Locating Dutrochet

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ESSAY REVIEWS

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Henri Dutrochet (Henri du Trochet, 1776–1847). Le matérialisme mécaniste et la physiologie générale. By Joseph Schiller and Tetty Schiller. Paris: Albert Blanchard, 1975. Pp. 227. 50 francs.

Dr Joseph Schiller, who died in December 1976, contributed much to the historical study of biology in the period 1750–1850. His first book-length work, *Claude Bernard et les problèmes scientifiques de son temps* (1967), has not had the recognition it deserves, partly because strictly internalist history of science is no longer a sufficient goal for most researchers, and partly because Dr Schiller worked outside the major social networks of the subject. It was perhaps this situation which drew him to write his study of Henri Dutrochet (1776–1847), for, in Dr Schiller's opinion, Dutrochet was a *frustré* of the history of science, who after a lifetime's research, mostly carried out in the isolation of a small country château during the French Restoration, enjoyed an Indian summer of recognition in Paris only to have his achievements neglected by modern historians of science.

Dr Schiller's present work is the second major attempt to draw attention to Dutrochet. The first was that of A. R. Rich who in 1926, the centenary of Dutrochet's discovery of osmosis, published a long and useful paper outlining his life and work, and stressing his cell theory. In Rich's opinion, Dutrochet's cell physiology was superior to that of Schwann for which it was a likely but unacknowledged source.[1] Now, fifty years later, Dr Schiller and Mme Schiller have pursued essentially the same approach in presenting this collection of previously unpublished correspondence and autobiographical pieces. The Schillers have provided a biographical and iconographical sketch to accompany Dutrochet's *Notice sur ma vie*, very useful notes enlarging on the *Notice sur mes ouvrages* (1846), and short chapters on the scientific conceptions and influence of Dutrochet. The material includes significant items which were previously unknown, and all scholars interested in this area are much indebted to Dr Schiller for ferreting out this material and getting the collection published, as well as for the throughness of the notes and the subtlety of the discussion. Though nothing presented here forces any radical change in our understanding of Dutrochet, the book provides a most useful aid and a most stimulating challenge for future research.

The *Notice sur ma vie* together with a number of letters from military archives sheds new light on Dutrochet's early life and military adventures. Dutrochet appears to have joined his father and brother in the army of the Vendeans, but the strength of this attachment remains in doubt. Unfortunately there is little new material on Dutrochet's time as a medical student, or on his long years at his mother's home, Chareau, but it is useful to know that he trained a local youth to help him in his scientific work. It seems that Dutrochet, like Charles Bonnet before him, was unable to concentrate for long periods on detailed anatomical studies without precipitating ill health. Thus when he planned a collaboration with Gilbert Breschet to study the salamander he insisted that Breschet carry out the anatomy of the adult; he would examine the patterns of development.

Dutrochet was a rural, not a metropolitan, investigator; his interest in the work of other men similarly placed is nicely illustrated by his visit to England, which Dr Schiller has thoroughly researched. Dutrochet spent most of the visit with Thomas Andrew Knight at Downton.

The *Notice sur mes ouvrages* is the last of a series of résumés which Dutrochet wrote at various stages of his career, some of which were published. This account is particularly useful for the detailed descriptions of Dutrochet's last few years of research, which are further illuminated by the paper 'De l'influence des agents extérieurs sur les êtres vivans et des phénomènes vitaux qui en résultent' here printed for the first time—an assured, reflective discussion, free alike of the forced analogies and the crude mechanism which were at times features of Dutrochet's work.

The letters presented here include an important early letter to Cuvier at the Académie des sciences but the majority are to Etienne Geoffroy Saint-Hilaire, Dutrochet's friend and adviser for twenty-five years or more.[2] These letters, bought some years ago by the Muséum national d'histoire naturelle, beautifully demonstrate Dutrochet's eagerness for recognition at the Académie, and show how Geoffroy was able to help him towards election as a full member in 1831, after a number of earlier vacancies had fallen to the scions of the 'great families' of the Muséum.

Dr Schiller's discussion of Dutrochet's *conceptions scientifiques* concentrates on the two linked convictions which give unity to his work: that plant and animal physiology are aspects of the same study, and that this general physiology is a part of physics. It might be argued that the first is more fundamental than the second, and that the roots of the first and the evolution of the second deserve more attention, but no one will dispute the significance of Dr Schiller's essay. In his usual concise style, he has demonstrated the strong centripetal forces in an apparently diffuse corpus of research.

The chapter on the influence of Dutrochet seems to me less satisfactory, perhaps necessarily. It contains quantities of useful information about citations of Dutrochet by later physiologists, especially those who worked in Germany, but little pattern emerges. The importance of Dutrochet's work on osmosis was hard to deny, but its significance for animal physiology was for long uncertain. The problem of 'action at a distance', under which rubric Dr Schiller discusses the influence of Dutrochet's work on the sensitive plant, seems rather artificial; some of the links with Charles Darwin's work, rather strained. Dutrochet's contribution to cell theory will not be understood properly until the whole area is better explored than at present. In this case as in others Dutrochet's 'problems' were not discrete or well defined, so that the tracing of 'influence' is more than usually unhelpful.

Because the present volume includes references to almost all the surviving material by or about Dutrochet, it may be worth adding a few further details for completeness. The Académie possesses a number of manuscript contributions by Dutrochet, in some cases significantly different from the published versions; a few manuscript notes by Dutrochet on Flourens' *Recherches* of 1824 are extant; I have a MS. *cahier, Catalog de ma bibliothèque, 1810*, which records an extensive collection of books on medicine and natural history. The *Royal Society catalogue of scientific papers* gives a very useful, though not complete, list of papers with their various reprintings. I know of only one significant paper which is listed neither there nor in the present work: an account of rhinoplasty in India as recounted to Dutrochet by his brother-in-law.[3] Dutrochet's interest in this topic *c.* 1817, and his attempts to perform grafts in experimental animals accord beautifully with his evident conviction that animal tissues were fundamentally the same in structure and properties as plant tissues.

Dutrochet's library and herbarium were dispersed some years ago so it is possible that more material will still come to light. Almost certainly any such discoveries will have but a minor effect on our understanding of Dutrochet.

Much more significant advances will result from a deeper study of the whole period, so that biology can be properly related to the society which produced it. This work is under way in a number of places and within a few years the whole aspect of early nineteenth-century life sciences will have changed. It will then be possible to 'place' such figures as Dutrochet, who have until recently appeared as exceptions to generalizations about French anatomical physiology and German materialism.

