres of Echinus obtained by shaking at the two-cell stage.[51] He concluded that Roux's principle of organ-forming germ areas had been refuted, at least for Echinus, while the possibility of artificially produced twins had been demonstrated.[52] Roux had earlier done experiments using frog eggs in which he observed abnormal development if one of the blastomeres was destroyed at the two-cell stage and the remaining one left to develop.[53] He had concluded that differentiation was dependent on the influence of already differentiated parts so that if some part was artificially removed later blastomeres would no longer be capable of self-differentiation.[54] The idea that differentiation after the initial cleavage was dependent meant that during each cleavage there was a qualitative division of nuclear material resulting in progressively more separated material which in turn determined the position of the later differentiated organs. Although Driesch's conclusion was rather modest -- he did not immediately suggest he had disproved Roux's interpretation of dependant differentiation except in the case of Echinus -- he suggested that perhaps it had wider implications for amphibian blastomeres as well as those of Echinus, but that experimental methods had to be improved. He noted that leaving the killed blastomere in contact with its viable partner might have interfered with the living blastomere's ability to regulate itself, a supposition which was later confirmed experimentally.[55]

Driesch's experiments confirmed to him that a combination of approaches was necessary in embryology. He expressed his belief in an important early theoretical work, Biology as an Autonomous Basic Science in which he made direct reference to the Kantian principle of teleology as one of the basic tenets of his theoretical framework in biology.[56] He argued that "where causality ceases, natural science must also end. Only one mustn't forget that at this point the connection to the teleological form of judgement begins."[57] He concludes his study with a quotation from Kant's Critique of Judgement in which Kant claims that "It is indeed quite certain that we cannot adequately cognize, much less explain, organized beings and their internal possibility according to mere mechanical principles of nature, and we can say boldly it is alike certain that it is absurd for men to make any such attempt or to hope that another Newton will arise in the future who shall make comprehensible by us the production of a blade of grass according to natural laws which no design has ordered. We must absolutely deny this insight to men."[58] Does Driesch's agreement with Kant's statement mean that he thought morphogenesis was inexplicable? This can hardly be the case, since he went on to produce his famous Analytical Theory of Organic Development only a year later, rather, Driesch felt that morphogenesis could indeed be analysed successfully, but not in an exclusively mechanistic fashion.

If we turn now to Driesch's Analytical Theory, we can see how his interpretation of the experimental work on the development of isolated blastomeres derives from his theoretical framework and further, how this interpretation marked the starting point upon which all subsequent investigation into development was based. It is important to remember in this context that how one interprets experimental results is based not only on one's observations per se, but also, and perhaps more importantly, on the theoretical framework one applies to reach that interpretation. What this means is that even though anyone using Driesch's methods and materials would have observed the same results, the subsequent interpretation of those results would not have necessarily coincided with Driesch's. His interpretation may seem to follow as if by logical necessity, but from an historical standpoint that interpretation was "necessary" only because Driesch made it from within a particular theoretical framework.

To begin with, Driesch clarifies what he calls the "basic philosophy" [Grundansicht] of his theory.[59] He says, "If I call the fate of each blastomere its prospective value, then I can summarise my experimental results as follows: The prospective value of each blastomere is a function of its position in the whole."[60] He points out that there is nothing "mystical" about the effect of position as Roux had assumed (presumably Driesch is referring here to some earlier criticism of his position by Roux, but there is no reference given) but rather that the effects of position can be explained in physiological terms. Although some historians[61] have assumed that Driesch's analysis is mechanistic because he goes on to discuss various chemical and physical means by which differentiation proceeds, such an interpretation fails to take into account the relationship between teleology and mechanistic causality. What Driesch is, in effect, arguing is that although one can certainly analyse the physiological processes by which differentiation proceeds, one can only explain the various processes with reference to the whole.

For example, in a section on the independence of basic morphogenetic processes, Driesch considers in what sense the development of certain parts of an organism can be seen as independent.[62] He notes on the one hand that different germ layers can develop independently (e.g., the development of the ectoderm of Echinus is independent of the presence of the complete endoderm.[63]) but, on the other hand, that each part develops according to a "specific formative cause"[64]which derives from its relation to the whole. He concludes, "That part A develops independently with reference to part B only shows that the specific cause of its differentiation does not lie in B."[65] Thus, he argues, the fact that these independently formed parts (in the earlier noted sense of independent) together form a functional whole indicates that a kind of "second order harmony"[66] exists beyond that which can be explained in purely physiological terms. When Driesch uses the expression "harmonious equipotential system" to describe the egg, he means it in the sense of this second order harmony, not just in the sense of physiological harmony, although this exists as well. In fact, Driesch uses a different expression, "functional harmony", when he is referring to physiological events.[67]

Driesch later referred to the theory of development presented in the Analytical Theory as his "machine theory" of life.[68] In some ways this appelation is appropriate, but it should not be conflated with the notion of a "mechanistic theory" of life because, in spite of the verbal similarity, the "machine theory" is certainly not mechanistic. Driesch makes this clear in a section of the Analytical Theory called "Where Teleology is Really Valid".[69] Driesch starts out by saying that one can compare physiological events to the working of a machine. He then claims that one can extend this machine analogy to the morphological realm provided (and this is a crucial limiting circumstance) one thinks of the finished structure as already "given" in the egg.[70] This "given" is then the "real domain of teleology".[71] Driesch concludes that "On the basis of this 'given', this 'machine', we understand with the help of chemistry and physics the functioning [of the organism] which is in principle very probably causal..."(In a footnote Driesch clarifies that he is referring to the very probable basic similarity of physico-chemical processes in both organic and inorganic realms; that is, he is denying the notion of a special "life substance".)[72] In other words, Driesch's so-called "machine" actually is the teleological principle, or rather, a personification of that principle. This analogy seemed sufficient as a means of conceptualising the working of the teleological principle, at least based on the experimental results that Driesch had achieved up to that point. However, he was never totally satisfied with this formulation and was soon to revise his views on this point based on further experiments, this time on regulation at the gastrula stage.

Before discussing this later formulation of the teleological principle, it is important to review Driesch's observations in the Analytical Theory since these constitute his major contribution to experimental embryology. Driesch was not the first scientist to study the processes of differentiation and regulation during embryogenesis, but he was the first to provide a theoretical framework on which further investigation could be based. That framework is Driesch's description of the egg as a "harmonious equipotential system" in which the "prospective value" of each embryonic cell is a function of its position in the whole. Further, as his experimental work demonstrated, the "prospective potency" of each cell is always greater than its "prospective value" since the latter can change if the relative position of the cell is changed. Driesch's theoretical framework forms a basis for embryological research even to this day.[73] For example, the gradient theory and the theory of nuclear equivalence are based on this framework. I have shown that Driesch was able to develop this framework precisely *because* of the teleological principle which informed his viewpoint. The "harmonious equipotential system" has a specifically teleological meaning for Driesch; in fact, it could not have been conceived *without* the teleological component. Once the conceptualisation had been made in these terms, there was nothing to prevent other scientists from redefining it within purely materialistic terms; that is, in the more narrow physiological sense of "harmonious". The historical record shows, however, that this immensely heuristic framework arose as a result of a teleological perspective.

What changed in Driesch's later formulation of his theory of development was *not* his fundamental belief in the necessity for a teleological component in the explanation of morphogenetic processes, but rather his conception of the teleological component itself. In the *Analytical Theory*, as we have seen, he thought teleology could operate like a machine. The teleological component was the "machine" which built the organic machine. In other words, the result of all the physico-chemical processes was ultimately coordinated and determined by a sort of machine which worked to a pre-set plan. As Driesch continued his experimental work, he came to the conclusion that certain morphogenetic events such as the "regulatory response" could not be adequately explained in what he now called "static-teleological" terms. He published his new position in an 1899 article entitled "The Localisation of Morphogenetic Processes. A Proof of Vitalistic Phenomena".[74] The work which led to this new position was begun in 1895 when Driesch took a completed sea urchin gastrula and cut it in half near the equator so that each half retained both ectoderm and archenteron.[75] The result was that each half healed over the incision and subsequently developed into a proportionally normal but smaller pluteus. How could this regulation be explained -- more precisely, what aspects of the regulatory response had to be accounted for in order to explain how regulation had occurred? The problem is not a simple one because the severed gastrula does not seem to be responding in terms of the particular location of the cut, but rather as though it somehow sensed the various elements which need to be rectified all at once. Not only must it reorient its parts, but also it must restore the proper proportions within these parts while taking into account the fact that there is less material.

It is important to remember that even in the Analytical Theory, Driesch had not seen his description of physiological processes such as chemical and contact induction processes between cells and tissues[76] or the directive role of the nucleus[77] as providing a complete explanation of development. These processes were the means by which the teleological "machine" built an organism. Driesch had, however, explained certain features such as the proportionality of the three sections of gut in purely physiological terms; that is, terms of induction. The later experiments at the gastrula stage highlighted a new problem -- this proportionality was maintained in spite of the fact that from the point of view of chemical stimuli, these would have necessarily been reduced by the cutting of the gastrula, and moreover, reduced arbitrarily, while the receptive cytoplasmic filters would similarly be arbitrarily relocated. Driesch therefore concluded that this regulatory response was "essentially not a mechanical, but a specifically vitalistic sort of phenomenon."[78] He eventually clarified what "specifically vitalistic" means and it is here that one finds the explanation of the distinction between "static" and "dynamic" teleology.

