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Unexpected Convergence between Science and Philosophy: A debate on determinism in France around 1880

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ABSTRACT. In 1878 the mathematician Joseph Boussinesq pointed out a structural analogy between some features of living beings and singular solutions of differential equations. Sudden transitions between ordinary and singular solutions could represent sudden release of energy in biological process and in the fulfilment of free will. He assumed that a guiding principle rather than a physical action might lead the system beyond the threshold of singular points. Deterministic processes, which corresponded to ordinary solutions, gave way to indeterministic processes, which corresponded to singular solutions. Alongside the mathematical pathway, a different conceptual stream had already emerged in the second half of the nineteenth century. Both physicists and physiologists made use of concepts like triggering actions and guiding principles in order to represent explosions and unstable equilibrium in inanimate matter, and the complex interaction between volitions and motions in human beings. A third conceptual stream was represented by philosophical debates on the problematic link between deterministic physical laws and free will. The new issues stemming from the fields of mathematics, physics, and life sciences found room in philosophical journals, but the interest of philosophers gradually faded away towards the late 1880s. At the same time, the majority of mathematicians and physicists had never shown a systematic interest in this subject matter. We find in Boussinesq an original and almost isolated attempt to merge mathematical, physical, biological issues into a consistent philosophical framework. However questionable his research programme might be, it was actually a daring and systematic one. In the twenty-first century, some philosophers of science rediscovered the problematic link between determinism and singular solutions of differential equations. The memory of late nineteenth-century debates had already disappeared, but recently Marij van Strien has put forward a direct comparison between those debates and recent theses on determinism.

Keywords: determinism, singular solutions, triggering action, free will

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Introduction

The present paper deals with a debate on determinism and free will that took place in the second half of the nineteenth century, and more specifically around 1880, mainly in French speaking countries. Some mathematicians, physicists, physicians, and philosophers were involved, and both the boundaries and the bonds among different disciplines were fiercely shaken. Unexpected meaningful links among mathematical technicalities, the equations of mechanics, sudden release of energy, the emergence of life, and free will were under scrutiny. Unexpected structural analogies among different processes also emerged. The French mathematician Joseph Boussinesq played an important role in that process of cross-fertilisation among different disciplines: he put forward an original research programme, where different traditions of research really converged. We find the integration among mathematical researches, where singular solutions of differential equations were involved, researches in physiology, where concepts like "*Auslösung*" and "*principe directeur*" had recently emerged, and physical sciences, were transformations of energy in general, and concepts like "*trigger-work*" and "*travail décrochant*" were at stake. After some debate between mathematicians, Boussinesq's programme of research faded away, and was handed over to philosophers. Both language and concepts underwent a remarkable change: the debate on determinism was transformed into an exclusively philosophical debate, where mathematical or physiological issues offered the opportunity to raise traditional philosophical issues.

Papers dealing with some aspects of this historical subject have been published over time. Recently the philosopher of science Marij van Strien inquired into the relationship between twenty-first-century conceptions of determinism and nineteenth-century debates on the same subject. She was interested in a direct comparison between two different historical contexts, and Boussinesq played an important role in her research. My research is historical rather than philosophical: it aims to cast light on debates where different disciplines and different meta-theoretical attitudes were involved. I would like to explore the scientific context, in particular the specific contexts of mathematics, physics, philosophy, and life sciences. I would like to show that Boussinesq's researches were the result of an original integration among different traditions, where different issues found a provisional synthesis. It seems to me that the time is ripe for recasting the whole subject, and focusing on both Boussinesq's mathematical and philosophical approach, and its wide context.

The first section is devoted to Boussinesq's original integration between mathematics, science, and philosophy. The second section inquires into the mathematical and physical interpretations of differential equations. The third deals with physicists and physicians who attempted to shed light on explosive processes in inanimate matter, and energy thresholds in living beings. The fourth explores the French philosophical context, where the problematic link between natural laws and free will was sharply debated. In the last section I confine myself to some remarks on later debates and historical reconstructions. In the last decade, some researches on the mathematical and philosophical aspects of determinism have been put forward in the context of the philosophy of science. The interest in Boussinesq's research programme has recently re-emerged, and it deserves to be briefly commented.

1. Joseph Boussinesq on differential equations, living beings, and free will

In 1878, Joseph Boussinesq, a mathematician of the Lille Faculty of Science, published a remarkable essay in Paris, under the long and demanding title *Conciliation du veritable déterminisme mécanique avec l'existence de la vie et de la liberté morale*.[2] The essay, a book indeed, was introduced by a report the philosopher Paul Janet had read before the *Academy of Moral Sciences* on 26th January of the same year, and was subsequently published in the *Comptes Rendus* of the Academy. The journal also hosted a shortened version of Boussinesq's essay, which corresponded to "the philosophical section". Janet stressed that the subject matter was "very specific and technical", and he found it useful to draw the attention of philosophers to "the main idea". The core of Boussinesq's book was both mathematical and philosophical, because he described "some instances of perfect mechanical indeterminism". Some differential equations led to "*branch points* [*points de bifurcation*]", where solutions gave rise to two different pathways. From the physical point of view, a material system could evolve towards different directions, and the actual direction was unpredictable. In the corresponding mechanical model, "a body could rest or move, forwards or backwards, to the left or to the right" (Janet 1878, pp. 3 and 12-13).

