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VITALISM IN NINETEENTH-CENTURY SCIENTIFIC THOUGHT: A TYPOLOGY AND REASSESSMENT

WHAT was the true significance for chemistry and physiology of Wöhler's synthesis of urea? Did vitalism exert an inhibiting or a progressive influence on the growth of physiology in the nineteenth century? How can the differences between mid-nineteenth-century German and French reactions against vitalism be explained? These are some of the numerous as yet unresolved problems concerning the precise historical significance of nineteenth-century vitalistic thought.

Though recent discussions of the issues do not seem to have produced a consensus, they have yielded at least a couple of helpful methodological suggestions.

First, rather than simply labelling this or that scientist as a 'vitalist', it may be of more value to investigate his whole theoretical position, and determine the role of vitalistic concepts within this whole.[1]

Secondly, it is a mistake to regard vitalism as one doctrine, or set of doctrines. More attention to the varieties of vitalism is required.[2]

I propose to take up both of these suggestions by presenting a three-dimensional typology of vitalism based in part on a classification of the different roles ascribed to vital forces or powers.

One consequence of this typology is that it brings out very clearly the fact that a classification of nineteenth-century biologists based on the criterion of their vitalism or non-vitalism does not correlate at all with one based on their respective sides in the most fundamental theoretical and methodological issues of the period. That is to say, the issues which divided, say, vitalists from one another can be shown to be more significant than those which divided vitalists from non-vitalists. Indeed, in some cases

I have derived much benefit from comments made by Dr J. H. Brooke, of the University of Lancaster, on an earlier version of this paper.

[1] E. Mendelsohn, 'Physical Models and Physiological Concepts: Explanation in Nineteenth-century Biology', *The British Journal for the History of Science*, 2 (1964–5), 203. Also T. O. Lipman, 'Vitalism and Reductionism in Liebig's Physiological Thought', *Isis*, 58 (1967), 168.

[2] B. S. Jørgensen, 'More on Berzelius and the Vital Force', *Journal of Chemical Education*, 42 (1965), 395.

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the sole issue separating a vitalist from a non-vitalist was little more than a terminological dispute.

A second consequence of the typology is that it allows a reposing of the questions with which I began this paper in new terms—terms which, I think, render the problems themselves more tractable. I hope to be able to outline the sort of solution to some of these problems which my typology makes more readily available.

I shall begin by presenting a definition of vitalism which, despite its shortcomings, will serve to specify in a preliminary way the range of theories and concepts with which I shall be dealing.

Vitalism is the belief that forces, properties, powers, or 'principles' which are neither physical nor chemical are at work in, or are possessed by living organisms, and that any explanation of the distinctive features of living organisms which did not make reference to such properties, forces, powers or principles would be incomplete.

Several problems arise immediately from this attempt at definition. According to what criteria is a scientific explanation to be considered complete or incomplete? What distinguishes a physical or chemical force (property or power) from one which is neither physical nor chemical? What epistemological or ontological status do the vitalists' powers, properties or forces have?

The answers to these questions are almost as numerous as the vitalists themselves. My definition is inclusive of all their views only in virtue of its vagueness and ambiguity. What is required, then, is some attempt to give precision by providing not a definition, but a typology of vitalisms. Some attempts at this have already been made.

First, Kemeny[3] attempts to represent the varieties of vitalism on a single spectrum, with 'extreme' vitalism at one end, 'extreme' mechanism at the other. The inadequacies of such a scheme are fairly obvious. There are, as I hope to show, several different sorts of vitalist doctrine which do not admit of any placing on a continuum of greater or lesser extremism. Was Bichat more extreme than Müller? If we decide that he was, have we made a decision of any importance? Another difficulty with Kemeny's scheme is that it presupposes that vitalism is somehow opposed to mechanism, and only to mechanism. But, of course, there are varieties of mechanism, and some vitalists were, arguably, also mechanists (Müller?). Further, some anti-vitalists have characterized their positions not as mechanist, but as 'materialist' or 'physicalist'.

[3] J. G. Kemeny, *A Philosopher Looks at Science* (Princeton, 1959), 211ff.

In short, Kemeny's scheme is much too simple, and is, one suspects, based on very scant historical knowledge.[4]

Bent Søren Jørgensen[5] distinguishes what he calls physiological from chemical vitalism. The distinction he makes is, I think, viable, though, of of course, it is not intended as an exhaustive classification of vitalisms. However, the terms 'physiological and 'chemical' are, it seems to me, inappropriate to demarcate the particular doctrines he discusses.

The closest Jørgensen gets to a definition of 'physiological vitalism' is where he says: 'The physiological point of view merely denies that life can generate itself; but once it is generated, by multiplication or creation, it is subjected to the general natural laws, although in a very complex manner.' This form of words is ambiguous, but on any interpretation the text of Berzelius which Jørgensen presents as an example of physiological vitalism does not satisfy his definition. More to the point, it seems to me that the thesis that life cannot be spontaneously generated from inorganic matter (the most *obvious* reading of Jørgensen's definition) is not, properly speaking, a physiological thesis.

By contrast, chemical vitalism, in Jørgensen's sense, asserts 'the existence of a chemical affinity which acts only within the living body and which is directed against the usual (inorganic) affinities'. But the difficulty is that this definition obscures the distinction between those theories which asserted simply that a distinctive type of compound (organic compounds) with special properties could be formed only under conditions to be found in living organisms, and those which asserted, *further*, that the distinctive 'vital' properties of living *organisms* were dependent on the operation of a vital force, whether or not these properties were in turn dependent on the special properties of organic compounds. In other words, Jørgensen's term 'chemical' vitalism covers (and so does not distinguish between) doctrines belonging to organic chemistry and physiology respectively.[6]

Finally, Toulmin and Goodfield[7] distinguish four kinds of vitalism. First is the conception of the vital principle as an incorporeal agency (Van Helmont's 'Archeus', Stahl's 'Anima') which causes the distinctive vital

[4] Kemeny makes some astonishing historical judgements. For example: '. . . so far Physics and Chemistry have been able to do very little for Biology', Kemeny, *ibid.*, 212. This in 1959!

[5] B. S. Jørgensen, *op. cit.* note 2, 395.

[6] I have some sympathy with Lipman's more fundamental criticism of Jørgensen's attempt to distinguish kinds of vitalist theory (*Journal of Chemical Education*, 42, 396). However, Lipman does concede that there is value in assessing the role of vitalist doctrines within the thought of different scientists. If a classification of vitalist theories and concepts can help in this, it is not clear why such a classification may not be worthwhile.

[7] S. Toulmin and G. J. Goodfield, *The Architecture of Matter* (London, 1962), paperback edition, reprinted 1968, 366ff.

phenomena. Scepticism concerning incorporeal agencies lead to forms of vitalism in which the attempt to discover the causes of vital phenomena were eschewed in favour of the discovery of laws governing the vital phenomena themselves. The third sort of vitalism attributes organic behaviour to distinctive non-chemical constituents of the organic body (Descartes' 'animal spirits' and Borelli's 'nervous juice') whilst the fourth and final sort conceived of the vital force as distinct from but comparable with such forces as gravity, chemical affinity, and electricity (Liebig).

Despite some intuitive appeal it seems to me that the Toulmin/Goodfield classification fails because it is based on no clear principles of classification. I think there are fundamental theoretical and methodological differences between vitalists which are not brought into the open by dividing them up in this way.

My point here can best be made by proceeding directly to my own alternative typology. As I have already said, my typology is a three-dimensional one. I shall begin by explaining what these three dimensions are and outlining the sorts of vitalism they enable us to distinguish. I shall then illustrate the main distinctions of my typology by applying them to some representative nineteenth-century vitalist theories. Finally I shall attempt to bring my typology to bear on the clarification of two of the problems of historical exegesis posed at the beginning of this paper.

I

First, vitalist theories will differ according to the degree of epistemological scepticism (or its converse, metaphysical daring) which governs their construction. Among those early nineteenth-century French biologists who made their epistemological assumptions explicit, there was widespread opposition to the speculative system-building of such men as Lamarck and Buffon. The epistemological sceptics drew on the work of Newton and Locke which had made its impact on French thought through the influence of Condillac and the later Idéologues.[8] It was the radical empiricism of these scientists which was to culminate in the 'positive philosophy' of Comte.

In general, this epistemological scepticism took the form of a distinction between 'first' or 'hidden' causes ('Agents' or 'Principles') which were,

[8] O. Temkin, 'The Philosophical Background of Magendie's Physiology' and 'Materialism in French and German Physiology of the Early Nineteenth Century', both in *The Bulletin of the History of Medicine*, **20** (1946), 10–35 and 322–7 respectively. See also G. Rosen, 'The Philosophy of Ideology and the Emergence of Modern Medicine in France', *The Bulletin of the History of Medicine*, **20** (1946), 328–39.

and might have to remain, unknown, and the many and varied phenomena which result from those hidden causes. The phenomena were accessible to observation, and the proper business of science (at least, at this stage in its development) was to establish generalizations on the basis of careful observation of the phenomena. Speculation about the underlying *causes* of phenomena was forbidden (at least for the time being).