The need for new approaches is evident from the ways in which the different sections of Dr Schiller's book fail to support each other. The biographical sections are scarcely related to the discussion of the content or method of Dutrochet's work. The *responses* of Dutrochet's contemporaries in France to his work are scarcely mentioned, an omission which does *not* arise from want of data, rather from a lack of interest in responses which are not 'influences'.

In adopting this mode of presentation Dr Schiller echoes Dutrochet's self-presentation, best seen in his 'corrected papers' of 1837, a collection prefaced by a profession of Dutrochet's faith in the unity of physiology and its place as a department of physics. Dutrochet had little to say about the development of this attitude, or its position in the intellectual configuration of contemporary France; neither does Dr Schiller who, when describing interests or attitudes, tends to use such broad categories as 'materialism', whereby Dutrochet is linked with most of the late nineteenth-century German physiologists and with the chemists of the French Revolution. But such broad strokes obscure important features of Dutrochet's career and his mature thought. This is not a matter of simple omission acceptable in a short appetizer of a study; the omissions affect the treatment. For example, there is no need to resort to *ad hoc* explanations for Dutrochet's use of such terms as 'vital force', once his relationship to contemporary French life sciences is understood. There is every reason to locate in the traditions of French science much of that neglect of Dutrochet which Dr Schiller wanted to locate in modern historiography.

No one was more aware than Dr Schiller that much work remains to be done on Dutrochet and early nineteenth-century physiology, and his book provides an excellent tool for this further work. I would like, therefore, to extend this review to offer some preliminary suggestions as to how Dutrochet fits into the pattern of development of the *sciences naturelles* in France.

The question can perhaps only be answered by following his career and making comparisons at certain critical points with the careers of his contemporaries. Of these, the most useful for our purposes is Magendie, whose work on absorption overlapped that of Dutrochet on endosmosis. What follows is a tentative attempt to 'situate' Dutrochet. I am fully conscious that it needs expanding; I look forward to its correction as studies of figures more central than Dutrochet appear.

The background to Dutrochet

Dutrochet began his research career, like most of his fellow medical students, responding to *idéologie*, and in the conviction that the method of analysis was the key to the advancement of the natural sciences. His interpretation of this method is well seen in the *Théorie des habitudes et des sympathies* of 1810, where by comparing habits of the sensory and of the motor systems he reached the *principles* that while repetition of sensation decreased its effect, repetition of an action increased its facility.[4] But by the early 1820s, as physiology in France bloomed, the preoccupations of the ideologists were, like the Revolution, becoming a topic for historical study.

The Empire and Restoration saw a number of deep shifts in the basis of French culture and science. French political and intellectual culture came to be dominated by those Constitutionalists who saw themselves as conservators of the Charter; mostly liberal in politics and eclectic in philosophy, they frequently came from atypical, often Protestant, backgrounds: such figures as Guizot, Royer-Collard, Rémusat, and Victor Cousin in politics and philosophy; the Cuviers, A. P. de Candolle, and Brisseu de Mirbel in the life sciences; the links between the two groups were close.[5]

The influence of this scientific establishment, of Cuvier and his botanical associates, can hardly be overestimated. The second post-revolutionary generation of life-scientists were, often in the most direct sense, their heirs: Adolphe Brongniart, the son of Cuvier's close associate Alexandre Brongniart; J. B. A. Dumas and Victor Audouin, both brothers-in-law to Adolphe; Pierre Flourens, a protégé of Cuvier (and of Geoffroy Saint-Hilaire) and, like Dumas, a 'discovery' of A. P. de Candolle; Henri Milne-Edwards, younger brother of William Edwards and a close friend of Dumas and the younger Brongniart. These are the men who rose to prominence under the July monarchy and who dominated the life sciences in France until the liberal Empire.

The common characteristics of their science seem to represent a radiation, in both animal and plant studies, from the notion of organism which underlay the taxonomic work of Georges Cuvier and A. P. de Candolle. For our present purposes, five features of this Muséum physiology were particularly important. It emphasized actions of organs rather than properties of tissues; it resisted the tendency to reify such properties as *irritability;* it created some space for physical and especially chemical explanations of certain aspects of material change, especially *nutrition;* the model was equally applicable to any kind of animal or plant; and—the converse of the last—it involved no particular ordering of functions or organisms. Dr Schiller makes this last point when he describes Cuvier as having broken the link between physiology and taxonomy which was a feature of much late eighteenth-century science.

The extension of this kind of physiology from mammals to lower animals and plants formed a major part of the research effort of Cuvier's contemporaries and followers. The plant physiology of Mirbel and of A. P. de Candolle contains many references to Cuvier and his school, and is thereby separated from the mechanism of much eighteenth-century plant physiology. The comparative physiology of Desmoulins, of Milne-Edwards, and of Audouin is best seen as a product of Cuvier's zoology and Magendie's experimentalism. Flourens' experiments on the central nervous system of the vertebrates used ablation to separate the sites of the actions producing movement and those producing sensation. On the death of Cuvier in 1832, Flourens took a chair of Comparative Anatomy at the Muséum.[6]

If the Muséum was one of the major sources of the ideas which underlay the experimental physiology of the Restoration, its advocate was Magendie, a private teacher of physiology, textbook author and a journal editor, who from 1821 was a member of the Académie. Magendie had left the medical faculty in order to set up a private school of physiology. Because of his politics he was unable to obtain an official teaching position under the Restoration governments, but through his writings and his Académie position he did have a considerable impact on the young physiologists of the 1820s. The creed of his physiology, set out in a 'manifesto' of 1809, had much in common with the physiology of actions which we have connected with the Muséum. By 1820 he had become more and more an empiricist, but also more and more interested in the applications of the physical sciences of his fellow Academicians to physiology.

In the early 1820s, the young men of the Muséum and the associates of Magendie were part of a major upsurge of physiological research. Physiology was understood to encompass studies which we might call comparative, environmental, developmental, pathological, chemical or physical, as well as experimental studies on vertebrates. In this broad sense, it was very much the subject of the day. But by 1830, in the face of the clinical obsessions of the medical school, the failure of the Académie to create a section for physiology, and the conservatism of the Muséum under Cuvier, the momentum was considerably reduced. Some of the better-connected young men were on their way to successful careers in comparative physiology, a safe compromise between experimental physiology and zoology, but there remained little of that exciting web of physiological problems that had promised to connect the life sciences and fructify them.