Different versions of the essay circulated in 1878 and 1879. In 1878 both the complete version (Boussinesq 1878a, 256 pages) and the short version (Boussinesq 1878b, 65 pages) were published: in the latter no mathematical equation appeared. In 1879 the complete version was also published in the *Mémoires de la société des sciences, de l'agriculture et des arts de Lille* (Boussinesq 1879a, 257 pages, a one-page "Erratum" included). At that time Boussinesq had already published remarkable researches in the field of mathematical physics, and in particular fluid dynamics. In 1868 he had mathematically analysed the water flow in river bends, and in 1877 he published a treatise on the same subject (Boussinesq 1877a). Unfortunately the book was overlooked by the scientific community because both mathematicians and engineers were not at ease with his theoretical and mathematical approach: theoretically or mathematically oriented scholars were not interested in his practical results, and engineers did not manage to appreciate his results because of that approach.[3]

The complete version of his essay on determinism and free will could be attractive to mathematicians, because it concerned the solutions of differential equations; it could be attractive to physicists, because it dealt with the relationship between equations of motion

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[2] After having spent fourteen years at the University of Lille, Boussinesq held the chair of "*Mécanique physique et expérimentale*" at the Paris faculty of Science until 1896, and then the chair of "*Physique mathématique et calcul des probabilité*". In both cases he succeeded Henri Poincaré.

[3] In 1964 the American engineer Robert P. Apmann remarked that Boussinesq was only 26 years old in 1868 "and had yet to make his reputation". The papers the engineer James Thomson published in 1877 on the *Proceedings of the Royal Society* were acknowledged as an important contribution to the field (Apmann 1964, pp. 427-8 and 433-4). Some decades ago, the American mathematician John Guckenheimer noted that Boussinesq's researches, in particular his "equations which describe fluid flow in a convecting layer" were subsequently developed by the meteorologist Edward Lorenz in the early 1960s (Guckenheimer 1984, p. 325). For the role played by Boussinesq in the history of hydrodynamics, see Darrigol 2009, pp. vii-ix and 233-8, and Darrigol 2002, pp. 136 and 150.

and actual behaviour of natural systems, and philosophers, because it cast doubt on determinism in the context of natural philosophy. Janet pointed out that Boussinesq had explored scientific possibilities far beyond mechanical determinism: specific features of differential equations might account for the existence of living beings. This was, in reality, the most important issue at stake: could the creative power of life emerge from some kind of intrinsic uncertainty or "deficiency [insuffisance]" in mathematical procedures and mechanical laws? This perspective appeared far more interesting than the two traditional pathways taken by scientists: the reduction of life to mechanical processes, and "the vital principle of ancient schools of thought", which opposed the mechanical approach. The title of the book called into play the compatibility [conciliation] between two apparently divergent issues: mathematical determinism and the emergence of life. In reality Janet hinted at an even more sensitive issue: the possibility of a mathematical account for "human free will" (Janet 1878, pp. 18 and 21).

In the end, after a short historical review, the philosopher Janet stressed what he considered the key concept of the book: science itself could "not exclude some kind of phenomenic indeterminism", or in other words, "some degree of contingency" in the natural world. According to Janet, Boussinesq's work could be looked upon as a scientific implementation of Boutroux' philosophical thesis on the contingency of natural laws. Boussinesq had managed to fulfil the reconciliation between "two fundamental laws of our mind", namely "the law of efficient causality", and "the law of finality or progress". The former required that "everything must be explained by an antecedent", and that "the effect cannot contain more than its cause", whereas the latter required that "we add indefinitely something new, which is not intrinsically contained in the antecedent" (Janet 1878, pp. 20 and 23).

Boussinesq was a mathematician, and he was interested in explaining the mathematical aspect of that compatibility or reconciliation between "true mechanical determinism", on the one hand, and "the existence of life and moral freedom", on the other. Nevertheless, in his "*Avant-propos*" he devoted more than ten pages to a historical review and meta-theoretical remarks. He claimed that "the specific material features of life" could be accounted for by specific solutions of differential equations, which were "seats of convergence and bifurcation of the integrals" for those equations. He reminded the readers that neither recently departed nor still living "physiologists or chemists" relied on "the existence of "particular vital actions". Nor did the majority of them claim that living beings were the seat of "accelerations and chemical reaction" intrinsically different from those taking place in other "material systems" (Boussinesq 1878a, pp. 25-6).[4]

