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Mechanism, vitalism and organicism in late nineteenth and twentieth-century biology: the importance of historical context

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Abstract

The term 'mechanism' has been used in two quite different ways in the history of biology. Operative, or explanatory mechanism refers to the step-by-step description or explanation of how components in a system interact to yield a particular outcome (as in the 'mechanism of enzyme action' or the 'mechanism of synaptic transmission'). Philosophical Mechanism, on the other hand, refers to a broad view of organisms as material entities, functioning in ways similar to machines—that is, carrying out a variety of activities based on known chemical and physical processes. In the early twentieth century philosophical Mechanism became the foundation of a 'new biology' that sought to establish the life sciences on the same solid and rigorous foundation as the physical sciences, including a strong emphasis on experimentation. In the context of the times this campaign was particularly aimed at combating the reintroduction of more holistic, non-mechanical approaches into the life sciences (organicism, vitalism). In so doing, Mechanists failed to see some of the strong points of non-vitalistic holistic thinking. The two approaches are illustrated in the work of Jacques Loeb and Hans Spemann.

Keywords: Mechanistic materialism (Mechanism); Holistic materialism (holism); Organicism; Vitalism; Jacques Loeb; Hans Spemann; Jakob von Uexküll; Wilhelm Roux; Hans Driesch

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1. Introduction[1]

The term 'mechanism' has been used widely in the life sciences at least since the seventeenth century. Embodied in the 'mechanical philosophy' of Thomas Hobbes (1588–1679), Pierre Gassendi (1592–1655), René Descartes (1596–1650) and Robert Boyle (1627–1691), among many others, it came to dominate the epistemology of virtually all western science (Durbin, 1988, p. 179). But what does the term 'mechanism' mean, especially in relation to living organisms? How has it been applied—to what sorts of entities, processes, methodological approaches, ontologies—and how has that meaning changed? Is the concept of 'mechanism' context-dependent? These are of course very large and complex questions. However, I would like to explore one aspect of the meaning given to 'mechanism' in the life sciences during the period roughly from 1900 to mid-century. During these decades there was much discussion, and heated debate, about what was referred to explicitly as the mechanistic approach and about why it was or was not the best way to try to understand living organisms. These debates influenced virtually every area of biology from the then newly-born Mendelian genetics to established fields such as physiology, cell biology and even evolutionary biology. An exploration of what 'mechanism' meant in the early twentieth century raises philosophical issues that are still very much at the heart of discussions among biologists and philosophers of biology grappling with the ever-increasing sense of living organisms as highly complex, dynamic and self-regulating systems. The older notions of 'mechanism' are breaking down; but to better understand why, and what sorts of epistemologies are taking their place, an historical examination of 'mechanism' and its various formulations in the past century can be illuminating. The general issues debated in 1920 and 1990 were not so different; the way the issues were formulated and the alternatives available were.

In this paper I argue that the 'mechanistic conception of life' (as Jacques Loeb referred to it in his address to the Monist league in 1911) was embedded in the scientific and socio-political context of its time at a variety of levels: from the large-scale sweeping changes associated with rapid industrialization and the resulting social alienation rampant in Europe and the United States, to the disillusioning effects of World War I and, finally, to the attempt by biologists to create a 'new biology' that mimicked the methods of physics and chemistry and promised the potential for 'engineering' of organisms for human purposes on a wholly new scale. In particular I argue that the 'mechanistic view' became a rallying point for a younger generation of biologists (born roughly after 1865) who sought to upgrade and professionalize those aspects of biology that had previously been considered largely descriptive,

[1] I originally prepared this paper as a talk at the annual Reichenbach Conference at Washington University in the fall of 2003. At the time, I had not read the excellent essay, 'Embracing complexity: Organicism for the 21st century' by Scott Gilbert and Sahotra Sarkar (Gilbert & Sarkar, 2000). Their discussion has expanded my own understanding of certain issues, associated with mechanism and its opposites, vitalism and organicism as philosophical views. It was particularly helpful to me to read their discussion of the strains of organicism they see among embryologists in the United States in the 1920s and 1930s, and of geneticists in the 1980s and '90s.

speculative and not amenable to experimental analysis (e.g. embryology, heredity, cytology, evolution). Given this context, the concepts embedded in the term 'mechanism' were much more than philosophical abstractions. They had a social meaning that colored profoundly the way the term was used and understood, both by those who supported and those who opposed the mechanistic view.

As currently employed (and for a good part of the twentieth century as well), the term 'mechanism' had two very different meanings, what I refer to as 'philosophical Mechanism' (with a capital 'M'), and 'operative or explanatory mechanism' (with a small 'm'). Philosophical Mechanism is the view that likens organic (or other complex) entities to the interaction of material components in a machine. It derives from the mechanical philosophy of the sixteenth and seventeenth centuries, but has evolved through several periods of prominence in biology to the present (Durbin, 1988, p. 179; Dijksterhuis, 1961, pp. 431 ff.; Boas, 1952). Philosophical Mechanism is just one of several forms of materialism and encompasses the associated concepts of atomism and matter-in-motion (Lange, 1879–1881, quoted in Durbin, 1988, p. 179). Operative or explanatory mechanism, on the other hand, is the step-by-step description/explanation of how the components in a system interact to yield a process or outcome: for example, how an enzyme molecule interacts with its substrate molecule to yield a product, or how neurotransmitter molecules interact with membrane-bound receptors to produce an action potential. Operative mechanism is concerned with both the components and activities involved in understanding how something works or how a particular cause leads to a particular effect, and has been discussed by Machamer, Darden and Craver (2000) and a host of others in this issue. As a result, in this paper, I will deal primarily with philosophical Mechanism particularly as it was the focus of discussion during the early years of the twentieth century about the proper way to study and understand biological phenomena.

I should add that the concept of Mechanism during the period 1890–1940 was used in both an ontological and epistemological sense. However, since most life scientists at the time were less prone to delve into ontological discussions about their Mechanistic view—about whether organisms really were nothing more than complex machines and the like—the following discussion focuses primarily on advocates of Mechanism as an epistemology, as a practical approach to investigating and describing the properties and functions of organisms, and not as any necessary statement about the 'way the world is'.

In the nineteenth and twentieth centuries, Mechanism, particularly as applied to living systems, was often juxtaposed in a highly polemical way to a variety of alternative views, such as 'vitalism', 'holism' or 'organicism'. Historically, Mechanism has changed its meaning, or at least embodied different meanings at different times depending on the nature of the debates in which it was involved (Dijksterhuis, 1961, p. 431; Boas, 1952). It is thus impossible to give it a single definition. What I will do in this paper is try to situate Mechanism within the larger framework of questions about the nature of organisms and the ways to study them that formed an important epistemological debate throughout much of the twentieth century. That aspects of these debates resonate well into the twenty-first century makes the discussion more than simply an arcane historical curiosity.

