‘The Memory of Life Itself’:

Bénard’s Cells and the Cinematography of Self-Organization

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Abstract

In 1900, the physicist Henri Bénard exhibited the spontaneous formation of cells in a layer of liquid heated from below. Six or seven decades later, drastic reinterpretations of this experiment formed an important component of “chaos theory”. This paper therefore is an attempt at writing the history of this experiment, its long neglect and its rediscovery. It examines Bénard’s experiments from three different perspectives. First, his results are viewed in the light of the relation between experimental and mathematical approaches in fluid mechanics, leading to a re-examination of the long-term reception of Bénard’s results among fluid dynamicists up to the chaos craze, whereby the traditional emphasis placed on mathematical physics is counterbalanced by greater attention to experimental approaches. Second, we focus on Bénard’s own way of using his results as analogies that could help grasp something about the reason why inorganic matter may structure itself in ways reminiscent of living forms. This is shown to resonate strongly with Prigogine’s work in the 1960s and 1970s. Third, Bénard’s adoption of the cinematograph as his preferred experimental instrument is interpreted as having reinforced his long misunderstood belief that he had exhibited a form of self-organization essential to the understanding of life.

Keywords: Bénard cells, Poincaré, chaos, fluid mechanics, cinematograph, self-organization.

1. Introduction

Among the invisible things instrumental and mathematical technologies promised to unveil at the beginning of the twentieth century was “self-organization.” In 1900, the young physicist Henri Bénard set up an experiment at the Collège de France in Paris to study the dynamical behavior of a thin layer of liquid, about a millimeter deep, heated from below. Experimenting with liquids of various viscosities, he invariably observed the formation of small cells that tended to stabilize in the shape of hexagons after a short period of instability. Although this struck Bénard as being reminiscent of phenomena characteristic of the living world, interest in the result quickly subsided. More than seven decades later, this simple experiment would become a stepping stone of chaos theory and indeed of all attempts to deal with self-organization and the spontaneous emergence of order. “It is interesting to note,” some physicists recently wrote, “that Bénard’s work was not broadly known prior to this 1970’s explosion, although a limited community had continued to work on the subject, without major new discoveries.”[1]

How should one write the history of neglect? In the history of science, countless are the instances of intuitions not followed, of intriguing phenomena explained away by flawed reasoning, and of promising but short-lived encounters between different scientific approaches. When several decades or centuries later these instances regain relevance, their neglect suddenly becomes conspicuous, and in need of explanation. Traditional teleological accounts of neglect such as the rehabilitation of unsung scientific genius fallen into unjust oblivion are now greeted with salutary suspicion. Indeed, one could argue that it is only in hindsight that those initial instances become significant such that the question of their neglect hardly arises: what needs to be explained is instead the later crystallisation that conferred new meanings to them.

Yet, it sometimes occurs that particular historical configurations do indeed prefigure in significant ways the ones that will subsequently, and sometimes after much delay, be built on those bases. Time seems to have been wrapped around and folded onto itself; an earlier epoch appears to be directly providing meaningful resources for people living and working much later, while the interpretations of the earlier episodes are significantly modified by later events; the least one could claim is that both epochs seem to be "resonating" with one another.[2] In such cases, the task of social historians of science, who must never cease to insist on accounting for historical development at the local level, becomes more difficult. In my view, the historiography of neglect has to pay attention to three aspects: the initial configuration, the later period crystallisation, and modes of transmission between them. Historians' attention need to be drawn to both initial and final configurations *on their own terms, but also in the light of each other*. If they can be found, roads travelled only by the few, which connect both periods, have to be mapped out in details. Historians need to account for the fact that these roads failed to draw a crowd, as well as for why they were nonetheless kept open. And when the later blossoming of a research area does indeed echo an earlier configuration, historians can identify with greater precision the resources that were ready to be mobilized by a new generation of scientists.

In recent time, chaos has been a scientific field where historiographical problems of this kind have sprung up repeatedly. When around 1975–80 the mathematical theory of dynamical systems was adopted by a few physicists as a convenient language in which to cast some of their results, a great wave of excitement ensued.[3] People from many different fields (fluid mechanics, population dynamics, meteorology, solid-states physics, etc.) were mobilized in an intricate process of disciplinary convergence. As a result of this movement, whose long-term consequences might have been more limited than enthusiasts may then have thought, some important conceptual reconfiguration nonetheless occurred. While some hailed chaos as a new scientific revolution—the third of the century after relativity and quantum mechanics, it was sometimes claimed—others pointed out that it had first been exhibited towards the end of nineteenth or the beginning of the twentieth century.[4] This singular history has intrigued actors and debates have raged among commentators ever since. Most however agreed on one point—namely, that the revisitation of many parts of Henri Poincaré's work (his memoirs on curved defined by differential equations, his study of the three-body problem in celestial mechanics, his pioneering work in dynamical systems theory and topology, his contributions to ergodic theory, and so on) played crucial parts in the emergence of chaos in the mid-1970s. The recovery of Poincaré's role in the history of chaos is another example for the history of neglect I want to explore here.

While it is no doubt true that Poincaré's work foreshadowed concerns and introduced key concepts and methods used in chaos theory, one is hard pressed to explain why the great burst of activity only took place several decades after his death (Aubin & Dahan Dalmedico, 2002). This gave rise to various attempts at accounting for the "nontreatment" of chaos over several decades, most systematically explored by the philosopher Stephen Kellert (1993, see also Lorenz, 1993, p. 125). In traditional interpretations, it is said that Poincaré's work simply was forgotten, which explains why its legacy was not maintained. There were two main reasons why accounts of neglect in terms of forgetting have been unsatisfying: because they eschewed the admittedly arduous task of re-examining the coherence of the large parts Poincaré's lifework related to modern dynamical systems theory and because such accounts constantly downplayed the myriads of roads along which various parts of his heritage was transmitted to modern 'chaologists' (Aubin & Dahan Dalmedico 2002, pp. 279–82)."[5]

Now, looking at Poincaré's work through chaotic lenses has deeply modified the view we have of his work. Emphasizing the material culture in which his contributions to physics and celestial mechanics took place, Peter Galison (2003) for example reaffirmed older views according to which Poincaré's intents were far from revolutionary. Instead of questioning Newtonian physics, Poincaré wished to fill in the blanks still left out of the picture. To this end, he developed new conceptual tools (such as dynamical systems theory) that would help to unveil fundamental, structural relationships between things. There thus was a form of "optimistic modernism" (Galison 2003, p. 74–5) in Poincaré's conventionalism, which, I contend, could only be properly understood from the post-chaos point of view.

In a similar vein, the convergences and reconfigurations connected with the fashion for chaos can give rise to new insights about the relationships between physical laws, mathematical and instrumental tools, and the phenomenon of life at the time of Bénard.[6] As Evelyn Fox Keller (2002) has recently agued, throughout the twentieth century, physical and mathematical scientists used different types of approaches to try to "explain" life. Focusing on the physical chemist Stéphane Leduc, Keller has in particular brought to light a fascinating scientific milieu where mimetic experiments with nonliving materials were enrolled in support of the materialistic conception of life. From her account, it clearly appears that, like instability in dynamical systems, the new understanding of life as a dynamic process of self-organization that emerged in the 1970s both benefits from taking a look back at work done around 1900 and modifies the way we may take that look.