Some of these epistemological sceptics, nevertheless, made use of such concepts as 'vital property' and 'vital power' in their biological theory-construction. Some (arguably Cuvier) disobeyed their own epistemological prescriptions in doing so, however, others did not.[9] For these biologists, the vital powers were conceived of as general relationships arrived at through observation of phenomena. No underlying agency was hypothesized. Such vitalist theories I shall call 'phenomenalist'. My class of phenomenalist vitalists corresponds to Toulmin and Goodfield's second sort of vitalist.

In contrast to the phenomenalist vitalists are all those vitalists[10] who do hypothesize the existence of a non-physico-chemical agency or entity underlying the observable vital phenomena which are its effects. I shall call such vitalists 'realist vitalists'. This class includes the proponents of a number of rather different conceptions concerning the nature of the vital principle (incorporeal agencies, distinctive material constituents, forces or powers, etc.). Some of these differences can be brought out by situating each particular thinker not only on this, but on the other dimensions which I shall propose. However, I shall not consider here those realist vitalists who hypothesized the existence of incorporeal agencies or distinctive (non-chemical) material constituents in the living body (Toulmin and Goodfield's first and third kinds of vitalism). I shall not consider them here because such doctrines had virtually no influence on the main streams of biological thought in the nineteenth century.[11]

The second dimension of my classification distinguishes vitalist theories in terms of the formal character of the explanations they propose. I shall distinguish three main 'locations' on this dimension. First, vital powers were sometimes conceived of, not *as* minds or souls, but as analogous to

[9] See this paper, pp. 25–9 for a discussion of Bichat as a phenomenological vitalist.

[10] On pp. 29–34 I discuss Müller's theory as exemplifying realist vitalism.

[11] This remark requires a little qualification. Vitalist explanations of embryological development and regeneration sometimes utilized conceptions of quasi-mental vital forces, but even so (as in the case of Müller) the operation of these forces according to laws of blind necessity was also usually stressed. See this paper, p. 31.

them in that they were supposed to operate rationally in pursuit of some aim or goal. Explanations of vital phenomena in terms of such a conception of the vital powers would thus be teleological in form. I shall refer to this sort of vitalism as 'teleological' vitalism.[12] It is important to recognize that only rather few vitalists in the nineteenth century conceived of vital powers in this way, and even they made use of teleological explanation only in limited fields of study. In particular, teleological vitalism was associated with attempts to explain development and the differentiation of structure (*i.e.*, especially embryology). Interestingly, there is a widespread tendency among commentators to identify vitalism as a whole with teleological explanation in biology.[13] This is, of course, quite mistaken.

Other vitalists (especially, but not exclusively, physiologists working in the first half of the century in Germany) thought of their vital powers as being exercised 'according to laws of blind necessity', laws which could be discovered by observation and experiment, but which would not be reducible to the laws of physics and chemistry. Such ideas about the exercise of vital powers committed their proponents to explanations of the form now commonly referred to as the 'covering law' form of explanation. This was so independently of whether a phenomenalist or realist conception of the vital power was accepted. I shall call vitalism of this sort 'nomological' vitalism.[14]

Thirdly, along this dimension, there were some few vitalists who stressed the *variability* of the vital powers in such a way as to invite the interpretation that the exercise of the vital powers was not subject to law-like regularity of any sort. Nevertheless such vital powers were presented as *explaining* vital phenomena. If such forms of vitalism are acceptable as providing explanations at all, they provide explanations which are neither teleological nor nomological. I shall call such forms of vitalism 'non-nomological'.[15]

The third, and final, dimension along which I shall attempt to situate

[12] Müller again provides a clear example; see this paper, pp. 34–5.

[13] See, for instance, C. G. Hempel, *Aspects of Scientific Explanation* (New York, 1965), 304, and T. Schwann, *Microscopical Researches*, H. Smith (trans.) (London, 1847), 186ff.

[14] See pp. 35–6 for examples.

[15] It might be argued that my separate class of 'teleological vitalism' should have been included as a sub-class of non-nomological vitalism. Against this is the consideration that nineteenth-century teleologists tended, rightly or wrongly, to think that teleological explanations could also be nomological. In any event, my conception of non-nomological vitalism is intended to cover just that range of vitalist theorists who argued from the variability of the vital phenomena, irrespective of whether this involved the attribution of 'purposes' to the organism. See, for examples, this paper, pp. 36–7.

different sorts of vitalist doctrine concerns the fields of study in which vitalist explanations were attempted. Jørgensen's distinction between chemical and physiological vitalism is based on a recognition that confusion readily arises from failing to make clear the different problems to which vitalists addressed themselves. However, I shall not use the terms 'chemical' and 'physiological' in the same way as Jørgensen does for reasons which I have already indicated.

Those biologists whose field of concern was primarily with problems of growth and development—with morphogenesis and ontogeny—were faced with problems of a quite distinctive kind. Eighteenth-century work in embryology left the nineteenth century a legacy of problems whose solution in terms of physics and chemistry seemed a very remote prospect indeed. In addition, the relative success of the epigenetic over the preformation 'theory' in embryology predisposed biologists in favour of some sort of vitalism. According to the epigenetic theory an undifferentiated germ-cell had to be thought of as possessing the power to organize itself into the complicated structure of an adult organism. It is hard to see how any physical or chemical power then known could fulfil such a role.

The problem of explaining the process by which a mature organism develops from an amorphous germ-cell, together with the problem of how tissues regenerate after wounding, seemed to make special demands. There was, as well as a progressive increase in size, increasing differentiation of structures, and complexity of organization, resulting in the full integrated and 'harmoniously functioning' adult organism. Faced with facts like these the response of many biologists was to seek explanations, not in terms of antecedent conditions for each stage, but in terms of 'rational' or 'creative' agencies which moulded the developing organism in accordance with predetermined ends. I shall call teleological and other sorts of vitalism produced in connection with the problems of embryology and regeneration 'morphogenic vitalism'.[16]

Distinguishable from the problems of morphogenesis but, of course, also intimately related to them, are problems connected with functions and processes occurring in organisms considered in abstraction from any structural changes.

Foremost among the physiological problems to which vitalist explanations were addressed in the nineteenth century was the puzzling independence of the behaviour and properties of living organisms from the (destructive) influences of the environment. For some it was the pro-

[16] Examples of this sort of vitalism are discussed on pp. 30–2 of this paper.

duction of 'animal heat' (Liebig) which was most puzzzling, for others the resistance of living organisms to decomposition seemed to require vitalist explanations (Cuvier, Müller). For Bichat it was the 'spontaneity', variability, and irregularity of animal behaviour which was most striking. Others were impressed by the dependence of the 'vital phenomena' on the integrity of the whole organism.

The maintenance of a constant internal organization despite continuous interchange of materials between organism and environment (Cuvier, Liebig, Blumenbach); the formation and maintenance of organic compounds (Müller, Liebig); and the differences between the behaviour of living and newly dead organisms which have not yet undergone decomposition (Cuvier) were further distinctive features of organisms which seemed to require vitalist explanation. I shall call the vitalist theories developed to meet this range of problems 'physiological' vitalism.[17]

Finally, particularly during the early part of the nineteenth century, it was not at all clear that the laws and theories which were making headway in the new domain of inorganic chemistry were applicable to those compound substances which made up organic bodies. The unification of organic and inorganic chemistry took place slowly and unevenly, allowing plenty of leeway for those who maintained the permanent separation of the disciplines.[18] One reason for maintaining the autonomy of organic chemistry was the belief that special forces or powers found only in organic bodies were necessary for the formation of organic compounds. The elements combined to form organic compounds against their properly chemical affinities, in a mode of combination quite unlike that characteristic of inorganic compounds, and under the influences of distinctive forces or powers.[19]

[17] See this paper, pp. 32–4.

[18] As Dr J. H. Brooke has pointed out to me, this leeway became rather narrower after 1830.

[19] See this paper, pp. 32–4. This was also the (much earlier) view of Cuvier (see especially *An Introduction to the study of the Animal Economy*, J. Allen (trans.) (Edin., 1801), 3). There is some dispute about whether the great chemist Berzelius was a chemical vitalist in this sense. Certainly he speaks in a number of places (*A View of the Progress and Present State of Animal Chemistry*, London, 1813, and in *Lehrbuch der Chemie*, Dresden and Leipzig, 1847, Vol. IV, pp. 135–8, quoted in Jørgensen, *op. cit.* note 2) of the vital powers, and of the possibility of their being permanently unknowable. On the other hand, both Jørgensen (*op. cit.* note 2) and Brooke (J. H. Brooke, 'Wöhler's Urea and its Vital Force?—A Verdict from the Chemists', *Ambix*, 15 (1968), 84–114) argue rather convincingly against the view that Berzelius was a 'dogmatic' vitalist, committed to the separation of the domains of organic and inorganic chemistry. I suspect that the problem of situating Berzelius in my scheme indicates the importance of a further distinction—which it does not make—between organic chemistry and 'animal chemistry', to use the terminology of the time (it is the distinction which Brooke makes on p. 366 of his 'Organic Synthesis and the Unification of Chemistry—A Reappraisal', *The British Journal for the History of Science*, 5 (1970–1),

Clearly the relationship between such 'chemical vitalism' and some forms of physiological vitalism was very close. For someone, like Müller, who sought to explain the vital phenomena in terms of the behaviour of organic compounds, chemical vitalism provided the main justification for a vitalist position in physiology. On the other hand, it was quite possible to be a physiological vitalist whilst rejecting vital forces and powers in organic chemistry (Liebig).