Part of the difficulty in understanding the meaning of Mechanism in biology is that it is still routinely used in both the philosophical and operative/explanatory senses. Virtually every discussion I have with my biological colleagues involves some mention of 'the mechanism' by which the phenomenon under investigation takes place. It can refer to very specific processes, such as the nucleophilic attack by the reactive group of an enzyme on an exposed covalent bond of its substrate, or to a whole category of reactions such as cell signal responses due to protein kinase A (PKA) second messengers. Indeed, much of the research in biology today, whether in molecular, organismic, or population biology, is focused on unveiling the explanatory mechanism by which a biological process takes place.

While differing in breadth and focus, philosophical and operative/explanatory mechanism have overlapping meanings. Deriving from the machine analogy, both consist of the expectation that any process can be described in terms of its component parts and the activities through which they interact to produce an outcome (Machamer, Darden, & Craver, 2000). A machine functions by the interaction of separate, knowable parts: the turning of a flywheel or motion of a piston and valves. Both imply the existence of material entities, individual parts that interact to produce some effect. Both also involve the assumption that some sort of organized relationship exists between those individual parts such that they work together harmoniously. For example, just as the cogs in a gear-shift mechanism fit together to produce forward motion of a car, so during enzyme-mediated catalysis, a substrate fits into the active site of its enzyme in such a way that specific chemical bonds in the substrate are exposed to specific active groups in the enzyme, allowing a reaction to take place.

As a result of the overlap in meanings of the term mechanism, philosophical Mechanists are generally, but not always, operative mechanists; conversely, and in a more general sense, not all operative mechanists are necessarily philosophical Mechanists. It is possible to study a very specific mechanism (one enzyme-catalyzed reaction) while still recognizing that it operates in a more holistic way in the cell or tissue context where it occurs. Similarly, it is possible to espouse a highly sophisticated Mechanistic philosophy, and not necessarily be interested in studying specific operative mechanisms, as we will see in the discussion of the work of Jacques Loeb in Section 3.

Although Mechanistic thinking in the philosophical sense has tended to dominate much of Western biology in the past three hundred years, there have been periodic voices raised in protest against what at times seemed like Mechanists' over-simplistic way of viewing organisms. The alternative view that organisms were somehow qualitatively different from machines punctuated the development of Mechanistic interpretations of life, producing a pendulum swing at times away from Mechanism, at times back toward it, with each swing, however, altering in various ways the research programs that were under dispute (the nature of the nerve impulse, the relation of genes to chromosomes, or of genes to embryonic development). The periods of the most blatant promotion of Mechanistic thinking include the mid-to-late seventeenth century (for example, Descartes's pneumatic theory of muscle contraction, and Harvey's pump model for the heart and valves), the later eighteenth century

(Vaucanson's duck and other automata, Hales's hydrostatic explanation of the movement of water in plants, and chemical analogies of the stomach to a retort), the mid-nineteenth century (Helmholtz and the Berlin 'Medical Materialists' of the 1840s and '50s, whose champion, Helmholtz, studied nerve conduction as a form of electrical impulse), and much of the twentieth century (beginning in the late 1880s with Wilhelm His's mechanical theory of embryonic development, and embodied in Wilhelm Roux's program for developmental mechanics, *Entwicklungsmechanik*, in 1895). It is on this latter period (roughly 1895–1940), that the present paper will focus with two case studies: the 'mechanistic conception of life' put forward by German-born émigré physiologist and animal behaviorist Jacques Loeb (1859–1924) and an alternative epistemology propounded by embryologist Hans Spemann (1865–1940), who tried to fashion a more holistic approach to the organism by looking at tissue and organ-level interactions in development.

2. Working definitions: mechanistic materialism and its alternatives

Because the uses of Mechanism are intertwined with a variety of philosophical terms and concepts, which have not only changed over time, but are subject to continued discussion and debate today, I start with working definitions that can be applied to the specific case studies that follow The philosophical terms with which I will be concerned are: materialism, Mechanism (mechanistic materialism), reductionism, holism (holistic materialism), dialectical materialism, vitalism and organicism. All have been used in the twentieth century in discussing the nature of organisms and the best way(s) to go about investigating them. And all have contributed to ongoing debates about whether organisms are merely specially structured collections of molecules, or when they embody something qualitatively different and inexplicable in physical and chemical terms.

2.1. Materialism and mechanistic materialism

'Mechanism' is a form of materialism, gaining ascendancy during the scientific revolution as mechanistic materialism, or the mechanical philosophy (Boas, 1952). In the Mechanistic view the world appears as a mosaic of separate, independent parts. A detailed description of each of these parts and their interactions would produce a complete description of the system (a machine or an organism). From the Mechanistic point of view, the proper way to study any system is to take it apart (the analytical method) and determine the characteristics of the individual, isolated parts under as controlled a set of conditions as possible. For some Mechanists, such as biochemists studying enzyme kinetics, the lowest level of analysis might be a purified enzyme (along with perhaps its requisite co-factors) and its substrate. For others, such as cell biologists studying respiration, the lowest level of organization might be a group of molecules attached to a specific cell membrane (e.g. the cytochromes attached to the inner mitochondrial membrane). To understand a given biological process, the Mechanistic strategy has been to start by isolating those parts by some analytical procedure and identifying them structurally and functionally. Because of their commitment to analysis, Mechanists have tended to hold to some kind of reductionist strategy, that is, the belief that to understand higher level processes it is necessary to investigate them at lower levels of organization: for example, cells in terms of molecules, organs in terms of cells, organisms in terms of organ-systems. In this formulation Mechanistic materialism is downward looking, proceeding from higher to lower levels of organization in the search for the most general and basic explanation.

In discussing the relationship between parts and wholes, it is sometimes assumed that Mechanists have no appreciation of complexity and no interest in how components in a system interact to produce an overall effect. This is not the case for most Mechanists. The point of Mechanistic analysis is to identify the components of a complex system in order to understand how they work together in relation to the whole. For Mechanists, investigating the components separately reveals their individual characteristics in a way that studying them only as part of the whole does not. However, it is a tacit assumption of most Mechanists that once the characteristics of each component are known, their relationship to each other and to the whole will become apparent. It was a cardinal principle of early twentieth-century Mechanistic philosophy that the properties of living systems can be understood in terms of the laws of physics and chemistry, Thus, the lowest level of organization to which Mechanists of that era strove to understand living organisms was that of atoms and molecules.