Some scientists had professed a sharp reductionism: ordinary physical-chemical forces had to rule "all kinds of motions inside living beings", and life could not be looked upon as "a cause in itself". Boussinesq thought that those claims were in contrast with common experience, in particular the observation of "volitional motions" of muscles. Although physical and chemical processes could account for the more complex processes taking place inside living cells, they could not account for the assembling of "cells and organs of

[4] He listed "Alexander von Humboldt and Berzelius, among the departed", and "Claude Bernard and Berthelot, among the living" (*Ibidem*, p. 26).

specific shape". The body of knowledge of life sciences appeared as a network of assumptions and experiences which did not perfectly fit with each other, and he found that the introduction of a specific "*guiding principle*" in biological processes was the wiser choice (Boussinesq 1878a, pp. 30-31).[5]

He made reference to some remarks the physiologist Claude Bernard had put forward in 1867. He had assumed two kinds of "forces" inside living beings. The first had been labelled "*operating forces* [*forces exécutives*]", and they were assumed to act in the same way as in "unanimated bodies [corps bruts]", whereas the second had been labelled "*guiding or evolutionary principles*", because they had to be "morphologically active". The latter did not have to be identified with the previous vital principles, because "organic morphology" was based on "general physical-chemical forces". According to Boussinesq's reconstruction, the conflation between physical laws and the creative power of morphogenesis led to a scientific determinism which was more sophisticated than the purely mechanical determinism: even some features of "freedom" could stem from it. He was looking for a sound determinism, which was not to be confused with "fatalism"; determinism and freedom appeared as subsequent stages rather than alternative features of natural processes. In reality, a "free action" could take place only during "the guiding stage", while determinism was "absolute" during "the operative stage" (Boussinesq 1878a, pp. 28-9).[6]

Boussinesq reminded the reader that mathematicians and engineers had put forward something like Bernard's guiding principle. In 1861, the mathematician Antoine Cournot had spoken of "a principle of harmonic unity, global direction, and homogeneity", whereas in 1877 the mathematician and engineer Adhémar Barré de Saint-Venant had introduced a vanishing "trigger work [*travail décrochant*]", which was not so different from the small amount of force required to pull a gun trigger. However he found that new concepts and new words were unnecessary, and specified that a guiding principle was not in need of a corresponding mechanical force, however negligible might it be. Divergent solutions of differential equations, or in other words "bifurcations in the integrals of the equations of motion", offered a suitable mathematical model for the creative power of life, which acted "in its own specific way", and "should not borrow its way of action from physical forces" (Boussinesq 1878a, pp. 31-33; Saint-Venant 1877, pp. 421-22; Cournot 1861, pp. 364, 370, and 374).[7]

Boussinesq devoted the first chapter of the book to mathematical and philosophical foundations. At first he reminded the readers that the alliance between differential equations and physical laws for "inorganic matter" had been the "natural crowning

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[5] He mentioned, in particular, Emile Du Bois-Reymond and Thomas Henry Huxley as upholders of a sharp reductionism.

[6] Boussinesq quoted from Bernard's *Rapport sur la marche et le progrès de la physiologie générale en France* (Bernard C. 1867, p. 223). He also quoted from the second volume of the treatise that Berthelot had published in 1860, *Traité de chimie organique fondée sur la synthèse*. Berthelot acknowledged that chemistry could not account for "the level of organisation" of living beings, even though "the chemical effects of life" stemmed from "ordinary chemical forces" (Boussinesq J. 1878, p. 29; Berthelot M. 1860, p. 807).

[7] It is worth specifying that Saint-Venant did not make use of the expression "*travail décrochant*", even though he made use of the verb "*décrocher*".

achievement" of a successful scientific practice during "three centuries". On the other hand, he specified that the unquestionable scientific success in the comprehension of the inanimate world did not entail an underestimation of the specific pliability of living beings, and the creative power of human mind. Although some scientists upheld the purely deterministic nature of life, and poked fun on the illusion of human freedom, the mathematical model of differential equations was not in contrast with the actual practice of freedom (Boussinesq 1878a, pp. 37-9). On the contrary, to deny human freedom corresponded to denying an important feature of differential equations.

> Je me propose d'établir qu'une pareille conclusion, négatrice de toute vraie et active liberté, de toute influence de la vie sur la matière, est en désaccord avec la logique, et qu'elle n'a pu se produire que par l'omission d'un fait analytique important. Ce fait consiste en ce que des équations différentielles, même parfaitement déterminées, … sont loin d'être assimilables à des équations finies qui donnerait directement ces états en fonction du temps et des circonstances initiales. En effet, l'intégration introduit fréquemment, dans les quantités dont des équations différentielles font connaître seulement la dérivée ou les accroissements infiniment petits, une indétermination pour ainsi dire illimitée, lorsqu'il existe ce que les géomètres appellent des *solutions singulières* (Boussinesq 1878a, pp. 39-40).