By now it should be clear that the three dimensions I distinguish in my typology are logically independent of one another. That is to say that a particular position on any one of the dimensions will be logically compatible with any of the positions on each of the others. But, of course, not all combinations were equally widely distributed amongst nineteenth-century vitalists. As I have already suggested, for instance, vitalists who tackled the problems of morphogenesis tended also to adopt a teleological form of explanation.

A final point about the typology is that it can readily be extended to provide a framework for classifying non-vitalist theories. These, too, will have epistemological presuppositions, will involve the use of one or another form of explanation, and will be directed to the solution of a more or less clearly specified range of problems. Clearly, then, where vitalist modes of explanation are replaced by non-vitalist ones, the classification of both on these criteria will give a systematic guide to the extent and nature of the historical transformation involved. The relevance of this to the solution of some of the problems of historical exegesis with which I began may already be apparent. I shall develop this point in the last part of this paper.

First, however, I shall try to illustrate the main distinctions of my typology by applying it to the work of some representative nineteenth-century vitalists. These vitalists (Bichat, Bernard, Liebig, Müller) are selected not simply because they are representative, but also because the interpretation of their texts and of the relations between them has a direct bearing on the recent historical controversies to which I shall turn in the final section of this paper.

II

One of the most influential of the group of epistemologically sceptical physiologists who were active in France at the beginning of the century was Xavier Bichat. Goodfield[20] says that his vitalism was 'descriptive',

363–92). It was the problem of the chemistry of the brain and nervous system that early on seemed to push Berzelius into a vitalist position.

[20] G. J. Goodfield, *The Growth of Scientific Physiology* (London, 1960), 60ff.

as opposed to the 'explanatory' vitalism of those who were committed to the existence of 'agencies' or 'entities' of a non-physico-chemical kind. There is an obvious affinity between Goodfield's distinction and my distinction between phenomenalist and realist vitalism. However, her characterization of Bichat's vitalism as 'descriptive' is not merely unilluminating—it is seriously misleading. Speaking of physiologists such as Bichat, Goodfield says:

> The variability and complexity of animal behaviour were—for them—obvious facts, at a level of straightforward description.[21]

But if this is all one has to believe to be counted a vitalist then vitalism is not only still very widespread, but it is also true! As Goodfield points out Bichat thought of the 'vital properties' as having more or less the same role in this theory as 'gravity' in Newtonian physics. But the word 'gravity' as it occurred in Newtonian physics was hardly 'straightforwardly descriptive'. The concept of gravity served to connect such apparently widely disparate phenomena as free-fall, planetary motion, and the swing of the pendulum into one theoretical structure, and enabled the prediction of hitherto unobserved phenomena. If Bichat assigned a similar role to his notion of vital property, then this was also proffered as an explanatory concept. Of course, it might be that Bichat's vitalism fails to be explanatory, but my point is simply that if it fails to be explanatory then this must be for reasons other than that Bichat does not hypothesize underlying agencies or entities. To deny this would be to deny explanatory import to Newton's theory of gravitation and to post-Copernican astronomy.

What I regard as Goodfield's error here may be connected with her use of a misleading example in giving an account of Bichat's theory. She says that he used the vital properties to explain, for instance, 'the action of a muscle'.[22] But Bichat's explananda were the 'vital phenomena' (sleeping, waking, feeding, exercising, etc.)—that is, characteristics of whole organisms, rather than of their 'elements'. This point is fundamental. It was Bichat's most important contribution to have emphasized the importance for physiological explanation of the analysis of organisms into their constituent systems, organs and ultimately tissues and to have presented a systematic classification of the tissues.

The vital phenomena were thus to be explained as the outcome of the 'combined and adjusted' activities of the tissues (including muscular

[21] *Ibid.*, 61.
[22] G. J. Goodfield, *ibid.*, 67.

tissue). These activities of the tissues were thought of by Bichat as identical with the exercise of the vital powers[23] (Bichat analysed the vital *properties* as *powers* of the tissues which possess them). Since the relevant vital power of voluntary muscle is its 'animal contractility', the action of the muscle in contracting is simply the exercise of this power. Clearly, in a case like this, there would be strong objections to the claim that simply having a power explains the exercise of the same power.[24] This may be why Goodfield declines to call Bichat's vitalism 'explanatory'.

However, the pattern of physiological explanation proposed by Bichat is quite different. To hold that the 'contractility' of the muscles of an animal, among other things, explains its *locomotion* is at least not *obviously* questionable on logical grounds. This, I think, was Bichat's actual position and it enables us to make rather more sense of his parallel between gravity and the vital properties.

The point of Bichat's constant appeal to Newton can be brought out by considering the famous passage from the General Scholium of his *Principia*:

> Hitherto we have explain'd the phaenomena of the heavens and of our sea, by the power of gravity, but have not yet assigned the cause of this power . . . But hitherto I have not been able to discover the cause of those properties of gravity from phaenomena, and I frame no hypothesis. For whatever is not deduc'd from the phaenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental science.[25]

This passage is readily interpreted[26] as asserting that the term 'gravity'

[23] It is not clear to what extent and in what ways Bichat thought of the *non*-vital properties of tissues as contributing to the production of the vital phenomena. But the widespread interpretation of Bichat (see, for instance, Goodfield, *ibid.*, 68; C. Bernard, *Introduction to the Study of Experimental Medicine*, H. C. Greene (trans.) (New York, 1949), 59–60; J. T. Merz, *A History of European Thought in the Nineteenth Century* (New York, 1965), II, 385) that he thought of vital and physico-chemical properties as always in opposition to one another is mistaken. Vital properties opposed physico-chemical forces in the *environment* only.

[24] There are, of course, cases where the mere attribution of a power does explain its exercise. This is so, for instance, where the exercise of the power is known under some other description which can be corrected in the light of a power-ascription to the relevant agent. A non-technical case would be the explanation of an apparent kidnapping by an ascription of the (legal) power of arrest to the 'kidnapper' (a policeman). However, this does not apply to the case of the vital power of 'contractility' of muscles—the exercise of this power is already known as 'contraction'.

[25] Sir I. Newton, *Philosophiae Naturalis Principia Mathematica* (2nd edn., 1713), A. Motte (trans.) (London, 1729), II, 392.

[26] The correct interpretation of this passage is, of course, the subject of extensive controversy. Relevant and useful discussions may be found in R. E. Butts and J. W. Davis, *The Methodological Heritage of Newton* (Oxford, 1970), especially N. R. Hanson's 'Hypotheses Fingo'; G. Buchdahl, *Metaphysics and the Philosophy of Science* (Oxford, 1969), 333ff., 385ff; I. B. Cohen, *Franklin and Newton* (Harvard, 1966), 103ff., 127ff.

refers to the general facts of mutual attraction[27] between bodies as stated in the law of gravitation. The cause of this mutual attraction is unknown, and it would be unscientific to speculate about it at least at the present state of knowledge. Nevertheless, the mutual attraction can justifiably be adduced to explain a wide range of different phenomena.

Newton is here distinguishing two levels of explanation, the distinction being based on two different relationships: the relationship between the 'cause' of gravity and gravitational attraction itself, on the one hand, and relationship between gravitational attraction and phenomena (tides, planetary motion etc.) on the other hand.

A similar distinction is to be found in Bichat's work. In the *Physiological Researches* Bichat says this:

> In the study of nature, principles are certain general results of first causes (causes premières) from whence proceed innumerable secondary results. The art of finding the connection of the first with the second is that of every judicious mind. To seek the connection of first causes with their general effects is to walk blindfold in a road from whence a thousand paths diverge. . . . Let us suppose causes, and attach ourselves to their general results.[28]

Whilst in the *General Anatomy* we get:

> Sciences, both physical and physiological, are composed of two things, first, the study of phenomena, which are the effects; second, the investigation of the connections that exist between them, and the physical and vital properties (propriétés vitales) which are the cause.[29]

In these passages Bichat uses cause/effect terminology to refer to two very different sorts of relationship. First, there is the relationship between 'first causes'[30] and the 'principles' or properties of beings, which are the 'general results' of first causes. Second, there is the relationship between

My aim is not, of course, to provide a definitive interpretation of Newton but rather to give a reading of the passage which both is plausible and renders intelligible Bichat's analogy between the status of gravity in Newton's work and his own vital properties. For my argument to be complete here it would be necessary to situate Bichat with respect to the received traditions of Newton-interpretation in France at the turn of the century.

