CHAPTER 5

The Rejection of Mechanism

1. Introduction: Mechanism and Its Opponents

Not all scientific investigators see the development of mechanistic explanations as a critical constraint on their models. Even when there is general agreement in identifying a particular higher-level system as a locus of control for some phenomenon, those who do not accept a mechanistic paradigm may reject any attempt at further decomposition and localization of the sort described in Chapter 4. Decomposition and localization are seen as yielding only spurious explanations. As we will illustrate with several cases in this chapter, decomposition has been rejected by some for failing to provide an adequate explanation of life or of mind. Critics of decomposition and localization frequently attack the view in its simple form, what we have called direct localization. Whether or not these objections apply to more complex forms of localization such as those we describe in Part III, they do sometimes catch the shortcomings of direct localization. To mechanists it appears that those who repudiate localization are thereby repudiating scientific explanation, just as to opponents it appears clear that direct localization is a bogus explanation.

These debates are often driven by a variety of disparate concerns, from the religious and philosophical to the empirical and experimental.[1] Thus, in the evolving dispute between epigenesists and preformationists, Harvey and Descartes embraced the view that development must involve the emergence of organized forms from undifferentiated forms under the influence of the environment as a virtual corollary of their mechanistic commitments. In contrast, by the late seventeenth century, Malebranche, Malphigi, and Swammerdam began to look to development as the unfolding of preformed structures. Yet this shift to preformationism did not signal an abandonment of mechanism so much as a recognition of its limitations. The Cartesian vision of a mechanistic physiology was unable to explain the regular development of organisms, or the differentiation of organs and their reproduction. Others, such as Maupertuis, moved away from mechanism in a different fashion, allowing even the smallest elements in living organisms a capacity for intelligence and memory. Differences in the conceptions of acceptable explanations and acceptable methods loomed large in these debates, as in the other cases we will discuss.

Those who reject the search for mechanistic explanations have flown a variety of banners. We shall be concerned with only a few. *Dualism* (with regard to psychological activities) and *vitalism* (with regard to physiological activities) are two labels commonly adopted by opponents to mechanism—or applied to them by their mechanistic adversaries. The positive doctrines espoused by mechanists and antimechanists vary widely, as do the corresponding strengths and weaknesses. What they hold in common is hardly more than the view that something other than mechanistic processes are essential in understanding life and mind. Such a view can be held for quite compelling reasons and does not simply represent an irrational resistance to mechanism.

In the case of psychological theory, the most prominent opponents of mechanism are Cartesian dualists, though *epiphenomenalists* such as T. H. Huxley (1863, 1874), and *emergent materialists* such as R. W. Sperry (1969), count no less as opponents of mechanistic psychology. All, however, are mechanists in pursuing physiology, and all deny that the resources of physiology will suffice in assaulting the citadel of the mind. It is, in large part, their attempt to pursue mechanism that leads them to vitalism or dualism. Psychological dualists, such as Karl Popper and John Eccles (1977), are clear on what additional element they think necessary to deal with the phenomena of consciousness. With Descartes they maintain that cognition or consciousness depends on a different kind of substance, that the material substrate of even the most complex machines is inadequate to yield cognitive or conscious processes; moreover, the differences in substance foreshadow differences in capacities. In its most famous formulation, in the seventeenth century, this substance is not an alternative kind of matter out of which one might design engines of thought, but a substance whose primary attribute *is* thought. Dualists do not propose to explain consciousness or thought, but rather take them as primitive. Epiphenomenalists and emergent materialists eschew novel substances, but embrace the novel character of the conscious processes that pushes them beyond the reach of ordinary mechanistic explanations.

The vitalist position in physiology is even more varied. Some vitalists do hold out for the existence of a vital force or power and attribute to it the capacity to make ordinary matter into a living substance. But even here there is variety: Some vitalists treat this vital power as a unique entity in nature, while others, such as Justus Liebig, see it as quite closely connected with other, such as gravitational and mechanical, forces. Moreover, a significant number of vitalists are better thought of as emergent materialists or as *holists*. They do not posit a special substance or even acknowledge special powers, but argue that the distinctive properties of living organisms only appear once ordinary matter is organized in a particular way. They thus affirm a version of holism according to which the properties of life are treated as properties of the whole that cannot be refined into the properties of the parts, even when relations are taken into account. Some researchers, such as those physiologists Lenoir (1982) refers to as "teleomechanists," claim only that the special properties of life result from the way matter is organized in living things. This organization accounts for the teleological character of living systems; moreover, it could not be the result of simple underlying mechanical causes. For other scientists, such as those adopting some equipotentiality of particular systems, the holism is even more clearly an opposition to mechanism. The claim is made that because there is no differentiation of function between parts of the system, one cannot view the whole system as machine assembled from such parts.

In this chapter we shall examine four episodes in the life sciences involving researchers who did repudiate decomposition and localization, as well as the mechanistic assumptions on which these strategies rested, but did not thereby reject the importance of scientific investigation. These researchers included Pierre Flourens, Xavier Bichat, Theodor Schwann, and Louis Pasteur. The views expressed, at least in retrospect, were wrong, despite the eminence of the scientists involved. Our interest, however, is not in whether the views were erroneous, and even less in how the views were subsequently disproved, but in the factors that led them to repudiate mechanism and the effect this rejection had on the development of their disciplines. The terms in which their opposition was cast had implications both for the development of their own programs of research and for the development of those of their opponents. As always, the positive program of research is partly characterized and defined in contrast to the most significant alternative.

2. Flourens and the Integrity of the Nervous System

We noted in the last chapter that there were a variety of controversies, both in academic circles and in more popular forums, concerning phrenology. The first major theoretical challenge to Gall came from Pierre Flourens, with the support of Georges Cuvier. Flourens had flirted with phrenology early in his career, and even attended a course with Gall in 1815, but as he came increasingly under the patronage of Cuvier[2] he came to oppose the materialism he had once found attractive and became the premier defender of Cartesian dualism. Flourens's experimental results were heralded at the time as decisive refutations of specificity of brain action and were accepted doctrine for nearly forty years. Flourens opposed Gall for his materialism first and foremost, and secondly for his organology.[3]

Flourens isolated two propositions, which he took to be central to Gall's views: first, that the understanding resides exclusively in the brain; and second, that "each particular faculty of the understanding is provided in the brain with an organ proper to itself" (1846, p. 18). There is, he said, "certainly nothing new in the first one, and perhaps nothing true in the second one" (ibid.). The second proposition became the immediate target of Flourens's famous and elegant experiments supporting the equipotentiality of the nervous system. In these experiments Flourens extirpated parts of the cerebral lobe of pigeons. He found that the animal's visual abilities systematically weakened with successive excisions until they were totally lost. At the moment that sight was lost, Flourens found that other sensory faculties were lost as well. The conclusion was expressed in the principle of the "Unity of the Nervous System":

> Independently of the *proper action* of each part, each part has a *common action* with all the others, as have all the others with it. (1824, p. 241)

This is a straightforward denial of decomposability and an insistence on the integration of neural functioning. The result was a defense of.an equipotentiality of action within the cerebral hemispheres:

> All sensations, all perceptions, and all volition occupy concurrently the same seat in these organs. The faculty of sensation, perception, and volition is then essentially one faculty. (Ibid.)