In the past decades, "selforganization" imposed itself with great ideological force and at the expanse of much historical abuse (Paslack & Knost 1990; Dumouchel & Dupuy, 1983). Loosely grouped with other striking-sounding labels (cybernetics, chaos, emergence, complexity theories—the list could go on almost indefinitely), self-organization was supposed to account for the spontaneous emergence of forms in inanimate matter, one of the major tenets of the materialistic conception of life. While one can find contemporary uses of the term, in the sense of the organization of the self (Wright, 1908, p. 613), it clearly is anachronistic to say that making self-organization visible can capture Bénard's ambition when he presented his work on *tourbillons cellulaires*, or "cellular vortices," around 1900. To unpack the cultural environment in which Bénard performed his experiments seems to me to deepen our understanding of the relations between the physical sciences and the life sciences in the first decades of the twentieth century. By associating the self-organization of nonorganic matter with the phenomenon of life and by using the dynamical tool by excellence—the cinema—to analyze it, Bénard's vision is no less premonitory than Poincaré's. In this sense, we can now reclaim for Bénard's experiments a significant place in the reinvention of reality that occurred around 1900.

In the following, I shall approach Bénard's experiments from three different perspectives. First, I want to examine carefully his results in the light of the problematic relation between experimental results and their mathematical exploitations, within the domain of fluid mechanics. This leads us to a re-examination of the long-term reception of Bénard's results among fluid dynamicists up to the chaos craze, whereby the traditional emphasis placed on mathematical physics and Poincaré is counterbalanced by greater attention to experimental approaches. Second, I want to underscore Bénard's use of analogies to grasp something about the way inorganic matter may structure itself in ways that are reminiscent of living forms. I will contend that those analogies informed Bénard's later adoption of the cinematograph as his preferred experimental instrument. This technical choice provides the third perspective I want to adopt here with the aim of showing that the use of the cinema reinforced Bénard's long misunderstood belief, which admittedly he himself never fully developed, namely that he had exhibited a form of self-organization essential to the understanding of life.

2. Mathematics and instruments in Bénard's experiment

In his scientific biography of Bénard, the physicist Eduardo Wesfreid pointed out that the jury's report for Bénard's doctoral dissertation was not free from criticisms. His results were deemed not susceptible to add much to current knowledge, apparently because he had put too little emphasis on the theoretical explanation of his experimentally-derived laws.[7] Acknowledging that Bénard's mathematics seemed clumsy, positivist philosophers found enough justification for his method in the beauty of the facts experimentally uncovered (Walbois, 1901). Such reactions are to be understood in a context where the relationship between experimental physics and mathematical physics was being revaluated due both to new advances in precision and the changing character of theoretical physics.

French physics, around 1900, relied on instruments, and mathematics was one of them—perhaps the cheapest. When Bénard's mentor, Marcel Brillouin, was chosen to succeed Joseph Bertrand at the chair of general physics and mathematics of the Collège de France, his ability for articulating experimental work in physics with mathematical analyses was deemed precious. Unlike Poincaré the theoretician, Brillouin was praised for possessing "to guide himself . . . the double instrument of mathematics and experiment."[8] But despite this appraisal, the administration of the Collège had little money to spare on laboratory facilities for Brillouin's chair in "general physics and mathematics." But for him to teach a physics course without a research laboratory largely opened to students was unthinkable. Arguing for more space, monies, and assistants, he suggested, on 17 December, 1905, that the laboratory of experimental phonetics at the Collège de France be transformed into a laboratory of general physics and mathematics. All this apparently to no avail.

In these conditions Bénard got into contact with Brillouin, as well as with Éleuthère Mascart who was the professor of experimental physics at the Collège de France. Born in 1874, Bénard was admitted to the École normale supérieure in 1894, the same year as physicist Paul Langevin and mathematician Henri Lebesgue. In 1900–01, Bénard was hired as Brillouin's assistant for his course on the general properties of fluids for which he was asked to rework old experiments from Poiseuille on the viscosity of fluids in thin channels.[9] His work on cellular vortices was carried out at the same time but independently.

We now have excellent surveys of the history of fluid mechanics showing that *ca*. 1900 this was a hot topic (Darrigol, 2005; Eckert, 2006). Fundamental theories were well established but mathematical physicists were unable to account for new experimental results, such as Osborne Reynolds' clear display of the turbulent regime. That, for almost a century now, practical hydraulics was nearly completely divorced from hydrodynamics hardly seemed to bother anyone. On the contrary, foundational hopes such as those popularized by William Thomson, Lord Kelvin remained high and many still imagined that new, unsettling electron theories might soon dissolve in a correct understanding of the hydrodynamics of ether flows. After having studied the flight of birds with his chronophotograph, Etienne-Jules Marey had started to shoot the behavior of smoke streams past various obstacles (Didi-Hubermann & Mannoni, 2004). In 1896, Otto Lilienthal's fatal crash during a gliding experiment reinforced the glaring need for a new theory of flight theory that was stirring excitement for the new field of aerodynamics.

Above all fluid mechanics was a domain where the usefulness of mathematics for the practice of experimental physics and the invention of machines was put to the test. "Never has practice displayed so much disdain for theory, and at the cost of so many catastrophes," Brillouin lamented in a review paper about recent progress in the design of flying machines (1895, p. 766). When he chose to speak on "the relations between experimental physics and mathematical physics” at the 1900 International Congress of Physics, Poincaré wished to explore the consequence of his conventionalism in experimental science. For him, experimentation was the only source of truth in the sciences but mathematical physics had a unique role to play for generalizing its results. Experimenting was guiding science toward complexity; mathematization toward simplicity. Poincaré ended his paper with an insightful remark: “If Tycho had had instruments ten times more precise, there would have been no Kepler, no Newton, no Astronomy. It is unfortunate for a branch of science to be born too late, when the means of observation have become too perfect [i.e. precise]” (Poincaré 1900, 1175). Fluid mechanics around 1900 was hindered by instrumental advances that offered observations that seemed too precise to be handled mathematically, for example those of Osborne Renolds about turbulence or Étienne-Jules Marey. But this did not stop the young Bénard.

3. Bénard’s experimental setup

Consider a horizontal layer of liquid heated from below. In response to heating, the lower strata of the fluid expand and become lighter than the overlying strata. The warmer and lighter layers at the bottom tend to rise, while the cooler and denser top layers tend to sink. Thus described, convection is responsible for many large-scale atmospheric and oceanic phenomena, as well as the roiling of a heated broth. In a scientific way, it expresses the popular wisdom: “Heat rises.” This was known by 1797, and surely much before, when Benjamin Thompson, Count Rumford studied the transport of heat in fluids. To describe the phenomenon, the term *convection* was introduced by William Prout in 1834 (Verlarde & Normand, 1980).