Driesch's basic argument is that the role which he assigned to teleology in the Analytical Theory was too limited. He had talked about different types of harmony in development but had envisioned a "static-teleological characteristic" as the basis for the various kinds of harmony.[79] Static teleology is machine-like because although an end is assumed, for example an organ with certain proportions like the three-part gut, that end is achieved by the action of a chain of chemical stimuli and responses which are set into action because of the relative position of embryonic cells. The cells are oriented in a certain way with respect to the whole and their differentiation is ultimately based on that orientation. Regulation is therefore ultimately dependent on whether or not the original orientation can be reproduced once some part has been removed. Regulation is purposive but it could be carried out on a machine-like basis under certain circumstances.

For example, imagine an engine connected to two sources of fuel so that if one source is depleted an automatic signal activates the second and the engine keeps running. The ultimate purpose of the system is to keep the engine running but it can be activated by purely mechanical means such as a sensor which sets off a signal when one fuel source is depleted. This sort of response is static or fixed even though such a system presupposes an end or purpose. Now imagine a person who is monitoring the engine and who notices that the first fuel source, though not depleted, has become blocked. The signal will not be sent by the sensor to switch to the second fuel source because there is actually still fuel available from the first source. The monitor will switch to the second source because, even though the problem was unforseen from the point of view of how the system was set up, the monitor can override the pre-set controls in order to ensure that the ultimate purpose -- keeping the engine going -- is achieved. It is relatively easy to explain regulation of the halves of a divided blastula like the case of the unattended engine. All that is needed to set the regulatory response into action is the loss of contact between the two cells which, in our analogy, is just like the sensor activating a signal when the fuel source is depleted.

It is not so easy to account for regulation at the gastrula stage because the proportions of fore-, mid-, and hind-gut are already set and an arbitrary incision (so long as it occurs near the equator so that each half contains ectoderm and endoderm) at this stage does not have such simply explained results. Returning to our analogy, there is no one signal or even sequence of signals which could set into motion the complex regulatory response. Like the observer, the cut gastrula perceives an unforseen problem (the blockage of the fuel source is like the disappearance of the other half) and overrides typical procedure in order to remain a functioning whole. Driesch points out the difficulties in trying to explain this regulatory response in simple cause-and-effect terms.[80] If one begins by saying that the re-oriented, re-proportioned half is the "effect", the question one has to ask is "what relation is there between effect and cause" and given this relation how can one "characterise the sorts of phenomena which we observe."[81] The re-orientation can hardly be considered as an "effect" of the arbitrary cut, at least not in the purely mechanical sense, because the same effect is produced regardless of the precise location of incision, just as our fuel source could be blocked for a number of different reasons, but still "cause" the operator to pull the switch. It should rather be seen as an effect of a much more direct intervention of the teleological sort, a "responsive phenomenon" [Antwortsgeschehen] or "indeterminate adaptive phenomenon" [indeterminirtes Anpassungsgeschehen].[82] This responsive phenonenon is characterised as "dynamic-teleological" because it is in no way pre-set or fixed, but rather is flexible in the same way that our operator is flexible in keeping the engine running.

The differences between the systems described above are obvious, but so are their basic similarities, that is, the idea of goal orientation. Having an operator to ensure continued functioning constitutes an improved system, but not a fundamentally different system. Both are qualitatively different from a true mechanical situation where the notion of any sort of back-up system is absurd. For example, it would be meaningless to talk about a "back-up" for gravitation if a dropped ball is impeded by a table from falling to the ground. The essence of a mechanical situation is its predictability. A ball pushed off the table <u>always</u> falls to the ground, it <u>doesn't</u> jump back onto the table because it wanted to stay there. An organism <u>does</u>, however, try to maintain itself as a viable whole.

Driesch concludes with a theoretical section on "The Concepts of 'Teleological' and 'Vitalistic'."[83] It is here that he first sets out his theory of vitalism and in doing so he takes the position that teleology is a broad concept which subsumes both "static" and "dynamic" versions. Only the dynamic sort of teleology is truly vitalistic, therefore Driesch concludes that his new theory of development can rightly be termed "vitalist".[84] In other words, Driesch has defined vitalism in a narrow sense which is determined by how one understands the teleological component in development. His position has changed *only* insofar as his view of how teleology operates has changed. Rather than concentrating on the difference between static and dynamic versions of the teleological principle, let us review the similarities but in a slightly different context.

In his later presentations of his theory of vitalism, most notably in his book The Science and Philosophy of the Organism, Driesch always referred to the concept of "entelechy" rather than dynamic teleology.[85] He says, "Entelechy, as we know, is a factor in nature which acts 'teleologically'."[86] He does not give it a specific ontological status because he "leaves open" the question of the relation of entelechy to both substance and causality.[87] He finally chooses to view entelechy as an ordering principle although under special circumstances the principles of substance and causality may be applied as well.[88] No matter how one defines entelechy relative to substance and causality, then, it functions somehow as an ordering principle. From the standpoint of experimental embryology it is not crucial to analyse entelechy itself, as long as its existence is taken into account in any analysis of development. Although the analysis of entelechy might be an interesting philosophical problem, it is not a problem for science. On the other hand, the fact of entelechy, in whatever form, is important for science because it contributes a unique perspective from which to view organic development. But this is virtually the same as saying that a teleological component results in a unique perspective. Driesch may be correct in making a distinction between static and dynamic teleology, but from the point of view of developing a framework for scientific research, this distinction would not alter one's basic research approach.

From an historical perspective, how are we to understand the shift in Driesch's position? Most historians have taken the position that Driesch's new theory does indeed represent a sudden "switch to vitalism". His earlier work then seems easy to fit into a more mechanist approach and to then link with the theoretical framework of many contemporary biologists. His early references to teleology are seen as a somewhat inexplicable aberration in what is otherwise a truly reductionist framework. I have shown, however, that the "switch" in Driesch's thinking was from the static to the dynamic teleological concept. Teleology in some form was always an integral part of Driesch's theoretical framework. In other words, there are aspects of continuity (the teleological principle) and aspects of discontinuity (the precise working of that principle) in Driesch's theoretical framework. The historical problem is whether one should understand Driesch's scientific career as having been guided by two quite separate theoretical approaches or by one approach in which certain important variations occur. The preceding exposition has taken the latter view. Now it is necessary to give a critical appraisal of the former view, which is held by a number of analysts.

CHAPTER II

CONTEMPORARY ANALYSIS OF DRIESCH

A CRITICAL REVIEW

Any study of the work of Hans Driesch must somehow reconcile his participation in the programme of Developmental Mechanics [Entwicklungsmechanik], pioneered by Wilhelm Roux (1850-1924), with his philosophy of vitalism. In the previous chapter, I argued that all the scientists working in Developmental Mechanics shared a rationalist approach and further, that Driesch's vitalistic framework based on Kant's teleological principle was logically consistent with that rationalist approach. Traditionally, however, historians have accepted Developmental Mechanics as the product of a mechanist philosophy of biology and have regarded Driesch's experimental work as having been conducted within this framework. Vitalism, in this view, is then seen as a philosophy which Driesch turned to towards the end of his experimental career; in other words, it is not a part of Driesch's work in science, but rather something which led to his departure from science and the beginning of a new career in philosophy. Jane Oppenheimer, who laid the groundwork for much of the research in the area of history of embryology, is a well-known proponent of this view.

In an essay devoted to elucidating the main contributions to experimental embryology of Roux, Driesch and Theodor Boveri (1862-1915), Oppenheimer states, "Well, Driesch read philosophy, to which he was inclined, and became lost in admiration of the regulatory powers of the embryo, and eventually, after performing a number of other important experiments, he became a professor of philosophy and a vitalist."[1] In the editors' introduction to an early paper by Driesch, Oppenheimer and her co-editor, Benjamin Willier remark, "The results of his [Driesch's] experiments of separating the blastomeres *drove him to vitalism*, since he could not conceive of a machine which, when divided, could reconstitute two whole new machines like its original self."(my italics)[2] This view of Driesch as figuratively throwing up his hands and deciding that the problem of organic development was inaccessible to scientific investigation is once again reinforced by the observation that Driesch left his scientific career and became a professor of philosophy. There are a number of unexamined beliefs on which this view is based, none of which is supported by a close examination of the historical record.

The first and perhaps most important underlying assumption which needs to be reconsidered is that of the fundamental incompatibility between Driesch's later vitalism and his participation in the Developmental Mechanics programme. The source of the incompatibility presumably rests in the fact that Developmental Mechanics is the product of a mechanistic or, more specifically, reductionistic philosophy of biology. The very presence of the word "Mechanics" in the designation of the programme certainly does nothing to discourage one from thinking of it as thoroughly mechanist. But how well, one must ask oneself, does the term "mechanist" describe Roux's actual programme?

A really exhaustive answer to this question would require a thesis of its own, but there exists in the literature on Roux some persuasive arguments against the appropriateness of the term "mechanist" to describe his Developmental Mechanics programme.[3] Cassirer points out, for example, that Roux's concept of a "typical occurrence", an indispensible feature of his whole conceptual framework, was in no way explicable in terms of strict mechanism, rather, it was only understandable within the framework of the Aristotelian concept of "form". By "typical" Roux meant typical for organisms of a particular species, so that one might say that in spite of similar conditions or a similar environment, the eggs of a chicken and a duck each develop in a "typical" fashion. Such a concept requires the notion of an original and specific "disposition" of an organism which is fulfilled in the course of development. This language is certainly teleological even though as Cassirer stresses, Roux did not draw any metaphysical conclusions from this Aristotelian concept of form but used it in a purely methodological way.[4] He concludes, "From now on *developmental* mechanics speaks in something like another language, far removed from the usual language of mechanics and possessed of its own dictionary and its own syntax."[5] If one accepts these arguments, a different picture of Driesch's relation to this programme emerges. His later work must not be seen as a major departure from earlier work commonly associated with Developmental Mechanics; rather, a strong thread of continuity is apparent because Developmental Mechanics *itself* is not truly mechanistic.