Flourens did not categorically reject faculties of the mind; however, though granting that the faculties were nominally distinct, he said that if a commitment to the independence of the faculties also meant that "each faculty is a real understanding"—as it manifestly did, in the case of Gall and Spurzheim—then it would destroy the "unity of the understanding." This view lay at the heart of Flourens's metaphysics and was supposedly vindicated by his experimental results:

> It has been shown by my late experiments, that we may cut away, either in front, or behind, or above, or on one side, a very considerable slice of the hemisphere of the brain, without destroying the intelligence. Hence it appears, that a quite restricted portion of the hemispheres may suffice for the purposes of intellection in an animal.
>
> On the other hand, in proportion as these reductions by slicing away the hemispheres are continued the intelligence becomes enfeebled, and grows gradually less; and certain limits being passed, is wholly extinguished. Hence it appears, that the cerebral hemispheres concur, by their whole mass, in the full and entire exercise of the intelligence. (1846, p. 34)

According to this view, the brain became merely the organ on which the immaterial mind worked its will, and the various mental activities became but aspects of a single and unitary mind. The mind, Flourens says, citing Descartes, is indivisible and lacking parts.

Flourens did not wholly deny differentiation in brain structures. Indeed, he is often regarded as the first to demonstrate experimentally that distinctive structures have their own distinctive functions. Flourens noted that destruction of specific regions of the brain leads in turn to specific deficits: removal of the cerebellum leads to loss of locomotive action; removal of other regions affects sight only; other regions were specialized for respiration; and removal of the hemispheres leads to a loss of what Flourens called "understanding." Moreover, these losses left the other capacities intact. Flourens thus concluded that there are *exactly* four "particular organs":

1. The cerebellum, regulating locomotion.
2. The tubercula quadrigemina, regulating sight.
3. The medulla oblongata, regulating respiration.
4. The "brain proper."

The latter was "the seat, and the exclusive seat of intelligence" (ibid., p. 31). He concluded:

> Everything concurs then to prove, that the encephalon, in mass, is a multiple organ with multiple functions, consisting of different parts, of which some are destined to subserve the locomotive motions, others the motions of respiration, &c., while *one single one, the brain proper, is designed for the purposes of the intellection*.
>
> This being conceded, it is evident that the entire brain cannot be divided, as the phrenologists divide it, into a number of small organs, each of which is the seat of a distinct intellectual faculty; for the entire brain does not serve the purposes of what is called the intelligence. (Ibid., pp. 32–33)

The unity of the mind came to be reflected in the unified and coordinated activity of the cerebral hemispheres, or in their "common action." The higher cognitive functions assigned to the understanding could not be further decomposed. Thus, having assigned different functions to the four regions of the brain, decomposition and localization could do no more; the brain proper could not be divided, so its higher cognitive functions could not be localized in its components.

Accordingly, Flourens regarded Gall's views as issuing from a false abstraction. Flourens observed that Gall thought "each faculty of the soul must have its proper organ; in one word, he looks upon the outer man, and constructs the inner man after the image of the outer man." Gall thus began by observing a diversity in the expressions of intelligence and projected a diverse array of mechanisms. Gall's whole philosophy, according to Flourens, "consists wholly in the substitution of multiplicity for unity"

(1846, p. 47); Gall substituted "a multitude of little understandings or faculties, distinct and isolate" for "General Intelligence," or what was properly the "Understanding" and was correctly understood as a "unit faculty" with a multiplicity of applications. Flourens then concluded "that an explanation, which is words merely, adapts itself to any and to every thing" (ibid., p. 38):

> You observe such or such a penchant in an animal, such or such a taste or talent in a man; presto a particular faculty is produced for each one of these peculiarities, and you suppose the whole matter to be settled. You deceive yourself; your faculty is only a word—it is the name of the fact—and all the difficulty remains just where it was before. (Ibid., p.39)

The challenge is a familiar one to the contemporary ear, and was hardly new with Flourens. Opponents of faculty psychology, from Galen and Descartes to Herbart and Moliére, traditionally objected that explanations in terms of faculties were spurious precisely because they simultaneously classified behavior and explained it *in the same terms*; that is, behavior was classified, and the classification was projected to a lower level, which then supposedly explained the behavior. Flourens likewise regarded the "explanations" of the phrenologists as nothing more than redescriptions of the very phenomena needing to be explained. Such explanations were considered either vacuous or speculative. In either case, they were to be rejected.

The response from supporters of faculty psychology was equally familiar. Gall rejoined that the "intellectual faculty and all its subdivisions, such as perception, recollection, memory, judgment, and imagination, are not fundamental faculties, but merely their general attributes" (Gall and Spurzheim 1810–1819, 4:327). His mechanistic commitments made it impossible to understand how "there should be any peculiar organ of the reason" (ibid., 4:341). There was no hope of a mechanistic explanation of reason understood as a general, all-purpose capacity. The only hope for mechanism was a decomposition of the functions of the mind. Flourens also saw decomposition and mechanism as integral to one another; accordingly, he denied that either was viable. What supposedly showed the reality of the separate faculties, according to Gall, was the possibility of their being independently manifested. If they could be separately manifested they must be distinct, even if they were generally expressed together. The fact that they could be simultaneously affected by extirpation or stimulation, as Flourens had shown, could do nothing to show that they were one. What was required, rather, was a demonstration that they could *not* be differentially extirpated.

In addition to methodological criticisms of Flourens, the phrenologists raised technical objections to the use of artificial lesion data:

> Several natural philosophers have endeavored by mutilations, viz. by cutting away various parts of the brain, to discover their functions. These means have been pursued without fruit and will remain useless. They are too violent, and several faculties might be retained without being manifested; at all events they cannot teach more than may be ascertained in the healthy state. . . .
>
> The best method of determining the nature of the cerebral functions, is that employed by Phrenologists: it is to observe the size of the cerebral parts in relation to particular mental manifestation, and it is the third principle of phrenology, that in the same individual, larger organs show greater, and smaller organs less energy. (Spurzheim 1832, p. 12)

Gall observed, in an extended discussion occupying much of volume 6 of *On the Functions of the Brain and Each of Its Parts*, that ablation could not provide a useful experimental method because it required researchers to know beforehand the extent of an organ in order to get significant results. One would need more precise methods than were technically feasible and more adequate controls.[4]

The conflict remained at an impasse. Phrenological evidence was insufficient to show that decomposition and localization did any serious explanatory work. The antimechanistic stance of Flourens and his supporters denied the relevance of the data, treating it as a simple artifact. For some time Flourens's position was the dominant establishment view. It would not be until Broca (see Chapter 6) produced his own evidence concerning localization of function that this orthodox conception of brain action would be successfully challenged.