Convection was the phenomenon Bénard sought to investigate. Despite the limitations of Brillouin’s laboratory, Bénard was able to apply a wide array of precision instruments to the problem at hand: micrometers, thermometers, photography, and chronophotography. He filled a 15-centimeter wide circular vat with a thin film of various liquids (water, ethylic alcohol, benzene, paraffin, ether) which he then observed from above. The floor of the vat was heated to a fixed temperature while the upper surface of the liquid was kept in contact with air. On 9 April, 1900, Bénard announced his principal results and succinctly described the cellular division of the plane in the permanent regime. He gave empirical laws he had derived linking cell size with temperature and heat flux and reproduced a vertical section of flows in one cell. But for him the results had great importance because this was the "first example of a physical phenomenon where uniform conditions in the plane give rise to perfect cellular structure" (Bénard 1900a, p. 1007).

In a second paper, Bénard (1900b) gave a fuller description of two methods he had used to achieve his results: suspension particles and optic. Different types of particles suspended in the liquid gave rise to various phenomena that could be observed directly: opaque particles drew the shape of the cells (center and boundaries being clear while the intermediary surface was filled with a *dégradé* smooth toward the center and abrupt close to the edges); highly-reflecting laminar corpuscles produced an even more textured impression; depots could eventually be seen at the center of the cells; floating particles however tended to accumulate at the intersection of three cells. These conclusions were summarized in sketches of various cross-sections of the liquid in motion. Optical methods took advantage of the fact that the fluid surface was not totally flat. Interference patterns produced by the light reflected on a mirror at the bottom of the vat could be photographed. Inspired by the methods developed nearly half a century earlier by Léon Foucault in trying to make very regular parabolic mirrors, this method, Bénard wrote, was the most precise one allowing him to define the contour of the cells.

In lengthier publications, Bénard (1900c; 1900d & 1901) insisted on what he perceived as the most important feature he had made manifest: the spontaneous tessellation of the surface into hexagonal cells which he insisted reminded one of cellular structure. His meticulous analysis of fluid motions giving rise to this appearance used a great variety of methods where photography figured prominently. Both methods of visualization (suspension particles and interferometry) produced effects that could me registered photographically. Accordingly, the now famous clichés he reproduced in his publications often showed this partition of the plane into cells. Once photos were taken, Bénard measured several quantities on them. He counted the number of cells he could observe, and then drew them on separate sheets of paper. Bénard's analysis was rather elementary (simple geometry for describing the shape of the cells, measurement of various quantities and expression numerical laws both with algebraic expressions and curves on paper), but crucial to his conclusions expressed as simple geometrical laws.

For Bénard, mathematics thus was a tool helpful to describe the phenomena he had exhibited, but not a formal structure to understand its deep nature. He lauded himself for having completely solved a problem of hydrodynamics without any prior information of a theoretical nature. He admitted that he had refrained from any "attempt at coordinating" the empirical laws he had derived with "the equations of motion of viscous fluids presenting finite differences in temperature" (Bénard 1901, p. 142). In the eyes of his community, Bénard's attempts however seemed too modest. In tune with Poincaré's optimistic modernism, physicists thought that fluid mechanics could help furthering the understanding of the relation between mathematics and experiments as well as between microscopic and macroscopic theories of matter. In the book to which Bénard had contributed, Brillouin for example claimed that experimental and theoretical considerations were easier to interpret jointly in the study of viscosity. With appropriate experimental facilities, he hoped to pursue such studies and like Jean Perrin link the molecular hypothesis with the macroscopic study of fluids. In a context where “modern electronic theories” were attracting great interest, such investigations had to be encouraged, because they dealt with interactions between molecules rather than internal to the atom (Brillouin 1907, p. 2:137). Promising nothing of the sort, Bénard’s ambition were not, for all that, modest but lay elsewhere: “one has to get used to looking at biological phenomena, no matter how complex there might be, as simply resulting from a play of forces identical, at bottom, with those whose effect we study in physical and chemical phenomena” (Bénard 1900d, 1328). In fluid mechanics as much as in physical biology, however, his insights into the dynamical understanding of forms lay dormant for many years to come. Before we turn to a further analysis of his thoughts on biology, let us first examine his legacy in the physical sciences.

4. The troublesome legacy of Bénard’s experiment in physics

Since 1900, countless papers have been written about the phenomena exhibited by Bénard. This alone indicates that if there is a history of neglect to be written about Bénard’s experiment, it is not a history of how it was forgotten, for it never was! Only some of Bénard’s own ideas about why his experiment was interesting have been neglected by later readers. Informed by chaos, most retrospective looks have failed to place the inception and reception of Bénard’s experiment in its proper setting. Immediate reception was rather confidential. One finds work by G. Cartaud on microscopic cellular structures in metals, by Camille Dauzère on the solidification of liquids (which led Bénard to conjecture that lunar craters might have been formed by phenomena similar to those he had described), and by Henri Deslandres on cellular vortices in the solar atmosphere (see also Bénard 1908a & 1912). Bénard’s results had found their natural place in the analogical thinking characteristic of the mimetic experiment tradition which was destined quickly to fade away due to the success of fundamental, structural, and atomistic approaches in physics.[10]

Widespread enthusiasm for Bénard's experiment therefore had to wait much longer. Prior to the late 1960s, it was believed that the problem posed by Bénard's experiments had been essentially solved by James William Strutt, Baron Rayleigh, in 1916. In the first theoretical account of the phenomenon based on fundamental principles, Rayleigh wished to see "how far the interesting results obtained by Bénard in his careful and skillful experiments can be explained theoretically" (Rayleigh 1916, p. 432). Using the method of small oscillations laid out in his *Theory of Sound,* Rayleigh explained convection in terms of an imbalance of forces. The force causing the bottom lighter layers to rise was called buoyancy and it increased with the difference of temperature. Opposed to it, there was a dissipative force, or friction, due to viscosity. When the temperature gradient between the bottom and the top was small, the forces canceled. Heat was propagated by diffusion only. No current was created and the liquid stayed immobile. Convection arose when buoyancy overcame viscosity. The relative importance of these two forces, Rayleigh showed in a manner recalling Reynolds' work on turbulence, was measured by a pure dimensionless number *Ra,* later called the Rayleigh number:

$$Ra = \frac{g\alpha d^3 \Delta T}{\kappa\nu};$$

where g was the gravitational acceleration, d and ΔT, respectively, the distance and the difference of temperature between the plates, α the thermal expansion coefficient, κ the coefficient of thermal diffusion and ν the kinetic viscosity, the latter three quantities being physical characteristics of the fluid. Rayleigh's theory predicted the existence of a critical Rayleigh number, depending on the geometry of the cavity but not on further physical properties of the fluid, at which a stationary convective current was set in motion. Rayleigh's criterion was applied to study global meteorological phenomena, convection in stars, and plate tectonics. In Bénard's configuration, the onset of convection occurred at a critical Rayleigh number of the order of 2000.