In this way the specialisms institutionalized under the Directory confined the activities of the post-revolutionary generation; but they did not prevent the advertisement of a new *biologie*. Indeed as experimental physiology failed to become established, so its contributions were added to those of the German investigators and seen in the form of a unified science of life. This is a movement which has received little or no attention but it is of considerable significance for understanding Dutrochet and the response to his work.

After Napoleon had finally been removed, literary culture had exploded; an expanding press provided the Paris intelligentsia with constant discussion of English politics and German culture. In biology, as in literature, the influence of Germany was considerable, though not always acknowledged. In some cases, admiration of 'the university of Europe' fed residual or newly grown interest in the Philosophes. But even where the German biology had French roots, these had been developed in a way which horrified the scientific and medical administrators, as it excited the more adventurous young biologists. Were not German epigenetic doctrines a marked advance on Bichat's tissue classification? Was not the idea of single animal form an advance on the four types of Cuvier? Could not the 'arrest of development' of various organs explain congenital malformations? Above all, did not consideration of the animal series and the parallel development of higher animals provide an *ordering* of biology? The embryo and the polyp or infusorian pointed to the basic functions of life and its necessary structure, the increase in complexity was accessory. And did not the homogeneity, or globular structure, of these primitive forms point to the fact that living bodies were differentiated out of homogeneous organic matter, through globules, to fibres and vesicles, to tissues and organs? Lamarck, then old and dying, was rarely acknowledged as a source. Erasmus Darwin was more frequently mentioned, but the excitement came from the German present, where discoveries such as Rathke's pouches were being widely discussed while still unknown in France.

The transmission to France of the various elements of this grand science was slow and subject to useful criticism. The privileged, widely-cultured, Hippolyte Royer-Collard could have found few to agree with the whole of the ambitious scheme which he set out in his remarkable medical thesis of 1828, but he could point to aspects accepted and developed by French authorities: to the histology of Béclard and Breschet, the anatomy and embryology of Etienne Geoffroy Saint-Hilaire and Serres, the cell theory of Turpin, the botany and chemistry of F. V. Raspail. Most importantly for the development of physiology, the emphasis of such biologists as Blainville on the life of the tissues provided a necessary element in the biological yet experimental physiology developed by two pupils of Magendie, Michel Foderà

and, later, Claude Bernard.[7] Royer-Collard could also point to the general physiology of Dutrochet.

The late 1820s saw a broad and vocal opposition ranged against the government; liberals and radicals, Saint-Simonians and romantics found common cause. So too in the life sciences, a number of alternative approaches were ranged against the narrowly defined zoology of the Muséum. German transcendentalism, revolutionary materialism, even the search for God's great plan were associated in various ways, often under the title of *biologie*. Their exponents shared not only a speculative turn of mind, but a readiness to unify that which was separated in the conservative functionalism of a Cuvier; this emphasis on unity and development seems to link their *biologie* with the attacks on eclecticism in philosophy and the defence of historicism in the study of society.[8]

We should not underestimate the influence of *biologie* during the 1820s. William Edwards (associate of Magendie and author of a classic of positivistic experimental physiology), suggested in 1826 that parts of a plant could dissociate in water into separate animalcules.[9] Some of his aspirations were shared by the Muséum protégés (see, for example, Henri Milne-Edwards' paper on globule theory) but they were not pursued in a scientific environment where the careful description, classification and analysis of restricted problems had become the recognized method of attaining a position.[10] In Geneva, A. P. de Candolle seems to have enjoyed a greater freedom and he used it to introduce much Goethian metamorphosis into his botany.[11]

The outsiders of Restoration science, those who had long regretted the excessively descriptive, unadventurous science of a Cuvier, came in from the cold after 1830. When Cuvier died in 1832, the place of this great administrator was to some extent taken by Etienne Geoffroy Saint-Hilaire, sage of the political left and its romantic adherents.[12] But if the radical political movements of the July Monarchy were supported by the old and by the young, rather than by the middle-aged, the *biologie* of the July Monarchy seems to have been mostly for the old. By this time science, it seems, was losing its attraction for young men outside the Paris intelligentsia; they were attracted to literature and the fine arts and there was no inflow of young scientific talent in France in the 1830s which could compare with that of the early 1820s. The young men of the 1820s who had been interested in biology or physiology in the broad sense had succeeded in the narrower Muséum sciences or they had gone. The second post-revolutionary generation at the Muséum were perhaps more concerned to display the areas colonized than to explore new areas; they lacked the drive of the conquerors. It is hardly surprising: France was no longer at war, and its major industry was *goût*.[13] Until Claude Bernard (dramatist *manqué*) and Louis Pasteur (atypical chemist) managed to influence the government of the liberal Empire, the position of physiology in France was precarious.

How different the situation in Germany where biology developed more easily from an aspect of natural philosophy to a science within the closely linked philosophical and medical faculties of the universities, as the speculative nationalism of the Napoleonic period gave way to the liberal materialism of the rising bourgeoisie. In the visions of the philosophers such 'transition figures' as Mueller, Purkinje, and Schwann found problems if not methods. The romantic heritage remained strong enough for the brave generalizations of a Schwann or a Virchow to guide a mass of detailed researches on microstructure, development and pathology.[14]

Given this background we are in a better position to understand Dutrochet's

writings and the reaction to them in France. Before 1810, Dutrochet might best be described as a product of cross-fertilization between medicine and the natural history taught in Paris; from 1810 to 1830 he linked his previous ambitions to the concerns of those experimental naturalists who, like him, did 'private' research, often in the countryside. And between 1815 and 1830, a new generation of life scientists appeared. Dutrochet's work joined with theirs in the flood of physiology in the early 1820s; but even then it often seemed crude. By 1830, his work was praised most by those who rated originality highly and were tolerant of over-reaching and retraction. As French zoology and botany grew more conservative, Dutrochet, undeniably an original, never entered the Pantheon of Natural Scientists. He was hardly mentioned by Bernard.

Dutrochet's difficulties were in large measure those which stunted *biologie* in France. His isolation made it possible for him to pursue the experimental study of living stuff; the intellectual distance between Paris and him, increasing over the years, made his contributions less acceptable than they might have been even without such titles as *The immediate agent of vital movement revealed . . .* (1826). I do not mean to suggest that Dutrochet was a romantic; he cannot be classed with the Naturphilosophen any more than with the German materialists of 1848. He exemplifies a possible and fruitful development of the science of the Empire which did not become a highroad of Restoration science because the considerable technical difficulties of the subject were accentuated by the institutional, generational and political configurations of French science.