3. The Vitalist Opposition to Mechanistic Physiology

As we noted at the beginning of the chapter, vitalism embraces a variety of positions, ranging from the staunchly antimechanistic to those generally congenial to mechanism, but maintained that it was necessary to expand the range of forces in order to explain processes found in living beings. We will restrict our focus in this section to two positions to which the term *vitalism* has been attached: Bichat's rejection of mechanistic accounts of respiration, and Schwann and Pasteur's rejection of chemical accounts of fermentation. These views, like those of Flourens', are of interest in large part because they represent alternatives to mechanistic programs examined elsewhere in this book. They are also interesting because these researchers understood the mechanistic program—indeed, in some domains were advocates of it—but saw compelling reasons to insist on limits to that program.

Bichat's Opposition to Lavoisier's New Chemical Physiology

Lavoisier, in addition to contributing to a revolution in chemistry, applied the new chemical theories to physiology, proposing that biological respiration was comparable to ordinary combustion, only involving a fire without a flame. Numerous researchers, many with roots in chemistry, pursued the path proposed by Lavoisier and attempted to explain a number of physiological functions in purely chemical terms. (In Chapter 3 we briefly discussed attempts to develop evidence for various possible sites for animal respiration.) In addition to Lavoisier, Bertholet and Gay Lussac set out to analyze the chemical constitution of plants and animals, generally in terms of their elementary composition, and to explain the processes by which food substances became transformed into the tissue of an organism (see Holmes 1974). To other investigators, however, the attempt to explain the processes of life in chemical terms was misguided. They were convinced that processes occurring within living organisms were different in kind from those occurring outside living organisms.

One spokesperson for this opposition was Xavier Bichat, a French anatomist and surgeon toward the end of the eighteenth century (see Albury 1977). In some respects Bichat seems to be a paradigmatic mechanist, explaining physiological functions by tracing them back to the properties of components localized within the system. He asked what physical structures in the body could account for the variety of phenomena exhibited by the various organs of plants and animals. He differentiated several kinds of tissues in the body, each with its own primitive properties. Bichat here carried out a program of direct localization: Having differentiated twenty one types of tissue, he attributed a different and distinctive set of traits to each. He explained that these traits appeared in the organs themselves because they were composed from tissues that already possessed the traits. Various organs differed in their properties because they were composed of different tissues with different intrinsic qualities. The properties of the organs were thus "explained" by showing that the organs contained tissues that themselves exhibited the properties in question. Bichat compared this explanatory strategy to decomposing chemical compounds into chemical elements:

> Now these separate machines [the organs] are themselves formed by many textures [tissues] of a very different nature, and which really compose the elements of these organs. Chemistry has its simple bodies, which form, by the combinations of which they are susceptible, the compound bodies; such are caloric, light, hydrogen, oxygen, carbon, azote, phosphorus, &c. In the same way anatomy has its simple textures, which, by their combinations four with four, six with six, eight with eight, &c. make the organs. (Bichat, 1801/1822, cited in Hall 1951, p. 69)

Bichat thus seemed to be well embarked on a typical mechanist program, pursuing a strategy of direct localization. The next step in the mechanist program would have been to transfer the inquiry to a lower level, decompose the functions assigned to the tissues, and localize these functions within components of the tissues themselves. One would thus explain how the tissues had the properties they did in terms of their composition. The following move would have been to frame this model in terms of the kind of chemical theory being developed by Lavoisier and other chemists. Yet, at this juncture, Bichat rejected the mechanistic program and advanced two arguments against any attempt to account for the properties of tissues in more basic terms. One was based on a fundamental indeterminacy in vital processes; the other depended on their self-regulation.

In characterizing the properties of tissues, Bichat used two concepts central in the investigations of Albrecht von Haller: *irritability* (or *contractility*) and *sensibility*. Irritability, by Haller's definition, was the inherent ability of muscles to contract. Sensibility was an attribute of the nerves that allowed response to external stimuli. Haller treated these as basic features of living organisms and argued that although these activities could be measured, they could not be explained in more fundamental terms. Moreover, irritability was an attribute of animal muscle-tissue that was independent of the soul. Haller remarked, in an early passage, that the heart of dead animals continued beating, from some "unknown cause . . . hidden in the fabric of the heart itself" (1739, 2:129). The ultimate source of these attributes was God, who "gave to bodies an attractive force and other forces, which once received are exercised" (1756–1760, 1:xii).

According to Bichat the problem was even more serious than Haller had envisioned. Bichat argued that one could not even obtain accurate measurements of these basic properties because the sensibility and irritability exhibited by the tissues continually varied and so could not be determinately measured. This indeterminacy, for Bichat, marked a critical difference between the organic and inorganic worlds. While nonliving entities adhered to strict deterministic laws, living entities did not. Living things were at every instant undergoing some change in degree and kind:

> They are scarcely ever the same. . . . In their phenomenon nothing can be foreseen, foretold nor calculated; we judge only of them by their analogies, and these are in the vast proportion of instances extremely uncertain. . . . To apply the science of natural philosophy to physiology would be to explain the phenomena of living bodies by the laws of inert body. Here . . . is a false principle. (Bichat 1801/1822, p. xx; cited in Goodfield 1960, p. 69)

On account of this indeterminacy, Bichat objected to applying the principle of deterministic science to living systems:

> One calculates the return of a comet, the speed of a projectile; but to calculate with Borelli the strength of a muscle, with Keill the speed of blood, with Lavoisier the quantity of air entering the lungs, is to build on shifting sand an edifice solid itself, but which soon falls for lack of an assured base. This instability of vital forces marks all vital phenomena with an irregularity which distinguishes them from physical phenomena [which are] remarkable for their uniformity. It is easy to see that the science of organized bodies should be treated in a matter quite different from those which have unorganized bodies for their object. (1805, p. 81; cited in Goodfield 1960, p. 68)

While Bichat's first argument focused on indeterminacy, his second argument contended that living things evaded and opposed the laws of nature. This provided them with self-regulatory capacities, which, as Bichat saw, were necessary if living organisms were to survive in the face of natural forces that would tend to destroy them. For example, the temperature of ordinary objects gradually comes into equilibrium with the temperature in the environment. However, warm-blooded animals will oppose this tendency and maintain themselves at a set temperature regardless of their environment. Thus, Bichat came to conceive of the vital properties of living organisms as standing in opposition to those of inorganic nature. The properties of inorganic nature tended to tear down living organisms, and these organisms' vital properties were required to oppose these inorganic properties. He consequently characterized life as "the sum of all those forces which resist death" (1805, p. 1). Since living organisms opposed the natural tendency of nonliving nature, they could not be explained in terms of the processes of nonliving nature alone.