Henceforth, the history of Bénard's experiment, what motivated it, and how it was received, was almost exclusively approached through the lens of Rayleigh's criterion. The duality between instrumental and mathematical technologies that was at play in Bénard's work was lost. Although he himself went on to study hydrodynamics for at least three decades, his later work was never afterwards greeted with the same excitement. Rayleigh's account of the Bénard phenomenon had tamed its most surprising aspects away and given the illusion that it lay within the reach of his brand of mathematical physics, that Bénard's cells could be understood without considering them as dynamical systems. Although convection played an important role in many fields of engineering and science, very few physicists thought it worthy of deeper study. Those who did, like Subramanayan Chandrasekhar (1857 & 1961), mainly focused on studying the way in which the system reacted when submitted to rotation, magnetic field, or a combination of the two. For physicists, the simple Rayleigh-Bénard problem, as it came to be labeled after 1916, had simply been solved.[11]

By 1978, the situation had changed considerably. In Grenoble, a symposium devoted solely to convection welcomed 57 papers by 65 participants from 15 different countries (Hopfinger et al., 1978). What happened in between was of course "chaos." Just like the revival of Poincaré's work in dynamical systems, the renewal of interest in convection, and in Rayleigh-Bénard especially, was sudden and widespread. Well beyond the confines of fluid mechanics, mathematicians, solid-state physicists, chemists, and biologists paid renewed attention to the phenomenon which can be seen as a nonmaterial "boundary object."[12] But it would be a mistake to attribute this increase of interest in convection just to the emergence of chaos. While it certainly played a role in accelerating some convergences, chaos could just as well be seen as stemming from the intense study of the Rayleigh-Bénard system undertaken for reasons that had little to do with an enthusiastic embrace of a “dynamical systems approach.”

Before we ask why the Rayleigh-Bénard system was so widely studied and discussed in the 1960s and 1970s, let us focus on a revealing misunderstanding that followed Rayleigh’s interpretation of it. An arcane corner of physics, which may not have been among its most exciting topics, Rayleigh-Bénard convection nonetheless generated a huge body of literature over the years. Reviewing the field in 1973, Madrid physicist Manuel G. Velarde wrote: “In fact, too many papers have been published . . . leading to much confusion since the publication of Lord Rayleigh’s paper, just because people did not try to repeat Bénard’s experiments under different conditions and/or did not want to contradict the beautiful and masterly analysis of Lord Rayleigh” (Velarde 1983, p. 514). However, not until the 1950s was it realized that Rayleigh had provided an account for a phenomenon quite different from the one observed by Bénard (Block 1956; Pearson 1958). “We are saying that Bénard convection in the limit of an extremely thin fluid layer under thermal gradient has nothing to do with Rayleigh’s instability criterion described above! Indeed Bénard cells can be induced from heating from above! Or by a horizontal heating if the fluid layer is vertical!” (Velarde 1983, p. 476). The experiment was even carried out in zero gravity on board the Apollo XIV spacecraft, and gave rise to the same hexagonal patterns. Further studies showed that the Bénard phenomenon was almost entirely due to surface tension. It became customary among hydrodynamicists to call the problem of explaining the appearance of hexagonal cells the Bénard problem, while reserving the term ‘Rayleigh-Bénard’ for qualifying convection problems in terms of loss of stability. “It seems fairly well established nowadays [1973] that for standard . . . fluids, hexagons . . . appear when surface tension is involved in the problem, . . . whereas rolls and only rolls are the structure of Rayleigh convection” (ibid., p. 479). In the early 1970s, it seemed striking to Velarde that all convection problems beyond the simple linear analysis by Rayleigh had not been tackled earlier:

> Quite to my dismay I must confess that the solutions to the simplest non-linear Rayleigh[-Bénard] problem . . . has not yet appeared in the literature! Not much, in fact, has actually been advanced (73 years after Bénard's original experiments!) since the original and simple analysis of Rayleigh. . . . This obviously shows the mathematical difficulties involved in such simple physical problems (ibid., p. 514).

In spite of the "over-production of publications on a particular subject," there had not been "any real breakthrough in understanding" concerning the Rayleigh-Bénard system (ibid.). Was this solely an effect of "mathematical difficulties"? This situation raises an interesting question, as was already noticed by some early chaologists:

> It is striking to note that twenty years ago [i.e. 1964] little more was known about [turbulence] than at the beginning of the nineteenth century when Navier was setting down the equations governing the flow of a fluid. . . . And yet fluid mechanics is a domain easily accessible to experiment: no laboratory machinery comes anywhere close—in complexity and in cost—to the accelerators used to study subnuclear particles! Despite its banality, this observation raises questions which historians of science will one day have to address: that of the underlying causes (circumstantial or epistemological) of the relative stagnation, in a discipline which has never lacked for practical and economic motivation (Bergé et al., p. xiii).

Some of these claims cannot withstand even a superficial historical look. To claim that little more was known about turbulence in 1964 compared to the 1820s is obviously grossly exaggerated (Aubin, 1998, chap. 7). The experiments that served to establish the chaotic behavior of fluid flows at the onset of turbulence crucially depended on resources that were far from being available in the 1820s, such as computers, lasers, and liquid helium. Despite claims to the effect that these experiments were of “nineteenth-century style” (Gleick 1987, 192), one only needs to compare the conditions in which Bénard performed his experiments at the Collège de France and those in which his successors at the École normale supérieure worked in 1978 (Libchaber & Maurer 1982) to see how crucial modern technology was in order just to envision such experiments. Most significantly, it was only after the nature of turbulence had been redefined in terms of strange attractors by David Ruelle and Floris Takens (1971) that people could claim that “real breakthroughs” in the study of turbulence had been achieved (Aubin 2006).

Viewed from the perspective of fluid mechanics, the neglect of the Bénard phenomenon therefore seems quite relative. Between 1900 and the 1970s, different instrumental and mathematical environments had changed its meaning completely. From a curiosity explained away by a simple analysis of force imbalances, it was turned into a crucial experiment for testing new hypotheses about the onset of turbulence. Meanwhile, the normal science machinery had cranked out a considerable number of eminently forgettable studies (perhaps also in the waiting to be rediscovered). But as pointed out above, encounters between various research agenda around the Rayleigh-Bénard system predated the recognition that dynamical systems theory and chaos would become its natural home. As we shall now see, the fluid mechanics standpoint exhausts neither the reasons why the experiment could become fashionable again in 1970s nor the full meaning it had for Bénard himself around 1900. For this, we need to explore further the analogy with living matter.

5. Neovitalism and the analogy with living matter

The appearance of order exhibited by Bénard was sure to captivate those who tackled it for the first time. “A fascinating aspect of the Rayleigh-Bénard instability,” a physicist wrote, “consists in the existence of a remarkable periodicity—or if one prefers the existence of a perfect order—in the organization of the convective cells, an order that cannot fail to surprise the one who sees it for the first time" (Bergé, 1976, p. 24). When future Nobel laureates such as Ilya Prigogine (Glansdorff & Prigogine 1971) or Pierre-Gilles de Gennes (1975), Fields medalist like René Thom (1975), and later chaos theorists, showed renewed interest in the Rayleigh-Bénard phenomenon, Rayleigh's solution no longer seemed satisfying. Their ambition was boundless and they often associated this phenomenon with processes characteristic of the living world.