This is not the place to try and outline a full scientific biography of Dutrochet, but I would like to try to indicate his place in contemporary science by discussing his 'conversion' to the 'method of experiment and observation' as a means of relating his work to Muséum science and to *biologie*.

Dutrochet's 'conversion' and his approach to physiology

Looking back over his career Dutrochet recalled the impact of his reading of Spallanzani soon after his return from military service to Chareau. He had been converted to the method of experiment and observation, had given up purely rational physiology, and had begun to investigate such topics as the dessication and revivification of rotifers. He had even—though exactly when remains obscure—burned his long manuscript on rational physiology, *Recherches sur les mouvemens et les lois de la vie*, from which his published memoir on *Habitudes et sympathies* had been extracted.

This conversion was stressed by Pierre Coste in his Éloge of Dutrochet. Looking at our record of it, in the perspective of now standard historical writings on physiology in France, it is not difficult to see a switch from physiological systems to experimental physiology; from some kind of vitalism to some kind of materialism; or to see a parallel to Magendie's well known 'manifesto' of 1809. All three readings are erroneous: there is enough evidence concerning the beginnings of Dutrochet's research to reach a subtler interpretation in which those features of his physiology which formed the basis of his own individual stance in the 1820s, and those features which Dutrochet shared with the Museum physiologists and with Magendie, are teased apart. I want to suggest that Dutrochet's work, from the very beginning, was in tune with the physiology of actions. His 'conversion' was not parallel to Magendie's, at least in terms of physiological explanation; rather it compares with Magendie's subsequent passage to empiricism as a response to the apparent paucity of reliable information in physiology. Dutrochet's goals in 1809 were

similar to those of Magendie; Spallanzani gave him an insight into his own ignorance and a possible way forward.

The success of this interpretation of Dutrochet depends on our ability to demonstrate a continuity of purpose across the period of 'conversion'. The key evidence here is the letter which Dutrochet wrote to Cuvier in November 1811, accompanying a memoir on the *formation des êtres organisés*. Dutrochet wanted Cuvier's approval in order to publish a long work from which this extract was taken. This longer work was never published, nor was the extract, but the recurrence of similar titles in a number of memoirs submitted by Dutrochet to the Académie gives ground for supposing that the extract was the first record of studies which finally reached the public in the successful papers of 1821 on the *parties végétantes des animaux vértebrés*. Certainly the 1811 extract concerned the application to animals of principles of the *mouvement de végétation* first observed by Dutrochet in plants.[15]

It is very unlikely that Dutrochet had carried out many experimental or observational studies on this topic by 1811. Indeed, this omission probably accounts for the failure of the extract and its successors. The extract, and even more so the unpublished longer work, were probably 'rational' discussions of the principles of growth. When we remember that the *Habitudes et sympathies* of 1810 was taken from a long manuscript, *Recherches sur les mouvemens et les lois de la vie*, it is difficult to avoid the conclusion that the long manuscript was more or less the same in both cases. As will be indicated below, the *Habitudes et sympathies* discusses physiology in terms of actions rather than of vital properties, is not restricted to human or even to animal physiology, and does involve some discussion of changes in body structure and function in response to environmental stimuli. I can see no reason to postulate any fundamental change in Dutrochet's opinion as to the purpose of physiology and the acceptability of different kinds of explanation between this memoir and those of the 1820s. I am therefore inclined to the opinion that his views on these matters were worked out in a long 'rational' memoir begun at about the time of his graduation in Paris, and consulted, perhaps extended, during his first years of research at Chareau. In Paris, Dutrochet had sought to uncover the principle of habit; at Chareau he sought to uncover the principle of growth. This would seem to be the ambition which lay behind the studies in comparative embryology during his first decade of research. The paper on rhinoplasty adds support to this contention. That Dutrochet's thoughts on the principle of growth remained unpublished owed less to his reticence than to the fact that he was now in correspondence with the Académie rather than simply being a member of the greater medical community of Napoleonic Paris.

We can afford some latitude here and still maintain the major point—that there is a continuity of ambition running across the 'conversion', linking his reflections as a graduate in Paris with his prize-winning experimental physiology of the 1820s. The 'conversion' did not involve any sudden forsaking of general problems for limited problems capable of solution. It was the strictly controlled science of the Académie which taught that to Dutrochet, as it did more quickly and more completely to the younger men of the Restoration. In his early research on growth, as in his study of habits, Dutrochet was concerned with principles.

Nor was this a 'conversion' to materialism away from vitalism. I would not deny that some of Dutrochet's physiology is clearly of the kind sometimes known as mechanistic materialism, his analyses of plant movement soon after his discovery of osmosis providing good examples. But this was not Dutrochet's most characteristic attitude. As I have tried to show elsewhere, he was usually

very conscious that plant movements, or the plant's capacity to raise up sap, were vital actions, determined not by external causes alone, but rather subject to an internal determination which gave to the actions a quality which he called spontaneity.[16] His use of the term '*force vitale*' in the *Influence des agents extérieurs* need not be interpreted as Dr Schiller interprets it, as a concession to conservatism on an official occasion; it is quite consistent with his frequent reference to vital actions, responses depending on the particular state of the animal or plant, a state which could be characterized by its 'energy' or force, the *réaction vitale* to external forces.

Such an analysis is consistent with his stress on the *action sensoriale* in his notes on Flourens' *Recherches* of 1824,[17] as it is consistent with his stress on the active nature of sensation in the *Habitudes et sympathies* of 1810. All seem to me to be close to the attitude exemplified in the following quotation:

> point un résultat mécanique de l'action exercée sur mes organes, ou d'une simple communication de mouvements soumise à des lois necessaires, fixes, invariables, comme le choc de corps à corps; qu'il y a une action réelle et propre à l'organe sensitif qui se dirige lui-même suivant des lois particulières et donne le *ton* plutôt qu'il ne le reçoit . . .[18]

The quotation comes from Maine de Biran's *Influence de l'habitude sur la faculté de penser* (1803). It is not unlikely that Dutrochet had read this piece. In any case, he was engaged with similar questions shortly afterwards. His work shows the same kind of dissociation from the *idéologie* of Cabanis and De Stutt de Tracy; *sensibilité* is rejected as a key concept linking the physical and the moral; the physical is separated as motion; physiology is formulated in terms of the actions of organs.[19]

We are led naturally to a comparison with Magendie's 'revolt from Bichat', recently analysed by Albury. In the rejection of *sensibilité* and espousal of what we have called action physiology, Magendie and Dutrochet were at one before Dutrochet retired to Chareau. Magendie needed a manifesto in 1809 because he was setting himself up as a physiologist. Dutrochet only waved his flag when he too began to publish obviously physiological studies (see the 'Introduction' to the 1824 collection[20]). Both were warnings and proclamations of the distance between the new physiology and that of the followers of Bichat and Chaussier.