In their attempt to model biology after the physical sciences, Mechanists emphasized the importance of experimentation. This was one feature that younger biologists thought would help place biology on the same footing as chemistry and physics. Experimentation allowed the biologist to distinguish between alternative hypotheses; hypotheses that were incapable of experimental test—for example, that living systems were organized by a non-chemical, non-physical 'vital' force—were considered worthless. Experimentation thus served as a corrective against unbridled speculation.

2.2. Holistic and dialectical materialism

Standing in opposition to mechanistic materialism in the later nineteenth and early twentieth centuries were a variety of views that can be clustered under the name 'holism' (also called 'organicism') and that were, as the name implies, concerned with how complex systems function as a whole. Holistic biologists sought to provide an approach that differed from what they saw as the piecemeal, oftentimes naïve and oversimplistic views (especially of organisms) promoted by Mechanists. In the early twentieth century two categories of holistic thinking can be discerned: materialistic and non-materialistic, though the two were often conflated. Among the former are included holistic materialism and dialectical materialism, and among the latter 'vitalism'. Holistic and dialectical materialism share a materialist epistemology, seeking to account for living processes as functioning wholes within the framework of known physical laws. Vitalism, on the other hand, claimed that living organisms defy description in purely physico-chemical terms, because organisms possess some non-material, non-measurable forces or directive agents that account for their complexity. Vitalism was regarded by early twentieth-century mechanistic materialists such as Jacques Loeb, T. H. Morgan or Wilhelm Roux as fuzzy-minded and subjective nonsense that offered no concrete research agendas, and provide no real guidelines for practical investigation.

Gilbert and Sarkar (2000) have pointed out that even though other versions of holistic thinking in the early twentieth century shared with Mechanism a strictly materialist epistemology, they were nonetheless largely ignored or rejected because they were perceived as keeping 'very bad company'—either with vitalism of one sort or another, Nazi 'organicism', or Marxist dialectical materialism—all of which advanced some kind of holistic view (Gilbert & Sarkar, 2000, pp. 4–5).[2] Yet, in substance, holistic materialism, and the formally-developed sub-category, dialectical materialism, stand in sharp contrast to vitalism in not postulating *any* forces that cannot be understood in terms of the known laws of physics and chemistry. What united all forms of holism was the clear recognition that living organisms were capable of activities that had no counterpart in the machine world: self-replication, purposeful or ordered response to stimuli, elaborate self-regulatory capabilities, and the incredible efficiency of their energy transduction. To understand these complex interactive processes, it was necessary to get beyond the individual parts to look somehow at the whole system or process. In the period 1900–1940, finding methods—conceptual and experimental— to accomplish these investigations was a major goal of holistically-oriented, but materialist biologists (including the embryologists Hans Spemann, Ludwig von Bertalanffy, Joseph Needam and Paul Weiss, and the geneticist Richard Goldschmidt).

As part of their opposition to mechanistic thinking, holistic materialists emphasized the importance of distinguishing between levels of organization in a complex system (in studying organisms this could include the atomic, molecular, cellular, tissue, organ-system, organismic, population, or ecosystem levels), proceeding to investigate each level on its own terms. The holistic approach does not preclude starting with a reductionist, analytical breakdown of a complex system into its component parts, but it does emphasize that this is not enough. For example, in studying the function of the mammalian kidney holistic materialists might first determine all of the tissue and cell types of which the kidney is composed. These might be studied initially in isolation using methods of histology, cytology and cell physiology. However, holistic materialists argue that it would also be necessary to study the whole, functioning where its response to variables such as blood pressure, ion concentration and hormones could be revealed. Studying one-by-one the ten or so individual cell types that make up the kidney could not be expected to provide a full picture of how the kidney functions as a whole within the intact organism. The nephron, the main filtration site in the kidney, depends for its function completely on its

[2] Gilbert and Sarkar note that holism has had in the past, and has in its various incarnations today, some bizarre fellow travelers, from Volkish nature-worship under the Third Reich to New Age Rhetoric of spiritualization of matter today (ibid., p. 5).

interaction with cells in different regions of the kidney, from the cortex, where the first major filtration of numerous ions occurs, to the medulla, where much selective reabsorption takes place. Each level of organization within the kidney—cellular, tissue and organ—has its own properties and characteristics that cannot be understood only by examining them separately. It is in their appreciation of the concept that each level of organization in a complex system has its own special properties, and that these must by studied by techniques appropriate for that level, that holists differ in one significant way from Mechanists.

For holistic biologists, complex systems (even very simple ones) show emergent properties that are the product of the individual parts plus their interactions (what we today call *synergistic effects* are an example of emergent properties). To those holistic thinkers committed to materialistic explanations, emergent properties were not mystical, since they resulted from the very material interaction of the parts of the whole in any system. But emergent properties could not be predicted from knowing only the individual components making up a given system. Explanations relevant to a given level had to be derived from studying that level itself and not merely by isolating its lower-level components.

A more formalized version of holism, known in the twentieth century as dialectical materialism, became the official philosophy of science (and social science) in the Soviet Union after 1917 and the People's Republic of China after 1949.[3] Dialectical materialism shares all the basic characteristics of holistic materialism—concern with levels of organization, interaction of parts, and emergent properties—but adds several important features. The most unique and defining of these is the dialectic insistence itself: that all processes can be best understood in terms of the interaction of opposing forces, or agents within a system, and between any system and its external environment. A classic example of dialectical materialist thinking applied to biology has been Darwinisim, where the dynamics of evolutionary change is constantly fueled forward by the interaction of two opposing processes: heredity (faithful replication) and variation (non-faithful replication) (Prenant, 1943, p. 138). With either process by itself there is no evolution; with both present evolution becomes inevitable. A chief feature of dialectical materialism, its advocates point out, is that it provides a way to investigate and understand the dynamic change that characterizes all systems in the universe. Random events may of course affect the way in which any system changes, but more constant factors driving change lie at a deeper level internal to the system itself. Further discussion of the characteristics of dialectical materialism take us too far afield from the present paper. The point of mentioning it here is to indicate that there was in the 1920s and 1930s another version of holistic thinking that was overtly and self-consciously materialistic and clearly devoid of mystical or vitalistic overtones. That it was not more consciously pursued was a result of the

[3] Some Western biologists (in particular), such as J. D. Bernal, J. B. S. Haldane and Marcel Prenant, did consciously use and promote dialectical materialism, but reaction to the rise of Communism after 1917 largely drowned out their attempts to give dialectics a prominent place in philosophy of science. For a more thorough discussion of dialectical materialism, see Allen (1980, 1991), Graham (1986).

political climate following in the wake of the Bolshevik revolution and the ensuing gulf between Russian and Western science and philosophy (Graham, 1986).

To see how one explicit form of Mechanistic philosophy was put into practice as a scientific research program, the career of Jacques Loeb is particularly instructive.