To the few who then bothered to go back to the dusty volumes holding his lengthy papers, the proximity of Bénard's outlook with new claims made in favor of a dynamical understanding of life came as a surprise. In fact, in 1900, Bénard was the first to be surprised when he realized that a physical experiment had given rise to structures that reminded him of living tissue. As opposed to Heidelberg zoologist Otto Büschli, Bénard had no intention to contribute to biology when he designed his experiment: "Studying a purely physical problem, in conditions as simple as possible, I was never guided by considerations foreign to physics" (Bénard, 1900d, p. 1328). Prompted, he said, by the interest manifested by some naturalists, he was led to state the implications of his results on biology explicitly:

> The shape of the currents I was able to observe in liquids offering no other heterogeneity than temperature differences are, if I am not mistaken, especially interesting and novel in that they are examples of remarkably simple physical phenomena able to create from scratch a cellular structure that seem, up until now, to be particular to living beings and characteristic of the organic world.[13]

To have been able to reveal a phenomenon of self-organization previously thought to arise solely in living tissues seemed particularly significant to Bénard. Pondering on the "consequences" of his experiments "from the point of view of biological theories," he concluded: "Purely physical research, such as this, might perhaps have some interest in the eyes of scientists who do not despair of reducing the complex phenomena of life to the general laws of inorganic nature."[14] In his more popular account published in the *Revue générale des sciences,* Bénard was even bolder. Living matter, he wrote, was formed of liquids or semi-liquids that were divided in stable cells. Regular hexagonal assemblages were very common in living tissue and therefore should correspond to a "general law of stability." Even though the phenomena he had exhibited arose spontaneously through the action of heat, it was "legitimate to predict that the same structure . . . could emerge under the action of forces altogether different: for example, in the cases of the complex mixtures in protoplasma, it could result from simple phenomena of diffusion and osmosis." He went on:

> In spite of the coarseness of our present means of investigation to observe what is happening inside a living cell, it would be contrary to the scientific mind to condemn in advance this line of research as necessarily unproductive. Many bold speculations may be backed by the history of experimental science, which provides enough examples of alleged chasms that are today bridged in spite of Metaphysics that deemed them unbridgeable.[15]

The imperialistic incursion of a physical scientist into biology is never necessarily welcome. As Toulouse physicist Henri Bouasse, known for his penchant for polemics and his *franc-parler,* expressed it tactlessly, physics "may be taken as the ideal-type of a complete experimental science. All other [sciences] strive to resemble to it" (quoted in Meyerson, 1921, p. 1:126). In the early twentieth century, however, vitalist theses went through a significant revival. In his the presidential address in front of the biology section at the 1911 meeting of the British Association for the Advancement of Science, Scottish zoologist D'Arcy Wentworth Thompson (1911) forcefully drew attention to the contemporary re-emergence of vitalism. A real and urgent question, this was perhaps the greatest a biologist could tackle. Without citing Bénard, Thompson emphasized the important role of surface tension for the understanding of biological processes such as the frequent structuration of matter into hexagonal shapes. The type of mathematical tools Thompson considered, such angle measurement, minimization of surface, etc., was similar to Bénard’s (see Thompson, 1961, pp. 104–7).

Debates about vitalism, and especially the role physics and chemistry could play in the understanding of the nature of life, therefore provided an opening for attempts such as Bénard’s. Although it seems to have been little noticed by contemporary vitalists or their opponents, the contribution made by Bénard was, as we have seen, wholly in favour of a physicalist interpretation of life, by suggesting that some simple structures could arise from a purely physical system, without resorting to any sort of vital force. Among the various phenomena observable in living matter, Bénard singled out self-organization. “Neovitalism,” as it was sometimes called, never was a well structured movement, but it left the room open for a certain number of scientists to oppose it by trying to show that some manifestations of life could be explained solely on the basis of known chemical and physical laws (Séliber, 1910; Bosc, 1913; Mourgue, 1918). Brillouin (1900) himself published articles where he put his expertise as physicist in the service of physiology. Of course in a context where fundamental laws in physics themselves were in the process of being fundamentally revised, to exclude *a priori* that any sort of vital force existed hardly was tenable. In a popular work, the Jena physicist Felix Auerbach (1910) promoted the notion of “ectropy,” the biological antithesis of entropy. More famously, embryologist Hans Driesch, philosopher Henri Bergson, and many others believed that evidences in favour of the existence of some vital principles (entelechy, *élan vital*) were now conclusive.

Focusing on Leduc’s chemical models, which she says were “almost self-evidently absurd,” Keller showed how active was the field of “synthetic biology” in this period (Keller, 2002, pp. 307 & 11). She explained that the role played by those models in “demonstrat[ing] that complex forms—comparable in complexity to those found in the living world—could be brought into existence by recognizable physical and chemical processes" (ibid., p. 11–2). In Leduc's own words, since living beings were composed of the same chemical elements as the mineral world, since the same physical was at play in them as in nonliving matter, "what constituted the [living] being was form and structure." Pushed to its limit, this reasoning implied that "biology was part of the physical chemistry of liquids" (Leduc 1910, p. 6). Bénard never expressed his views concerning vitalism, nor did he state a clear definition of life. Probably sensing that this would not lead him far career-wise, he never even pursued the intuitions he put forward in 1900–01. But the suggestions with which he concluded his papers are clear indications that the reason he thought his experiments could contribute to the debate about vitalism hinged on one particular propriety of living matter—its spontaneous structuring in hexagonal shapes. For him, this was definite evidence against the need to resort to vital forces to account for the way in which inanimate or living matter acquired its structure. To resituate Bénard's experiments in the vitalist debate of the first decades of the twentieth century thus gives a framework within which, even though the term had yet to appear, they truly were about "self-organization" as one of the characteristic features of life. But, on must notice that instability, turbulence, and chaos were not in Bénard's picture.

6. Order out of chaos: Prigogine and the Bénard system

About Bénard cells, a philosopher wrote "the general thrust of this apparently childish example is physical and cosmic. . . . It is therefore possible to explore the idea of a universe where order and organization are constituted in turbulence, instability, deviance, improbability, and the dissipation of energy" (Morin, 1977, p. 41). While the instrumental and mathematical contexts had, as we have seen, been radically overhauled since 1900, the anti-mechanist climates of both periods on the contrary had a lot in common. The same scientists disillusioned with Newtonianism and reductionism who extensively revisited the work of vitalist biologists and philosophers often paid attention to the Bénard instability. The role de D'Arcy Thompson's and Allan Turing's work (1952) for René Thom or of Bergson for Prigogine is well attested (Keller 2002, pp. 93–5). Biologist Henri Atlan (1971) also saw his attention drawn to Bénard cells.

Let us focus on the way in which the biological analogy was crucial in Prigogine's wish to promote the study of the Bénard phenomenon. In 1971, he published together with Paul Glansdorff a book that would reach a large audience despite its technicalities. In this book, they wrote that the Bénard system "is an especially enlightening example of the degree of unification that our method allows us to achieve between problems pertinent to thermodynamics and hydrodynamics" (Glansdorff & Prigogine 1971b, p. 3). For them, the Rayleigh-Bénard problem (which they most often conflated with the Bénard problem) provided one of the main examples of the application of a method first developed for nonequilibrium thermodynamics. As early as 1955, Prigogine had started extending his study of irreversible thermodynamics to the nonlinear domain having possible applications to biology in view (on Prigogine, see Brans et al. 1988; Stengers 1997). In 1967, Glansdorff and Prigogine distinguished between two kinds of structures in matter, those arising in equilibrium (e.g. crystals) and those arising in out-of-equilibrium conditions, which they called "*dissipative structures.*"[16] A few years later, Prigogine defined them as follows: "beyond a critical level dissipation can become an organizing factor, destabilize the disordered state, . . . and drive the system to an ordered configuration. Hence the term *dissipative structure*" (Prigogine, 1975, p. v). The two most intensely studied exemplars of dissipative structures were the Rayleigh-Bénard system and the periodic chemical reactions such as those exhibited by Soviet chemists Belousov and Zhabotinsky in 1950s, which instead of tending towards an equilibrium state, exhibited oscillatory behaviors (Prigogine 1968, p. vii–viii).[17] These chemical systems, which Prigogine approached from the analogy with hydrodynamic instability, were particularly interesting in that they exhibited a spontaneous appearance of order in time and space under the influence of dissipation. Instability gave rise to structure, chaos to order.