It is important to stress that Oppenheimer does *not* discuss the changes in late nineteenth century embryology in terms of a conflict between mechanist and vitalist approaches to biology, since in essence she does not admit that there *is* a vitalist approach within science. Rather, her focus is on the rise of an experimental methodology which replaced the older descriptive approach in embryology. In this view, the methodological shift explains the switch in interest from phylogeny to ontogeny. Oppenheimer even goes so far as to equate the beginning of the Developmental Mechanics programme with the beginning of experimental or, to use a more modern term, analytical embryology.[6] While Oppenheimer is correct in stating that this period saw the rise of the experimental method, one cannot equate it exclusively with a shift in interest to ontogeny. The French scientist, Laurent Chabry (1855-1893), for example, performed very similar experiments to Roux but still worked in the Cuvierian taxonomic tradition. This was demonstrated by Frederick Churchill in his article, "Chabry, Roux and the Experimental Method", in which he shows that while these scientists contemporaneously used an experimental approach and even performed similar experiments destroying early blastomeres of Ascidian or frog embryos, they represented widely different scientific traditions.[7] Churchill states: "That their beliefs differed becomes apparent when historians contrast Chabry's spatial use of mechanical metaphors and quest for anatomical laws with Roux's analytic employment of metaphors and functional approach."[8] A given methodology can support a variety of scientific or metaphysical traditions. Experimentation, in the sense of active manipulation of the object of study, is clearly not a sufficient means of characterising a scientific tradition. Oppenheimer's explanation of the changes in late nineteenth century embryology mistakenly assumes this to be the case.

Oppenheimer clearly wants to portray philosophic vitalism as a dead end in terms of any interpretation of experimental results. For example, in trying to evaluate the relative contributions of Roux and Driesch, she argues, "Roux had the perspicacity to appreciate that the embryo could be grappled with experimentally; Driesch, though he made a great experimental contribution to embryology, lacked it, and was so steeped in metaphysics that he finally made his option for philosophy proper."[9] He was really driven from the straight and narrow! To give her the benefit of the doubt, Oppenheimer must mean that Driesch was so steeped in a *particular* metaphysic (i.e., vitalist), since all good science has *some* metaphysical foundation. The argument is then made that, "Roux interpreted the egg for the first time as a mechanism mechanically analysable by outside interference; Driesch envisioned it as ruled by an entelechy as spiritual as any *deus ex machina* must be."[10]

Leaving aside, for the moment, Oppenheimer's very cursory dismissal of the concept of entelechy, this whole line of argument hardly makes sense. How could Driesch make "a great experimental contribution" while presumably being unable to appreciate, as Roux did, that "the embryo could be grappled with experimentally?" Why would anyone work in experimental embryology if he or she were convinced that the embryo was not susceptible to such investigation? Further, Oppenheimer fails to note that the concept of entelechy is only fully developed by Driesch about ten years after his experimental career began. As I have shown, Driesch's concept of entelechy was a *result* of the interpretation of his own experimental results as well as of similar results by other researchers, but Oppenheimer argues as though its development had nothing to do with experimental work at all. In addition, as I have already indicated above, Roux's analysis of the egg or embryo was itself far from mechanistic, relying on the methodological, if not metaphysical, use of teleological concepts.

Oppenheimer makes one very sweeping generalisation about what she calls the "great progressive minds of embryology"; it is that they are investigators who have learned "to address the embryo by the right question; and these are the men who have derived their intuitions primarily from the study of the embryo itself."[11] Roux is identified as belonging to this group along with Aristotle,[12] Caspar Friedrich Wolff and Karl Ernst von Baer, among others. The other group of investigators, which includes Goethe, Geoffroy St. Hilaire and Ernst Haeckel as well as Driesch, "examined their embryos to fit their observations into philosophical patterns already set and rigid" and "were the minds whose philosophical patterns delayed rather than accelerated the course of embryological progress."[13] Only a few sentences earlier Oppenheimer had granted that Driesch made "a great experimental contribution" even though he was unable to appreciate the susceptibility of the embryo to experiment. Now he is suddenly identified as one who delayed the course of embryological progress.

Clearly, if one is to accept Oppenheimer's claims about "embryological progress", one will need some crucial information which has not been provided. For example, how does one know if and when one is addressing the "right question" to the embryo? How can one tell if a theory delayed or accelerated "the course of embryological progress"? Oppenheimer tells us that even those in the "progressive" group, "started out from philosophical and theoretical premises, but in such a way that they relegated the initiative of answering their problems to the embryo itself; they could do so only because it was the embryo that gave them their clues as to how to ask their questions."[14] Again, one must ask exactly how one group's philosophical premises interfered with direct access to "clues" from the embryo while another group's did not.

It should be fairly clear by now that Oppenheimer is employing a double standard (with all the advantages of hindsight) to support her categorisation of progressive versus regressive investigators in embryology. The progressive group includes anyone whose work can be reinterpreted within the framework of current science as somehow leading up to that science. The regressive group, on the other hand, consists of those whose work cannot be made to fit within the current scientific framework. In other words, Oppenheimer's sole criterion for defining embryological progress is whether or not historical theories seem to be in line with currently existing theory. She does not even offer a way of determining this correspondence other than by arguing for some unexplained superiority on the part of certain investigators in their ability to discover clues from "the study of the embryo itself." Briefly, then, Oppenheimer's categorisation is both tautological and presentist. Acceptance of Oppenheimer's view has prevented serious attempts to reevaluate the validity of Driesch's contributions. Even those who do attempt to analyse Driesch's work in embryology more or less independently often make some of the same basic errors as Oppenheimer, as well as a number of others.

In his article, From Machine Theory to Entelechy: Two Studies in Developmental Teleology, Frederick Churchill sets out to examine what he refers to as two "explanations of development" put forth by Driesch in 1894 and 1899 respectively.[15] Although this study embodies a high level of technical sophistication, its emphasis on the aspects of discontinuity in Driesch's work tends to reinforce the myth of Driesch as being led to vitalism later in his career. In this vein, for example, Churchill states: "That Driesch turned away from his earlier machine-theory of life and eventually from science altogether should not conceal the fact that he had been a highly ingenious experimenter and that many of his theoretical discussions of embryological events carried on in a style very similar to Roux's own quasi-philosophical forays."[16] Unlike Oppenheimer's naive view of a too metaphysical Driesch versus a "great experimenter" Roux, Churchill's depiction of both scientists as having a philosophical style recognises a fairly common feature of nineteenth century German embryology; that is, a tendency for scientists to have had more background in philosophy and to view philosophy as more relevant, not to mention integral, to their scientific pursuits than is the case today.

Nevertheless, Churchill still implies, although to be fair he does not state explicitly as Oppenheimer does, that Driesch's later theory and his "turning away" from science are somehow causally connected. Driesch's theory of entelechy develops from his scientific work but it leads to a break with science. Admittedly, it is very tempting to take this view for a number of reasons, but it rests on false assumptions. In the first place, such a view assumes that if a person makes a career shift from experimental science to philosophy, that move in and of itself shows that he or she has "turned away from science altogether." But this is absurd! People change careers for all sorts of reasons which have nothing to do with beliefs. In Driesch's case, at least one highly plausible alternative exists. It would have been extremely difficult for Driesch to achieve the position of Ordinarius in a medical faculty of a German university given his vitalistic approach to biology, not to mention his criticism of the system which divided biology into two faculties, philosophy and medicine.[17] As I already mentioned in the previous chapter, many of the best experimental scientists were associated with institutes rather than university faculties per se. Driesch had been able to pursue an independent research career thanks to his private means, but he had no connection with any university for a large part of that career. He was probably able to get a position in the philosophical faculty at Heidelberg in 1909 only as a result of his impressive record in research and, perhaps more importantly, because of his selection as the Gifford lecturer at the University of Aberdeen in 1907, which was a highly prestigious position. In any case, it was several years *after* Driesch presented the theory of entelechy that he completely gave up doing experimental work, so one might at least claim that Driesch himself saw no incongruity between his experimental work and his philosophical position.[18]

Churchill's more narrow internalist view of Driesch's later and more explicitly vitalistic theory of embryogenesis is based on an assumption that Driesch was searching for a mechanistic explanation but was forced into vitalism because of the inadequacy of that explanation to account for the so-called "regulatory" response. He argues that Driesch's problem was the result of his failure to explore more deeply the nature of the nucleus.[19] In fact, he concludes, "I suspect Driesch's ultimate turn [to vitalism] was closely linked to his conviction that the nucleus was comprised merely of a uniform mixture of chemical ferments; that is, that the chromosomes had no formatively significant organization."[20] Further, Churchill contends that because the early Driesch was so strongly "reductionist" (in the sense of reducing cellular events to simple chemical stimuli and responses) he was unable to explain development on this basis since "he denied himself the possibility of utilizing a broad range of internal cellular "formative" events."[21] He contrasts this approach with that of August Weismann who discussed the role of chromosomal elements in development and Theodor Boveri whose experiments "fruitfully pursued that approach *in vivo*."[22] This thesis needs to be examined more closely because it contains a number of flaws.