Although Bichat thought the indeterminacy and self-regulation of living organisms ruled out any mechanistic attempt to explain the properties of tissues in more basic terms, he did not therefore oppose empirical and experimental inquiry into the operation of living organisms. His tissue theory was the result of just such an inquiry, and in it he developed experimental techniques to identify the particular forms of sensitivity and irritability manifested by different tissues. What Bichat's principles did, though, was limit the inquiry into these tissues to a descriptive mode: empirical investigation should only *describe* what sorts of sensibility and irritability different tissues exhibited and not try to explain *why* the tissues manifested these properties by appealing to other properties found in nonliving matter.[5] Thus, Bichat, like Haller, treated these forces as basic forces in nature: "To create the universe God endowed matter with gravity, elasticity, affinity, etc., and furthermore one portion received as its share sensibility and contractility" (ibid.).

Bichat rejected the mechanistic program at the point that requires an explanation of the properties of tissues in terms of properties of inanimate matter, because these tissues seemed to possess features that were not exhibited by, and in fact seemed opposed to those of, inanimate matter. It appeared to him quite inconceivable that any purely physical machine could possess these properties. Indeed, if direct localization is the model of how to develop a mechanistic explanation, his objections appear well taken; for direct localization assumes that we will find some lower-level component with the properties of tissues, and it is clear that no purely chemical constituent could have such properties. Bichat was quite aware of the attempts by his contemporaries to carry out mechanistic investigations, but he contended that their efforts would necessarily be futile and that sensibility and irritability must be accepted as basic properties. One could characterize them and differentiate tissues in terms of their possession of these properties, but one could not explain these properties mechanistically. Proper scientific explanation was limited to this descriptive mode, just as Newtonian theory was restricted to describing the phenomena. And just as Newton had to accept gravity as a fundamental force, irreducible to mechanical push and pull, the physiologist had to accept sensibility and irritability as fundamental forces, irreducible to mechanistic chemistry. Vital forces could not be observed and were therefore not proper objects for mechanistic explanation.

François Magendie, one of the most prominent physiologists in the generation after Bichat, as well as others such as Johannes Müller, repudiated Bichat's case for indeterminism and brought a greater quantitative focus to physiological research. Yet Magendie remained a vitalist and rejected attempts to provide a mechanistic explanation of all living phenomena. While Bichat had drawn a parallel between vital forces and Newtonian forces (like gravity), all the while seeking to develop a physiological description that accepted indeterminacy, Magendie insisted that without precise quantitative laws to describe these forces, no science was possible. Magendie (1809) also rejected the idea of developing a taxonomy of tissue types in favor of examining organs and their interactions within animals. As he expressed it, his aim was "to abolish the two vital properties known under the names of animal sensibility and animal contractility and to consider them as functions." To do this Magendie popularized the technique of vivisection, whereby one could interrupt the activity of a component within the system and see what consequences it had on the overall functioning of the system. He was able to differentiate the sensory function of the posterior roots of the medulla and the motor function of the anterior roots, as well as to identify the control of the fifth cranial nerve over touch and of the cerebellum over balance (Magendie 1821; see Albury 1977).

Like Bichat, Magendie seemed well on the way to a decomposition of vital functions and a localization within bodily tissues. While Magendie did contribute information that was critical to developing a mechanistic explanation of vital functions, he did not embrace mechanism applied to these functions. This is not to deny that he attempted to unravel the chemical and physical processes operating within the organism. For example, he conducted various studies on the effects of poisons in the body. While part of his goal was to isolate the active agent in the poisons, he did *not* try to analyze everything at this chemical level. He performed physiological experiments to determine where poison entered the body: by separating the rear quarter of a dog so that only the blood vessels remained intact and then showing that poison injected into that portion still entered the animal's circulation and took effect (cf. Olmstead 1944), he established that poison did not enter only through the lymphatic vessels. Yet, in carrying out these physiological experiments, Magendie accepted that there was a level of analysis beyond which he could not go. He was unable to explain nutrition or action in mechanical terms, as these processes took place at a level that was imperceptible and hence not subject to experimentation, at least given the tools at hand. His opposition therefore appears more pragmatic than principled: without appropriate experimental techniques to identify components and their contributions, he saw there was no hope of discovering the operative mechanisms.

Bichat's and Magendie's opposition to the mechanistic explanation of physiological processes in terms of chemical mechanisms provides an interesting study in contrasting styles. Bichat pressed principled arguments designed to show that physiological processes exhibited distinctive properties, inexplicable in physical or chemical terms, which therefore could not be identified with physical or chemical processes. Magendie, by contrast, offered technical rather than principled arguments against mechanism in physiology: lacking the means to reveal physical or chemical processes underlying vital processes, any proposed mechanistic account was doomed to be idle speculation.

The Explanation of Fermentation

Bichat's objections to mechanism were general and sweeping; he opposed *any* attempts to account for the properties of living tissues in mechanistic terms. By the middle of the nineteenth century, however, a more focused opposition arose, one directed at fermentation. Proponents of this objection tried to show that fermentation was one physiological process irreducibly linked to living cells.[6]

The framework for modern chemical thinking about fermentation stems from Lavoisier, who viewed it as a two-part process: sugar was broken down into simpler components, one of which was subsequently oxidized:

> The effects of venous fermentation upon sugar is [*sic*] thus reduced to the mere separation of its elements into two portions; one part is oxygenated at the ex-

pense of the other, so as to form carbonic acid, while the other part, being disoxygenated in favor of the former, is converted into the combustible substance called alkohol; therefore, if it were possible to re-unite alkohol and carbonic acid together, we ought to form sugar. (Lavoisier 1799, p. 196)

At the beginning of the nineteenth century, Gay-Lussac (1810) and Thénard (1803) extended the inquiry of Lavoisier, establishing (in modern symbolism) the following general equation for fermentation:

$$C_6H_{12}O_6 \Rightarrow 2CO_2 + 2C_2H_5OH.$$

The only thing that seemed to require explanation was what prompted the decomposition of sugar into these components. Gay-Lussac's proposal typified early-nineteenth-century thinking. Following Appert's (1810) discovery that one could preserve food by sealing it from air and heating it, Gay-Lussac proposed that exposure to ordinary oxygen was what initiated the decomposition process (see Fruton 1972).

The case for the involvement of living organisms in fermentation was developed by Cagniard-Latour (1838), Schwann (1837), and Kützing (1837).[7] All three were microscopists. Cagniard-Latour's first observations of yeast, made at the outset of the nineteenth century, showed yeast to be a fine, granular material. New microscopes, introduced in the 1830s, allowed him to observe yeast reproducing through budding, an activity typical only of living organisms. He also ascertained that the number of globules was always proportional to their weight. Thus, he concluded that yeast could not be a simple chemical substance, but was instead a living body: "brewer's yeast, this widely-used ferment, is a mass of small globular bodies that can reproduce themselves . . . and not simply an organic or chemical substance, as has been supposed" (Cagniard-Latour 1838, p. 221). Because it could not move, Cagniard-Latour concluded that yeast was a plant. He further contended that its production of alcohol and carbon dioxide from sugar was the result of its growth.