It would seem then that Dutrochet's 'conversion' at Chareau did not concern the aims of physiology or the interpretation of physiological phenomena. It did concern the state of physiology and the means for its advancement. Magendie, more after 1809 than before, realized that physiology had yet to be created; Dutrochet, on reading Spallanzani, realized, as several late eighteenth-century naturalists had realized, that huge areas of physiology, especially of the lower animals and plants, were scarcely understood at all. Magendie was to 'redo' much of the eighteenth-century physiology; Dutrochet was to explore new areas, especially that of vital actions at the level of microstructure.

In the study of the simpler forms of life Dutrochet saw an opportunity to uncover its basic secrets. In this conviction he submitted his sketch of the movement of vegetation in plants and animals, and began careful studies of rotifers and of comparative embryology. He stopped analyzing known human phenomena; he began analyzing plants, embryos and tissues. His published work between 1810 and 1820 was mostly anatomy and embryology; around 1820, as physiology in Paris received its impulsion from Magendie and Flourens, Dutrochet also began to study process and action rather than form and development.

This concern with lower animals and plants, that separates Dutrochet

from Magendie and his fellow medical physiologists, links him to such figures as Trembley, Bonnet, Spallanzani, and Lamarck, as is evidenced by numerous references within his work. In a sense, Dutrochet, by settling at Chareau, had stepped back from the growing specialization and professionalization of Paris science to the wider views of the eighteenth century. His materials and scope were those of a Bonnet, yet his attitude was crucially different. He was not concerned with the chain of beings, or even with the scale of animals; he was concerned with the similarity of structure and function in animals and plants, as it was revealed by progressive anatomical and physiological analysis. In particular he was convinced, in opposition to Lamarck, that there was no fundamental difference between animals and plants.

It is tempting to look for the influence of Erasmus Darwin here, but unnecessary, for the whole structure of Muséum physiology invited the parallel treatment of plant and animals. Cuvier's report of 1809 discussed plant physiology in much the same way as animal physiology; A. P. de Candolle's work on plant movements used the same framework as studies of animals, though cautioning against the identification of plant and animal irritability. Etienne Geoffroy Saint-Hilaire was wont to suggest that botany could only be done properly by zoologists. A disciple of Blainville excused himself for not giving a proper treatment of plants in his *Biologie*; the first volume of the *Annales des sciences naturelles* (1824) called for the insights of animal physiology to be applied in plant physiology, and vice versa.[21]

Dutrochet was remarkable, not in calling for cross-fertilization between plant and animal studies, but for actually carrying out this programme, and this during the 1820s when most of the younger life scientists were adjusting to the continuance of the existing disciplines, and Magendie was less and less ready to generalize, even about medical physiology. In 1809, Magendie had hoped that the advance of physiology would show how the different actions of the organs were produced by the vital force acting through different molecular architecture; by 1826 he would have thought this a dream, as Michael Foster was still to do fifty years later.[22]

But Dutrochet thought that his work on microstructure and osmosis had actually brought him to his goal. He was so praised in 1830 in a long review in the *Revue française*, the author of which was almost certainly Hippolyte Royer-Collard, exponent of *biologie*, nephew of the philosopher and future Professor of Hygiene in the Medical School.[23] Dutrochet had demonstrated the unity of microscopic structure, as his patron Geoffroy Saint-Hilaire had demonstrated the unity of composition of the various animal groups. He had explored a virgin field in science and discovered the intimate details of life's operations, where the study of molecular form became the study of function.

I have discussed these contrasts in method in some detail because they seem to offer the best hope of 'situating' Dutrochet. I am suggesting that he should be seen as an exponent of the new physiology, in as much as he formulated his physiology in terms of actions and used experimental intervention to elucidate these actions. He was atypical in his 'country naturalist's' interest in plants and lower animals; atypical too in his determination to extend the process of experimental analysis to the point where a single form or unit is uncovered, and seen as basic to animals and plants. Such ambition may have been common and acceptable in the first decade of the nineteenth century; by the third, as the contrast between the methods of Cuvier and those of Geoffroy Saint-Hilaire grew sharper, the search for unitary form was politically and intellectually suspect.[24]

The taxonomic range of Dutrochet's physiology, his particular interest

in lower animals and plants, linked him to the *biologistes* who continued to use the animal series, and more generally, genetic analysis, as their major conceptual tool. But Dutrochet rarely used this mode of approaching animals and plants; he used a component analysis more characteristic of Muséum physiology. He was not then a *biologiste* in a strict early nineteenth-century sense. He was a physiologist; one whose interest in basic form and basic function encouraged that suspicion of physiology which was a legacy of the Revolution. Men a generation younger might avoid that stigma by sticking close to comparative anatomy, maintaining disciplinary boundaries, and conducting limited analyses of organ functions in the various groups of higher animals; Dutrochet's early studies in comparative embryology suggest that if he had lived in Paris, he might have learned to do this kind of thing. But he didn't. His work was thereby more interesting and less acceptable to the Académie and the Muséum, where the established figures greeted it with suspicion.

The rave reviews of Dutrochet's work came from the exponents of *biologie*; in the eyes of the more cautious, he was handicapped by his habit of using slight evidence to proclaim the conquest of new principles. At a time when such intellectual ambitions as his were becoming increasingly suspect, his enthusiasms were the more irritating, his errors less forgivable. All readers of Dr Schiller's book ought to bear in mind that between 1821 and 1831 Dutrochet claimed to have demonstrated: marked formal resemblances between the overall structure of animals and plants; the nervous system of plants; the immediate agent of all vital movement; that life could be interpreted as a state of capillary electricity; and that muscle fibre analogues could be formed by passing an electric current through blood. If we add a number of errors in the quantitative study of osmosis which were avoided by researches of greater sophistication, then we shall better understand the response of the increasingly professional physicists and comparative anatomists in Paris. Such condescending reviewers as Magendie, Auguste Saint Hilaire (no relation), Desmoulins and Adolphe Brongniart were presented with an easy target.[25] Though no one could dispute that osmosis was an interesting effect, it was very easy to argue that Dutrochet's work needed redoing by researchers more attuned to the standards of the scientific specialities.

The criticism was not without basis, but it ignored the possibilities within the work which could have been taken up, and which in Germany were taken up. In concluding this essay, I would like to consider briefly Dutrochet's later work on cells and in physics, and to indicate a little of their potential.