The essential problem with Churchill's interpretation is the implicit assumption that Driesch would have prefered a mechanist explanation, but he was "forced" into vitalism by his too simplistic view of the nucleus. There is nothing in the historical record to suggest that this was the case, rather all the evidence points to the contrary. For example, in one of his earliest works published in 1891 on the mathematical-mechanical examination of morphological problems in biology, Driesch is careful to distinguish between elements of morphology which are susceptible to mathematical or mechanical investigation and those which are not.[23] While Driesch believes much can be explained in mechanical terms, he expresses a very Kantian approach to biology[24] when he says, "That we cannot carry on the following discussion [of morphogenesis] in terms of a systematic construction is a result of the very nature of things. Even though we shouldn't in any way underestimate the value of the genuinely mechanical efforts with which we are now occupied, we mustn't be blind to the fact that it is only fragments which are available [vorliegen] to us; fragments perhaps which have not touched on the really essential part of living forms at all."[25] Driesch is not merely saying that the current state of knowledge is fragmentary, rather he is making the same point as Kant that what is available to human knowledge is *fundamentally* restricted.

In his analysis, Churchill mentions briefly that Driesch supported his teleological interpretation of the organic world by appealing to Kant. He then continues, "Setting aside the question of whether he [Driesch] properly understood the Critique of Teleological Judgement, it is clear that he embraced a teleological approach..."[26] Churchill raises doubts about Driesch's understanding of Kant but he never returns to that issue, nor does he offer either his own interpretation of Kant's position or any indication of just what he finds objectionable about the teleological approach Driesch adopts in his famous study of 1894, The Analytical Theory of Organic Development. This absence of discussion makes it difficult to understand Churchill's later attempt to contrast Driesch's use of teleology in 1894 with that in the 1899 study, "The Localisation of Morphogenetic Processes, a Proof of Vitalistic Phenomena".[27] His own evidence from Driesch's writing seems to contradict the point he is trying to make that Driesch's "declaration of vitalism" in 1899 constituted a turning away from his earlier "machine-theory" of life.[28]

For example, Churchill refers to the last section of the Analytical Theory where "Driesch reaffirmed his intention to stay within the strictures of what he considered Kantian teleology."[29] It is precisely in this section that Driesch refers to the "given" structure of the organism as both the "real domain of teleology" and the "machine",[30] leaving no doubt that the "machine" controlled the physiological processes of development.[31] Churchill says, "Causal analysis revealed the harmony in the developmental processes, teleology explained the givenness of the starting structure."[32] But I have already shown that Driesch distinguished between the overall harmony in developmental processes which could only be accounted for through teleology (the harmonious-equipotential system) and the physiological harmony (functional harmony) which was subject to causal analysis.[33] In other words, Churchill is wrong in saying that Driesch believed harmony in development could be causally analysed. He felt that only the harmonious processes themselves could be analysed. In the machine-theory the regulatory response is triggered by physico-chemical stimuli, but the response itself is nonetheless teleological not mechanical. In the later theory Driesch argued that the response is triggered not by a specific mechanical stimulus, but rather by the ability of the organism to perceive the "atypical" situation and act to ensure "typical" development, as Churchill notes.[34] Thus, Churchill's evidence from the Analytical Theory and "The Localisation of Morphogenetic Processes" actually reveals the underlying continuity of Driesch's teleological approach to morphogenesis, rather than demonstrating a sudden switch[35] to vitalism.

As to Churchill's argument that Driesch was "forced" into vitalism because "he failed to recognise the extent to which his machine-theory had snared him by the promise of the simplistic chemical solution as suggested in

Herbst's catalogue of taxic and trophic responses",[36] while plausible, it is not historically correct. While it is true that Driesch did not accept chromosome theory in 1899 (along with the majority of embryologists at that time), this fact in no way initiated his vitalist views. Further, when Driesch did accept the importance of genetics and the organisation of genes in chromosomes, he did not see it as a reason to reject vitalism, rather, he found these new theories completely compatible with his own.[37] Thus, Churchill's claim is logically as well as historically suspect.The problems which Driesch had raised in the three crucial areas of regulation, regeneration and reproduction could not be solved by genetics and, in fact, remain problems today.

Churchill also claims to find support for his argument in Driesch's own memoirs, but his evidence is somewhat misleading. The section to which he refers is one in which Driesch recalls beginning to think in terms of a vitalist explanation of his experimental results as early as 1895. This certainly sounds as though Driesch changed his mind about something, but was the change from a mechanist to a vitalist explanation of development? It seems clear that the answer to this question must be no. Driesch says he began to think in terms of a vitalist explanation, *not* in a general sense, but in terms of his *experimental results*.[38] In other words, while his general approach had always been a vitalist one, it was only later in his career that he began to give a what he called a specifically vitalist interpretation to earlier experimental work which he had previously felt could be sufficiently described in physico-chemical terms. This refers, of course to the problem of the triggering of the regulatory response, not to the nature of the response itself, which was always seen as teleological. He *never* felt that the whole of embryogenesis could be explained in mechanical terms. Specifically, Driesch gradually came to the realisation that his earlier view of how the principle of teleology operated was not sufficient as an explanation of regulation or regeneration whereas his later view adequately accounted for these phenomena.

Another study which presents Driesch as starting out with a mechanist approach and only turning to vitalism when experimental results could not be explained is that of Horst Freyhofer. In his book, The Vitalism of Hans Driesch: The Success and Decline of a Scientific Theory, Freyhofer sets out to explain why vitalism became popular within science in late nineteenth century Germany and why that popularity declined so rapidly in subsequent decades.[39] Actually, Freyhofer devotes very little space to Driesch's scientific work but analyses at great length his theory of vitalism as a "general theory of knowledge and reality."[40] This analysis is philosophical rather than historical and, as such, it fails to answer the question of why vitalism rose and fell as a theory within science during Driesch's lifetime. From a philosophical point of view, Freyhofer's criticism of Driesch's theory may or may not seem convincing, but from the point of view of history this criticism is irrelevant. Freyhofer merely reiterates many of the contemporary arguments against Driesch as well as adding a few more recent ones from positivist philosophy, but such arguments do not add to historical understanding.

Indeed, Freyhofer's metaphysical argument begs the historical question. Even if one could demonstrate the superiority of one metaphysical position over another, it would have no bearing on the historical issue of the popularity of a given metaphysical position during a given time period. For example, Freyhofer quotes chemist Alwin Mittasch, who argued in 1936 that the Drieschian concept of entelechy explained nothing but merely defined the limitations of science.[41] In other words, entelechy is, in this view, extra-scientific. The historical question one might ask here is not whether Mittasch's argument is correct but why the trend in the scientific community was to favour such an argument at the time. The answer to that question cannot be found in philosophy but must be looked for in the historical record. For example, it could perhaps be argued that the popularity of the views of the Vienna Circle in the 1930's as opposed to the earlier popularity of neo-Kantianism influenced the decline in popularity of the vitalist approach in biology. Anyone with a sense of history cannot fail to be aware of the long-standing battle between vitalist and mechanist approaches to biology from the time of Aristotle and Democritus right up to the present. One or other approach may have dominated for a time but never exclusively. Freyhofer seems to ignore this historical reality and to assume that vitalism declined simply because it was an inferior metaphysical basis for scientific research.

As to his relatively brief discussion of Driesch's work as an experimental scientist, Freyhofer unquestioningly presents the standard view of Driesch's switch from his earlier "machine-theory" to his theory of entelechy as a "switch of root metaphors".[42] Freyhofer reveals his confusion between the underlying metaphysic and the interpretation of experimental results when he says, "Driesch subsequently gave up the mosaic theory and adopted an as yet loosely defined vitalist idea, soon to be known as regulative theory."[43] The problem with such an interpretation is that it assumes mosaic theory to be conceivable only within a mechanist framework while regulative theory must be thought of as vitalist. Freyhofer thus misses the point, which is that each theory can be compatible with either metaphysical approach so that showing Driesch as having switched theories does not automatically demonstrate that he thereby switched metaphysics. In any case, Freyhofer makes a fundamental error in associating "machine-theory" with mosaic theory.[44] Driesch *never* supported the latter; in fact, his earliest experiments were crucial in disproving the main tenet of that theory, the principle of organ-forming germ areas which had been developed by Roux and August Weismann.[45]

Freyhofer cites a passage from Driesch's memoirs which is also used by Churchill in his article to support the view of a dramatic shift to vitalism.[46] In this rather well-known passage, Driesch refers to a "quite fundamental change" in his position which came upon him suddenly during a walk in the woods near Zurich in 1895.[47] In Freyhofer's analysis this quotation is not put into any context, but Churchill at least gives an explanation of just what the problem was which caused Driesch to question the earlier "machine-theory"; that is, the regulatory capacity of the embryo.[48] Churchill's discussion of the problem fails only when he moves from the experimental work, per se, to his interpretation of the underlying metaphysic. Freyhofer's description of the experimental side is almost non-existent and his insistence on using jargonistic philosophical phrases to describe Driesch's theory switch is, to say the least, obfuscating.[49] Further, since Freyhofer and Churchill both recognise that Driesch reverted to his earlier position in an article subsequent to the "sudden conversion", they have the problem of explaining his inconsistent behaviour. This they do in an unconvincing manner.

Given a commitment to the interpretation of Driesch as having made a sudden switch to vitalism as early as 1895, both Freyhofer and Churchill then explain away the 1896 study, "The Machine Theory of Life: a Word of Explanation",[50] by arguing that Driesch was virtually bullied into recanting his vitalistic approach because of adverse criticism from both Roux and Emil du Bois-Reymond.[51] Churchill at least gives some evidence to support his interpretation but Freyhofer offers the reader nothing to support his argument that the "devastating criticism of the prestigious Du Bois-Reymond ... called the young Driesch to order."[52] In the first place, there is nothing in "The Machine Theory of Life" which indicates that Driesch was backing down from his position. Churchill misunderstands Driesch's point when he quotes him as saying, "What I represented therefore was absolutely not 'Vitalism', but, at least as far as living phenomena come into the question, was directly *the current view of physico-chemical dogmatism.*"[53] This quote is, in fact a reply to a specific criticism made by Roux.