3. Jacques Loeb and the mechanistic conception of life

3.1. Loeb's background

Born in the Rhein provinces of Prussia in 1859, Jacques Loeb was, in the words of historian Donald Fleming, 'a child of his time' (Fleming, 1964, p. xi). 1859 also saw the publication of Darwin's *Origin of species* and Marx's *Critique of political economy*, while the preceding year had seen the publication of Rudolf Virchow's influential essay, 'The mechanistic conception of life', all three works codifying an explicitly materialistic interpretation of the natural and social worlds. From reading Schopenhauer at an early age, Loeb had become interested in the problems of human motivation and the nature of the will, moving through several disciplines and European laboratories in an attempt to find the most direct approach to this problem. Starting as a student of philosophy, he quickly determined that nothing would come of what he saw as mere 'word-mongering' (Fleming, 1964, p. xii), so he turned toward biology and medicine. Entering the University of Strasburg in 1881, Loeb completed his M.D. degree with a thesis under Friedrich Goltz. He followed his mentor in attacking the highly atomistic nature of the then popular theory of cerebral localization (Pauly, 1987, pp. 25–26). After some abortive job opportunities in Berlin, in 1886 Loeb took a position as Assistant in physiology under Adolf Fick at Würzburg (1886). Fick had been a student of the Berlin medical materialists and espoused the view that life was nothing more than an expression of the principles of physics, particularly of electricity. Embedded in this Mechanistic view was the assumption, as in physics, of a strictly deterministic universe where, if one knew all the inputs, it would be theoretically possible to predict the precise outcome of any process.

While Loeb's philosophy was solidified by his association with Fick, it was the experimental plant physiologist Julius Sachs, who was also at Würzburg, and the physicist Ernst Mach, at the time in Prague (later Vienna), who provided a more concrete and lasting direction for his emerging research program (ibid., p. 29). From Sachs, he gained an appreciation not only for the fine details of experimental method, but also an interest in the specific problem of plant movements, or tropisms, one of Sach's fundamental research interests. From Mach, he 'expanded a rather narrow methodological reform . . . into a radical revision of the nature of biology and its future purposes' (ibid.). By the time Loeb initiated a correspondence with Mach (the two never met) he realized he had found an ally in the struggle to avoid pseudo-mechanistic explanations that employed non-existent metaphysical entities such as atoms or other unseen elements. What mattered was predictability, not only because it at least was a concrete indication of the reliability of an hypothesis, but also because it indicated the ability of the scientist to control phenomena. Mach considered science important primarily for its importance in controlling nature for human purposes. Historian Philip Pauly has argued that despite the incongruities that it might seemingly encompass (the fact that Mach was often held up as an idealist and anti-materialist, especially by Marxists), Mach was a much greater influence on the form of Mechanistic thinking that Loeb developed than has been previously realized. Mach was a major influence on what Pauly calls Loeb's 'engineering ideal in biology' (ibid.).

It was only after his emigration to the United States in 1891—to teach first at Bryn Mawr College (where he met Thomas Hunt Morgan, with whom he was to maintain a lifelong friendship), moving subsequently to the Universities of Chicago and California (Berkeley), and finally the Rockefeller Institute in New York—that Loeb's mechanistic philosophy became explicit and took its most concrete shape. Although Loeb was ultimately to give up on the application of engineering in the areas of his own greatest interest, animal behavior and physiology, that work had been a major motivating force early on for the development of his mechanistic philosophy. Loeb's research on artificial parthenogenesis (the development of an unfertilized egg into an adult organism) provides a useful illustration of what 'mechanism' meant as he developed it in the early twentieth century.

3.2. Experiments on artificial parthenogenesis

No process in late nineteenth or early twentieth-century biology remained more of a bastion for metaphysical explanations than that of fertilization of the egg by a sperm and the subsequent developmental events that it triggered. Once the sperm has penetrated the egg, a cascade of events take place, rapidly in most cases, leading to the first cleavage, in which the single-celled zygote becomes a two-celled embryo (the two cells are referred to as blastomeres). This remarkable series of events raised many questions: How does the entrance of sperm into the egg cytoplasm initiate division? Does the point of penetration determine the plane of division, and if so how does the plane of the first division affect the future axis (anterior–posterior, dorsal–ventral) of the organism? What role, if any, does the egg cytoplasm play in the course of development? And, of course, the question of questions: How does the egg end up giving rise to so many different cell types during the process of embryonic differentiation?

The most serious attempt to deal with some of this cluster of questions in a rigorous, experimental way prior to Loeb had come from the work of Wilhelm Roux and Hans Driesch between 1888 and 1892. Their focus had been on the nature of differentiation. Roux championed a mechanistic view known as the mosaic model (or the Roux–Weismann mosaic theory, as it was known, since the same idea had also been proposed by the cytologist and evolutionary theorist August Weismann) that claimed with each cell division hereditary units were parceled out in such a way that each cell generation received increasingly specialized particles; by the time differentiation was complete each cell type (muscle, nerve, skin) contained only the particles determining that cell's specific characteristics. One prediction from this hypothesis was that if one of the first two blastomeres was killed or removed, the results would be 'half-larvae', since already half the determiners were parceled out into each daughter cell. Roux tested this prediction by puncturing one of the first two blastomeres of the frog egg with a hot needle and raising the subsequent developing larvae as far as they could develop. The results fitted the predictions: Roux got some half-embryos (the majority died from infections in the cultures) developing through at least the gastrula stage. Differentiation could thus be described as a mechanical process that resulted in an adult mosaic organism, with each cell type containing only the active determiners for its special characteristics. From this work Roux developed an entire research program, known as *Entwicklungsmechanik*, complete with its own journal (*Archiv für Entwicklungsmechanik der Organismen*), which he edited. Roux's *Archiv* became a major organ for expounding his own version of the Mechanistic view.

Roux's results did not go unchallenged for long. In 1891 a young German embryologist, Hans Driesch, working at the Stazione Zoologica in Naples, carried out a series of experiments similar to Roux's. Using the sea urchins that were so plentiful in the Bay of Naples, Driesch separated the first two blastomeres (instead of killing one) of the sea urchin embryo by vigorously shaking. If Roux's mosaic principle were true, Driesch predicted that he, too, should get half-embryos. In fact, however, he found that the separated sea urchin blastomeres each developed into a complete, though smaller-than-average sized embryo. Indeed, many of the embryos developed to at least the larval stage known as a Pluteus. Driesch interpreted the results as counteracting Roux's strictly mechanical model. No machine could reconstruct the whole out of individual parts. The sea urchin embryo acted as what Driesch called a 'harmonious equipotential system', and for the next seven years he carried out experiments to study the characteristics of self-regulation and adjustment to altered conditions in embryonic development. Eventually, by the early 1900s, however, he despaired of finding a Mechanistic, physico-chemical solution to the problem, and adopted an increasingly vitalistic interpretation. He claimed that embryonic development was guided by an 'entelechy', an organizing, directive force that consumed no energy, was immaterial, but was the factor that distinguished living from non-living matter. Eventually, Driesch abandoned experimental biology altogether for philosophy.