Dutrochet and Schwann on cells

Dutrochet was 59 when he came to live in Paris. Early in the July Monarchy he had still much to give—for example his important demonstration that respiration in plants was directly comparable to respiration in animals—but in reading over his 1837 collection and comparing it with the publications of the 1820s, it is possible, I think, to sense the draining away of the high scientific ambition and its replacement with a comprehensive curiosity appropriate to a man who had, at last, arrived in the bourgeois Monarchy. He gave up his wilder claims listed above; he allowed several basic elements of microstructure, several basic forms of plant movement. He also formulated two interpretations which were to be of the greatest significance for German biology.

Some time during the 1830s, Dutrochet concluded that muscle fibres were not made up of globules and that he had overestimated the significance

of his experiment on the electrolysis of blood.[26] Similarly he concluded that mould filaments were not the product of spontaneous generation, but grew from pre-existent spores: not out of the medium but in it.[27] In his 1837 account of the growth of feathers he wrote:

> Ainsi la nutrition s'opère chez un animal à système vasculaire, comme elle s'opère chez un insecte; le liquid nutritif est de même épanché dans les interstices organiques et c'est là que les parties vivantes avec lesquelles il est en contact le prennent pour servir à leur développement.[28]

an attitude which was to find an echo in the action of 'nutritive centres' advanced by the Scottish romantic John Goodsir and used by Virchow in the establishment of modern cell theory.[29] That Dutrochet reached this attitude through the study of insects and, very likely, through his studies on moulds, is yet another indication of the power of *biologie*, of the enormous significance for the physiology of higher forms of observations on lower.

If we add this point to those made by Dr Schiller about Dutrochet's cell theory we find that he was at one time or another in possession of three key features of cell theory: unity of structure, an appreciation of the life of the parts, and a theory of cell function. That these opinions had so little impact poses a challenge. Why was Schwann's work on cell theory, with its crude model of cell formation, so much more influential? Our answer requires attention to both social and philosophical, as well as technical factors.

The microscope came into use among biologists in France during the 1820s. The technical failings of their instruments were one of the difficulties which helped discredit their programme, and decrease the market for the better instruments introduced at the end of that decade. In Germany, however, the major inflow of young men to the biological sciences followed the introduction of better microscopes. In France, these improved instruments were sometimes a useful aid to comparative anatomy, but in Germany, the common interest in developmental studies made them a basic tool.

That Dutrochet in 1837 was more or less correct by modern standards is not relevant here; his 'mistakes' during the 1820s had not been productive enough for their rectification to be seen as an advance. Schwann's 'mistakes' were more fortunate, because the notion of a direct, *gradual*, formation around a nucleus gave a fruitful research programme to a goodly number of microscopists, who developed and corrected his work.

Dutrochet on physics

If it is true that *biologie* failed in France, and the study of living things remained dominated by physics, organic chemistry, and comparative anatomy, then we might expect Dutrochet, after coming to live in Paris, to define his relationship to these existing disciplines, especially since he was a member of the Académie in a section (*économie rurale*) that did not represent his real interests.

Dutrochet had often before wandered into the investigation of problems which came under the umbrella of physics, but it was the interpretation of osmosis which involved him in a more or less continuous debate with physicists. After he came to Paris he collaborated with physicists in measuring plant temperatures and he busied himself in his last years with the investigation of surface tension phenomena. The introduction to his 1837 memoirs defined his hopes for *physiologie générale* in terms of the physical phenomena of life,[30] whereas his first major work had opened with a discussion of *sensibilité*.

This interest in physics was not unusual during the 1830s; Anthelme

Richerand remarked in the tenth edition of his elementary physiology text that the greatest change in the subject since the book's first edition in 1801 had been the development of physical and chemical explanations.[31] Magendie's interest is well known. Yet we should take care to specify the nature of these interests if we are to understand their roots and subsequent developments.

I have suggested elsewhere that Magendie's was a rather crude attempt to utilize the physics of the Académie in his own work—an attempt encouraged by the prestige of the physicists and Magendie's professional isolation. Dutrochet's attitude in the 1830s probably owes something to these same forces, in as much as he then took pains to relate to the subject of physics rather than simply straying into the territory of the inorganic. But Dutrochet's attitude was not that of Magendie. Dutrochet rarely tried to apply physics; he tried to *discover* it, in organic bodies—initially, perhaps, in the expectation of uncovering a distinct organic physics; later in the conviction that organic physics was a part of general physics. By 1837, having dropped the claim that there was one immediate agency of all vital movement, he was on the lookout for various other pieces of 'peculiar physics' which might explain the action of living bodies. Indeed, as Dr Schiller very cogently points out, his exploration of fluid movement in *Chara*, etc., were done in the hope of finding new pieces of peculiar physics so as to *enlarge physics*. His approach was then far more comprehensive than Magendie's, but his materials were obscure. I suppose his work on surface tension must have seemed very eccentric, even to those who shared his hopes for physics in physiology. The analyses of blood flow by Magendie's associate Poiseuille must have seemed much more sensible than the old man and his camphor.[32]

Yet it was Dutrochet's wider view which, in the few years before his death, began to be adopted by German physiology. Brücke, Du Bois Reymond and Helmholtz were, it appears, of a similar opinion as to the possible necessity of new physics. They seem to have reached their starting point through a Kantian analysis of the problems of physiological explanation; Dutrochet reached his end point through a lifetime's work springing from the desire to find, by analysis, the basic principles of form and of function. Correspondences of philosophical position such as these cannot be treated as matters of 'influence', but we ought to note that as Brücke and Du Bois Reymond thought out their position in the early forties, they were much concerned with Dutrochet's work on osmosis.

Brücke's medical dissertation took diffusion and osmosis as its subject. He was rather condescending towards Dutrochet's disavowals of materialism—no good Kantian would produce such philosophical crudities—but he recognized the very important advance. The passage is worth quoting in full:

> it cannot be denied that Dutrochet by his writings opened a new treasure-house of physiology which the labour of generations will scarcely exhaust, for he grasped that the parts of the living body are not properly divided into solids and liquids but more correctly into contained and containing parts; that the living body was composed of cells, not cavities continuous one with another but vesicles placed side by side; and that the diffusion of fluids through their walls, in the living body, belongs to statics and mechanics no less than do simple capillary attraction and that mechanical force by which the circulation of the blood is ruled and the individual parts of the body can be rendered flaccid and turgid.[33]

These terms of praise recall those of the 1830 article in the *Revue française;* but they were written in a context where such work could actually be taken up. In France, Dutrochet's work had attracted interest but had failed to take root in an intellectual *milieu* which never shook off the shape which the

Directory and Empire imposed on the post-revolution generations: the Académie dominated by the physical sciences; a Medical School almost entirely clinical; the Muséum, for the administrators and the presenters of Natural History, that mainstay of polite culture in nineteenth-century France. Dutrochet's general physiology, like *biologie*, was burdened with German overtones, and undertones of the *philosophes*. They scarcely stood a chance until France seemed threatened more by the progress of German science and industry than by the memories of the Revolution.