It is important to understand the nature of the attack on Driesch in order to appreciate his response and it is crucial to keep in mind the historical context of the debate. Very early on in his paper Driesch notes that Roux accuses him of viewing living processes and typical embryonic development [Eibau] as being "very simple".[54] Far from backing down, Driesch attacks Roux for implying a distinction between simplistic [grob] and sophisticated [fein] physico-chemical processes which are never clarified and which, in Driesch's opinion, cannot be meaningfully defined. Further, he points out that Roux's own interpretation is not entirely unlike his own, and where he talks of each "living phenomenon" [Lebensgeschehnis] as not "chemico-physical" and of "purposefully acting mental capacities" [zweckthaetiger seelischer Leistungen] he too could be labelled a vitalist.[55] Finally, Driesch argues that the way out for Roux between the Scylla of "simplistic" physico-chemical processes and the even more dangerous Charybdis of some form of vitalism leads him to an unsatisfactory solution which contains an obvious internal contradiction. He says, "Roux inveighs against the interpretation of each individual living phenomenon [Lebensgeschehnis] in a physico-chemical manner, only to finally give a physico-chemical explanation himself."[56]

I think Driesch makes a valid point when he argues against some artificial distinction between simplistic and sophisticated physico-chemical processes and suggests instead that one should differentiate between processes which are physico-chemical and those which are not,[57] by which he means teleological processes like regulation. That was precisely the view which Driesch did present in The Analytical Theory, as I indicated above. Roux was arguing that Driesch's theory was "vitalistic" because his understanding of physiological processes was unsophisticated; that is, that a more "sophisticated" treatment of physico-chemical processes could have accounted for the development of form. Interestingly, Churchill in effect recapitulates this argument himself when he suggests that Driesch's "dilemma" and subsequent "declaration of vitalism" resulted from his reliance on the "promise of the simplistic chemical solution ... of trophic and taxic responses."[58] I have already pointed out above that the fact that Driesch did not recant his vitalist views even after accepting chromosome theory indicates that Churchill's explanation is not in accord with the historical record. Driesch pointed out correctly that his theory took into account all the latest experimental evidence then available, which did not include extensive knowledge of chromosomes. Chromosome theory, as mentioned earlier, became fasionable only much later. Thus, he had indeed presented "the current view of physico-chemical dogmatism" and was therefore merely claiming that he had not presented an over-simplified view of "living processes"; i.e., the physico-chemical events associated with embryogenesis.

In fact, Driesch's "machine-theory" did go beyond that physico-chemical dogmatism because it was a fundamental feature of his approach that this dogmatism could *never* account for the purposefulness of form. So why did he seem to object so strenuously to the vitalist label? For one thing, Driesch was loath to be forced into Du Bois-Reymond's camp of "neo-vitalists" which included anyone who rejected a purely mechanist, i.e. physico-chemical, explanation, whether on scientific or religious grounds. Driesch objected to the conflation of his scientific theory with others which had no basis in science but were predominantly theological. Specifically, he argued that DuBois-Reymond made no distinction between the "metaphysical-theological discussions" of a vitalist named Rindfleisch and Driesch's own presentation of the "problem of vitalism" with respect to experiments on regulation. Driesch makes his point even clearer when he continues, "One would like to be spared the word 'neovitalist': Bunge has his view, I have my view, another has a third view on the fundamental problem [das Fundamentale] of life. If these views have only somthing negative in common are they therefore identical, or even similar?"[59] Driesch wanted to dissociate himself from any specific vitalist camp but he recognised an affiliation with other scientists who shared his approach such as Oskar Hertwig or Gustav Wolff, whose work on lens regeneration he had cited earlier in the paper.[60] Far from backing down or being "called to order", to use Freyhofer's image, Driesch was standing up for his theory as being a valid scientific approach and, in effect, was refusing to be tarred with Du Bois-Reymond's excessively broad brush of "Neo-vitalism". Similarly, Churchill, Freyhofer and Oppenheimer can be seen as committing the fallacy of guilt by (false) association.

The last aspect of "machine-theory" which Driesch clarified but did <u>not</u> back down from was the concept of a "formative drive" [Bildungstrieb]. The first part of his argument is somewhat difficult to restate in English because it relies on a grammatical distinction which, while it also exists in English, is less clear to the English speaking reader. Restated in more general terms, the point seems to be that the formative drive can be examined only as a result not as a process.[61] Driesch then switches to the term "life force" [Lebenskraft] and remarks in his concluding sentences that "at this time we can only say one thing; that is, that this 'life force' is in any case not a 'force' in the sense of a specific type of energy. But then, what is it?"[62] This was the question which Driesch eventually tried to answer with his concept of entelechy, which was discussed in the previous chapter. Driesch's article of 1896 is nothing more than a clarification of his position for those who obviously misunderstood it, and not a reversal of his earlier views. His "discovery" in the Zurich woods was of the distinction between static and dynamic teleology, not a sudden conversion to a teleological viewpoint per se.

This he had always had.

The attempt to separate Driesch's scientific work from his vitalist philosophy is given a new twist by Reinhard Mocek in his book, Wilhelm Roux-Hans Driesch: On the History of Developmental Physiology of Animals ("Developmental Mechanics").[63] As a traditional Marxist, Mocek approaches his study from the standpoint of dialectical materialism and his analysis is shaped by his commitment to keeping within the framework of that agenda. In looking at the contributions to biology of both Roux and Driesch, Mocek is in effect measuring each researcher's approach to the problems of developmental biology against the yardstick of the "correct", dialectical approach. Unlike any of the other analysts considered so far, Mocek does not implicitly take the position that mechanism is the preferred, "normal" metaphysical approach to science. In fact, from the standpoint of dialectical materialism, mechanism is almost as flawed as vitalism. This fact has some interesting implications for Mocek's analysis in which an attempt is made to reinterpret not only Driesch's vitalism but also Roux's mechanism.

Although at times Mocek's analysis seems a bit contrived as a result of his own overriding epistemological concerns, it at least tries to come to grips with the important problem of the philosophical context surrounding an approach to scientific analysis. As Donna Haraway notes in her review of Mocek's book, "most other treatments of Roux and Driesch have not developed the important neo-Kantian context of *Entwicklungsmechanik* at all, or only grudgingly."[64] Insofar as Driesch is concerned, Mocek argues that his vitalism was a result of his experimental work in biology. He says: "Biology is the basis of vitalism, but not the reverse."[65] However, while he does give serious attention to the Kantian element of teleology as an integral part of Driesch's "machine-theory", Mocek's analysis of the 1899 switch to vitalism is unexpectedly reminiscent of Churchill's, complete with the obligatory mention of the walk in the woods near Zurich in 1895. He explains the difference in Driesch's "machine theory" of 1894 and the theory presented in 1899 as follows: "What was still accepted as the harmony of chemical releases with the receptivity of the germ in the 'Analytical Theory' now becomes defined as a specific vitalist result, removed from the purview [Zustaendigkeitsbereich] of known formative stimuli and made an independant factor."[66] In other words, he is following Churchill's analysis in seeing the "harmony of chemical releases with the receptivity of the germ" as a harmony which could be explained in mechanistic fashion rather than one which could only be accounted for in a teleological fashion in any case.

Mocek's explanation is correct as far as it goes, namely in recognising that the problem Driesch identified was whether the source of the regulatory response was mechanical or not, but unfortunately it gives the impression that Driesch had been satisfied with a chemical solution to the problem of morphogenesis in the Analytical Theory which is not true. There was still an "independant factor" even in the "Analytical Theory" and that was the "formative drive" [Bildungstrieb]. A little earlier in his exposition, Mocek had argued that both Driesch and Roux had been confronted with the problem of "morphological purposiveness" [Zweckmaessigkeit]. Roux had found a solution based on functional adaptation within a Darwinian frame of reference, but according to Mocek, Driesch's view of evolution theory as "nothing but an explanatory authority [Erklaerungsinstanz] for ontogenetic processes led (him) to a mystification of purposiveness ..."[67] Mocek seems predisposed to view the concept later to be known as entelechy as an expression of mysticism because it is a non-material factor, but the postulation of such factors is not unknown even in other areas of science.[68] The concept of entelechy was ultimately described by Driesch as a kind of "ordering principle" but while he thought of it as non-material, that did not therefore automatically mean it was a "mystical" concept.

Thus, although Mocek sees a connection between Driesch's science and his philosophy the flow goes in only one direction, from biology to philosophy. I believe this interpretation begs the question of how science and philosophy are related in general and it relies on the well-worn assumption, introduced by Oppenheimer, that Driesch's vitalism could not have been a positive factor in the development of scientific theory but must have been a retreat from science altogether in the face of an unanswerable question. If Mocek were only claiming that there are aspects of Driesch's later work in philosophy which go beyond anything that he dealt with, or even thought one could deal with, within a scientific framework, then I would have no problem agreeing with his position. I also think that his work in biology was a necessary prerequisite to the further development of his philosophical position after 1910 when he finally left biology to concentrate on philosophy. The fact remains, however, that his metaphysical commitment to the necessity for a teleological as well as causal analysis of embryogenesis influenced the way he approached embryology from the very earliest stages of his career. However modern analysts may view the concept of entelechy, it is clear that

Driesch himself saw it as an improvement on his earlier model <u>because of</u>, not in spite of the greater role it assigned to the teleological principle.