[27] Sir I. Newton, *op. cit.* note 25, I, 262.

[28] M. F. X. Bichat, *Physiological Researches on Life and Death*, F. Gold (trans.) (London, 1815), 79–80.

[29] M. F. X. Bichat, *Anatomie Générale, appliquée à la Physiologie et à la Médicine* (Paris, 1801), C. Coffyn (trans.) (London, 1824), iii.

[30] Bichat seems to have meant something like 'hidden' or 'underlying' cause by this term. There were no theological connotations. Thus his criticism of Stahl and Barthez for proceeding from 'first causes' amounts simply to the methodological point that they unjustifiably suppose the existence of hidden causes.

the principles or properties of beings and the innumerable 'secondary results' of them, the phenomena.

The correspondence between Bichat's and Newton's two types of relationship is, I think, obvious. Likewise, there is a correspondence between the epistemological prescriptions which both men associate with their distinction.

Bichat's prescription was only to 'suppose' first causes and investigate instead the phenomena and their relationship to the properties of beings, though he does not seem to have ruled out knowledge of first causes as in principle unattainable. But it does seem that such knowledge is of a different kind from knowledge already attained (whether it is knowledge of a non-scientific kind, or scientific knowledge of a recalcitrant kind is unclear).

Similarly, Newton (lacking the requisite experimental and observational evidence) refused to 'frame an hypothesis' about the cause (cf. Bichat's 'first cause') of gravity. However, his use of the phrase 'but hitherto' suggests that he did not regard knowledge of the cause of gravity as *in principle* unattainable.

So much then for the phenomenalist character of Bichat's concept of vital power and the epistemological position with which it is connected.

For my example of realist vitalism I shall now turn to the theory of Johannes Müller (1801–58). Müller was a figure of great importance as a teacher in the second quarter of the nineteenth century, many major figures in the rising generation of German physiologists being drawn from among the pupils of his innovatory teaching-cum-research laboratory in Berlin. In this institution students were encouraged to utilize the new experimental and conceptual tools of chemistry and physics, vitalistic explanation being used only as a last resort, after the failure of physical and chemical methods of analysis.

For Müller physiological phenomena were to be explained in terms of the properties of the 'proximate principles' (organic compounds) which made up the living body. Müller's vital principle, the 'organic' or 'organizing' force was adduced to explain the formation and maintenance of these compounds in opposition to the chemical affinities of their elementary constituents. The force was thought of by Müller as analogous in its mode of action to other, physical and chemical forces, but distinct from them.

Müller displays the 'realism' of his conception of the vital force when he criticizes Reil's view that the organic force is merely the result of the special

mode of combination of elements and the form of organic bodies.[31] Since bodies are structurally identical before and immediately after death, Reil must suppose the existence of some material constituent which is lost at death, so accounting for the loss of the vital phenomena, or he must concede, along with Müller himself, that the vital phenomena depend not only on the combination of elements in the living body, but also upon a vital energy or force independent of those combinations.

Similarly, in his discussion[32] of the quiescent state of seeds before germination and egg-cells before incubation (which he terms 'capability of living', the term 'life' being defined as 'the manifestation of the organic or vital force'), Müller asserts that 'a certain combination of elements' and the presence of the vital principle are necessary conditions for the manifestation of vital phenomena. But they are not jointly sufficient. Only in the additional presence of what Müller calls the 'vital stimuli' (warmth, atmospheric air, water, nutriment) are the phenomena of life manifested. The vital phenomena are thus *caused* by the organic force, and *conditioned* by the vital stimuli. This distinction between causes and conditions was not peculiar to Müller—or indeed to vitalists—and the readiness to use it can generally be taken as indicating a commitment to theoretical entities or agencies.

Müller's work can also be used to exemplify what I have called morphogenic vitalism. After introducing the notion of the organizing force as a solution to the problem of the formation and maintenance of organic compounds, Müller goes on to contrast the way in which crystals are formed, by the aggregation of particles all subject to the same laws of crystalline attraction to the way in which organisms are formed, by the contemporaneous development of different tissues and organs in an arrangement 'rationally adapted to the exercise of the forces, a most excellent harmony of the organization with the faculties intended to be exercised'.[33] Responsible for this difference is the vital force in a new role[34] —that of generating (though only from *organic* matter—Müller did not believe in spontaneous generation from inorganic matter) the organs in the arrangement necessary for the proper functioning of the adult organism. Initially, the vital force is present in the amorphous 'germinal disk' and

[31] J. P. Müller, *Handbuch der Physiologie des Menschen für Vorlesungen* (Coblenz, 1834), W. Baly (transl.), *Elements of Physiology* (London, 1837), I, 26.

[32] *Ibid.*, I, 28.

[33] *Ibid.*, I, 20. The second (1840) English edition has 'pre-ordained' instead of 'most excellent'.

[34] There seems to be no reason, save theoretical economy, why Müller should not have postulated a distinct force here. Certainly the force in this role is ascribed different characteristics and new mode of operation.

forms the organs of the developing organism 'strictly in accordance with what the nature of each requires', so as to harmonize with each other in their functioning. It is the continued action of the force into adulthood which sustains the harmonious functioning of the different organs.

Now, when Müller describes the arrangement of tissues and organs effected by the vital force as a rational adaptation, it is clear that he means more than that the arrangement is intelligible. He seems to mean that the developing organism, or more precisely, the vital force which controls development, is itself capable of exercising rationality. At the very least he must mean that the organism develops according to some rationally devised plan.

But whatever is meant by 'rational' it must be taken not to imply an imputation of consciousness to the vital force. Against Stahl, Müller argues that the 'organising principle' does not act consciously in forming the body:

> The formative or organising principle, on the contrary, is a creative power modifying matter, blindly and unconsciously, according to the laws of adaptation.[35]

There are strong grounds for saying that Müller attempts to attribute quite inconsistent characteristics to the organizing principle. For the principle to form the organs in accordance with 'what the nature of each requires', and to do so rationally would imply that each 'act' of the principle were 'chosen' with foresight of the consequences. This is, apparently, what Müller simultaneously rules out with the phrase 'blindly and unconsciously'. Müller resorts to metaphysics for a solution to his difficulties. Perhaps the principle is to be thought of as a 'creative archetype or Platonic Idea', at some time in the distant past 'infused into matter', and giving its characteristic form and properties to each species.[36] The appeal to Platonic Ideas here is inappropriate, though the Aristotelian notion of a Form is more in keeping with what Müller seems to require. However, it is probable that the more immediate source of Müller's explanatory model is the incursion of Romantic philosophy (especially Goethe and Schelling) into biology in the shape of 'Naturphilosophie'. Müller had been influenced by this movement in his youth, but by 1827 had rid himself of that influence except in this area.[37]

[35] J. P. Müller, *op. cit.* note 31, I, 25.

[36] *Ibid.*, I, 25.

[37] E. Mendelsohn, *op. cit.* note 1. It is perhaps relevant to note that in the second English edition of 1840 Müller comments on his own speculations concerning Platonic Ideas that such things are not 'an object of science' (I, 26).

Interestingly, Müller was not alone in being unable to carry over the conceptual and technical advances that were being made in physiology and chemistry into embryology.

As late as 1865, for instance, we find Claude Bernard, who had dispensed with vital powers and forces in physiology, saying this:

> . . . what distinguishes a living machine is not the nature of its physico-chemical properties, complex as they may be, but rather the creation of the machine which develops under our eyes in conditions proper to itself according to a definite idea (une idée définée) which expresses the living being's nature and the very essence of life.[38]

Next, to illustrate the distinction between physiological and chemical vitalism I shall compare very briefly Müller's contributions in these areas with those of Liebig. As we have already seen Müller based his vitalism in physiology on a form of chemical vitalism. It seemed to Müller to be reasonable to believe that living organisms were composed entirely of elements found commonly in inorganic nature. However, those elements were present in the form of 'proximate principles' or organic compounds which were not found in inorganic nature.

Müller presents a couple of distinguishing criteria for organic and inorganic compounds, and goes on to give the outlines of a theory to explain them. First, the elements making up organic compounds do not combine in constant proportions, as they do when they form inorganic compounds. Second, organic compounds are not just absent from inorganic nature—their artificial synthesis is impossible.[39] These differences suggest to Müller that both the mode of combination and the energy by which combination is effected in the organic compounds are peculiar.

His explanatory theory, which Müller attributes to Berzelius and Fourcroy,[40] is that inorganic compounds are formed by the binary combination of elements (or compounds), whilst the organic compounds are formed by the *direct* combination of three or four elementary substances. These

[38] C. Bernard, *op. cit.* note 23, 93.

[39] J. P. Müller, *op. cit.* note 31, I, 1. J. H. Brooke ('Wöhler's Urea . . .', *op. cit.* note 19) provides an excellent analysis of ambiguities in this widely held thesis.