Although a strong proponent of Roux's *Entwicklungsmechanik* program, Loeb at first saw no contradiction between Driesch's results and a Mechanistic interpretation. He, too, had experimented with sea urchin eggs at Woods Hole, and found that fragments of embryos, or even eggs, could be stimulated to produce full embryos under the right conditions (Pauly, 1987, p. 95). It was when Driesch declared himself an unabashed vitalist that Loeb saw an unbridgeable gap in their work. To Loeb, vitalism was a form of non-materialist philosophy, an unacceptable metaphysics that served no function other than curtailing research. Driesch's own career was proof enough of the dead end to which vitalistic thinking could lead.

Loeb chose to focus on the problem of fertilization, rather than later events surrounding axial formation or differentiation. Nothing could have been more basic to the distinction between living and non-living matter than this moment in the initiation of a new life. It was also known, however, that under natural conditions, the eggs of some species (ants, bees, wasps, aphids) undergo normal development without being fertilized, a process known as parthenogenesis (Loeb, 1986 [1900]). The question Loeb posed was: could cleavage be induced in unfertilized eggs by known chemical or physical agents? If that were possible, Loeb reasoned, it could lead to an investigation of the specific chemical and physical factors that are involved in normal fertilization. He had begun experiments with sea urchin eggs while working at the Stazione Zoologica in Naples in 1892 (where Driesch was still in residence), and took up the problem again at the Marine Biological Laboratory in Woods Hole after his arrival in the United States.

Loeb's experiments involved placing sea urchin eggs in sea water of varying osmolarities (concentration of ions), and of varying types of salt combinations (sodium, calcium, potassium and magnesium chloride). The ideas behind these experiments came from Sachs, who had studied the effects of varying salt concentrations on plant processes such as rates of water uptake, transpiration and photosynthesis, and from the physical chemist Svante Arrhenius, whose theoretical studies of ionization convinced Loeb that biological phenomena were controlled by precise ionic concentrations.[4] Living phenomena such as fertilization thus had to be approached from the standpoint of physical chemistry, and not from morphological methods such as those of traditional cytology. Loeb's relationship with embryologist and later *Drosophila* geneticist T. H. Morgan (beginning at Bryn Mawr) was important here, too, for Morgan had also spent part of a year working with Driesch at the Naples Station on the effects of various physical and chemical conditions on the course of early development. Morgan was particularly interested in how experimentally induced changes in early embryogenesis manifested themselves as 'pathologies' in the later embryo. Studying the pathology and its causes would thus illuminate the normal process. While Loeb also saw the value of studying the 'abnormal' in order to understand the 'normal', his deeper motivation lay in replicating that normal process by known physico-chemical agents. His model here was the work of Lavoisier and Laplace on animal respiration, to which he referred in his 1911 talk to the Monists: '[Theirs] was the first attempt to reduce a life phenomenon, namely, the formation of animal heat, completely to physico-chemical terms' (Loeb, 1964a, [1912], p. 6).

When Loeb placed unfertilized sea urchin eggs in hypertonic sea water (water containing a higher concentration of ions than that found in the egg cytoplasm or in normal sea water), he found that on returning them to normal sea water, the eggs started to divide (though at first none went beyond early blastula stage) (Loeb, 1900, pp. 326–327). Excited by this discovery, Loeb attempted to isolate which factors were responsible for the initiation of cleavage and to see if he could get development to proceed to the Pluteus larva stage. He first initiated a series of experiments in which he systematically varied the concentrations of solutes (starting with magnesium chloride, followed by potassium, calcium and sodium, then on to sugar and urea) while holding the concentration of the others constant. This series was followed by another in which he varied the time of immersion of the eggs.

[4] Loeb and Arrhenius were both at Würzburg in 1886–1887, but it is not clear that they met at this time. However, it is unlikely that Loeb did not at least learn of Arrhenius's work at the time through Sachs, and they did become fast friends after Loeb's emigration to the United States (Pauly, 1987, p. 108).

Finally, Loeb was able to come up with both the proper mixture and concentration of ions (60 cc of 2.5 N $MgCl_2$, and 40 cc normal sea water) and immersion time (2 hours) that would yield not only a large number of cleavages, but also would support development through the Pluteus stage (the furthest that development, even with normal fertilization, could be carried in the laboratory) (ibid., p. 329).

Loeb was excited by his results: he had mimicked a 'vital' process by adjusting ionic concentrations and had produced normal, though 'fatherless' sea urchins. As he wrote: 'I consider the chief value of the experiments ... to be the fact that they transfer the problem of fertilization from the realm of morphology into the realm of physical chemistry' (ibid., p. 332). Others soon showed that purely physical stimuli, such as pricking the egg with a needle, could also initiate parthenogenesis. These were all positive confirmations that life could be understood by employing the concepts of physics and chemistry, and testing them empirically by controlled experiments. Loeb's interest in this work was not in finding the operative *mechanism* by which fertilization actually triggered development, but rather in demonstrating that living processes followed the laws of physics and chemistry, and through proper experimental investigation could be brought under human *control*. Operative mechanisms might be interesting for someone to work out in detail, but that was not Loeb's primary focus. He was not so much motivated to reduce fertilization to one specific physico-chemical process (though he would have been interested in any such attempt). Rather, his interest was to demonstrate that the process was not beyond investigation by physico-chemical means, and most important, not beyond manipulation through experimentation. Science represented progress for Loeb precisely because it led to control, to the engineering of nature. As he said in 1903:

> We cannot allow any barrier to stand in the path of our complete control and thereby understanding of the life phenomena. I believe that anyone will reach the same view who considers the *control* of natural phenomena as the essential problem of scientific research. (Loeb, 1903, p. 25, quoted in Pauly, 1987, p. 114)

Control of nature could only be accomplished, in Loeb's view, by banishing all those medievalisms that had held humanity back for so long: superstition, mysticism, metaphysics and the belief in vitalistic, non-physical, non-chemical forces. The 'mechanistic conception of life' represented this confluence of ideologies that guided Loeb throughout most of his career. For his work on parthenogenesis, Loeb was nominated in 1901 by over a hundred individuals in ten countries for the first Nobel Prize in physiology or medicine (Fleming, 1964, p. xxiv; Pauly, 1987, p. 101).