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NOTES

[1] A. R. Rich, 'The place of R. J. H. Dutrochet in the development of the cell theory', *Bulletin of the Johns Hopkins Hospital*, 1926, *39*, 330–65.

[2] The relationship between Dutrochet and Geoffroy Saint-Hilaire is worth further study. Dutrochet's writings include limited support for the philosophical anatomy of Geoffroy Saint-Hilaire, who in turn was a notable opponent of the vitalism of the Bichat school. The date of their friendship is therefore relevant. Dr Schiller suggests (p. 14) that we must abandon the obvious interpretation of most of the evidence—that the disagreement at the Académie which led to their friendship occurred in 1818—because it preceded Geoffroy Saint-Hilaire's support for Dutrochet's candidature at the Académie, and Dutrochet's name was on a list of candidates submitted by Lacépède on 17 November 1817. True, but on this occasion Dutrochet was placed well down the list of candidates. On 22 February 1819 a new list was presented for a vacancy in zoology, and Dutrochet was elected. It seems to me quite reasonable to suppose that it was this later submission to which Dutrochet referred in his autobiographical sketch (p. 82), in which case the evidence for 1818 or early 1819 is strengthened.

Another minor, but significant, question of date arises over the letter to Cuvier about endosmosis printed on pp. 201–2 and there dated '24 Octobre 1826'. This date would place it *before* Dutrochet's announcement of endosmosis to the Académie, and such a sequence seems very unlikely. According to my notes, the month on the manuscript appears to be '9 bre' i.e. Novembre. The contents of the letter were read to the Académie on the 27 November 1826.

[3] H. Dutrochet, 'Examples of the reunion of parts totally separated from the rest of the body', *Plastic and reconstructive surgery*, 1969, *44*, 288–9, translated and reprinted from *Gazette de santé*, No 9, 21 March 1817.

[4] This difference is the effect of repetition was one which Maine de Biran had stressed in his attempt to separate passive and active faculties. See his *Influence de l'habitude sur la faculté de penser*, Paris, 1803.

[5] Useful bibliographies for the culture of early nineteenth century France are given in, G. de Bertier de Sauvigny, *La restauration*, 2nd edn., Paris, 1955, and J. Droz, *Europe between revolutions, 1815–1848*, London, 1967. Robert Fox, 'Scientific enterprise and the patronage of research in France, 1800–1870', *Minerva*, 1973, *11*, 442–73, contains much on scientific culture. Also see Henri Gouhier, *La jeunesse d'Auguste Comte et la formation du positivisme*, 3 vols., Paris, 1933–41; G. Boas, *French philosophies of the romantic period*, New York, 1964; Ch.-M. Des Granges, *La presse littéraire sous la restauration, 1815–1830*, Paris, 1907; D. Bagge, *Les idées politiques en France sous la restauration*, Paris, 1952; E. Bréhier, *Histoire de la philosophie*, ii. fasc. 3: *le XIX^e siècle*, Paris, 1932; the considerable literature on Stendhal constitutes a useful source for 'background'.

[6] The lives and works of the scientists and naturalists grouped around the Muséum can be explored in a number of biographical studies including William Coleman, *Georges Cuvier, zoologist*, Cambridge, Mass., 1964; P. Nicard, *Étude sur la vie et les travaux de M. Ducrotay de Blainville*, Paris, 1890; Louis de Launey, *Les Brongniart*, Paris, 1940; G. Legée, 'Cuvier (1769–1832), Geoffroy Saint-Hilaire (1772–1844) et Flourens (1794–1867)'. *Histoire et biologie*, 1969, fasc. 2, pp. 10–34; Théophile Cahn, *La vie et l'oeuvre de Etienne Geoffroy Saint-Hilaire*, Paris, 1962; J. M. D. Olmsted, *Francois Magendie*, New York, 1944; J. Schiller, *Claude Bernard et les problèmes scientifiques de son temps*, Paris, 1967.

The conceptual structures of early nineteenth-century biology are under investigation in several places, partly as a result of the work of Georges Canguilhem and Michel Foucault. See for example, *Revue d'histoire des sciences*, 1970, *23*, and 1972, *25*, for articles and discussions on Cuvier and Geoffroy Saint-Hilaire respectively. William R. Albury's 'Physiological explanation in Magendie's manifesto of 1809', *Bulletin of the history of medicine*, 1974, *48*, 90–9, provides a foretaste of scholarship to come from Albury and also from Michael Gross of Princeton. Karl Figlio's recent paper 'The metaphor of organisation', *History of science*, 1976, *14*, 17–53, points a way towards an integration of the content of science with its social context.

Useful, more or less contemporary, accounts of life sciences include, George Cuvier, *Rapport historique sur les progrès des sciences naturelles depuis 1789, et sur leur état actuel*, Paris, 1810; [Hippolyte Royer-Collard], 'De l'état actuel de la physiologie', *Revue française*, 1828, *3*, 28–66; [A. P. de Candolle], 'De l'état actuel de la botanique générale', *Revue française*, 1829, *8*, 33–56; Henri Milne Edwards, *Rapport sur les progres des sciences zoologiques*, Paris, 1867.

[7] On *biologie* in France see Georges Canguilhem's essays on Bichat, Comte, cells and physiology in *Études d'histoire et de philosophie des sciences*, Paris, 1970, and *La connaissance de la vie*, Paris, 1969; J. V. Pickstone, 'Globules and coagula: concepts of tissue formation in the early nineteenth century', *Journal of the history of medicine*, 1973, *28*, 336–56, and 'Vital actions and organic physics: Henri Dutrochet and French physiology during the 1820s', *Bulletin of the history of medicine*, 1976, *50*, 191–212; Hippolyte Royer-Collard, *Essai d'un système général de zoonomie*, (thèse doctorale), Paris, 1828, and 'Considérations sur le développement du foetus humain', *Revue française*, 1828, *5*, 77–119.