CONCLUSION

At the beginning of this study, the concepts of vitalism and mechanism were discussed and it was suggested that there exists a sort of definitional double-standard according to which vitalism tends to be given a "strong" definition while mechanism is "weakly" defined. I argued that only a weak definition is suitable for the purpose of determining the hard core of a scientific research programme since, by definition, only the most fundamental assumptions are needed to make up the hard core. Using this approach as a basis for defining vitalism for the purposes of this study, I then took the position that a vitalist approach had been an integral part of Driesch's theoretical framework. In other words, so far as Driesch's work as an experimental scientist was concerned, vitalism was a consistent part of the hard core of the research programme within which he worked. The fact that there were important differences between the earlier and later formulations of his theory of development was noted, but it was argued that these differences were not relevant in terms of Driesch's theoretical framework <u>within</u> science.

Driesch's description of the egg as a "harmonious equipotential system" was shown to have a specifically teleological meaning in his <u>Analytical Theory</u>. Similarly, the concepts of "prospective potency" and "prospective value" were conceived within a framework in which the concept of a teleological principle was essential. Turning from this earlier description to the much later one given in The Science and Philosophy of the Organism, it is important to note that Driesch's discussion of development employs these exact terms.[1] This fact suggests that, in spite of the new characterisation of the way teleology actually operates, the scientific observations which Driesch makes remain the same, broadly speaking, although there are differences in the finer details of his analysis. From a scientific perspective, then, the differences in the earlier and later formulations of the theory should be seen as differences *within* a research programme, not as differences *between* research programmes. Lenoir's examples of different varieties of teleomechanism provide an analogous situation except that in the cases he cites each variety is represented by a separate set of individuals, whereas in Driesch's case one person represents two varieties of the research programme at different points in his career.[2]

In the critical section of this study, one feature common to most of the analyses considered was the tendency to see vitalism either as something totally incompatible with science or as something one would turn to as a last resort when mechanist explanation seemed inadequate. Even the latter position implies that turning to vitalism means turning *away from* science. The only study which deviates slightly from this outlook is that of Mocek and the reason for this is not a more sympathetic view of vitalism but a less sympathetic one of mechanism. I have shown that the external evidence for this view, i.e., the argument that Driesch left science altogether after his "conversion to vitalism," is both historically inaccurate and logically suspect. Personal, institutional and political circumstances influenced Driesch's eventual association with a philosophical rather than a medical faculty. Further, he continued to do experimental research in embryology for more than ten years *after* his supposed "conversion to vitalism".

Why does it seem so difficult for all these analysts to see the possibility that someone would actually prefer a vitalist explanation rather than viewing such explanations as last resorts or admissions of defeat on the battleground of science? I suggested at the outset that historians tend to reconstruct science ahistorically, according to currently accepted metaphysical views. A common implicit assumption in the analyses considered seemed to be that there was a "correct" way of doing science which produced "good" scientific theories; that is, theories which were both consistent with the data and which suggested heuristic avenues of further research. This line of reasoning takes the position that from a *methodological* point of view the scientist should always opt for a reductionist, materialist approach because it is the most useful for producing empirical results. The history of science definitely does not uphold that position since some of the best results have been produced within methodological frameworks which we would now consider highly questionable.

Another problem with the approach just discussed is its failure to deal with Kant's position on biology. Remember that Kant had argued in the Critique of Judgement not only that the teleological approach was *consistent* with scientific investigation of living organisms, but that it was *absolutely necessary*. Others may consider it *not* necessary, but then they must account for their position fully; that is, they must counter Kant's position, not just disagree with it. Further, while many admit the heuristic value of teleology purely as a methodological approach, they take the position that at some point in the future a totally mechanistic explanation will be found. The basis for such a position is no less metaphysical than that of any vitalist who believes, along with Kant and Driesch, that teleological explanations are fundamentally indispensible, yet this view is often accorded the status of an unquestioned basic principle which requires no support whatsoever! When this view is carried over into the practice of history of science it implicitly downplays the importance of the underlying metaphysical foundations of science and it also fails to take into account the sociological aspect of science.

At the beginning of this century, the view was first put forth that we tend to accept empirical results more or less at face value because we are to a large extent socially conditioned to think in a certain way. In his book The Value of Science, Henri Poincare took issue with such a view expressed by a contemporary of his, LeRoy, who had taken the position that scientists *create* rather than *discover* facts.[3] Since then the discussion of the sociological factor in science has been an important, although often undervalued, aspect in the practice of history and philosophy of science. One of the more interesting sources for the sociological view is the 1935 study by Ludwig Fleck, The Genesis and Development of a Scientific Fact, where vitalism is discussed.[4] Fleck says, "This social character inherent in the very nature of scientific activity is not without its substantive consequences."[5] He goes on to explain that one of the consequences is that simple descriptive terms become slogans and "no longer influence the mind through their logical meaning" and he mentions "vitalism" in biology specifically as one of those terms which, whenever it is found in a scientific text, "is not examined logically, but immediately makes either enemies or friends."[6] Driesch's use of the term "vitalism" should be seen as simply descriptive. It was not a slogan for him even though it has been taken as such by many opponents of his position, both among his own contemporaries and among many of his modern-day critics.

Fleck's observations have important consequences for the practice of history of science. The historical task must be to reconstruct science so that the importance of the sociological aspect is not ignored. This does not mean that historians should search the record for any bizarre scientific theories which have long since been discarded. It does suggest, however, that in the case of theories like that of Driesch, whose "remarkable observations rank among the most important in the science of biology," according to a modern textbook in developmental biology,[6] more attention has to be paid to the role of metaphysical beliefs in generating empirical results. Ultimately, the sociological factors which help to determine metaphysical beliefs need to be considered within an historical context. The present study has only taken the first step towards this more complete analysis by trying to provide a more integrated view of Driesch's scientific achievements in an internalist framework. An externalist study might help to complete the historical picture.

ENDNOTES

Introduction

[1]A well known example of a scientific theory compatible with different metaphysical views is that of quantum mechanics. According to the traditional or Copenhagen interpretation, probability statements like the so-called 'uncertainty principle' [Unbestimmtheitsrelation] are fundamentally unavoidable, while the Bohmian interpretation holds that such statements are fundamentally reducible to statements which are not themselves probability statements. In a sense, the question of which interpretation is "correct" is meaningless. Kurt Huebner, in his book Critique of Scientific Reason, trans. by Paul R. Dixon, Jr. and Hollis M. Dixon (Chicago: University of Chicago Press, 1983), argues that both views "share the common error of seeing the statements and principles of physics as expressions of essential characteristics of nature or being." In fact, Huebner tells us, "The history of physics is a process in which this confusion of our own free constructions with the ontologically real constantly repeats itself." (p. 23)

[2]Specific secondary works could be cited ad nauseam, but this is unnecessary since a good introductory bibliography in essay form is to be found in William Coleman, Biology in the Nineteenth Century, (Cambridge: University Press, 1977) pp. 167-182.

[3]See, for example, Frederick Gregory, Scientific Materialism in Nineteenth Century Germany, (Dordrecht-Holland/Boston: D. Reidel Publishing Company, 1977).

[4]Timothy Lenoir, The Strategy of Life: Teleology and Mechanics in Nineteenth Century German Biology, (Dordrecht-Holland/Boston: D. Reidel Publishing Company, 1982).

[5]These terms were created by Dietrich von Englehardt as Lenoir notes in an earlier work, "The Goettingen School and the Development of Transcendental Naturphilosophie in the Romantic Era" in Studies in History of Biology, vol. 5, William Coleman and Camille Limoges, eds. (Baltimore: Johns Hopkins University Press, 1981). See note 6, p. 197.

[6]Ibid., p. 113.

[7]Ibid., p. 114.

[8]Immanuel Kant, "Prolegomena zu einer jeden kuenftigen Metaphysik die

als Wissenshaft wird auftreten koennen",(1783) Werke, vol. 4 (Berlin, 1903), p. 297. Quoted in ibid., p.113.

[9]A good discussion of this problem can be found in J.D. McFarland, Kant's Concept of Teleology, (Edinburgh: Edinburgh University Press, 1970), pp. 117-123.

[10]See Immanuel Kant, Critique of Pure Reason, trans. Norman Kemp Smith (New York: Macmillan & Co., Ltd., 1929; reprint ed., New York: St. Martin's Press, 1965), especially the section from pp. 384-484 which is devoted to the discussion of the antinomy of reason. Kant stresses the important difference between that which is conceptually possible, and that which is also perceptually confirmed, "when reason is applied to the *objective* synthesis of appearances." (p. 385)

[11]My discussion is based on that of Huebner in Critique of Scientific Reason, pp. 5-7.

[12]Clark Zumbach, The Transcendental Science: Kant's Conception of Biological Methodology, (The Hague: Martinus Nighoff Publishers, 1984), p. 106.

[13]Immanuel Kant, Critique of Judgement, trans. J. H. Bernard (New York: Hafner Press, 1951), p. 234.

[14]Ibid., p. 236.

[15]Ibid., p. 15.

[16]Ibid., pp. 15-16.

[17]Kant, Critique of Pure Reason, pp. 172-173.

[18]Kant, Critique of Judgement, p. 16

[19]Ibid.

[20]Ibid., p. 17

[21]A more complete discussion of this distinction of Kant's can be found in McFarland, Kant's Concept of Teleology, pp. 124-134.