By the time he came to Rockefeller University in 1910, Loeb's program in mechanistic science had solidified, and was one from which he would not retreat. For the remainder of his career (he died in 1924) Loeb focused more and more of his attention on the properties of colloids, particularly those of large molecules such as proteins or 'nucleins' (i.e. nucleic acids), which he felt held the key to life at the physico-chemical level (Pauly, 1987, p. 116). The nucleic acids, like some proteins, were capable of autocatalysis—that is, they seemed to act like enzymes for their own synthesis. No scientist, Loeb argued, could claim to have really created life until he (or she) produced matter that could organize, repair and reproduce itself by nuclein-generated autocatalysis. The 'secret of life', if there was one, was to bring together just the right combination of molecules, and to organize them in such a way that they could continually re-generate themselves. Reproduction was just a highly organized form of autocatalysis. Loeb was not a naive mechanist in the sense that he thought organisms were truly machines or merely chemical factories, such as some of the more blatant nineteenth-century mechanists such as Jakob Moleschott, who claimed that the brain secretes thought like the kidneys secrete urine (Nordenskiold, 1928, p. 450). In later works such as *The organism as a whole* (1916), Loeb conceded that organisms acted in coordinated, even purposeful ways, but still claimed these more complex processes could be best understood by analytical methods. He knew the machine imagery had its limitations. But pragmatically, he argued that the only way to proceed with knowledge that would lead to control over nature was through the Mechanistic approach.

3.3. Loeb and reductionism

Loeb's Mechanistic view was cast in the reductionist language of physics and chemistry. Loeb's form of reductionism sought to explain complex processes by examining their increasingly lower levels of organization. What Loeb meant by reductionism (and he did use the term) was that to trace higher level processes was down to particular physical and chemical reactions. Implicitly, this meant seeking to understand life in terms of atoms, molecules and their interactions. As he wrote to William James in 1888 regarding the study of perception of light by organisms: 'Whatever appear to us as innervations, sensations, psychic phenomena, as they are called, I seek to conceive through reducing them—in the sense of modern physics—to the molecular or atomic structure of the protoplasm, which acts in a way that is similar to (for example) the molecular structure of an optically active crystal' (Pauly, 1987, p. 38). Pauly has argued that Loeb recognized that organisms were able to do all the sorts of things they do because of a high degree of organization of their parts (cells, cell organelles, molecules and atoms). But because these processes were so complex, the only practical approach was to stick to the reductionist program that had proved so successful in the physical sciences. While at the University of Chicago, colleagues such as John Dewey and Charles Otis Whitman (Department chair and also Director of the MBL) urged Loeb to be more explicit about his Mechanistic and reductionist philosophy. He steadfastly refused, however, to say much more than to advocate such an approach as the only way for the life sciences to shake off the onus of being merely a descriptive, speculative, 'soft' science. Loeb's commitment to Mechanism was part of his larger program to make biology a hard science, and thus to professionalize it as an equal to the physical sciences. The new biologists of the early twentieth century were not gentlemen naturalists, but hard-nosed materialists who sought to understand the principles of living systems in order to bring them under human control.

4. Mechanism in the context of early twentieth-century holistic biology

Partly in response to Roux's program for *Entwicklungsmechanik*, and reinforced by aggressive proselytizing by Loeb and other Mechanists, a movement toward more holistic approaches in biology gained a certain currency in Europe and the United States by the 1920s. A variety of historians and philosophers of science have provided very thoughtful and comprehensive introductions to the holistic movement in the early twentieth century, so that little needs to be done here except to summarize some of their general conclusions and suggest how holism was viewed by the biological community, especially in the United States, much of which was already committed by 1900 to a Mechanistic view of life (Harrington, 1996; Ash, 1995; Gilbert & Sarkar, 2000).

4.1. Faces of holistic biology, 1900–1935

Historian Anne Harrington has suggested the variety of ways in which 'holism' in biology in the early twentieth century grew out of not only reaction to the mechanism of Roux, Loeb and others, but also as a reaction to the cultural fragmentation associated with 'modernism' (especially with Darwinism), World War I, urbanization and industrialization and the perceived increasing mechanization of everyday life embodied, for example, in Charlie Chaplin's 1936 feature, *Modern times*. The machine became the symbol of the impersonal society that had emerged with the industrial state, corporate and banking conglomerates (the Carnegies, Krupps, Morgans and Rothschilds) and increasingly bitter (and violent) labor-capital conflicts. The fragmentation was most painfully obvious in Germany following the harsh political and financial terms forced on it by the Versailles Treaty. In combination with disillusionment and embittered national pride, Germany produced a romanticized nationalism and nostalgia for a past when it was believed that humans were more harmoniously integrated into the natural world (Harrington, 1996, pp. 19–33). Thus the holistic view was intimately tied up with a variety of scientific and cultural trends that set it against the Mechanistic world view that many thought had led to 'modernism' and an undermining of morality, the social fabric and 'man's place in nature'.

In the period between 1900 and 1935, the holistic views included a wide spectrum of approaches with widely differing views on both the philosophical meanings of 'holism' and the practical applications of holistic thinking to scientific research programs. Driesch was, of course, the major and most extreme exponent, but there were many others, including Jakob von Uexküll, Ludwig von Bertalanffy and Henri Bergson. In one way or another each of these individuals advocated an anti-mechanistic, holistic approach, replacing the machine analogy with an organicism that repudiated the reduction of all living processes to simply matter in motion (Harrington, 1996). So what, they declared, if ions in solution in the cell cytoplasm, or in blood plasma obeyed Arrhenius's laws of electrolytic dissociation. That was not what made an organism 'alive'. What was important was to develop an insight into the wholeness of the organic process, to interact with the organism on its own terms, as a functioning whole, not a mosaic of separate parts. Thus, von Uexküll, a confirmed holist with a strong vitalistic tinge, expressed this view unambiguously in a tribute to Driesch's concept of the embryo as a 'harmonious equipotential system':

> Driesch succeeded in proving that the germ cell does not possess a trace of machine-like structure, but consists throughout of equivalent parts. With that fell the dogma that the organism is only a machine. Even if life occurs in the fully organized creature in a machine-like way, the organization of the structureless germ into a complicated structure is a power *sui generis*, which is found only in living things and stands without analogy. . . . It is not to be denied that vitalists are the victors all along the line. (Quoted in Harrington, 1996, p. 51)

It was against this sort of claim that Loeb's mechanistic worldview was directed. He saw such claims as mystical nonsense, introducing into biology a non-scientific metaphysics that he (Loeb) had been struggling since his student days to eradicate. It represented for him a resurgence of German romanticism, a new *Naturphilosophie* that was not only philosophically backward-looking but from a pragmatic point of view had no significant research potential. As a result of this 'romantic climate' in biology, Loeb often took an even more extreme position than he otherwise might have taken. Driving (and keeping) metaphysics out of biology required adopting an uncompromising, hardened Mechanistic line.