[40] How far Müller's attribution here was justified is open to question. As Dr Brooke has pointed out to me, Berzelius was much less wedded to the unitary view of organic compounds than Müller, and Müller's chemical hypothesis was by this time obsolescent (see also Brooke, *op. cit.* note 39, 91, and note 23, *ibid.*).

Without explicit commitment to vitalism, however, more or less the whole doctrine of the structure of organic compounds given by Müller can be found already in Fourcroy's *The Philosophy of Chemistry*, prefixed to the 1796 English Translation of *Elements of Chemistry and Natural History* (Edinburgh and London).

ternary and quaternary proximate principles which make up the substance of living bodies have a constant tendency to decompose into binary compounds, which, in fact, they finally do after death. This decomposition into binary compounds is in accordance with the 'elective chemical affinities' of their constituent elements.

How, then, are 'proximate principles' formed and maintained *against* the chemical affinities of their elementary constituents? This is Müller's answer:

> Chemical compounds, we know, are regulated by the intrinsic properties and the elective affinity of the substances uniting to form them; in organic bodies, on the contrary, the power which induces and maintains the combination of their elements does not consist in the intrinsic properties of these elements, but is something else, which not only counteracts these affinities, but effects combinations in direct opposition to them, and conformably to the laws of its own operation.[41]

Thus Müller's chemical vitalism involved not simply the assertion that organic compounds were not subject to the laws of inorganic chemistry but even that they were not, properly speaking, chemical compounds at all.

Given Müller's belief that the vital activity of organisms was dependent on the properties (especially their great 'reactivity') of the 'proximate principles' his chemical vitalism clearly implies a physiological vitalism. If the vital power is necessary for the formation and maintenance of organic compounds, it is also necessary for the maintenance of the life-activities which depend on them. The action of the vital power in influencing chemical, and hence physiological, changes in the body was to be thought of as analogous to the action of heat and light, and as conforming to discoverable laws. There was nothing about this power, apart from its apparent confinement to living organisms, which ruled out its being regarded as a new species of physical or chemical power. Müller did not, so far as I am aware, explain why he did not so regard it.

It is illuminating to compare the position of Müller with that of his great contemporary, Liebig. Liebig was not particularly consistent in his discussions of the role of vital forces, and in any case his views seem to have changed considerably throughout his career.[42] However, a more or less

[41] J. P. Müller, *ibid.*, I, 4. But note that in the Second English edition Müller omits 'in direct opposition to them' from the passage just quoted, and is generally less confident in his assertions of the existence of the vital force (2nd edn. I, 4).

[42] See especially T. O. Lipman, 'Vitalism and Reductionism in Liebig's Physiological Thought', *Isis*, **58** (1967), 167–85.

coherent and distinctive view can be obtained from the *Familiar Letters*.[43]

Liebig's view was that organic compounds were formed chemically, by the affinity of their ultimate particles, according to the same laws, and in the 'same fixed and immutable combining proportions' as inorganic ones.[44] Also Liebig asserted the possibility of artificial synthesis of organic compounds (whilst conceding that thus far most successful syntheses had used organic materials as raw-materials).[45]

On the question of the formation of organic compounds, Liebig's theory was that the elements combined to form them in accordance with their chemical affinities. However, organic compounds did differ from inorganic in that the 'compound atoms' of the former contained more atoms. The greater the number of atoms in a compound, the weaker were the forces of affinity between them. The weaker these forces of affinity, the easier it was for various extraneous factors (gravity, electric and magnetic forces, heat, light, etc.) to disrupt and decompose compounds. Consequently, the organic compounds were more unstable and more difficult to synthesize than the inorganic.

Liebig was not, then, a chemical vitalist. However, this did not prevent him from holding a vitalist position in physiology. An explanation was still required of how it was that organic compounds were formed and maintained in the living organism—not, how, despite their chemical affinities, but despite the many extraneous disruptive influences. Re-enter the vital force; this too was able to alter the strength and direction of the chemical force, and so counteract the disruptive effect of light, heat and the rest. As Liebig puts its:

> The vital principle annihilates the power of these forces to disturb the manifestation of chemical affinity; it promotes the formation of organic substances in a precisely similar manner as heat renders possible and favours the formation of inorganic compounds; that is, by removing altogether or diminishing the energy of other resisting forces.[46]

Finally, I shall very briefly outline examples of the use of teological, nomological and non-nomological forms of explanation by vitalists.

The morphogenic vitalism of Müller is most readily understood as

[43] J. von Liebig, *Familiar Letters on Chemistry*, 2nd series, J. Gardiner (ed.), (1844). Other editions, containing additional letters, appeared in 1845, 1851 and 1859.

[44] J. von Liebig, *ibid.*, *Letter VII*, 3rd edn. (1851), *Letter XIV*, 166.

[45] But Liebig did *not* believe in the possibility of direct synthesis of organic compounds from their elements: '. . . although we are altogether incapable of producing any of these compounds by direct combination of their elements'. Liebig, *ibid.*, 3rd edn., 175. See Brooke, *op. cit.* note 39.

[46] J. von Liebig, *ibid.*, 129. See also 3rd edn., *Letter XIV*, 174–5.

teleological in form. Müller's reference to his 'organizing power' as creative, and as producing a 'rationally adapted' arrangement of the organs, suggests that the action of the power can be understood only in terms of the outcome it produces, rather than the conditions under which it is manifested. The use of the notion of a 'creative archetype' has a similar implication.

On the other hand the view, attributable to Müller (when he is discussing the role of the vital power in physiology) and also to Liebig, that the exercise of the vital power is subject to discoverable general laws, was connected with an affirmation of the importance of experimentation in physiology. If the exercise of the vital power were related in a lawlike way with certain conditions, then the relevant laws could be discovered by maintaining some conditions constant whilst altering others. The correct method in science was, according to Liebig:

> . . . to test the truth or falsehood of our notions by producing the phenomenon ourselves, under varying circumstances, and by examining the conditions of its first appearance, varying these conditions, and closely observing the influence of these alterations.[47]

Scientific understanding of the action of the vital force was to be gained in precisely the same way. Liebig was opposed to those who misused the term 'vis vitae' to refer to an 'incomprehensible, indefinable something', and thereby obstructed scientific understanding.

> But this vis vitae is itself but a subject of investigation and in order to explore it, to comprehend its essences, to understand its operations and effects, the physician must pursue the same method which has been followed in natural philosophy and chemistry with such signal success.[48]

For both Müller and Liebig vital forces or powers were regulated in their operation by laws which could be discovered by applying the same experimental method as had proved successful in investigating other forces or powers in nature. Experimental methods were to be employed in physiology, and to be employed quite generally in physiology.

This position on the law-abiding character of physiological phenomena was by no means a foregone conclusion even in Germany at the time of Müller and Liebig. Even Liebig had to recognize that the results of many experiments aimed at the discovery of quantitative laws were very divergent and apparently inconsistent. Some experimenters had, for instance,

[47] J. von Liebig, *ibid.*, 25, 3rd edn. (1851), 15.
[48] J. von Liebig, *ibid.*, 24; 3rd edn. (1851), 14.

claimed that the body-heat produced by animals could be accounted for entirely in terms of the combustion of foodstuffs by oxygen taken in during respiration. Others, however, produced quantitative results which seemed to show that oxygen intake was far too small to account for the quantities of heat produced, and that some other source of heat—such as nervous action—must be supposed. One understandable response to this sort of situation was the rejection of quantitative methods as such, on the grounds that organisms behave in a fundamentally irregular, variable fashion.

Liebig, however, argued for the retention of quantitative methods, but stressed the need for great care in the interpretation of results. Different results obtained by experimentation into the production of animal heat were to be understood in the light of the great variety of factors affecting the rate of both respiration and heat loss, and the consequent difficulty of controlling all relevant factors.

The dominant vitalist tradition in France contrasted markedly with the German during this period. The variability and 'spontaneity' of the vital powers were there held to rule out the use of experimental procedures—and especially quantitative ones—to investigate their properties.[49]

This view is reported by Foster as widely held especially in medical circles in France during the early decades of the nineteenth century. He quotes Gerdy, a professor of surgery, for instance, as denying that 'vital phenomena remain identical under identical conditions'.[50]

Even experimentalists in physiology such as Magendie and Delaroche[51] defended their use of experiments by distinguishing within the vital phenomena between those produced by the vital force and those produced by physico-chemical forces. Experimentation was of use in gaining understanding of only the latter. So much was this conception of the variability of vital powers or forces entrenched in French physiology that Bernard was able to regard his rejection of vitalism as equivalent to the denial that matter had 'spontaneity'.[52]

The figurehead of this tradition of vitalist thought was Bichat, and accordingly it is to his work that I shall turn for an example of non-nomological vitalism. In the following passage Bichat contrasts the vital powers of organisms with physical laws:

[49] I understand from Dr Brooke, that recent (as yet unpublished) studies by John Pickstone indicate that this rather bald assertion requires at least some qualification, in view of the great diversity of approaches in the French physiology of the period.

[50] Sir M. Foster, *Claude Bernard* (London, 1899), 14.