Now, the role played by Prigogine in promoting the interdisciplinary study of the Rayleigh-Bénard system in the late 1960s was tremendous. In 1965, he organized a conference at the University of Chicago, with Chandrasekhar in attendance. A strong emphasis was put on the unifying prospect of using variational techniques in many fields of science, from statistical mechanics to hydrodynamics. The analogy between hydrodynamic instability and phase transition was forcefully emphasized, using convection as a test case (Donnelly, et al. 1966). In 1973 a Conference on Instability and Dissipative Structures in Hydrodynamics was held at Brussels, where Prigogine and his collaborators, fluid dynamicists, and a French group of physicists from Orsay led by Gilles de Gennes, tried to identify the commonalties of their respective approaches. Prigogine expressed the “analogies” he saw at play between different dissipative systems:

> The purpose of this volume is to present a number of problems involving hydrodynamic instabilities from the standpoint of irreversible thermodynamics of dissipative structures. We hope that the *analogies* with chemical kinetics and the existence of common underlying ideas in all phenomena involving the emergence of order in a previously disordered medium will stimulate further research in these fascinating areas (Prigogine, 1975, p. vi, my emphasis).

Besides his study on dissipative structures, Prigogine then saw recent developments of the mathematical analysis of nonlinear differential equations as a useful advance. To problems of instability, Prigogine contended “one may apply the powerful tools of the qualitative analytical-topological theory initiated by Poincaré, continued by Andronov, and completed to perfection by Thom [*sic*]" (Nicolis et al., 1975, p. 2).[18] Prigogine saw fluid mechanics as a traditional testing ground for new mathematical approaches:

> Fluid mechanics, which was the first field to show, more than a century ago, the emergence of patterns of order, has long been developed independently of irreversible thermodynamics and fluctuations. On the other hand, it has always been the privileged field where new mathematical techniques and ideas were tried and applied (Prigogine 1975, p. vi).

It thus was partly through his interest in fluid mechanics, an interest which was a direct consequence of the analogies he detected with chemical, thermodynamic, and biological phenomena, that Prigogine got interested in recent developments in dynamical systems theory, including Thom's catastrophe theory and Poincaré's qualitative theory. As a result of the history of fluid mechanics, scientists who then tackled the Rayleigh-Bénard then insisted not only on self-organization as Bénard and Leduc had done, but also on instabilities and their relation with the order/disorder dichotomy. In this case, it was the "mathematics of time" (Smale 1980) developed by dynamical systems theorists that provided the adequate tool for making sense of the dynamic character of the Bénard phenomena (Aubin, 2001; Aubin & Dahan Dalmedico 2002). Recall how Bénard who was obviously not in a position to use this kind of dynamical approaches had struggled with the proper way to account mathematically for his experimental results. It therefore becomes highly significant that he did in fact try to exhibit the dynamic features of the phenomena he studied. But he resorted to a new experimental tool, the cinematograph.[19]

The Cinema as a Physics Instrument

By using the new tool of the cinema Bénard's experiments made—if only for the very few—the dynamic nature of self-organization visible in a deep and interesting way. In this last section of my paper, I want to argue that Bénard's use of the chronophotograph and the cinematograph is intimately linked with his forays into analogical thinking applied to the dynamic understanding of living organisms. In my view, to look more closely at the way Bénard mobilized the cinematograph for his experimental work can further our understanding of the resonance between Bénard's experiments and chaos theory. Indeed, both approaches embodied an understanding of life in dynamical terms. Already in 1900, he started to experiment with a chronophotographic apparatus borrowed from the Gaumont Company (Bénard 1900d, 1327). For several decades, Bénard would put the chronophotograph and the cinematograph at the center of his experimental practice in various laboratories, at the Collège de France, at the universities of Lyon and of Bordeaux, and at the Institut de mécanique des fluides in Paris. Like Marey and others, Bénard realized that those instruments were especially suited for analyzing the rapid motion of fluids. In a study that prefigured the van Kármán street, Bénard wrote:

> Because I needed to operate with a very short time of exposure and to obtain many images in little time, I had to resort to a cinematographic setting . . . Set since 1904 in the basement of the Science Faculty in Lyons, in especially favourable circumstances to avoid perturbing trepidations, the apparatus . . . makes possible the complete chronophotographic study of the swirls produced by the uniform motion of solid, variously-shaped obstacles (Bénard 1908b, 840).

After WWI, Bénard and Theodor van Kármán were involved in a minor priority dispute about whether or not the series of alternate vortices produced behind a moving object in a fluid should be named the "*Kármánsche Wirbelstrasse*" or "*boulevard de Bénard*" (Weisfred, 2006; Van Kármán, 1967). What is more significant than priority however is the fact that Bénard saw in his use of cinematography the distinctive feature of his work. He sarcastically commented on van Kármán and Rubach's 1912 paper that they had merely pointed out that it would be possible to use cinematography, which he himself had done obtaining more precise results.

Recent historiography of the cinema has emphasized the close relationship between the development of this form of representation and science (Mannoni, 1995; Lefebvre 1996). Bénard's reliance on Léon Gaumont's generosity was very characteristic of the Collège de France. Gaumont who had been trained at Léon Jaubert's Observatoire populaire du Trocadéro and also at the Carpentier instrument-making firm, had a very strong interest in science. He was introduced to the cinema by George Demenÿ, a long-time assistant to Marey (himself a professor at the Collège). In 1909–29, the Gaumont Company produced several hundreds short films that purportedly formed a true "Encyclopaedia" which included several films on the subject of "turbulent vortices." In the Archives of the Gaumont Company, there are today six films representing the motion of turbulent vortices produced by Bénard and Dauzère. It is known that Bénard showed such films at the Easter 1914 meeting of the French Physical Society. According to Weisfred, the booklet he wrote as a school teacher's guide to go with the films described observation methods and, drawing attention to the beauty of descriptive physics, commented on the analogy between convective motion and biological phenomena.[20]

The evidence one can derive about Bénard's intent from the few remaining films would at first sight seem rather scant. The films mainly consist of rather long shots of the onset of the cell structure and its evolution until the liquid in the vat evaporates completely. No comment is made. Still, as the subtitles express, there is a definite impulse for classifying the various types of observable cells depending on the nature of the liquid or initial conditions. Attention is drawn to the fact that there are at least two "species" of cells and that they sometimes organize themselves in chains. Mostly, the phenomenon is presented as a dynamical one and the organization of inanimate matter that it shows is clearly meant to seem uncanny.