The existence of "singular solutions" of differential equations was the keystone of Boussinesq's scientific and philosophical design. He acknowledged that "inorganic nature" could "unquestionably" undergo the "supremacy of physical-chemical laws". From the mathematical point of view, those deterministic laws corresponded to ordinary solutions of differential equations. Singular solutions corresponded to bifurcations in natural processes, and were consistent with the emergence of life and free will. Ordinary solutions were consistent with a deterministic approach to nature, whereas singular solution could be put in connections with unpredictable processes in the context of life and mind.

> Ainsi la présence ou l'absence de solutions singulières, et de la *flexibilité* qu'elles permettent dans l'enchainement des faits, paraît fournir un caractère géométrique propre à distinguer les mouvements essentiellement vitaux, ceux surtout qui sont volontaires, des mouvements accomplis sous l'empire exclusif des lois physiques. Un être animé serait par conséquent celui dont les équations du mouvement admettraient des intégrales singulières, provoquant, à des intervalles très-rapprochés ou même d'une manière continue, par l'indétermination qu'elles feraient naitre, l'intervention d'un *principe directeur* spécial (Boussinesq 1878a, p. 40).

Boussinesq was aware that the reduction of nature to "matter and motion" was a meta-theoretical option, or a sort of "imagery", rather than an inescapable necessity emerging from the natural world. The result of a rough observation by means of human vision had given birth to a representation in terms of "shapes and their changes of place over time". The subsequent mathematical approach further simplified that simplified outcome of a specific sensorial process, and replaced uneven shapes with "ideal figures and abstract quantities". What Boussinesq labelled "*mechanical determinism*" was the most sophisticated version of that procedure, and corresponded to the differential equations of motions, where "second time-derivatives of spatial coordinates" could be derived from "some functions of those coordinates". In other words, the accelerations were assumed to be proportional to the applied forces, and forces consisted of "functions of coordinates, to be determined by means of observation". The future was predictable, and tightly linked to the knowledge "of present and past". Boussinesq labelled "general integrals" the formal solutions of the system of differential equations representing the physical system. When the actual numeric values of coordinates and velocities at a given time were inserted into the general solutions, these became "the particular integrals" or particular solution of the physical problem. Besides these ordinary solutions, which corresponded to mechanical determinism, there were those peculiar solutions that Boussinesq had already labelled "singular solutions". The name stemmed from the fact that some solutions led to infinite derivatives (Boussinesq J. 1878a, pp. 43, 46, and 48-9).[8]

When he began to analyse some instances of differential equations, he specified that he was not in search of equations describing "*living beings*". That kind of equations would have been extremely difficult to shape, and it would have been even more difficult to integrate. More specifically, two hindrances had to be overcome "in the analytical explanation of the material phenomena of life": first, the interactions between the living being and "the environment", and second, "the exchange of matter" between them. He confined himself to "fictitious examples", which did not deal with living beings in themselves, but corresponded to some structural features of complex systems, living beings included. In reality, he started from "equations of motion of a systems of points", because the mathematical model had to be "as simple as possible", and "in accordance with the general principles of mechanics". In brief, he attempted to show the structural analogy between some features of biological processes and some features of the singular solutions of differential equations for simple mechanical systems. He was aware of the apparent mismatch emerging from such simplified mathematical models: "the physical-chemical instability" of living beings contrasted with "the realm of pure mechanical laws", as far as life was in contrast with "death". Nevertheless, he was confident about the fruitfulness of his mathematical, scientific, and philosophical framework (Boussinesq 1878a, pp. 63-5).

The first instance he put forward was also "the simplest" and "the more abstract": the motion of "a tiny heavy body along a perfectly smooth curve", where "any friction" was excluded. He chose a tangent line to the curve as the z-axis, and its normal plane as the xy-plane. Physics and geometry required that "the tangential acceleration d^2s/dt^2 equals

[8] Today the expressions *general solution*, *particular solution*, and *singular solution* have the same meaning as at Boussinesq's time (James/James 1992, p. 121). Although Boussinesq looked upon "*mechanical determinism*" as a specific implementation of "mechanics", it is worth remarking that different conceptual streams in the tradition of mechanics, and in the tradition of natural philosophy, were at stake. The mechanical approach in terms of forces and differential was different from, and sometimes in opposition to, the mechanical approach in terms of matter in motion and collisions.

the projection $g(dz/ds)$ of gravity on the tangent line", where ds was the differential element along the curve. The equation of motion was therefore

$$\frac{d^2s}{dt^2} = g\frac{dz}{ds} \quad \text{or} \quad \frac{dv}{dt} = g\frac{dz}{ds} = g\frac{dt}{ds}\frac{dz}{dt} = \frac{g}{v}\frac{dz}{dt} \quad (1),$$

where v was nothing else but the velocity ds/dt. After having multiplied the last equation by $2v \cdot dt$, an expression "that never becomes infinite",

$$2v\frac{dv}{dt}dt = 2v\frac{g}{v}\frac{dz}{dt}dt \quad \text{or} \quad 2v \cdot dv = g \cdot dz,$$

Boussinesq integrated it, and obtained "the well-known equation of living forces", where v_0 was the initial velocity of the body:

$$v^2 - v_0^2 = 2gz, \quad \text{or} \quad v^2 = 2gz + v_0^2,$$

$$v = \pm\sqrt{2gz + v_0^2} \quad \text{or} \quad \frac{ds}{dt} = \pm\sqrt{2gz + v_0^2} \quad (2)$$ (Boussinesq 1878a, pp. 67-8).