[51] F. Magendie, *Précis Élémentaire de Physiologie* (3rd edn., Paris, 1833), 5.

[52] C. Bernard, *op. cit.* note 23, 60ff.

The first incessantly vary in their intensity, in their energy, in their development, are continually passing from the last degree of prostration, to the highest pitch of exaltation, and assume under the influence of the most trifling causes a thousand modifications; for the animal is influenced by everything which surrounds him; he wakes, he sleeps, reposes or exercises himself, digests, or is hungry, is subject to his own passions, and to the action of foreign bodies. On the contrary the physical laws are invariable, the same at all times, and the source of a series of phenomena at all times similar. Attraction is a physical power, it is always in proportion to the mass of brute matter in which it is observed; sensibility is a vital power, but in the same mass of matter, in the same organic part its quantity is perpetually changing.[53]

This is a most tantalizing and difficult passage, but I have space for only a very brief and partial commentary. One plausible interpretation of Bichat is that the exercise of the vital powers is so variable because it is influenced by such a wide variety of internal and external conditions. This interpretation would not rule out the possibility that the vital powers are subject to general laws. Such laws might be in principle discoverable by experimental analysis, though they need not turn out to be physical or chemical laws.

On such an interpretation, the point of Bichat's emphasizing the variability and instability of the vital powers (or their exercise) is simply to highlight the sheer number and variety of the conditions capable of affecting animal behaviour, and the complexity of the organism's response. Then there would be great technical difficulties in the way of discovering the relevant quantitative laws, but this would not be because no numerical relationships exist between the vital phenomena and their causes.[54]

Bichat does seem to have been interpreted in this way,[55] but not, generally, by his contemporaries. In the eyes of most of his contemporaries and followers, Bichat believed that the exercise of the vital powers was not related in a regular manner to external or internal conditions. This interpretation can be plausibly extracted from the above quotation from

[53] M. F. X. Bichat, *op. cit.* note 28, 80.

[54] This seems, in fact, to have been the position of Bichat's great contemporary, G. L. Cuvier. See, for instance, Cuvier's *Animal Kingdom*, M. McMartine (trans.) (London, 1834) from the 2nd (1829) French edn., 2. Here Cuvier says that

> An essential difference between the general sciences and Natural History is, that, in the former, phenomena are examined, whose conditions are all regulated by the examiner, in order, by their analysis, to arrive at general laws; whereas, in the latter, they take place under circumstances beyond the control of him who studies them, and who seeks to discover, amid their complication, the effect of some general principle already known.

[55] For instance, by G. J. Goodfield. See *op. cit.* note 20, 68ff.

Bichat, and has the implication that experimental analysis is not at all applicable to the scientific study of the phenomena of life.

This interpretation of Bichat has him asserting that the same animal, in the same internal state, subject to the same external conditions, might behave differently at different times (that is to say, it might or might not exercise its vital powers,[56] and to a greater or lesser 'degree'). Such a conception of the vital powers and their exercise faces very serious logical and philosophical problems. The denial that the covering-law form of explanation applies in physiology requires, surely, some sort of account of what form physiological explanation should take. Since Bichat used cause/effect terminology, and spoke of conditions 'affecting' and 'influencing' the behaviour of organisms, without giving any alternative account of causality in his more epistemological moments, we must suppose that either his work is fundamentally inconsistent, or that some other interpretation of him is required.

A less radical interpretation of Bichat which still has him denying the subjection of the vital powers to quantitative natural laws is this: changes in the behaviour of organisms are related to changes in external and internal conditions, but not in a determinate way. For example, maintaining other conditions constant, but raising the temperature of the environment may produce a different response in an organism today from the one it produces tomorrow, but it will produce some response which can be identified as a response to this same change in external conditions. This view is, I think, a plausible interpretation of Bichat's explicit statements on the variability of the vital powers, and is not obviously incoherent. It does not rule out experimental analysis as a source of physiological knowledge, though it does rule out the possibility that experiments might yield quantitative laws.[57]

This squares well with the fact that Bichat did engage in physiological experimentation, though it was generally designed to reach conclusion of a qualitative kind. In his *Physiological Researches*, for instance, he undertook to discover the influence of the death of the heart on the brain, concluding after vivisections on dogs that the life of the heart is necessary for the life of the brain.

This interpretation of Bichat is also consistent with his explicit criticism of quantitative methods in physiology. Borelli, he says, in calculating the

[56] Though, strictly speaking, vital powers are attributes not of organisms, but of their constituent tissues.

[57] Though there are exceptions to this. There is, for instance, a 'rigorous proportion' between organic sensibility and contractility. See Bichat, *op. cit.* note 28.

force of muscle, and Lavoisier, in calculating gaseous exchanges in the lungs, 'build upon a quicksand an edifice solid of itself, but necessarily decreed to fall for want of a foundation.'

III

Finally, I wish to turn to a discussion of the ways in which the typology I have just outlined might illuminate a couple of problems of historical exegesis.

The first problem I shall take is the notorious one of the significance of Wöhler's synthesis of urea for the fate of vitalism.

An early view which appears still to have some adherents is succinctly expressed by H. T. Pledge:

> It thus happened that when in 1828 Wöhler identified the compound obtained by treating potassium cyanate with ammonium sulphate and the 'urea' obtained from urine (and so 'organic'), his work was greeted as an organic synthesis; as a destruction of the vitalistic view; as, in fact, a charter of liberty for organic chemists.[58]

The firmest rejection of this estimation of the significance of Wöhler's synthesis was delivered in McKie's well-known 1944 paper,[59] in which he argued that the raw materials for Wöhler's 'synthesis' were themselves organic, being at that time classified as inorganic 'as a mere item of code'. McKie further argued that (given his definition of synthesis as 'the compounding of a substance from the elements that compose it') Wöhler had effected only a transformation, not a synthesis, and that vitalist doctrines were still widespread long after 1828.

But what McKie did not make clear (partly, it seems, because it did not seem to him to be a very interesting question[60]) was the question of the implications of syntheses for vitalist doctrines independently of whether Wöhler's 'synthesis' really was a synthesis. Also, beyond the assertion of what the significance of Wöhler was not, McKie had little to say about what it actually was.

Some recent studies, most notably those of Lipman[61] and Brooke,[62] have sought to present more detailed and balanced assessments of the

[58] H. T. Pledge, *Science Since 1500*, 2nd edn. (London, [1939] 1966), 126.

[59] D. McKie, 'Wöhler's Synthesis of Urea and the Rejection of Vitalism', *Nature* **153** (1944), 608–10.

[60] *Ibid.*, 609.

[61] T. O. Lipman, *op. cit.* note 1 and note 6. Also 'Wöhler's Preparation of Urea and the Fate of Vitalism', *Journal of Chemical Education*, **41** (1964), 452–7.

[62] See J. H. Brooke, the two articles cited, note 19.

significance of Wöhler's work. The main burden of Lipman's argument turns on the distinction between the creation of a living, organized body, and the creation of a non-living, organic compound. Vitalists thought of the vital force as necessary for the first, not the second. Thus Wöhler's synthesis was 'almost insignificant';[63] only the artificial synthesis of life itself could have been a serious threat to vitalism.

Brooke, too, argues that the organic/organized distinction has often been overlooked to the detriment of accounts of the significance of Wöhler's work. Brooke also presents an alternative, and convincing assessment of the relevance of Wöhler's synthesis, not so much for vitalism, but (as an example of isomerism) for contemporary debate about the importance of the structure, as well as composition, of chemical compounds.

But, most importantly, Brooke performs a valuable service of clarification with his discussion of ambiguities in the sentence, 'We cannot, from its elements, produce an organic body as Nature does'. His objective here is to resolve these ambiguities so as to support the insignificance-claim for the urea synthesis '*vis-à-vis* current vitalist beliefs'.[64] In the main, my account supplements Brooke's by bringing out the differential significance of the urea synthesis for different sorts of vitalism,[65] though I do take issue with Brooke on a number of points of detail.

Now to the outlines of my account. First, my typology distinguishes three main fields within which vitalist doctrines were maintained. It is quite clear that Wöhler's synthesis had no direct bearing on the problems with which morphogenic vitalists were engaged. Thus, Wöhler's achievement not only failed to falsify morphogenic vitalism—it was quite irrelevant to it.

But with the chemical and physiological vitalisms of the period the situation was much more complex. Some vitalists did hold that the vital force was necessary for the formation of organic compounds. For these theorists Wöhler's synthesis did constitute an empirical threat, but, of course, not a decisive one. In order to maintain a chemical vitalism such as Müller's against Wöhler's results it was necessary either to re-interpret those results, or modify the vitalist theory. This is sufficient to show that Lipman's general insignificance-claim is mistaken. The synthesis of urea was relevant to chemical vitalism.