[22]Lenoir, Strategy of Life, pp. 22-24.

[23]Ibid., pp. 12, 14.

[24]Ibid., p. 10.

[25]Ibid., pp. 146-149.

[26]Everett Mendelsohn, "Physical Models and Physiological Concepts: Explanation in 19th Century Biology", British Journal for History of Science 2 (July 1965):201-219, p. 213.

[27]Ibid.

[28]Lenoir, Strategy of Life, p. 132.

[29]Ibid., pp. 128-129.

[30]Ibid., pp. 195-196.

[31]Imre Lakatos, "History of Science and its Rational Reconstructions" in I. Lakatos, Philosophical Papers, eds. John Worrall and Gregory Currie, vol. 1: The Methodology of Scientific Research Programmes (Cambridge: Cambridge University Press, 1978) pp. 102-138.

[32]Lenoir, Strategy of Life, p. 12.

[33]Ibid., pp. 12-13.

[34]Ibid., p. 13.

[35]Ibid., p. 243. Although Lenoir only mentions Driesch and Hertwig, others such as Johannes von Uexkuell, Gustav Wolff and Emil Ungerer can also be identified with this group. See for example Uexkuell, Die Lebenslehre (Potsdam: Mueller & Kiepenheuer, 1930) or Wolff, Mechanismus und Vitalismus (Leipzig: W. Engelmann, 1902).

[36]Hans Driesch, The History and Theory of Vitalism, trans. C. K. Ogden (London: Macmillan & Co., 1914). See especially pp. 66-92 for a discussion of Kant's philosophy of biology.

[37]Ibid., p. 94.

[38]Lenoir, Strategy of Life, p. 9.

[39]Ibid., p. 220.

CHAPTER I

[1]Ernst Haeckel, Generelle Morphologie der Organismen Allgemeine Grundzuege der organischen Formen-Wissenschaft, mechanisch begruendet durch die von Charles Darwin reformierte Descendenz-Theorie, 2 vols. (Berlin: Georg Reimer, 1866); Haeckel, Natuerliche Schoepfungsgeschichte (Berlin: Georg Reimer,1868).

[2]Quoted in Ernst Cassirer, The Problem of Knowledge: Philosophy, Science and History Since Hegel, trans. William H. Woglom and Charles W. Hendel (New Haven and London: Yale University Press, 1950), p. 162.

[3]A good description of the rise of historicism in the nineteenth century can be found in Cassirer, Problem of Knowledge. See especially the section (pp.160-175) on Darwinism, which deals specifically with historicism in a

scientific context.

[4]Ibid., p. 173.

[5]Stephen Jay Gould, Ontogeny and Phylogeny (Cambridge, Mass. and London: The Bellknap Press of Harvard University Press, 1977), p. 76.

[6]Ibid., pp. 78-80. The quotation of Haeckel's famous statement appears on p. 78. It is from Haeckel, Anthropogenie: Keimes- und Stammes-Geschichte des Menschen (Leipzig: W. Engelmann, 1874), p. 5.

[7]Gould discusses Weismann's studies of caterpillars in Ontogeny and Phylogeny, pp. 102-109.

[8]See, for example, Frederick Churchill's evaluation of Driesch's failure to appreciate Darwin's theory of evolution, particularly the ability of that theory to explain the origin of animal forms, in his article "From Machine-Theory to Entelechy: Two Studies in Developmental Teleology," Journal of the History of Biology, 2 (Spring, 1969):175.

[9]Cassirer, Problem of Knowledge, p. 174.

[10]Ibid. The original quotation is from Driesch, Die Biologie als selbstaendige Grundwissenschaft (Leipzig: W. Engelmann, 1893), p. 27.

[11]Driesch, Die Biologie, p. 27.

[12]These ideas on dogmatic Darwinism are set forth in Driesch, The Science and Philosophy of the Organism, 2d ed. (London: A. & C. Black, 1929), pp. 161-175. The term "dogmatic Darwinism" first appears on p. 169.

[13]Ibid., p. 165

[14]Cassirer, Problem of Knowledge, p. 179.

[15]Scott F. Gilbert, "The Embryological Origins of the Gene Theory," Journal of the History of Biology 11 (Fall, 1978):310.

[16]Gilbert, in the article just cited, shows how modern gene theory had its origins in the work of a group of embryologists all working in the Developmental Mechanics tradition. This group was initially concerned with nucleus-cytoplasm relationships in developmental processes. Thus, although the gene theory which eventually resulted from this work had obvious implications for any theory of evolution, the impetus for the work did not come from within the framework of evolution theory per se, but rather from that of Developmental Mechanics.

[17]Lenoir in his book, Strategy of Life, (pp. 197-199) reports that Hermann Helmholtz took this position in a paper on fermentation. A current example of this sort of argument can be found in Churchill, Machine-Theory to Entelechy, pp. 184-185.

[18]See above, p. 8 for more on this.

[19]Lenoir, Strategy of Life, p. 242.

[20]Ibid.

[21]Reinhard Mocek, Wilhelm Roux-Hans Driesch. Zur Geschichte der Entwicklungsphysiologie der Tiere ("Entwicklungsmechanik") (Jena: Gustav Fischer, 1974), p. 105.

[22]See, for example, Thomas Hunt Morgan's treatment of this issue in his article, "Developmental Mechanics," Science, 7 (1898):158, where he stresses the need for a "more exact, more profound...new embryology." His emphasis is clearly on the need for a new experimental methodology and his rational approach is implicit rather than derived from a formal philosophical basis.

[23]The noumenon is also called the "thing-in-itself", while the phenomenon is the object of human sensible intuition. The former concept can only be defined negatively since human beings can only experience the phenomenal world.

[24]Kant, Critique of Pure Reason, p. 267.

[25]Gregory, Scientific Materialism, p. 147. Gregory's discussion of the conflict between neo-Kantians and materialists (pp. 145-163) is excellent in pointing out differing attitudes towards the place of philosophy in relation to science at this time.

[26]All of the following biographical information is from Driesch, Lebenserinnerungen. Aufzeichnungen eines Forschers und Denkers in entscheidender Zeit (Munich/Basel: Ernst Reinhard, 1951).

[27]Ibid., p. 33.

[28]Ibid.

[29]Ibid., p. 36

[30]Driesch, Tektonische Studien an Hydroidpolypen. I. Die Campanulariden und Sertulariden. Inaugural-Dissertation (Jena: Gustav Fischer, 1889).

[31]Driesch, Lebenserinnerungen, p. 54.

[32]Driesch, Tektonische Studien, p. 33.

[33]Ibid., p. 35.

[34]Ibid., p. 34.

[35]Driesch, "Das System der Biologie," Suddeutschen Monatsheften, no. 2 (1890):1-16.

[36]Ibid., p. 7.

[37]A good description of these institutional features can be found in Charles E. McClelland, State, Society, and University in Germany 1700-1914 (Cambridge: Cambridge University Press, 1980), pp. 258-287.

[38]Ibid., p. 279.

[39]Ibid., p. 280.

[40]Driesch, "Das System", p. 8.

[41]Ibid.

[42]Kant, Critique of Judgement, p.262.

[43]Driesch, "Das System," p. 9.

[44]Driesch, Analytische Theorie der Organischen Entwicklung (Leipzig: W. Engelmann, 1894).

[45]Driesch is here using the term "typical" in a specific sense, which is the same as Roux's use of the term. See below, p. 51 for more on this usage.

[46]Driesch, "Das System," p. 12.

[47]See Lenoir, Strategy of Life, pp. 145-147, where he discusses the contrasting approaches of Oken and Carus as opposed to Kielmeyer and Mueller, based on just this sort of distinction.

[48]Ibid., p. 148.

[49]Cassirer, Problem of Knowledge, p. 181.

[50]Gustav Wolff, "Entwicklungsphysiologische Studien I: Die Regeneration der Urodelenlinse," Archiv fuer Entwicklungsmechanik der Organismen, 15 (1895):380-412.

[51]Driesch, "Entwicklungsmechanische Studien I. Der Werth der beiden ersten Furchungszellen in der Echinodermentwicklung. Experimentelle Erzeugung von Theil- und Doppelbildung," Zeitschrift fuer wissenschaftliche Zoologie, 53 (1891):160-178.

[52]Ibid., p. 178.

[53]Wilhelm Roux, "Contributions to the Developmental Mechanics of the Embryo. On the Artificial Production of Half-Embryos by Destruction of \one of the First Two Blastomeres, and the Later Development (Postgeneration) of the Missing Half of the Body," trans. Hans Laufer, in Foundations of Experimental Embryology 2d ed., eds. Jane M. Oppenheimer and Benjamin H. Willier (New York: Hafner Press, Macmillan Publishing Co., 1974), pp. 3-37.

[54]Ibid., p. 37.

[55]Although there were earlier indications that Driesch was correct, the

first conclusive evidence was given by J. F. McClendon in his article "The Development of Isolated Blastomeres of the Frog's Egg," American Journal of Anatomy, 10 (1910).

[56]Driesch, Die Biologie.

[57]Ibid., p. 58.

[58]Ibid., pp. 58-59. The quotation is from Kant, Critique of Judgement, p. 248.

[59]Driesch, Analytische Theorie, p. 11.

[60]Ibid., pp. 11-12.

[61]Both Jane Oppenheimer and Frederick Churchill have argued this position. See Oppenheimer, Essays in the History of Embryology and Biology (Cambridge, Mass. and London: The M.I.T. Press, 1967), pp. 73-74 and Churchill, "Machine Theory to Entelechy", p. 167.

[62]Driesch, Analytische Theorie, pp. 92-94.