He stressed that the differential equation (2) had a slightly wider scope than the equation (1), because it was consistent with the solution $v = 0$. Among the possible solutions, $v = 0$ or $2gz + v_0^2 = 0$ was a "singular solution", since it made infinite the right-hand side of equation (1). In this case, in the original differential equation of motion,

$$\frac{d^2s}{dt^2} = 0 \quad \text{and} \quad \frac{dz}{ds} = 0,$$

which corresponded geometrically to an horizontal tangent line, and physically to a condition of equilibrium. Therefore singular solutions corresponded to points where the body was instantaneously at rest in a locally horizontal but unstable position. What the body might do afterwards was unpredictable, as it happened when a body was in equilibrium on the top of a dome. According to Boussinesq, only some kind of "guiding principle" was able to make the body lean towards right or left. Mathematical laws could not decide the behaviour of the body in those points: the integration of differential equations allowed the physicists only to compute the specific initial conditions v_0 that led to the conditions of unstable equilibrium. If $z = f(s)$ was "the equation of the trajectory", $v = 0$ corresponded to $2g \cdot f(s) + v_0^2 = 0$ or $f(s) = -v_0^2/2g$ (Boussinesq 1878a, pp. 69-70).

> En résumé, *les solutions singulières cherchées correspondent aux positions d'équilibre où le mobile arrive sans vitesse* ; celles-ci peuvent être, soit des sommets de la courbe, soit des points d'inflexions où la tangente est horizontale et que le mobile atteint en venant d'en bas, …. J'appellerai *points d'arrêt* de telles positions ; … en sorte que le mobile pourra, au gré du principe directeur, et sans que la lois physique (1) soit violée, s'y arrêter pendant un temps *quelconque*, puis effectuer son départ, *arbitrairement*, du coté vers lequel les s croissent ou du coté opposé, du moins dans le cas ordinaire où le point d'arrêt considéré est un sommet. […] Les point d'arrêt seront donc le *siège* du principe directeur, la région où se trouvera localisé son pouvoir, qui n'exercera que là sur le mobile (Boussinesq 1878a, p. 70).

The number of "stopping points" depended on the shape of the trajectory: whenever descending paths were followed by a new smooth top, the specific initial conditions could give place to other stopping points, and once more the system was at the mercy of "the guiding principle". Many possibilities were at stake: the body could rest on the stopping point, or could descend forwards or backwards, but different trajectories or initial conditions could lead the body to a permanent downward pathway: in that case, "the system was like *dead*", since "it was subjected only to mechanical forces". The concept of stopping point was an actual scientific concept on the borderline between mathematics and physics. On the contrary, Boussinesq's guiding principle was a questionable concept on the borderline between mathematics and philosophy. Boussinesq's meta-theoretical design was in tune with the meta-theoretical attitudes of some contemporary theoretical physicists: he was not loath to pursue a questionable alliance between the most advanced mathematical physics and the most speculative tradition of natural philosophy (Boussinesq 1878a, p. 74).[9]

The mathematical analysis of stopping points required a finer distinction between singular solutions and "asymptotic integrals", and Boussinesq undertook that analysis with great detail. Singular solutions corresponded to a body that reached the smooth top in a finite time. On the contrary, asymptotic integrals corresponded to a body that could reach the top only after an infinite time, at least "*from the abstract point of view*". In order to show the mathematical aspect of that difference, Boussinesq refined his model, and focused on the stopping point, which was assumed as the new starting point. Two mathematical steps were in order. He first wrote equation (2) in a slightly different form in order to focus on the differential element of time:

$$ds = \pm\sqrt{2gz + v_0^2} \cdot dt, \qquad \pm\sqrt{2gz + v_0^2} \cdot dt - ds = 0,$$

$$\sqrt{2gz + v_0^2} \cdot \left[dt \pm \frac{ds}{\sqrt{2gz + v_0^2}} \right] = 0, \qquad dt \pm \frac{ds}{\sqrt{2gz + v_0^2}} = 0.$$

[9] The daring integration among integral-differential equations, theory of probability, recently emerged physical concepts, and cosmological cogitations that Boltzmann had put forward the previous year might be looked upon as structurally akin to Boussinesq's daring integration among mathematics, physics, and philosophy.