As is well known, Müller argued that Wöhler had not succeeded in

[63] T. O. Lipman, *op. cit.* note 1, 457.
[64] J. H. Brooke, 'Wöhler's Urea . . .', *op. cit.* note 39, 84.
[65] In fact, Brooke's distinctions are themselves a useful tool in distinguishing types of vitalism.

synthesizing an organic compound since urea was not, properly speaking, organic.[66] It is clear that this manoeuvre had the effect of insulating Müller's vitalism from the implications of the synthesis of urea. However, in support of his insignificance-claim Brooke argues[67] that Müller (and Berzelius) had other motives for rejecting the synthesis as an organic synthesis. In particular, the synthesis seemed to threaten Müller's theory of the special mode of combination of organic compounds.[68] Only by refusing to allow urea as an organic compound could the theory be preserved.

Brooke is certainly correct here, but it does not seem to me that his point supports his insignificance-claim. It was precisely to explain the special mode of combination of the organic compounds, against the chemical affinities of their atomic constituents, that the hypothesis of the vital force was required. To have allowed the falsification of the theory of the special mode of combination of organic compounds may not have entailed the falsification of the vital-force hypothesis but it would have removed the main argument for that hypothesis. In short, the logical relationship between these two elements of Müller's theoretical position was so close that a significance-claim for the urea-synthesis *vis-à-vis* one of them is tantamount to a significance-claim for the other.

Since Müller's physiological vitalism followed from his chemical vitalism,[69] it is clear that any empirical threat to the latter, such as, I have argued, Wöhler's synthesis constituted, would also be a threat to the former.

However, we also saw that it was possible to find grounds for physiological vitalism other than in chemical vitalism. Liebig's work is a fairly clear example. For him, the vital force aided in the formation and maintenance of organic compounds only by counteracting external disrupting agencies. The artificial synthesis of organic compounds was quite consistent with this conception of the mode of action of the vital force. Further, Liebig's theory offered some sort of explanation of the technical difficulty of achieving organic synthesis.

Finally, a point about the differential vulnerability of different sorts of vitalism to empirical evidence of the sort provided by Wöhler. Both

[66] See, for instance, Müller, *op. cit.*, I, 3ff.

[67] J. H. Brooke, 'Wöhler's Urea . . .', *op. cit.* note 19, 91.

[68] See this paper, pp. 32–3.

[69] See this paper, p. 33. With regard to Müller's work, the remarks which Brooke quotes from R. Schubert-Soldern are not so wide of the mark as Brooke seems to suggest. See Brooke, 'Wöhler's Urea . . .', *op. cit.* note 19, 104.

Lipman[70] and McKie[71] seem to accept the view that vitalism was eventually rejected after a long process of accumulation of factual counter-evidence. Brooke[72] rejects this account but is unclear about what to substitute for it—vitalist views, he says, are too 'resilient and complex to be corroded by mere facts'. But why shouldn't complex views be falsified, just as simple ones can be? And what accounts for the resilience of vitalism?[73]

Müller's and Liebig's vitalisms were 'realist'—that is to say, they conceived of the vital power as an underlying agency whose manifestations were detectable by observation and experiment. A theory of this sort is relatively invulnerable to evidence. Any evidence which tells against established generalizations can be accommodated without relinquishing commitment to the existence of the agency underlying them by simply modifying the conception of the mode of action of the agency. The shift from Müller's to Liebig's conception of the mode of action of the vital force can be interpreted in this light.

Clearly a phenomenalist conception of the vital power (as was typical of French vitalism in the early nineteenth century), which identified the vital power as a relationship between phenomena, would be much more susceptible to counter-evidence.

Finally, I shall suggest some ways in which my typology may throw some light on a second problem of historical exegesis. This problem is identified by Mendelsohn, and concerns the contrast between French and German rejections of vitalism towards the middle of the nineteenth century and afterwards. Mendelsohn's view is that the reductionist approach of Schwann, Du Bois-Reymond, Helmholtz and others of their generation was a repudiation of earlier work—particularly that of Müller; above all, they are to be seen as a movement in reaction against the incursion of 'a prioristic' speculation in biology, the *Naturphilosophen* being the main culprits. By contrast Bernard's work, in France, though it involved the rejection of vitalism, was at the apex of a cumulative development. Bernard was able to synthesize the best of what had gone before in France, whilst Schwann and his colleagues overthrew the work of their predecessors.

[70] T. O. Lipman, *op. cit.* note 61, 457.

[71] D. McKie, *op. cit.* note 59.

[72] J. H. Brooke, 'Organic Synthesis . . .', *op. cit.* note 19, 374.

[73] It is, perhaps, important to emphasize that I here intend only to cast doubt on Dr Brooke's particular line of attack on the view that vitalism was rejected as a result of the accumulation of factual counter-evidence. I agree with Brooke that this account of the demise of vitalism is inadequate, though I would, perhaps, argue this on rather different grounds, having to do with the general failure of falsificationist accounts of scientific rationality.

In an earlier section of this paper I suggested that my typology of vitalisms was capable of an extension to cover non-vitalistic theories, so providing criteria by which to assess the nature and extent of any theoretical change. I have space only to attempt this in connection with the transition from vitalism to reductionism in Germany, and then only for the representative theories of Müller and Schwann. But this should be sufficient to show that a reassessment of Mendelsohn's conclusions is required.

According to Mendelsohn, Müller, though himself a vitalist, led his students 'into exploration of the very problems through which they went on to develop a radically new idea of what constituted an acceptable physiological explanation.'[74]

Of the new conception of physiological explanation, Mendelsohn has this to say:

> These men emerged from their initial studies believing that the vital functions under examination could be successfully explained with recourse only to physicochemical laws.[74]

The idea that Schwann was heading a movement for a 'radically new' approach in physiology is supported not only by an assessment of Schwann presented at a celebration in his honour at the Berlin Academy of Sciences (1878) but also by Du Bois-Reymond and by Schwann himself.[75] Evidence from the 'horse's mouth', so to speak, might be regarded as being quite decisive. However, Schwann and Du Bois-Reymond, though themselves participants in the theoretical changes under discussion, have the role of historians of those changes in the texts which Mendelsohn cites. As such, their assertions have no more authority than those of any other historian—perhaps rather less than some. Nevertheless, if grounds are discovered for doubting their own assessment of the significance of the changes they inaugurated, an explanation of their own historical misjudgments will be required.

First, though, an outline of Schwann's new conception of physiological explanation, and of his application of it.

In the *Microscopical Researches* (1839)[76] Schwann contrasts two predominant views on the fundamental powers of organized bodies. First, the teleological view:

[74] E. Mendelsohn, *op. cit.* note 1, 208.

[75] See Schwann's (1858) letter to Du Bois-Reymond and Du Bois Reymond's letter of 1841 cited in Mendelsohn, *op. cit.* note 1. As Mendelsohn points out, an early (1835) letter indicates that Schwann may have been rather more uncertain than this later reflections suggest.

[76] T. Schwann, *op. cit.* note 10.

. . . every organism originates with an *inherent power* which models it into conformity with a predominant idea, arranging the molecules in the relation necessary for accomplishing certain purposes held forth by this idea.[77]

Secondly, the physical view:

. . . the fundamental powers of organised bodies agree essentially with those of inorganic nature, that they work altogether blindly according to laws of necessity and irrespective of any purpose, that they are powers which are as much established with the existence of matter as the physical powers are.[77]

Schwann subjects the former view to criticism and advocates the acceptance of the latter. But what does Schwann mean when he says that the fundamental powers of organized bodies 'agree essentially' with those of inorganic nature? Does he mean that the powers of organized bodies are identical with those of inorganic nature? This, presumably, would be necessary if Schwann were to be correctly described as a 'reductionist'.[78] However, in further clarification of his view, Schwann says that his insistence is on explanation 'by means of powers which operate *like* the physical powers, in accordance with strict laws of blind necessity, whether they be also to be found in inorganic nature or not'.[79]

It seems that Schwann was prepared to countenance as conforming to the 'physical view', explanations of vital phenomena, not only in terms of physical powers as yet unknown, but also in terms of non-physical powers, so long as these non-physical powers share certain important characteristics with physical powers proper.

However, Schwann's own crystallization-theory of cell-formation was in conformity with a physicalism of a much more restrictive kind than this. His argument has two distinct phases—the first accomplishing a shift of the level at which explanation of developmental phenomena are to be explained to the cellular level, the second being an argument from analogy yielding the crystallization-theory as a conclusion.

Drawing on Schleiden's work, Schwann is able to assert the generalization that there is 'one universal principle of development for the elementary parts of organisms', and that this principle is the 'formation of cells'. The next question is: What is the cause of the vital phenomena? This cause must reside in the whole organism, or in the individual cells. But, in the case of at least one important vital phenomenon (growth) isolated

[77] *Ibid.*, 190.

[78] Several different *sorts* of reductionist thesis may, of course, be distinguished. See, for instance, E. Nagel's contribution to M. Claggett, *Critical Problems in the History of Science* (Madison, 1959).