[8] See, for example, Pierre Leroux, *Réfutation de l'éclectisme*, Paris, 1839. Leroux attacked the notion that philosophy was a specialism for a class of *érudits*. It was, he maintained, *la science de la vie*, which must follow the current of humanity. See also note 24 below.

[9] W. F. Edwards, 'La liaison du règne végétal et du règne animal', *Bulletin des sciences naturelles*, 1826, *8*, 188–9; also see *Le globe*, 11 Mai 1826, and 18 Mai 1826.

[10] See my 'Globules and coagula', op. cit. (7).

[11] See M. Guédès, 'La théorie de la metamorphose en morphologie végétale; A. P. de Candolle et P. J. F. Turpin', *Revue d'histoire des sciences*, 1972, *25*, 253–69.

[12] See, for example, the rich article by Frank Bourdier, 'Le prophète Geoffroy Saint-Hilaire, George Sand et les Saint-Simoniens', *Histoire et nature*, 1973, *1*, 47–66.

[13] See Fox, op. cit. (5).

[14] See, for example, Walter Pagel, 'The speculative basis of modern pathology', *Bulletin of the history of medicine*, 1945, *18*; Russel Maulitz, 'Schwann's way: cells and crystals', *Journal of the history of medicine*, 1971, *26*, 422–37; Brigitte Hoppe, 'Discussions histologiques et physico-chimiques au commencement de la cytologie au xixe siècle', *Actes, xiie congrès internationale d'histoire des sciences*, Paris, 1971, viii. 73–83.

[15] P. Coste, *Éloge de du Trochet*, Paris, 1866. The letters and autobiography are reproduced in the volume under review.

[16] These points are elaborated in my 'Vital actions and organic physics', op. cit. (7).

[17] The notes, discussed briefly in my 'The origins of general physiology in France, with special emphasis on the work of R. J. H. Dutrochet', University of London Ph.D. thesis, 1973, provide further evidence for the close links between Dutrochet's work on the sensitive plant and contemporary work on animal nervous systems.

[18] Maine de Biran, *Oeuvres* (ed. by Pierre Tisserand), Paris, 1920–, ii. 20.

[19] For detailed discussion of Maine de Biran's significance as the point of departure of much of nineteenth-century French thought from the tradition of the Idéologues, see his *Oeuvres*, op. cit. (18); also Bréhier, op. cit. (5); and the recent study by Réné Lacroze, *Maine de Biran*, Paris, 1970.

[20] R. J. H. Dutrochet, *Recherches anatomiques et physiologiques sur la structure intime des animaux et des végétaux, et sur leur motilité*, Paris, 1824.

[21] Cuvier, op. cit. (6); A. P. de Candolle, 'Expériences relatives à l'influence de la lumière sur quelques végétaux', in *Mémoires presentés à l'Institut des sciences, lettres et arts*, Paris, 1806, i. 329–50, and *Physiologie végétale*, 1832, i. 29–34; for Etienne Geoffroy Saint-Hilaire, see the interesting letter to him from Dutrochet (7 December 1821), on pp. 188–9 of the volume under review; Charles Dhéré, *De la nutrition, après les idées de Ducrotay de Blainville*, Paris, 1826, p. 12.

[22] F. Magendie, 'Quelques idées générales sur les phénomènes particuliers aux êtres vivants', *Bulletin des sciences médicales de la Société Médicale d'Émulation*, Paris, 1809, *4*, 145–70. Michael Foster, 'Physiology', in *Encyclopaedia Britannica*, 9th edn., Chicago, 1885, xix. 22.

[23] For biographical notes on Hippolyte-Louis Royer-Collard (1802–1850), see the entry in the *Dictionnaire encyclopédique des sciences médicales*. The *Dictionnaire des ouvrages anonymes* by Ant.-Alex. Barbier, 3rd edn., Paris, 1879, iv. gives him as the author of the physiology articles in volumes *1–15* of the *Revue française*. The content and sympathies of those articles make it likely that he was also the author of the article on Dutrochet which appeared in volume *16*. In this connexion it is of interest that Dutrochet was recommended for the Legion of Honour in a letter written to Cuvier from the Ministry of Commerce on 16 January 1831 and signed Royer-Collard. Presumably this was Pierre-Paul, the statesman, but Hippolyte-Louis had, in 1830, been appointed Director of the Section of Letters and Sciences of the Ministry of the Interior. In any case, it would seem likely that both uncle and nephew were involved in the proposal.

[24] There is a considerable literature on the Cuvier-Geoffroy dispute; see the works cited in (6) above.

Some suggestion of the close relationship between the life sciences and the study of society—the common problems of method, and thus the political sensitivity of the life sciences—can be gleaned by reading the discussions of historical method in Guizot's *History of civilisation*, (ed. by

W. Hazlitt), London, 1856, especially i. 291, where the method of science, (analysis from without of social states) is contrasted with the primitive method of synthesis, following the internal and moral chronology, the order in which facts succeed one another and reciprocally create each other.

[25] See the reviews of Dutrochet's *Recherches* (1824) by Desmoulins, *Journal complémentaire*, 1824, *18*, 263–73; 1824, *19*, 46–54; and by Auguste Sainte-Hilaire, *Bulletin des sciences naturelles*, 1824, *17*, 191–4; for Magendie's opinion see his *Précis élémentaire de physiologie*, 3rd edn., Paris, 1836, i. 4; for Brongniart's, which probably also represented that of his brothers-in-law, see his 'Notice sur Henri Dutrochet', *Mémoires d'agriculture, d'économie rurale et domestique*, Paris, 1853, pp. 421–42.

[26] Discussed in my 'Globules and coagula', op. cit. (7).

[27] See pp. 94, 142, of the volume under review; also R. J. H. Dutrochet, *Mémoires pour servir à l'histoire anatomique et physiologiques des végétaux et des animaux*, 2 vols., Paris, 1837, ii. 198.

[28] Ibid., ii. 383.

[29] R. Virchow, *Cellular pathology* (reprinted with introductory essay by L. J. Rather), New York, 1971.

[30] Dutrochet, op. cit. (27), i.

[31] Anthelme Richerand, *Nouveaux élémens de physiologie*, 10th edn., 3 vols., Paris, 1833–5, i. 'Préface'.

[32] See my 'Vital actions and organic physics', op. cit. (7).

[33] Ernst Brücke, *De diffusione humorum per septa mortua et viva*, Berlin, 1842, p. 43; for a recent review of the background of this thesis see David Galaty, 'The philosophical basis of mid-nineteenth century German reductionism', *Journal of the history of medicine*, 1974, *29*, 295–316.