He then chose a very general trajectory, which was expressed by the function

$$2g \cdot z(s) = f(s) = K^2 \cdot s^{2m} \cdot \left(\log \frac{a}{s} \right)^{2k},$$

and contained the free parameters K, m, a, and k. Obviously, in the new starting point, $v_0 = 0$, and therefore $f(s) = 0$ and $f'(s) = 0$. The first space-derivative of the function was:

$$f'(s) = 2K^2 \cdot s^{2m-1} \left[m \left(\log \frac{a}{s} \right)^{2k} - k \left(\log \frac{a}{s} \right)^{2k-1} \right] \qquad \text{(Boussinesq 1878a, pp. 72-3).}$$

When $s \to 0$, both $f(s)$ and $f'(s)$ vanished if $2m - 1 \geq 0$, which corresponded to a positive exponent for the variable s. He could therefore focus on the time required to reach that stopping point:

$$dt = \frac{-ds}{\sqrt{2gz}} = \frac{-ds}{\sqrt{f(s)}} = \frac{-ds}{K \cdot s^m \cdot \left(\log \frac{a}{s} \right)^k}.$$

A finite time corresponded to a singular solution, whereas an infinite one corresponded to an asymptotic integral. Under the condition $2m - 1 \geq 0$ or $m \geq 1/2$, the computation of dt or

$$\int dt = \int \frac{-ds}{K \cdot s^m \cdot \left(\log \frac{a}{s} \right)^k}$$

depended on the algebraic sign of the exponent m. Around $s = 0$, the qualitative trend of the last integral was not different from the qualitative trend to the integral

$$\int \frac{ds}{s^m}.$$

A well-known rule of convergence established that the integral converged for $m < 1$ and diverged for $m > 1$. In the first case the trajectories led to "stopping point corresponding to singular points" whereas the second case corresponded to asymptotic integrals (Boussinesq 1878a, pp. 74-6).

Boussinesq's second kind of differential equation was chosen among the best-known physical models: two bodies endowed with masses M and M_1 interacted by means of a force $F(r)$ of mutual attraction, which depended on their mutual distance $r=\sqrt{x^2+y^2+z^2}$. Although the problem was "less simple from the analytical point of view", it was however "the more elementary among those dealing with the motion of a real system". In the ordinary three-dimensional space, where the M coordinates X,Y,Z and the M_1 coordinates X_1,Y_1,Z_1 were functions of time t, the equations of motion had the structure

$$\frac{d^2X}{dt^2}=\frac{1}{M}F(r)\frac{X_1-X}{r},\quad \frac{d^2Y}{dt^2}=\ldots\ldots,\quad \frac{d^2Z}{dt^2}=\ldots\ldots$$
$$\frac{d^2X_1}{dt^2}=\frac{1}{M_1}F(r)\frac{X-X_1}{r},\quad \frac{d^2Y_1}{dt^2}=\ldots\ldots,\quad \frac{d^2Z_1}{dt^2}=\ldots\ldots$$

After having subtracted the first series of equations from the corresponding terms of the second series, and having simplified the symbols in accordance with the equalities

$$\left(\frac{1}{M}+\frac{1}{M_1}\right)F(r)=\varphi(r),\quad X-X_1=x,\quad Y-Y_1=y,\quad Z-Z_1=z\ ,$$

Boussinesq displayed the three component of the equation of motion:

$$\frac{d^2x}{dt^2}=-\varphi(r)\frac{x}{r},\quad \frac{d^2y}{dt^2}=-\varphi(r)\frac{y}{r},\quad \frac{d^2z}{dt^2}=-\varphi(r)\frac{z}{r}\,.$$

In reality, the motion took place in the plane that contained r, and therefore the three-dimensional mathematical problem could be transformed into a two-dimensional one. Moreover, the intrinsic symmetry of the physical problem made it convenient to replace the coordinates x,y with the polar coordinate system r,ϑ (Boussinesq 1878a, pp. 92-4).

I confine myself to reporting on the last stage of Boussinesq's development. The two equations of motion assumed the simple structure

$$\frac{d^2r}{dt^2}-r\left(\frac{d\vartheta}{dt}\right)^2=-\varphi(r)\quad\text{and}\quad r\frac{d^2\vartheta}{dt^2}+2\frac{dr}{dt}\frac{d\vartheta}{dt}=0. \qquad (3)$$

The second equation could be recast in a slightly different way,

$$\frac{dr}{dt}\frac{d\vartheta}{dt}+\left[\frac{dr}{dt}\frac{d\vartheta}{dt}+r\frac{d}{dt}\left(\frac{d\vartheta}{dt}\right)\right]=0,\quad \frac{dr}{dt}\frac{d\vartheta}{dt}+\frac{d}{dt}\left(r\frac{d\vartheta}{dt}\right)=0,$$

and then multiplied by r:

$$\frac{dr}{dt}\left(r\frac{d\vartheta}{dt}\right)+r\frac{d}{dt}\left(r\frac{d\vartheta}{dt}\right)=0,\qquad \frac{d}{dt}\left(r^2\frac{d\vartheta}{dt}\right)=0.$$