Schwann approached the identity of yeast from a different direction. He had been addressing the question of spontaneous generation and also followed up on Appert's discovery that one could suppress fermentation and putrefaction by heating and sealing organic material from air. Schwann showed that sealing off air was unnecessary to stop putrefaction as long as all air reaching the organic material was heated. He interpreted these results as showing that putrefaction was due to living organisms, which were present in ordinary, but not in heated, air. These organisms fed on the dead organic matter. Since putrefaction and fermentation were regarded as very similar processes, he proposed extending these findings to fermentation. He discovered, however, that normal yeast cells could maintain fermentation even when provided with only heated air, but that if they were boiled, fermentation could not continue. This led him to propose that yeast is a living organism, a fact further supported by his observation that corpuscles in brewer's yeast multiplied during the course of fermentation and that arsenic halted fermentation. He found additional support in the fact that the rate of fermentation increased in the presence of nitrogenous substances, which he attributed to the fact that yeast needed nitrogen for normal growth. Like Cagniard-Latour, Schwann interpreted alcoholic fermentation as a byproduct of the normal nutritive process in yeast:

> Alcoholic fermentation must therefore be regarded as a decomposition effected by the sugar-fungus, which extracts from the sugar and a nitrogenous substance the materials necessary for its own nutrition and growth, and whereby such elements of these substances (probably among others) as are not taken up by the plant preferentially unite to form alcohol. (1837, p. 192)

The proposal that fermentation required the activity of a living organism was vigorously attacked by a number of researchers who remained convinced that it was a purely chemical reaction. One of the strongest proponents of the chemical view was Jacob Berzelius. In his paper introducing the concept of a *catalyst*, he had proposed that fermentation was a catalytic process like others he considered:

> Of all the known reactions in the organic sphere, there is none to which the reaction bears a more striking resemblance than the decomposition of hydrogen peroxide under the influence of platinum, silver, or fibrin, and it would be quite natural to suppose a similar action in the case of the ferment. (1836; cited in Keilin 1966, p. 20)

Berzelius (1839) also criticized the proposals of Cagniard-Latour, Schwann, and Kutzing, arguing that yeast is simply a chemical substance, not a living organism. He proposed that it participates in fermentation and forms a precipitate resembling living cells.

The conflict over whether yeast cells were living organisms was bitter and heated. In 1838 Turpin prepared a paper for the French Academy in which he defended Cagniard-Latour's views. When a description and partial translation of the paper appeared in the *Annalen der Chemie* (Turpin 1839), it was followed by an anonymous article, widely attributed to Wohler and Liebig (the editors of the journal). The article (Anonymous 1839), entitled "The Unravelled Mystery of Spirituous Fermentation," ridiculed the view of yeast cells as living organisms by treating the view as holding that yeast cells were little animals shaped like distilling glasses, without teeth or eyes but with a stomach, an intestine, and urinary organs. According to the article, yeast cells digest sugar in their stomach, producing carbon dioxide and alcohol, the latter being expelled through a bladder the shape of a champaign bottle (see Fruton 1972, pp. 50ff.).

Although satirical, the article did raise a legitimate objection: identifying fermentation with the life processes of an organism did not itself produce any explanatory gain. Attributing fermentation to the digestive processes of a living organism did not explain the process unless one could detail the chemical reactions through which the digestion occurred. On the other hand, these authors overreacted to the idea that living organisms might figure in the process, thinking it was incompatible with any form of mechanistic explanation of the process. Schwann had already pointed to a way in which linking fermentation with life could still be compatible with a restricted form of mechanistic explanation—one construing the components of the cell as ordinary physicochemical constituents that are transformed by being adsorbed into living cells. While Schwann was not the first to have observed cells or to have claimed that they were the basic structural unit of living organisms, it was through his work that the cell theory gained general acceptance. The reasons for this were complex. One is that, due to improvements in microscope design and Schwann's ability as a microscopist, his observations were more credible than those of his predecessors. A more important factor, however, had to do with his theoretical argument that all cells, which may appear quite different in different tissues, are really the same kind of morphological unit, because they are formed in the same manner. He presented microscopic evidence to support the claim that cells are formed by a process analogous to crystal formation. Despite morphological differences that might appear later in development, the common origin of the various cells showed them to be the same kinds of living units. This was a highly mechanistic proposal, and the fact that he was advancing such a model encouraged a general acceptance of his cell theory (see Bechtel 1984a). Other researchers, including von Mohl, reported observing cell division, but could not offer a mechanical model to account for the process.

Schwann's commitment to mechanism may seem at odds with his defense of the claim that living cells, and not simple chemical structures, are the agents responsible for fermentation. Schwann, however, did not conceive of himself as totally giving up mechanism in defending his account of fermentation. He viewed living systems as themselves mechanical systems, but thought that the special kinds of structures found in living systems were the particular mechanical systems required to carry out tasks like fermentation. This becomes clear in the third part of his *Microskopische Untersuchungen* in which Schwann develops what he considers to be his speculative—and mechanistic—"theory of the cell." He divides the functions of cells into two kinds. He calls the first "plastic phenomena"; these relate to the process by which cells develop. The second he calls "metabolic phenomena"; these "result from chemical changes either in the component particles of the cell itself, or in the surrounding cytoblastema."

It is clear that Schwann intended metabolic phenomena to include such processes as fermentation and respiration, and that he expected to explain these phenomena in terms of the specialized constitution of the cell:

> The cytoblastema [i.e., the intercellular fluid], in which the cells are formed, contains the elements of the materials of which the cell is composed, but in other combinations; it is not a mere solution of cell-material but it contains only certain organic substances in solution. The cells, therefore, not only attract materials from out of the cytoblastema, but they must have the faculty of producing chemical changes in its constituent particles. Besides which, all the parts of the cell may be chemically altered during the process of its vegetation. The unknown cause of all these phenomena, which we comprise under the term metabolic phenomena of the cell, we will denominate the *metabolic power*. The next point which can be proved is that this power is an attribute of the cells themselves, and that the cytoblastema is passive under it. We may mention venous fermentation as an instance of this. A decoction of malt will remain for a long time unchanged; but as soon as some yeast is added to it, which consists partly of entire fungi and partly of a number of single cells, the chemical change immediately ensues. Here the decoction of malt is the cytoblastema; the cells clearly exhibit activity, the cytoblastema, in this instance even a boiled fluid, being quite passive during the change. (1839, p. 197–98)

Schwann, thus, did not fully abdicate mechanism. He thought that the fermentative power of cells was due to their constitution, yet he insisted that this constitution is peculiar to living cells. The metabolic power associated with the living cells alters the passive cytoblastema, making it into cellular substance, and gives it the power to produce chemical reactions such as fermentation. To the degree that Schwann rejects mechanism, it is because he does not think that ordinary chemistry alone can explain fermentation. To do that, it is necessary to consider the metabolic power that organizes these constituents into the cell.