So powerful were the forces of mechanistic thinking in establishing the 'new biology' that attempts at holistic alternatives, even when expressed in materialistic, non-mystical ways, were easily misunderstood or viewed with considerable suspicion. An example will illustrate this point, and also provide a contrasting approach to biological processes that ultimately had a significant impact in its own right: the work of embryologist Hans Spemann on embryonic induction.

4.2. Hans Spemann, embryonic induction and the 'organizer' concept

Although appreciating Driesch's motivations to treat the embryo holistically, Spemann could not agree with his overtly vitalistic approach. Trained at Würzburg in descriptive morphology, Spemann was greatly influenced while a faculty member there by embryologist and cytologist Theodor Boveri. Spemann took up experimental embryology in the late 1890s, eventually publishing a series of investigations on the differentiation of the eye lens in the vertebrate (frog) embryo. Spemann showed that a necessary step in the differentiation of head ectoderm tissue into the lens was contact with the underlying optic vesicle, an outgrowth of the anterior portion of the neural tube (itself derived from ectodermal tissue known as the neural plate). Spemann showed experimentally that if the optic vesicle were removed, the overlying ectoderm would not differentiate into a lens; conversely, if the optic vesicle were transplanted from the anterior to the flank or posterior region of the embryo, it would produce lenses in these otherwise eyeless regions. To this phenomenon Spemann eventually gave the name *induction*. As a materialist Spemann saw that induction required physical contact between the inducing and the inducible tissue: there was no 'action at a distance'. Although the process proved to vary considerably among different vertebrates, Spemann nevertheless saw it as a paradigm for how differentiation could be viewed as a series of inductive cascades of increasingly greater specificity (Hamburger, 1988, p. 18).

In this light Spemann, along with his graduate student Hilde Proescholdt, took the process of induction back to earlier embryonic stages, notably to the gastrula stage after invagination has created the blastopore (where the ball of cells that formed the blastula has pushed inward to form the two-layered gastrula—much like pushing your finger into a balloon). The gastrula in vertebrates soon develops its anterior–posterior axis and dorsal–ventral surfaces, which establishes the entire body plan for the organism. The question was: is there a master inductive sequence established by some portion of the gastrula that begins the axial differentiation? Proescholdt (who later married Spemann's first assistant, Otto Mangold), transplanted various regions of the early gastrula into the belly (that is, inside the developing gut region) of an older embryo. One tissue region, known as the dorsal lip of the blastopore, proved to have profound effects: it appeared to be, in effect, a master inducer. When transplanted into the belly of the host, dorsal lip tissue organized its own cells along with some of the host's cells to form a whole secondary embryo. Spemann eventually called the dorsal lip region the 'organizer', and it became the focus of work in his laboratory for over a decade (1924–1935). Ultimately, for this work Spemann was awarded the 1935 Nobel Prize in Physiology or Medicine (Hamburger, 1985).[5]

Although both Spemann and Loeb were ardent experimentalists, Spemann focused at the tissue and organ-system level of development, whereas Loeb focused at the cellular level eventually moving down to that of the colloidal molecule. Most important, Spemann did not believe, or organize his research program around, the necessity of reducing induction to any lower level of organization than the one at which he worked, tissues and organ-systems. Indeed, Spemann felt that induction could only be understood as a tissue-level process. The emergent property of induction itself would be lost if studied only at the cellular or molecular level. Spemann's view was very much in congruence with Driesch's concept of the embryo as a 'harmonious equipotential system' (a term that Spemann himself adopted and used). Although Spemann consciously took a more holistic or organicist approach, Spemann was not a vitalist; he thought that induction was a process based on interaction of material components—cells, tissues and organs—and that while molecules most certainly were involved, reduction to the molecular level would not reveal the essence of the process itself. For Spemann, the interesting processes took place at a higher level of organization, and had to be studied at those levels in order to reveal their workings.

Spemann had even tried once, rather half-heartedly, to take a reductionist approach and use heat-and alcohol-treated dorsal lip as an inducer to determine whether the process required living tissue or not. He got ambiguous results: out of a number of transplants there was only a single successful induction, and that one

[5] Hilde Mangold was killed in a tragic explosion from a gasoline heater in her kitchen in 1924, just as the first and most famous paper on the organizer effect was being published. According to Viktor Hamburger, who knew her well as a fellow graduate student under Spemann, her death deprived the scientific world of one of its most skilled and thoughtful investigators (Hamburger, 1984).

could be explained away as the result of an incompletely killed dorsal lip transplant. Spemann never followed up, but his own former graduate student, Johannes Holtfreter, did (Holtfreter, 1933). Using a classical reductionist approach, Holtfreter used ground-up, or heat-and alcohol-treated dorsal lip tissue as inducer. Because he controlled carefully for contamination and infection of the cultures, Holtfreter found that dead tissue was still able to induce a complete secondary embryo. Hamburger, who corresponded with both Spemann and Holtfreter, claimed that Spemann was clearly interested in, but troubled by these findings (Hamburger, 1988, pp. 96–99). He did not doubt the empirical results, but it was foreign to his way of thinking to pursue a line of investigation that was so reductionist. To him the essence of the organizer was to be found in its wholeness. While Spemann did not doubt that the organizer operated by some sort of chemical or physical process, his focus was on questions at higher levels of organization in the living embryo: effects of organizer tissue of different ages, or host embryos at different stages of development, or the ability of organizer from one species to induce secondary embryos in another species. Put simply, Spemann always sought to work upward from the tissue (dorsal lip) level to higher levels of organization (the induction of body axes and organ-systems) while Loeb sought to push downward from the cell to the colloidal suspension and ultimately to atoms and ions.

Part of the misunderstanding about Spemann's views in these matters is undoubtedly due to his own failure to express clearly and explicitly how his holism/organicism differed from the vitalism of Driesch or von Uexküll, who were such prominent exponents of the non-materialist, non-Mechanistic approach at the time. In evaluating Driesch's dilemma (i.e. that in recognizing the embryo's holistic qualities it became impossible to study development scientifically), Spemann wrote in 1938 that although Driesch had erred in believing that because simple mechanical, reductionist approaches could never solve the problem of embryonic development, he was still right to see the embryo as a 'harmonious equipotential system' (Spemann, 1938, p. 347). At the same time, Spemann would not go all the way with Driesch and postulate non-material causal factors such as an 'Entelechy', or renounce ultimately all physico-chemical and causal explanations. Experimental analysis could be used to understand the functioning of the harmonious equipotential system, but the embryo still had to be studied as a whole. It was not necessary to postulate an unknowable 'vital force', supra-organismic directive agency or teleological processes to carry out a causal analysis of development.