The constancy of the product $r^2(d\vartheta/dt)$ could easily be interpreted in geometrical and physical terms: it corresponded to the constancy of "the surface $(1/2)\int r^2\cdot d\vartheta$ described by the radius in unit time". At the same time, the differential equation assumed the simpler structure

$$r^2\frac{d\vartheta}{dt}=const,\qquad \frac{d\vartheta}{dt}=\frac{k}{r^2},\qquad \vartheta=k\int\frac{dt}{r^2}+k'. \qquad (4)$$

At this stage Boussinesq placed the last expression into the first differential equation:

$$\frac{d^2r}{dt^2}-\frac{k^2}{r^3}=-\varphi(r),\qquad \frac{d^2r}{dt^2}=-\varphi(r)+\frac{k^2}{r^3}. \qquad (5)$$

Singular solutions corresponded to the value r assumed when $d^2r/dt^2=0$ and $dr/dt=0$ simultaneously. Because of the two-dimensional nature of the problem, those values "*did not correspond to stopping points but uniform circular trajectories*", because a vanishing radial velocity led to a constant angular velocity in the above equations. In fact, when r assumed the constant value r_0,

$\vartheta=\frac{k}{r_0^2}t+k'$ (Boussinesq 1878a, pp. 96-7).

> Le mobile, parvenu sur un de ces cercles, le parcourra pendant un temps quelconque, *au gré du principe directeur*, et puis en déviera, soit, arbitrairement, vers le dedans ou vers le dehors, soit seulement dans un de ces deux sens, suivant que, dans le mouvement rectiligne correspondant …, le déplacement pourra ou ne pourra pas se produire à volonté en-deçà et au-delà du point d'arrêt. Une fois engagé dans sa nouvelle orbite, où son mouvement est représenté par un système d'intégrales particulières …, l'atome la suivra, selon les cas, soit indéfiniment, soit jusqu'à la rencontre d'un d'une seconde orbite singulière, soit même jusqu'à son retour à la première orbite (Boussinesq 1878a, pp. 97-8).

When Boussinesq focused on the connection between mathematics and physics, he stressed two elements: the mathematical expression of the physical force, and the specific value of initial conditions. He wondered whether the physical force, namely "the true expression of the action between two atoms", was consistent with "the existence of one or more circular orbits". This question fell outside the field of mathematics, and no demonstration was available: he confined himself to remarking that a positive answer appeared to him as "eminently probable". The second issue was equally important: both in the case of a body in motion along a given trajectory, and in the case of "relative motion between two atoms", only specific initial conditions allowed physical bodies to reach singular points or orbits (Boussinesq 1878a, pp. 99 and 105).

Boussinesq acknowledged that such conditions appeared "very difficult to realise", but once established, "the bifurcations occur indefinitely", and "the role of the guiding principle never comes at end". A daring conceptual shift from mathematics to biology led Boussinesq to interpret that "perfectly unstable equilibrium", namely the indefinitely lasting motion on circular orbits, as a rough mathematical representation of the supposed instabilities that allowed life to emerge. In other words, while the material system persisted in a singular condition, it "could not die", until "an external perturbation" suddenly modified that condition. If life corresponded to that extraordinary condition, where a specific law of force and specific initial conditions were required, death corresponded to "the supremacy of mechanical laws". A material system was *dead* when it was "at the mercy" of mechanical laws, and it was *alive* when it could successfully elude that mechanical drift (Boussinesq 1878a, p. 115).

On the physical side of instabilities, friction and viscous dissipation represented a very sensitive issue. On the one hand, they represented a sort of watershed between mathematical idealisation and reality; on the other, dissipative effects could prevent the body from reaching the singular point, in the case of a motion along a given trajectory. Nevertheless, specific singular integrals could emerge even in the case of viscous motions, or more generally when the acceleration d^2x/dt^2 did not depend only on the coordinate x but also on the velocity $v = dx/dt$. Boussinesq assumed a very general structure for the equation of motion:

$$\frac{d^2x}{dt^2} = f\left(x, \frac{dx}{dt}\right).$$

Since time t did not appear explicitly in the function $f(x,v)$, he recast the equation in terms of x and v:

$$\frac{d^2x}{dt^2} = \frac{dv}{dt} = \frac{dv}{dx}\frac{dx}{dt} = \frac{dv}{dx}v, \quad \text{therefore} \quad \frac{dv}{dx}v = f(x,v),$$

$$\frac{dv}{dx} = \frac{f(x,v)}{v} \qquad \text{or} \qquad dv - \frac{f(x,v)}{v}dx = 0.$$

The structure of the solution was $\varphi(x,v) = c$, and its differentiation yielded

$$\frac{\partial\varphi}{\partial x}dx + \frac{\partial\varphi}{\partial v}dv = 0 , \qquad \frac{\partial\varphi}{\partial v}dv = -\frac{\partial\varphi}{\partial x}dx , \qquad dv + \frac{\frac{\partial\varphi}{\partial x}}{\frac{\partial\varphi}{\partial v}}dx = 0 ,$$

$$\frac{1}{\frac{\partial\varphi}{\partial v}}\left(\frac{\partial\varphi}{\partial v}dv + \frac{\partial\varphi}{\partial x}dx\right) = 0 , \qquad \frac{1}{\frac{\partial\varphi}{\partial v}}d\varphi = 0$$ (Boussinesq 1878a, pp. 82-3 and 109-10).