The controversy between those who took fermentation to be a purely chemical process and those who saw it as the activity of living yeast continued for several decades. In these discussions the concept of fermentation was applied far more generally than just to alcoholic fermentation. It was applied to any reaction where nitrogen-based organic material, present in small quantities, catalyzed a chemical breakdown of substances into simpler components, without being consumed in the reaction. Thus, it included the breakdown of sugar into lactic acid in the souring of milk, or that of sugar into butyric acid in the souring of bread; the transformation of gums, starch, dextrin, and mannitol into dextrose; the formation of ammonia from urea; and the transformation of proteins, starches, and fats into simpler products by saliva, gastric juice, or pancreatic juice. In many of these cases there was no indication of the involvement of living organisms, thus creating a tension between two types of accounts for different fermentations. The proponents of the chemical accounts argued that all fermentations ought to be simple chemical reactions, and they viewed those who advocated a special role for living organisms in some fermentations as engaging in special pleading. The chief combatants advancing these two alternatives were Justus Liebig and Louis Pasteur.

Liebig, after initially simply endorsing Berzelius's theory, later advanced his own, one more reminiscent of Stahl than of Berzelius. A ferment, according to Liebig, was nothing but an unstable organic product, and fermentation was a chemical reaction, a decomposition conceived of as akin to oxidation. He attributed the appearance of living globules to "gluten or albumin [taking] a geometric form which [became] altered during the process of fermentation of beer, wort, or vegetable juices" (Liebig 1839). His own theory attempted to integrate the observation that nitrogenous material undergoes spontaneous putrefaction, while non-nitrogenous material must be brought into contact with putrefying substances (such substances caused sugar to ferment rapidly when the two were brought into contact). Yeast simply accomplished this to a higher degree. Liebig attributed this power to the kind of chemical activity occurring in the nitrogenous substances and held that these substances were, in turn, capable of passing this activity to others. Thus, it was not a particular material, such as Berzelius's catalysts, that induced fermentation, but simply unstable substances capable of passing movement onto other substances. In the fermentation of beer, yeast was essential not because of any organic activity, but because, in dying, it released chemicals into the medium, and these chemicals in turn initiated fermentation. Liebig's theory of fermentation, along with various theories of animal and plant metabolism that he developed in subsequent years, held considerable influence among chemists for some time.

Pasteur defended the position of Cagniard-Latour and Schwann, holding that living cells were essential to fermentation. As he said, "organization and life" are the cause of fermentation. He was led, however, to take up the question of fermentation as a result of his concern with a quite different set of problems. Prior to attacking the question of fermentation, Pasteur had been a pure chemist little interested in problems relating to physiology. He apparently first considered fermentation and observed yeast under a microscope in 1856, when he was asked to consult on some problems facing the local brewing industry (see Connant 1948). His intellectual interest in fermentation, though, stemmed from a purely chemical problem—that concerning the ability of certain solutions to polarize light. Pasteur thought that this ability was correlated with the asymmetry of molecular structure, but this correlation seemed to fail with amyl alcohols, a by-product of fermentation; he then distinguished two amyl alcohol isomers, only one of which polarized light. Pasteur was further interested in how the capacity to polarize light arose, since only substances produced in living organisms seemed to possess this capacity.

The first paper to result from his research on fermentation (Pasteur 1858a) dealt not with alcoholic fermentation but with lactic acid fermentation, a reaction most readily observed in the souring of milk. In the mid-nineteenth century the two reactions were conceived to be quite different physiological processes for metabolizing glucose.[8] Lactic acid fermentation, in fact, seemed to offer strong support for Liebig's conception of fermentation reactions, as it did not appear to involve living organisms in any way (Gerhardt 1856). The common procedure for trying to eliminate microorganisms from biological material—boiling the substrate and heating the air—did not impede lactic fermentation. Pasteur's goal was to demonstrate that, despite appearances, a "new yeast" figured in lactic acid fermentation:

> Just as an alcoholic ferment exists, namely, brewer's yeast, which is found wherever sugar breaks down into alcohol and carbonic acid, so too there is a special ferment, a lactic yeast, always present when sugar becomes lactic acid, and that if any nitrogenous plastic material can transform sugar into this acid it is because it is a food suitable to the development of this ferment. (1858a, p. 28)

He prepared a culture of yeast, boiled it to kill the cells, and seeded it with a lactic ferment. After incubating for a day, this lactic ferment induced an active lactic fermentation in the new solution and a release of CO_2. Though Pasteur initially used brewer's yeast to make the culture, he was quick to point out that any "nitrogenous" substance (that is, any protein) would suffice. Using microscopic observations, he also set out to show that the new yeast was very similar in appearance to brewer's yeast, which led him to the thought that they might be "neighboring species."

In subsequent work Pasteur turned directly to the study of alcoholic fermentation and set out to refute Liebig's interpretation of the process. He showed that when yeast was added to a liquid medium of sugar, ammonium tartrate, and inorganic phosphate in which there was no substance that could be construed as spontaneously decomposing (as Liebig's model required), fermentation still ensued. This inducement of fermentation could only be attributed to yeast. Moreover, he showed that when fermentation occurred some of the mass of the sugar substrate was not accounted for in the output, which pointed to the fact that some of the sugar had been taken up by the yeast. From such evidence Pasteur concluded that fermentation was essentially a process associated with life:

> Alcoholic fermentation is a process associated with the life and the organization of yeast cells and not with their death or putrefaction any more than it is a

> phenomenon of contact, in which case the transformation of sugar would be accomplished in the presence of the ferment without yielding anything up to it or taking anything from it. The chemical process of fermentation is essentially a phenomenon associated with vital action, commencing and ceasing with it. It is invariably accompanied by the growth and multiplication of yeast cells. (1860a, p. 359)

As to how yeast accomplished fermentation, Pasteur was agnostic. He allowed that the participation might come close to that envisaged in Liebig and Wohler's satire, but he also acknowledged the possibility that it might in fact be due to a peculiar chemical agent produced by the yeast that functioned as a catalyst:

> Will one say that the yeast nourishes itself with sugar so as to excrete it in the form of alcohol and carbonic acid? Will one say on the contrary that the yeast produces, during its development, a substance such as pepsin, which acts on the sugar and disappears when that is exhausted, since one finds no such substance in the liquids? I have no reply on the subjects of these hypotheses. I do not accept them or reject them, and wish to constrain myself always not to go beyond the facts. And the facts only tell me that all the fermentations properly designated as such are correlative with physiological phenomena. (Ibid., pp. 360)

Pasteur thus did not explicitly rule out the possibility that fermentation could eventually be explained in chemical terms. He insisted that the process was connected with the life of a cell and could not be totally separated from it, and the possibility of a chemical explanation was set aside as hypothetical and not needing an answer. His own methodology required that he not "go beyond the facts." Insofar as he resisted the attempt to decompose the living system and to attribute fermentation to component parts, and instead insisted on correlating it with living cells, though, he was rejecting the development of a mechanistic approach to explaining fermentation. He abjured speculation.