At the root of the documentary genre, the Gaumont Encyclopaedia was studied by Frédéric Delmeulle. The Gaumont Company had itself established “laboratories, small menageries, greenhouses, [and] all sorts of scientific instruments. It sends operators in the large botanical and zoological stations, in the large industrial centers to shoot documentary films on various manufacturing processes; savants, medical doctors, engineers, chemists, naturalists help them” (from a Gaumont brochure quoted by Delmeulle, 1999, p. 56). This led Gaumont to adopt a policy of support to scientists up until the 1920s with no significant break during World War I. In April 1914, Doctor Henry Billet (a professor of surgery at the école du Val-de-Grâce) writes in *Ciné-Journal*: “My predecessors were impaired by financial difficulties and abandoned [the idea of using cinema for their teaching] . . . when the idea struck me to call on M. Gaumont, who welcomed me in the most benevolent manner. He placed an operator at my disposal with all the necessary accessories, and thanks to him, I was able to demonstrate the necessity of equipping the École with indispensable apparatuses” (quoted in Delmeulle, 1999, p. 101).

But this policy was not the result of Gaumont’s wish to constitute a cinematographic encyclopaedia, but rather the reverse. “[I]t is particularly clear,” he writes, “that educational cinema stems in straight line from the popularization film, and that the latter is likewise a product of the first attempts to use cinema in the service of scientific research” (ibid., p. 56). Indeed, as Alice Guy has recounted, Gaumont’s atelier was a hybrid social space where scientists were more than welcome. A secretary in Gaumont’s Comptoir général de Photographie who had been probably been hired in 1894 and would be one of the first to have the idea to shoot fiction films (“La Fée aux choux” in 1896), she wrote:

> There, I met scientists such as the physicist Eleuthère Mascart . . . who astonished Gaumont by waiting more than half-an-hour in his office and having a very humble chat with me; Dr. Pierre-Emile Roux, Pasteur's disciple and successor; . . . the physicist Louis-Paul Cailletet., who had liquefied gases, air, and oxygen; Arsène d'Arsonval who applied high-frequency [electric] current to medicine thus inventing "*darsonvalisation*"; the astronomer and geographer Joseph Vallot who had installed his observatory at the top of the Mont-Blanc; . . . Doctor Charcot, the famous explorer who died on the *Pourquoi pas?*; Andrée who left for the North Pole on a balloon [and never came back]; the botanist and apiculturist Gaston Bonnier; Doctor Alexandre Yersin, microbiologist (to whom I supplied myself a cinematographic camera when he left for Hong Kong where he discovered the plague bacillus), as simple and friendly as schoolboy; the engineer Gustave Eiffel who built many bridges, the Eiffel Tower and began digging the Panama Canal; C.-A. François-Frank . . . whom I often helped to take pictures of ataxic, of animal breathings, exposed hearts, frogs that I would adorn with a small white flag in order to register their beatings—and who was always very kind to me; . . . Louis and Auguste Lumière; . . . the Brazilian aeronaut Alberto Santos-Dumont whose first flight we filmed . . . at the time when the Wright brothers were also flying for the first time (Guy, 1976, pp. 49–51).

Guy was also directly involved in scientific applications of the cinema. In his experiments on animal and human respiration, dog's heartbeat during dissection, the motion of the ataxic, the facial expressions of the demented, the Collège de France physiologist François-Franck was assisted by Guy and a colleague of hers Lucienne Chevreton who would later marry the physiologist. Tending the X-ray apparatuses which people flocked to see barely six weeks after Röntgen's discovery was announced at the Academy of Sciences, Guy was left with a scar on her hand (Guy, 1976, p. 80; Delmeulle, 1999, p. 61).

As a matter of fact, early cinematography was a delicate technique where scientific and technical concerns were to the fore.

> Today’s movie directors have no idea of the hardships and setbacks we had to go through to get those films; if a neglectful operator forgot to brush the velvet frame of the apparatus, the full length of the film would be scratched; if a negligent developer forgot to stir the developing tank, then dark zones would streak part of the stripe; water bubbles would form and poke holes in the negative; if solutions were too warm, emulsions would melt, sometimes it would be torn from its basis and destroy the result of our efforts. One had to deal with the instability of the stand, the lack of tightness of our containers, the unevenness of light. The emulsion of the film, much less sensitive than now, would only record a small part of the spectrum and gave out black-and-white images with brutal contrasts, etc. (Guy, 1976, p. 218).

The technical investment consented by Bénard when he chose the chronophotograph and the cinematograph therefore was by no means small. So, it is most surprising that there is so little memory remaining from Bénard’s cinematographic work. The chronophotography allowed no more than an analysis of fleeting dynamic phenomena at a more leisurely pace, just like Jules Janssen’s had wished to do when he invented the photographic revolver that is the ancestor of cinema (Canales 2002 & 2006; Launay & Hingley 2005). The cinematograph, on the other hand, was designed to recreate the studied phenomenon: “giving the observer the faculty of seeing again the scene that he studies as many times as necessary” (Anonymous 1909, p. 682). In the interwar period, the increasing importance of civil and military aviation gave a tremendous boost to the field of fluid mechanics. Lavishly funded institutes were set up throughout France. Hired at the Sorbonne’s Institut de Mécanique des Fluides, Bénard would go on making films about fluids some of which distributed by Gaumont. In the course of his career, he made, and carefully analyzed the data of, over 130 films which were apparently thrown away by German soldiers occupying the Fluid Mechanics Institute during World War II. Bénard's cinematographic legacy was not neglected; it was simply destroyed.

7. Conclusion

In the 1900 volume of the *Revue générale des sciences pures et appliquées,* Poincaré discussed the relation between experimental and mathematical physics, Brillouin put his expertise as physicist in the service of the physiology of nerves and Marinesco explored some of the application of cinematography to biology and art. In his experiments on convective cells published in the same volume, Bénard addressed all those issues in his own way. To express his belief that the spontaneous structuring of inorganic matter he had exhibited had something to do with the phenomenon of life, he used the best tool he had at his disposal for exhibiting a dynamic process: the cinematograph. In the way in which Bénard seems to have used it, the cinema preserved dynamical processes so that they could be measured; it conferred to their artificial reconstitutions a striking lifelike quality; but contrary to dynamical mathematical tools it was not in itself a means for understanding the analogy.

Yet, remembering that in his 1902–03 lectures at the Collège de France, the philosopher Bergson argued against the "cinematographic nature" of contemporary knowledge and in favor of a vitalist philosophy of becoming, we may wonder whether Bénard's cinematographic studies of lifelike qualities of fluids—or at least, the types of considerations that had led him to such speculations—were not implicit in Bergson's discussion. In his analysis, by focusing on snapshots, this type of understanding forewent the possibility of explaining becoming in general. In particular, the problem of how form arose in the living body, he claimed, was unthinkable with such an approach:

> Now, life is an evolution. We concentrate a period of this evolution in a stable view which we call a form, and, when the change has become considerable enough to

> overcome the fortunate inertia of our perception, we say that the body has changed its form. But in reality the body is changing form at every moment; or rather, there is no form, since form is immobile and the reality is movement. What is real is the continual *change* of form: *form is only a snapshot view* of *a transition.* Therefore, here again, our perception manages to solidify into discontinuous images the fluid continuity of the real. When the successive images do not differ from each other too much, we consider them all as the waxing and waning of a single *mean* image, or as the deformation of this image in different directions (Bergson, 1911, p. 301).