The source of most views that Spemann was a 'vitalist' comes from the final page of the book *Embryonic development and induction*, his Silliman Lectures at Yale, published in 1938. In this passage, Spemann stated that the

> processes of development, like all vital processes, are comparable, in the way they are connected, to nothing we know in such a degree as to those vital processes of which we have the most intimate knowledge, viz., the psychical ones. . . . even laying aside all philosophical conclusions, merely for the interest of exact research, we ought not to miss the chance given to us by our position between the two worlds. (Ibid., p. 372)

The problematic use of terms such as 'vital' and 'psychical processes' (which clearly could be construed as non-material and thus vitalistic) has led to confusion among many readers. Hamburger, however, saw through these terms to their materialist (if not Mechanistic) basis.

> [Spemann's comments] do not bear out the claim that his interpretation of regulation and of harmonious systems has a vitalistic aspect. On the contrary, he deserved credit for being one of the first to point out the weakness of Driesch's argument and to have shown that the phenomena of regulation and of the harmonious-equipotential system are accessible to experimental analysis. (Hamburger, 1988, p. 67)

Thus, seeing 'wholes' did not have to invoke mysticism, but it did have to be accompanied by the appropriate experimental methodologies. For Spemann this was the transplantation experiment at the tissue and presumptive organ level, technical operations at which Spemann and his students were exceptionally adept. But he was never interested in the reductionistic—that is, biochemical or genetic—functions, or levels of organization, by which the dorsal lip exerted its effect. In this sense he was content to have a reproducible experimental system that gave him the power to analyze causal relationships in tissue organization and differentiation, and at those levels to develop testable hypotheses.

The interesting lesson from this brief examination of Spemann in relation to the development of mechanistic philosophy, however, is the degree to which, at the time, any holistic approach might be easily seen as a step toward vitalism. As a result, many biologists failed to see holistic approaches (even those that were consciously materialistic and causal–analytical) as fruitful avenues for research. For example, although an embryologist by training, T. H. Morgan paid relatively little attention to Spemann's work in his later writings, even those about embryology. Morgan was even ill-at-ease with aspects of Loeb's *Organism as a whole*, claiming that some remarks in the book would 'be welcomed by just such people who would like to believe that . . . the more fundamental problems of nature are quite untouched by mechanistic principles' (he was referring here to some on Loeb's discussions of Mendelian genetics) (Pauly, 1987, p. 137). It was in this climate that mechanistic materialism was being developed in the early twentieth century, and it is in part this 'climate' that often pushed proponents of Mechanism toward more extreme and hardened claims. At the time, any nod toward holistic thinking was seen as a slippery slope that would lead inevitably to metaphysics and vitalism.

5. Conclusion

In this paper I have tried to characterize the 'mechanistic conception of life' that gained a considerable following in the life sciences during the early years of the twentieth century and distinguish it from contemporaneous holistic or organicist positions. Loeb's Mechanism drew heavily on materialist thories from the nineteenth century: the atomistic, and mosaic nature of matter (including living matter), the analytical (and to varying degrees reductionistic) method for understanding complex systems, commitment to experimental methodology, and the deeply-held belief that all living processes could be understood in terms of the known laws of physics and chemistry. The causes of living phenomena were to be sought at the most fundamental level of the organization of matter, molecules and atoms.

I have also tried to suggest that the intellectual and social context in which an explicitly held Mechanistic research program served several different aims in the development of early twentieth-century biology. (1) It provided a way in which biology could dissociate itself from the earlier tradition of natural history that focused too much on descriptive methods and speculative theories such as reconstructing evolutionary histories of various taxa. True science, younger biologists argued, looked not at hypothetical historical causes but at immediate proximate causes in terms of the material, knowable components of the process order investigation. This was, of course, the model of 'true' science exemplified by physics and chemistry. (2) Mechanistic science provided the basis for the growing attempt to professionalize biology and place it on the same rigorous and analytical footing as the physical sciences. Clearly, one component of that professionalization was to incorporate Mechanistic worldview and experimental/quantitative methods, which had advanced the explanatory power of physics and chemistry in the nineteenth century, but also their ability to predict and control nature. (3) By generating its own nemesis—that is, the new vitalism of Driesch and von Uexküll—the Mechanistic view became hardened, especially in the hands of proselytizers such as Loeb, into a strong program that self-consciously opposed any alternative organicist or holistic views—seeing them all as quasi-metaphysical and an experimental dead end. Thus, the form that Mechanistic thinking took in the early twentieth century reflected as much its context as its self-contained epistemology and consequently differed from earlier (eighteenth and nineteenth-century) mechanistic traditions. It was physico-chemical not merely mechanical, and with the growth of organic and physical chemistry in biology it emphasized reducing life processes to colloids, molecules and atoms.

For younger biologists, especially in the United States and England, the simplifications involved in mechanistic thinking, while to a certain degree limiting to a full understanding of organismic complexity, were often useful if the aim was to control the organism for a particular, narrow function (in genetics to breed for a certain trait, or in psychology to condition and control a certain behavior). While organisms are not machines, within narrow limits they can be forced to function like machines. So, for many younger biologists the Mechanistic approach had a number of attractive dimensions, which gave it considerable prominence in the early decades of the century. For many, it remained the rallying cry of the 'new biology', rescued from what were considered the doldrums of the old-fashioned natural history, riddled with speculative and non-testable hypotheses, and often lacking much utilitarian value beyond construction of putative taxonomic and phylogenetic relationships. Only by becoming like physics and chemistry could biology move on to a new level and take its place among the legitimate sciences.

Today, with the advent of computer technologies for handling large quantities of data, with the intellectual advent of systems theory, bioinformatics and 'complexity', the older mechanistic philosophy of someone such as Loeb may seem crude and oversimplistic. Holistic thinking is no longer so suspect in a variety of fields, from genomics (Commoner, 2002), to physiology, to population and developmental biology (Gilbert & Sarkar, 2000). Even one of the most widely used introductory college biology textbooks has incorporated discussions of the concepts of levels of organization and emergent properties in its early chapters, a theme that is maintained throughout the book to good effect (Campbell & Reece, 2002, pp. 2–4). As biology has begun to supplant physics as the pre-eminent science of the day, its philosophical underpinnings are not only becoming more sophisticated, but also less rigid. There is thus a greater need and opportunity for philosophy and biology to form a more interactive partnership, at the research bench and in the classroom, than ever before. It is my hope that through collective, interdisciplinary efforts, such as the conference that gave rise to this issue, such interactions will become increasingly frequent in the years ahead.

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