Although Pasteur rejected any mechanistic research into fermentation, he did not reject the attempt to study fermentation empirically. In fact, in his subsequent research, he established one of the important features of yeast life, which is now known as the "Pasteur Effect": yeast develops and causes fermentation in the absence of oxygen, losing this ability when oxygen is present. Pasteur described this as a process of *vie sans air*, arguing that the yeast cells carried out metabolic activities very similar to those found in aerobic organisms, but that they drew their oxygen from the metabolites into which oxygen was unstably bound.

Liebig eventually came to accept the idea of yeast cells as living organisms, but he still did *not* accept fermentation as a physiological process of living organisms. He argued that fermentation resulted from the disintegration of yeast—that when yeast cells ceased to grow, the bond that held them together deteriorated and a motion resulted, which was then imparted to the sugar. Liebig argued that the contribution of the living organism was simply to generate the substance that subsequently induced fermentation:

> It is possible that the only correlation between the physiological process and the phenomenon of fermentation is the production, in the living cell, of a substance which, by some specific effect analogous to that which emulsin exerts on salicin and amygdalin, brings about the decomposition of yeast and of other organic molecules. According to this view, the physiological process would be necessary for the production of this substance, but would have nothing to do with the fermentation. (1870, p. 6)

Pasteur (1872) responded in a brief note in which he pointed out again that even when there were no substances to decompose in the manner Liebig proposed, yeast still induced fermentation.

Opposition to Pasteur also came from chemists who viewed his model as vitalistic. This is particularly true of Berthelot, who remarked, "As to the vitalist opinions adopted by M. Pasteur regarding the real causes of the chemical changes operative in alcoholic fermentation, I do not believe that the moment is appropriate to discuss them" (1859, p. 692). Traube echoes a similar view: "Schwann's hypothesis (later adopted by Pasteur), according to which fermentations are to be regarded as the expressions of the vital forces of lower organisms is unsatisfactory" (1878, p. 1984). Berthelot and Traube found Schwann's and Pasteur's views vitalistic because they did not advance a purely chemical model, but inserted living organisms as entities performing functions not capable of being performed by nonliving entities. These critics insisted on restricting their focus to purely chemical agents.

Those who rejected the claim that there was something unique about living cells that enabled them to accomplish fermentation generally embraced the view that there was a chemical agent within yeast that induced fermentation. The positive results with yeast could thus be explained. Berthelot, for example, compared fermentation to processes that were recognized as purely chemical, such as the inversion of cane sugar (that is, the hydrolysis of sucrose, yielding dextrose and fructose). While inversion of cane sugar was carried out by yeast, Berthelot traced it to a substance he could isolate from the yeast cell by precipitating a cell-free extract in alcohol and then redissolving it in water. He proposed that living organisms contained both soluble and insoluble ferments, the latter of which remained closely bound to the cell structure and could not be isolated from it. The chemical reactions, he argued, were due to these ferments, although, with Liebig, he assumed these were produced by the living organism.

The idea that fermentation was due to a localized substance within yeast was also maintained by Traube:

> Nothing points against the assumption that the protoplasm of the plant cell either is itself or contains a chemical ferment which promotes the alcoholic fermentation of sugar; and further, that the activity of this ferment only appears to be tied to the cell in so far as no means have been found up to the present of isolating the ferment from the cell without destroying it. In the presence of air, the ferment oxidizes sugar by transferring free oxygen onto it; in the absence of air, it decomposes the sugar by transferring oxygen from one atomic group of the sugar molecule to another, yielding alcohol as a product of reduction and carbon dioxide as a product of oxidation. (1874, p. 886)

Thus, both Berthelot and Traube continued to advance proposals for direct localization. Supposedly biological processes were to be localized as the effects of chemical agents within the cell.

Pasteur rejected the distinction between soluble and insoluble ferments. He maintained that anything that could be isolated from the cell and produce chemical reactions should be excluded from the class of ferments. According to him the association with living cells constituted the mark of a ferment. Others were, of course, more ecumenical in their use of the term.

4. Conclusion: Settling for Descriptions

There is a common pattern to the four cases we have discussed in this chapter. Mechanists would offer models that appeared overly simplistic to those fully enmeshed in the complexities of the phenomena. Recognizing that the available mechanistic models could not deal with the phenomena that their experiments revealed, the opponents maintained the uniqueness of the phenomena. Often these opponents of mechanism were correct in pointing out the inadequacies of the then-current mechanisms, although wrong in their contention that no mechanism would ever suffice. Many of the appeals to vital forces that we have seen resulted from an inability to answer questions of interest at the level of organization at which research was then focused. Sometimes the appropriate level of explanation would involve a lower level, at which researchers then lacked the tools for carrying out empirical inquiry. Thus, Pasteur saw that explanations of fermentation in terms of underlying mechanisms were little more than crude speculation. In terms of the chemistry of the day, it could not be done. Similarly, Flourens saw that explanations of intelligence in terms of brain structure were unsupported by what was independently known of brain structure and function at the time. A systematic and principled account of the mechanisms was lacking. And it still is.

Almost as often, the appropriate level of explanation is higher than the one on which mechanists did focus research. We will discuss this in more detail in Chapter 7, but it can be briefly illustrated by Bernard's investigations into digestion and nutrition. Bernard began his research accepting the basic chemical models of digestive and nutritional processes common in the mid-nineteenth century. He assumed that there were highly localized chemical agents, catalysts, responsible for the processes in question. For example, Bernard's (1855) research leading to the glycogenesis of the liver was initiated under an assumption stemming from Liebig that all chemical reactions in the animal were catabolic, breaking down the complex foodstuff produced by plants into simpler products, with a release of energy. Thus, he sought the locus at which sugar was broken down within the body. Bernard planned to attack this problem and localize the reaction through a series of experiments in which he injected grape juice into animal blood systems and traced levels of concentrations by dissecting the animals at various times afterward. This led to the anomalous finding of heightened concentrations of sugar in the portal vein, upstream from the liver. Bernard then experimented with a dog kept on a protein diet and found the same result. Since it could not have originated in the animal's diet, Bernard asked from where the sugar came. He concluded that sugar was actually being formed in the liver from simpler constituents and was flowing back into the portal vein during dissection. This discovery undermined the simple, highly localized model that portrayed animal nutrition as involving simple assimilation and direct combustion. Nutrition had to be a much more complex process, involving both breaking down and rebuilding organic compounds; and it was clear that to develop a full account of nutrition one would have to examine the indirect path of chemical changes followed in the animal. This meant that nutrition could not be explained in terms of simple, basic, chemical reactions. It required, instead, an understanding of a complex sequence of reactions.