[79] T. Schwann, *op. cit.* note 10, 190. My emphasis.

cells display it, if given the right conditions. So, for these cells, the cause of growth is in the individual cell, the rest of the organism merely providing the conditions proper to growth. Now, as has already been shown, there is one universal principle of growth for all elementary parts. It follows that:

> . . . the question as to the fundamental power of organised bodies resolves itself into that of the fundamental powers of the individual cells.[80]

Schwann's transference of the level of inquiry is now complete. He is free to pose the problem of the cause of the vital phenomena as the problem of the powers of cells.

The vital phenomena manifested by cells are of two sorts—'plastic' and 'metabolic'.[81] Schwann's discussion centres around the question of the cause of the plastic phenomena—the combination of molecules to form new cells. This cause, which Schwann calls the 'plastic power', is of unknown nature. As a means of clarifying the nature of the plastic power, Schwann goes on to suggest a comparison with crystallization. On the basis of this analogy, he attempts to show that the laws of cell-formation are just what we should expect if cell-formation were a process of crystallization, making due allowances for the special characteristics of the substances undergoing crystallization in the formation of cells.

This allows Schwann to hypothesize the (contingent) identity of the plastic power and the chemical power of crystallization.

Schwann's theory of cell-formation was an attempt to solve at least some of the problems to which Müller's morphogenic vitalism was addressed, it was 'realist' in that it involved the postulation of an agency underlying and 'causing' the vital phenomena, and it was nomological in that it conceived of the action of the plastic power as law-governed.

Thus, though the crystallization theory was reductionist in character, it is fairly easy to situate within the framework which I initially erected for my typology of vitalism.

We are now in a position to assess the significance of the new conception of physiological explanation introduced by Schwann. We saw, earlier, how Müller's conception of the vital power was also realist in character. Both Müller and Schwann conceived of the powers whose nature they sought to discover as underlying observed phenomena, and as explaining those phenomena, the realist character of both conceptions being evidenced by a preparedness to distinguish 'causes' and 'conditions' for the occur-

[80] *Ibid.*, 193.

[81] The distinction closely corresponds to my 'morphogenic' and 'physiological'.

rence of the phenomena. This continuity between the two is of fundamental importance—had not Schwann thought of his 'plastic power' as an independently existing agency the question of its identity with the chemical power of crystallization could not have been posed, and the reduction could not have been hypothesized.

Further, Müller's physiological vitalism was of the nomological variety, as was Schwann's crystallization theory. Both the plastic power and the vital power in its physiological role obeyed laws of blind necessity.

It seems, then, that there were only two respects in which Schwann's conception of explanation differed from that of Müller. First, in that he was prepared to advance an explanation of developmental phenomena in terms of powers whose exercise was subject to blind laws, whereas Müller restricted such explanation to physiology and organic chemistry. But even this must be qualified in two ways. First, though Müller's morphogenic vitalism was teleological, Müller did attempt (unsuccessfully) to combine his teleology with a conception of the vital power even in its developmental role as operating blindly, according to fixed laws. Second, by altering the level of analysis of developmental phenomena to the cellular level Schwann avoided the key problems of differentiation and integration at the organismic level which had seemed to Müller to require a special form of explanation.

The second respect in which Schwann's conception of explanation might be thought to differ fundamentally from Müller's is that Müller was, whereas Schwann was not, prepared to countenance powers of a non-physico-chemical kind. On Schwann's side, it is certainly true that his crystallization theory was an attempt to provide an explanation of vital phenomena in terms of an already known and classified chemical power. However, this was not necessary in order for his theory to conform to the physicalist conception of explanation outlined in the *Microscopical Researches*. There is nothing in that conception which clearly rules out physiological explanation in terms of vital powers such as the one Müller hypothesized to explain the formation and maintenance of organic compounds. The only room for doubt here is that Müller conceived of his power as analogous to physical and chemical ones, but did not include it among them. On the other hand, as we saw, Schwann was prepared to accept explanation in terms of as yet unknown physical and chemical powers, and even non-physical or chemical powers, so long as these powers were like them in important respects. Add to this that neither biologist proffered a clear demarcation criterion for non-physico-chemical powers,

and it appears that there is very little foundation for the claim that, with respect to Müller, Schwann advocated 'a radically new idea of what constituted an acceptable physiological explanation.'[82]

If a radical discontinuity in German physiology of the nineteenth century is to be found anywhere, the rejection by Müller and others of his generation of the *a prioristic Naturphilosophie* is a much more likely place to look for it. It seems to me that Mendelsohn's account does not distinguish sufficiently clearly between *Naturphilosophie* and the rather refined forms of vitalism which lasted into the mid-century in Germany.[83]

If my main conclusion here is accepted, then a new problem emerges: how do we account for the over-estimation by Schwann and his contemporaries of the extent of their break with Müller? I do not claim to have the answer to this question, but it seems to me to be an intriguing one which demands further research.

[82] I believe that my argument here substantiates, at least for the examples of Müller and Schwann, the claim that I made on p. 18 that sometimes the differences between vitalist and non-vitalist could be little more than terminological disputes. In this case, much depends on precisely how Schwann's stricture concerning explanation other than in terms of physical powers or powers which operate like them is to be interpreted, and on what is or is not to be counted as such a power.

But Dr J. H. Brooke has raised an important objection to the procedure I adopt to reach this conclusion as to the triviality of the gap between Müller and Schwann. His point is that I show this on the basis of a series of *philosophical* distinctions, whereas what Mendelsohn may have had in mind was the question of the differences between Müller and his students on *scientific* matters. Possibly Mendelsohn was intending to imply a radically new science.

The charge that I have been laying undue stress on philosophical, at the expense of scientific, distinctions throughout the paper certainly would seem to have some force, and so a few words of explanation may be necessary. First of all, I concede that Mendelsohn's intentions are not always clear, but it does seem to me that his choice of phrase—'radically new *ideas of what constituted* an acceptable physiological explanation' (my emphasis) rather than simply 'radically new physiological explanations'—indicates that he does have in mind a *philosophical* revolution. And it is precisely new ideas about what constitutes an acceptable explanation which my typology is designed to capture. Different conceptions of causality, of regularity in nature, logical forms of explanations, and how these relate to different objects of knowledge are all, I think, legitimate objects of *philosophical* investigations, they are what my typology is intended to bring out and distinguish, and, finally, all are involved in deciding whether or not some particular scientific movement (in this case the one in physiology supposed to have been led by Schwann) has produced a radically new conception of what counts as explanation.

But to answer the more general point, it also seems to me that there are very close historical *and* conceptual relationships between 'radically new science' and radically new philosophy, such that I should take the non-existence of a fundamental philosophical break (if this could be fully demonstrated) as a strong indication of the absence of any major *scientific* break. It is this presupposition (concerning the connection between scientific and philosophical change) which is behind my general emphasis on philosophical distinctions, and it is, of course, a fairly widely held thesis, in one form or another. Perhaps this serves to make my own position a little clearer, though a thorough attempt to state and defend my views on the relationship between philosophical and scientific change is obviously well beyond the scope of this paper.

[83] As Driesch's discussion clearly shows, there is no necessary connection between *Naturphilosophie* and *any* form of vitalism. See especially H. Driesch, *The History and Theory of Vitalism*, C. K. Ogden (trans.) (London, 1914), 93ff.

However, one hint as to the direction in which one might look is provided by Nordenskiöld. Speaking of Müller, he says:

> He was University Warden during the years of the Revolution of 1848 and, being a conservative, came into repeated conflict with revolutionary-minded students.[84]

Also, according to Nordenskiöld, at least two of Schwann's most well-known contemporaries and fellow-pupils of Müller—Virchow and Henle—were persecuted by the Prussian authorities for their liberal views. A number of unanswered questions remains: How early did political differences between Müller and his students emerge? How widespread and intense was the political opposition to Müller amongst those students who Mendelsohn identifies as protagonists of the new conception of physiological explanation? Could it be that in distancing themselves from Müller's physiology they were giving expression to political differences which made it difficult for them to give due weight to the physiological continuities between them?[85]

To attempt answers to these questions would take me well beyond the scope of the present paper. However, I hope that I have already been able to show that the questions posed by Mendelsohn are inappropriate. The conception of physiological explanation held by Schwann and his contemporaries did not constitute a radical break from Müller's physiology. It follows that what requires explanation is not the mid-century revolution in German physiology, but the mid-century striking of revolutionary attitudes where there was no revolution.

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[84] E. Nordenskiöld, *The History of Biology* (Kegan Paul, 1929), 382.

[85] Dr J. H. Brooke has, quite correctly, pointed out that much further argument and documentation would be required to establish this speculation. He also doubts whether, in view of the frequency with which scientists have 're-written' history so as to emphasize their own originality, my political hypothesis is necessary. There do, though, seem to be special features of this case which tell against Brooke's point. First, the claim to revolutionary leadership is not made so much by Schwann himself, as on his behalf by his contemporaries. Secondly (and this requires much further research) there is a remarkable coincidence both temporally and geographically between the Müller/Schwann/Du Bois-Reymond episode, and the break which Feuerbach and Marx made with their erstwhile 'Young Hegelian' colleagues also in the name of 'materialism' though not now in philosophy but in history. There is even some direct overlap in the controversies generated by the two events.