[63]Ibid., p. 93.

[64]Ibid., p. 94.

[65]Ibid.

[66]Ibid.

[67]Ibid., p. 165.

[68]Driesch, "Die Maschinentheorie des Lebens," Biologischen Centralblatt, 16 (May, 1896):353-368.

[69]Driesch, Analytische Theorie, pp. 164-168.

[70]Ibid., p. 165.

[71]Ibid., p. 166.

[72]Ibid.

[73]This position is supported in a recent textbook, N. J. Berrill and Gerald Karp, Development,2d ed. (New York: McGraw-Hill Book Company, 1981), p. 204.

[74]Driesch, "Die Lokalisation morphogenetischer Vorgaenge. Ein Beweis vitalistischen Geschehens," Archiv fuer Entwicklungsmechanik der Organismen, 8 (1899):35-111.

[75]Ibid., p. 38.

[76]Driesch, Analytische Theorie, pp. 54-61.

[77]Ibid., pp. 45-48.

[78]Driesch, "Die Lokalisation," p. 39.

[79]Ibid., p. 65.

[80]Ibid., pp. 84-87.

[81]Ibid., p. 85.

[82]Ibid., p. 86.

[83]Ibid., pp. 102-106.

[84]Ibid., p. 103 and pp. 105-106.

[85]Driesch, Science and Philosophy.

[86]Ibid., p. 268.

[87]Ibid., p. 300.

[88]Ibid., p. 324. Driesch's reasoning here is somewhat circular, since he says that the principles of causality and substance can apply to entelechy "if united with the concept of wholeness." But, one might argue, the concept of entelechy cannot mean anything without a concept of wholeness.

CHAPTER II

[1]Oppenheimer, Essays, p. 75.

[2]Oppenheimer and Willier, Foundations, p. 39.

[3]See Cassirer, Problem of Knowledge, pp. 188-193. He cites similar criticisms to his own from a number of Roux's contemporaries, including M. Verworn, E. Pflueger, and J. Schaxel (pp. 191-193).

[4]Ibid., p. 190.

[5]Ibid.

[6]Oppenheimer, Essays, p. 64.

[7]Churchill, "Chabry, Roux, and the Experimental Method in Nineteenth-Century Embryology" in Foundations of Scientific Method: The Nineteenth Century, eds. Ronald N. Giere and Richard S. Westfall (Bloomington, Ind.: Indiana University Press, 1973), pp. 161-205.

[8]Ibid., p. 198.

[9]Oppenheimer, Essays, p. 163.

[10]Ibid., p. 164.

[11]Ibid., p. 166.

[12]It seems surprising that Oppenheimer would make the disttinction she does, considering that Aristotle was actually the first writer to develop a concept of entelechy; in fact, Driesch notes in History and Theory of Vitalism (p. 203) that he got the term from Aristotle, but that his usage was not properly Aristotelian. See Aristotle, De Anima, trans. Hugh Lawson-Tancred (Harmondsworth, Eng.: Penguin Books, 1986), pp. 155-159 for Aristotle's own definition of entelechy.

[13]Oppenheimer, Essays, p. 164.

[14]Ibid.

[15]Churchill, "Machine Theory to Entelechy," p. 167.

[16]Ibid., p. 177.

[17]Lenoir suggests in Strategy of Life (p. 280) that in the mid-nineteenth century the extreme competition for positions was a big external factor in the anti-vitalist campaign waged by the younger generation of scientists at the time. These anti-vitalists came to dominate medical faculties by the 1870's and were even more dominant when Driesch was active in experimental work. Paul Weindling's important pioneering study, "Theories of the Cell State in Imperial Germany" in Biology, Medicine and Society 1840-1940, ed. Charles Webster (Cambridge: Cambridge University Press, 1981), pp. 99-155, gives an indication of the many external factors such as the institutional and social structure of Imperial Germany which helped shape scientific views. Charles McClelland's State, Society and University provides an in-depth look at the difficulty breaking into the ranks of Ordinarius professors at this time, and is a good background to Weindling's study. See especially pp. 239-287, which deal with the expaansion of universities after 1870. Another useful source is Fritz Ringer's The Decline of the German Mandarins: The German Academic Community 1890-1933 (Cambridge, Mass.: The Bellknap Press of Harvard University Press, 1969) which focuses on the power of the Ordinarius professors within the system.

[18]"Die Lokalisation" was published in 1899; Driesch's last experimental paper was published in 1910. See Driesch, "Neue Versuche ueber die Entwicklung verschmolzener Echinidenkeime," Archiv fuer Enwicklungsmechanik der Organismen, 30 (1910):8-23.

[19]Churchill, "Machine Theory to Entelechy," p. 183.

[20]Ibid., p. 185

[21]Ibid.

[22]Ibid.

[23]Driesch, Die mathematische-mechanische Betrachtung morphologischer Probleme der Biologie (Jena: Gustav Fischer, 1891)

[24]Kant's main point throughout the section "Of the Principle of Teleology as Internal Principle of Natural Science" in the Critique of Judgement is that regarding "the empirical laws of natural purposes in organised beings it is not only permissible but unavoidable to use the teleological mode of judging as a principle of the doctrine of nature in regard to a particular class of its objects." (p. 229).

[25]Driesch, Betrachtung, p. 20.

[26]Churchill, "Machine Theory to Entelechy", p. 174.

[27]Ibid., pp. 179-185.

[28]Ibid., p. 177.

[29]Ibid., p. 176.

[30]Driesch, Analytische Theorie, p. 166.

[31]For more detail see above, p. 40.

[32]Churchill, "Machine Theory to Entelechy", p. 176.

[33]See above, p. 39.

[34]Churchill, "Machine Theory to Entelechy", p. 182.

[35]Ibid., p. 184.

[36]Ibid.

[37]See Driesch, Science and Philosophy, pp. 150-154.

[38]Driesch, Lebenserinnerungen, p. 109.

[39]Horst H. Freyhofer, The Vitalism of Hans Driesch: The Success and Decline of a Scientific Theory, European University Studies, Series XX, vol. 83 (Frankfurt am Main/Bern: Peter Lang, 1982).

[40]Ibid., p. 17.

[41]Ibid., p. 166.

[42]Ibid., p. 35.

[43]Ibid.

[44]Mosaic theory is so called because the information in the nucleus of

the egg is assumed to become progressively divided as cell division proceeds. The resulting cells are then seen as a "mosaic" of isolated units which, taken together, develop into the organism but which are determined before functional differentiation starts. Such a system would, by definition, have little or no regulatory capacity. The term "mosaic development" is still used, but without reference to a loss of nuclear material.

[45]For a description of the experiments leading to mosaic theory and Driesch's subsequent experimental refutation, see Oppenheimer and Willier, Foundations, pp. 3-50.

[46]See Freyhofer Vitalism of Driesch, p. 35 and also Churchill, "Machine Theory to Entelechy", p. 184.

[47]The original quotation can be found in Driesch, Lebenserinnerungen, p. 108.

[48]Churchill, "Machine Theory to Entelechy," pp. 183-184.

[49]For example, Freyhofer describes regulative theory as a decision by Driesch to commit himself "to the theoretical possibilities of the formist root metaphor-similarity" (Vitalism of Driesch, p. 35). The terminology refers to a not well-known philosophy and hardly serves to clarify anything.

[50]Driesch, "Die Maschinentheorie des Lebens. Ein Wort zur Aufklaerung," Biologisches Centralblatt, 16 (1896):353-368.

[51]See Churchill, "Machine Theory to Entelechy," p. 176, and Freyhofer, Vitalism of Driesch, pp. 35-36.

[52]Ibid., p. 36.

[53]Churchill, "Machine Theory to Entelechy," p. 176. The original quotation appears in Driesch, "Maschinentheorie", p. 365. The italics are in the original.

[54]Ibid., p. 354.

[55]Ibid., pp. 354-355.

[56]Ibid., p. 356.

[57]Ibid., p. 355.

[58]Churchill, "Machine Theory to Entelechy," p. 184.

[59]Driesch, "Maschinentheorie," p. 367.

[60]Ibid., p. 363.

[61]Ibid. The original quotation reads, "Mein Bildungstrieb ist, wie gesagt, nur im Perfectum thaetig gewesen, er 'hat' alles zweckmaessige gemacht, vorgesehen..." The grammatical distinction between perfect and

imperfect past tenses is that the former is used to indicate completion of an action in the past, while the latter indicates continuous or repetitive action in the past. In Driesch's context, purposiveness is seen as the "result" of the formative drive, but the "process" by which the formative drive works is unknown and possibly inexplicable.

[62]Ibid., p. 368.

[63]Mocek, Roux-Driesch.

[64]Donna Haraway, "Reinterpretation or Rehabilitation: An Exercise in Contemporary Marxist History of Science" in Studies in History of Biology, vol. 2, Coleman and Limoges, eds. (Baltimore: Johns Hopkins University Press, 1978), pp. 193-209.

[65]Mocek, Roux-Driesch, p. 149. Mocek presumably means this only in reference to Driesch.

[66]Ibid., p. 119.

[67]Ibid.

[68]For example, the so-called "spin paradox" in quantum physics requires that particle pairs interact instantaneously without any energetic exchange, per se.

CONCLUSION

[1]Driesch, Science and Philosophy, p. 52 and p. 88.

[2]See above, pp. 10-11.

[3]Poincare says, "Ce qu'il avait de plus paradoxical dans la these de M. LeRoy, c'etait cette affirmation que le savant cree le fait." See his book, La Valeur de la Science (Paris: E.Flammarion, 1912), p. 221.

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