Arguments for vitalism often stem from researchers seeking an explanation at an inappropriate level, which may be either too high or too low; likewise, arguments for localization and decomposition can suffer from the same problem. When researchers are operating at too high a level, they cluster together too many activities into one entity, so that it becomes mysterious how that entity is able to carry out the functions assigned to it. When researchers are at too low a level they may miss some of the complexity in the organization and behavior of the system that is critical to understanding how the system actually accomplishes its activity. The chemists seeking to explain metabolic processes in the nineteenth century were both at too high and too low a level: They clustered different physiological functions together, creating tasks that were too complex for any single chemical agent; thus, they viewed fermentation as a simple catalyzed reaction when it was actually complex. On the other hand, they tried to handle functions like nutrition at too low a level, thus missing many of the interactive activities needed to explain it.

The cases described in this chapter were not chosen because they show ways the mechanistic program expands or grows, but rather because they point to a common way in which researchers have opted out of that program. Two features unify these cases and point to a common rationale for rejecting mechanism. The first is that opponents of mechanistic models generally were responding, in part at least, to real limitations on mechanistic theories. They recognized limitations on the resources offered by the mechanisms, and saw that they could not offer explanations of the phenomena. The second is that the systems are complex and require sophisticated mechanisms, which at the time seemed unavailable.

Although the case for abandoning mechanism is generally made *after* an attempt to pursue a localizationist program, the logical point of departure is *before* one enters into the direct localization program. The critical point is whether researchers elect to pursue a strategy of decomposing phenomena into component processes and localizing them in appropriate subsystems. The opponent of mechanism denies the utility of localization and decomposition and seeks instead a better understanding of the phenomena. Once again, we can portray this as an additional choice-point, as in Figure 5.1.

The decision to abandon mechanism is not without its own cost. For example, the inherent weaknesses in Flourens's own positive program offset the strengths in his critique of Gall. In a memoir reviewing Gall's and Spurzheim's anatomical works, the critics, who include Cuvier, make the following remark:

> We cannot expect a physiological explanation of the action of the brain in animal life. . . .
>
> The functions of the brain . . . consist in receiving, by means of the nerves, and in transmitting immediately to the mind, the impressions of the senses; in preserving the traces of these impressions . . . ; [and] lastly, in transmitting to the muscles, always by means of the nerves, the desires of the will.
>
> Now these three functions suppose the mutual, but always incomprehensible influence of the divisible matter and the indivisible mind (*moi*); a hiatus in the system of our ideas never to be supplied, an eternal stumbling-block of all our philosophies. (Tenon et al. 1809, p. 38)

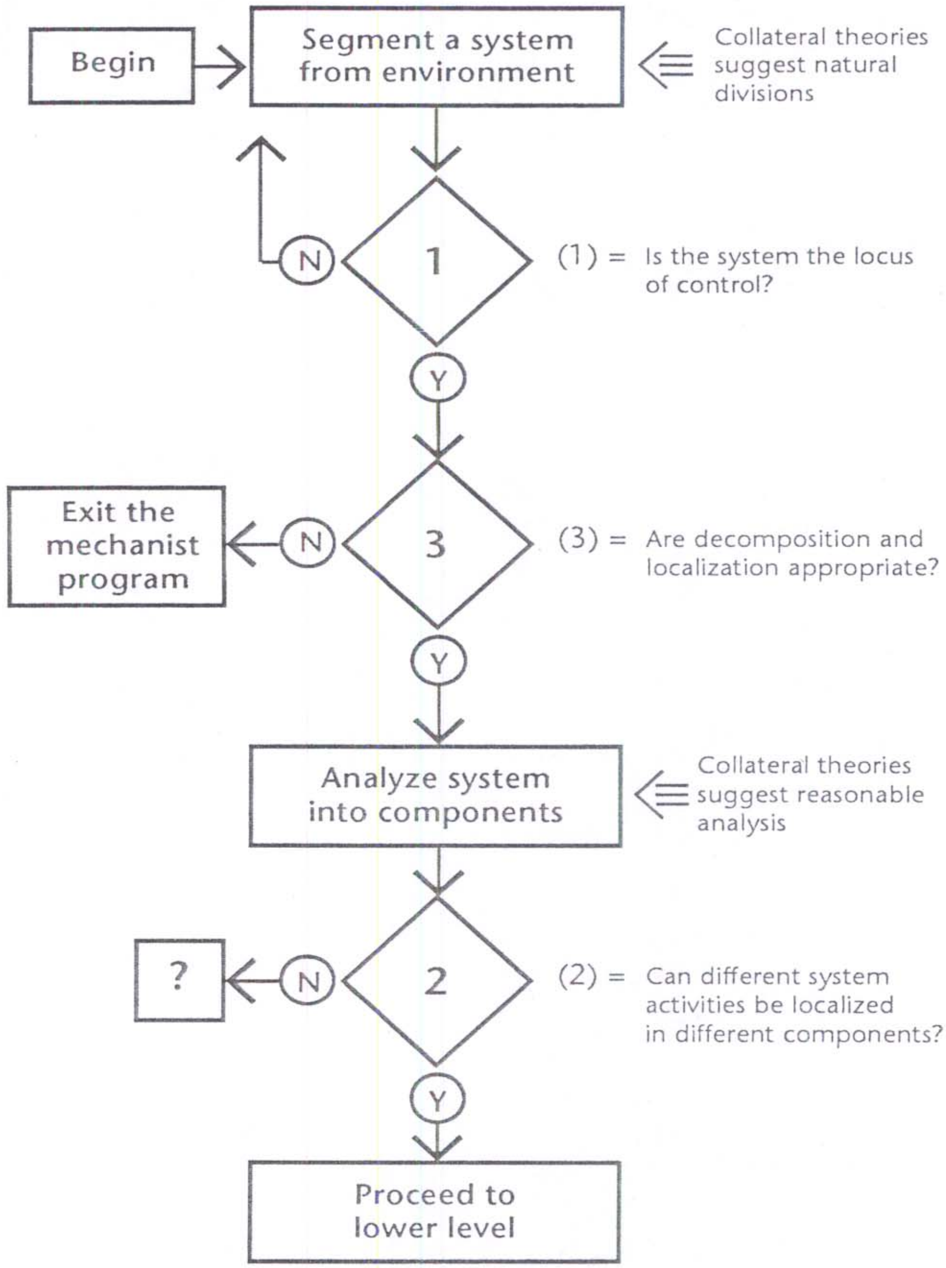

Figure 5.1. A Third Choice-Point. One response to attempts at mechanistic explanation involves a categorical denial of decomposition and localization, emphasizing a systematic description of the phenomena. This constitutes a rejection of mechanism.

A positive program for doing more than describing the functions is simply absent. The mechanistic program, however crude it might have been in the hands of Gall and Spurzheim, at least left us with a direction for research. Their opponents left us with only a "hiatus" which was "never to be supplied." Lacking a systematic program of research, the antimechanistic program is virtually guaranteed to fail in gaining adherents.