As the case of Bénard shows, Bergson might have been right when he pointed out that the understanding of form, its creation, its persistence and its destruction, was impossible using the cinematographic method. To deal with such themes in an alternative manner using qualitative mathematics was the projects of the D'Arcy Thompsons, the Thoms, and the Prigogines. It would be useful to remember that the recovery of Bergon's philosophy of form also was a project of the 1970s (besides Progogine, see Deleuze 1968 & 1983–85). Yet, the cinematographic understanding of the lifelike phenomena Bénard was concerned with is closer than ever to some developed recently, in particular the notion of "artificial life," or "A-Life," developed by Christopher Langton (1986) and others (Doyle 2004; Helmrich 2004; Emmeche 2004). Drawing parallels between Leduc's work, strongly disqualified in his time, and the contemporary excitement over A-Life, Keller emphasized the importance of war-related research in the 1940s for conferring its legitimacy of computer simulation. In the case of the Rayleigh-Bénard system, as well, the development of numerical approaches on the computer played an important part in the renewal of interest (to start with, with Edward Lorenz's work). The computer became the instrument that could be used to merge cinematographic knowledge with qualitative dynamics. In this context, Bénard's analogies were allowed to gain greater fame as well as deeper significance. In other words, the history of neglect perhaps is no more than the contingent history of insignificance.

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Endnotes

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[1] Mutabazi, et al 2006, p. 5. For another historical study written by a specialist of the field, see Koshmieder (1993).

[2] Schemes emphasizing the non linearity of time were imagined by Michel Serres and developed by Bruno Latour among others (Serres and Latour, 1995; Latour 1999); resonances are discussed by Ilya Prigogine & Isabelle Stengers (1984).

[3] The way in which abstract topology was introduced into physics is analyzed in Aubin (2001). An early account of the emergence of chaos, which also is manifesto in favor of the revolution, is given by Gleick (1987).

[4] Particularly adamant in proclaiming the revolution are Hayles (1990) or Abraham (1994); accounts emphasizing continuity with the past are to be found in Hirsch (1984) and Diacu & Holmes (1996).

[5] On Poincaré, let me cite: Gilain (1991), Chabert & Dahan Dalmedico (1992), Gray (1997), and Barrow-Green (1997). Likewise there are several historical studies working toward filling mathematical gaps between Poincaré and chaos: Israel (1993), Dahan Dalmedico (1996), Dahan Dalmedico & Gouzevitch (2004), and Aubin (2005).

[6] A useful exposition of the implication of chaos for biological thinking can be found in Glass & Mackey (1988).

[7] According to Wesfreid (2006), Bénard defended his dissertation on 15 Marc 1901 in front of a jury composed of Gabriel Lippmann, Edmond Bouty, and Emile Duclaux.

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[8] Cf. Maurice Lévy's report in Brillouin's personal file in the Collège de France archives. On the role of experimentation in French physics during the last decades of the nineteenth century, see Brillouin (1925).

[9] Bénard's experiments are discussed in Brillouin (1907), p. 1:152–9. Details about his Poiseuille experiments can be found in Bénard's notebook describing the experiments he performed for Mascart's course on electricity and magnetism in 1897–98, and Brillouin's on fluid mechanics in 1898–99 and 1899–1900, Bibliothèque de l'Institut, Paris (Ms. 5592). Let us also note that Bénard worked on the translation of Boltzmann (1904).

[10] The role of analogy in Bénard's thinking is discussed by Walbois (1901), p. 605. On mimetic experiments, see Galison & Assmus (1988) and Maas (2005), chap. 4. On the use of analogical thinking and mimetic experimentation in French astrophysics, see Le Gars (2007).

[11] This is not to say that Bénard convection was not tremendously important in meteorological, geophysical, and astrophysical contexts, which are beyond the scope of this paper. Similarly, Bénard's experiment played a part (although a much less prominent one than Taylor-Couette flows') in the development of the theory of hydrodynamic stability (Lin, 1955).

[12] Perhaps might it be preferable to refer to the Rayleig-Bénard phenomenon as a boundary system which, like a boundary object, served as a mediator between various communities. On boundary objects, see Star & Griesemer (1989). For more on the relation between the Rayleigh-Bénard problem and the emergence of chaos, see Aubin (2001) and Aubin & Dahan Dalmedico (2002), esp. pp. 308–10.

[13] "Les formes de courants que j'ai ainsi pu observer dans des liquides n'offrant d'autre hétérogénéité que des différences de températures, présentent, si je ne me trompe, l'intérêt tout spécial et nouveau d'un phénomène physique, remarquablement simple, créant de toutes

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pièces cette structure cellulaire qui, jusqu'à présent, semblait particulière aux êtres vivants et caractéristique du monde organique" (Bénard, 1900c, p. 1261).

[14] "des recherches purement physiques, du genre de celle-ci, présenteraient peut-être quelque intérêt aux yeux des savants qui ne désespèrent pas de ramener les phénomènes si complexes de la Vie, aux lois générales de la nature inorganique" (Bénard, 1901, p. 144).

[15] "Malgré la grossièreté évidente de nos procédés d'observation actuels pour observer ce qui se passe dans une cellule vivante, il serait contraire à l'esprit scientifique de condamner à l'avance ces recherches comme nécessairement impuissantes. De ces prétendus abîmes aujourd'hui franchis en dépit de la Métaphysique qui les déclarait infranchissables, l'histoire des sciences expérimentales offre assez d'exemples pour encourager bien d'autres astuces" (Bénard, 1900d, p. 1328).

[16] I. Prigogine and P. Glansdorff, "On Symmetry-Breaking Instabilities in Dissipative Systems," *Journal of Chemical Physics,* 46 (1967), 3542–50, on 3550. For a philosophical discussion, see A. Boutot, *L'Invention des formes. Chaos, catastrophes, fractales, structures dissipatives, attracteurs étranges* (Paris: Odile Jacob, 1993).

[17] Note that periodic reactions are also discussed by Mourgue (1918), p. 429.

[18] On the relation members of Prigogine's saw between dissipative structures and catastrophe theory, see Nicolis & Auchmurty (1974).

[19] On the role of cinematography in the developmental biology, see Keller (2002), p. 216; and Landecker (1999).

[20] Descriptions and stills from the movies can be found on the website http://www.gaumontpathearchives.com/. The six films have no author attribution and have been arbitrarily dated 1920. They have sequential reference numbers starting with 2000GS 05356. All are titled "Sciences physiques" with different subtitles : "Les tourbillons celulaires" (8'28''); "Les tourbillons cellulaires du spermacéti" (3'36''); "Les chaînes de

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tourbillons cellulaires dans l’éther” (11’45’’); “Les tourbillons cellulaires de l’éther” (5’49’’); “Tourbillons cellulaires isolés” (4’40’’); and “Les deux espèces de tourbillons cellulaires” (4’02’’).