If the solution $d\varphi = 0$ corresponded to “the first general integral” $\varphi(x,v) = c$, the solution

$$\frac{1}{\frac{\partial\varphi}{\partial v}} = 0 \qquad \text{or} \qquad \frac{\partial\varphi}{\partial v} \to \infty$$

corresponded to “one or more singular solutions”, which could be analytically expressed by an equation of the kind $v = \chi(x)$. Because of the arbitrariness of the constant c in the general solution, there were specific values of x that led to the same value of v both in the general and in the singular solution. The existence of these values assured Boussinesq that a passage “without any discontinuity from the general integral to the singular solution” could actually take place. The physical system could experience an indefinite number of transitions from ordinary trajectories to singular ones. Boussinesq remarked that, in this specific instance, the singular solutions did not depend on the choice of the initial conditions but on the analytical form of the force, in particular the analytical dependence on velocity. In the case of forces independent of velocity, the sum between kinetic and potential energy was actually constant in accordance with the expression $\varphi(x,v) = c$, and the two variables x and v could be separated. More specifically

$$\frac{1}{2}mv^2 + U(x) = c , \qquad \frac{1}{2}mv^2 - m\int f(x) = c , \qquad v^2 - 2\int f(x) = c' ,$$

and the derivative $\partial\varphi / \partial v = 2v$ was always finite. In other words, the existence of velocity-dependent forces led to specific singular solutions. He therefore stressed that “a guiding principle could become necessary not only in specific initial states of the moving body" (Boussinesq 1878a, pp. 82-3 and 110-11).

He was aware that both the conceptual and mathematical links between differential equations and life sciences were quite problematic. For the time being he confined himself to pointing out that "the physical-chemical instability in a system of two atoms" could be "maintained indefinitely" provided that suitable initial conditions were satisfied. Nevertheless, the persistence of singular states depended on "*the external conditions of isolation*", and those conditions could be easily fulfilled in the case of "a system of two atoms". On the contrary, living systems were open systems: they exchanged energy and matter with the environment. Only a weak kind of induction could lead him to guess that "a similar property", namely the existence of singular solutions, could also appear "in a system which lived in connection with an external world". Furthermore, he was aware that the emergence of singular states from purely physical conditions could "open the door to the belief in spontaneous generation". In this case, Boussinesq was not able to put forward a definite answer: he confined himself to pointing out that the initial conditions which were consistent with the establishment of singular solutions were actually "as specific as to lead to a negligible probability to be produced by pure chance" (Boussinesq 1878a, pp. 113-4 and 116).

In brief, from the mathematical point of view, two contrasting features emerged from singular solutions. On the one hand, Boussinesq stressed the improbability of the initial conditions leading to specific instabilities, and on the other, the *stability* of those instabilities. He specified that the stability of singular states and their improbability were independent of each other: the astonishing combination of stability and improbability was one of the hallmark of living processes.

> Quoiqu'il soit, l'exemple offert par un système de deux atomes, où la persistance de l'instabilité une fois produite se trouve réunie à une probabilité infiniment faible de première réalisation, suffit pour prouver qu'il n'y a pas nécessairement de proportion, de rapport fini, entre la stabilité d'une vie entendue comme je le fais, et la largeur du champ à l'intérieur duquel doivent être comprises les circonstances matérielles propres à son apparition. [...]
> D'ailleurs, le fait de l'impossibilité expérimentale de la génération spontanée, en ce qui concerne toutes les espèces connues, rapproché de celui de la longévité des mêmes espèces, ne semble-t-il pas être précisément la traduction physique du double résultat auquel nous a conduit le calcul, savoir, une probabilité ou étendue du champ d'une petitesse inassignable, à coté d'une persistance paraissant indéfinie dans les conditions assez favorables de milieu? (Boussinesq 1878a, p. 117).

In the last part of the essay Boussinesq outlined a historical-critical analysis of the mathematical approach to singular integrals. Mathematicians had been astonished by that kind of solutions, and they had not attempted to find a "*field of application*" in the natural world. At the same time, naturalists were not interested in mathematical models for living systems, not to mention the lack of actual mathematical competence. In brief, neither "zoologists" were able to handle the mathematical toolbox, nor mathematicians were interested in inquiring into the mathematical features of “those peculiar material systems which we call living bodies”. During the nineteenth century, singular integrals had sometimes attracted the attention of mathematicians, but always from a purely mathematical point of view, or in connection with mechanical problems. Boussinesq mentioned Siméon Denis Poisson, Jean-Marie Constant Duhamel and Antoine Augustin Cournot’s researches, and briefly commented on their texts (Boussinesq 1878a, pp. 121-30).

In the end, he synthesised the issue he had raised, and the result he had achieved: physical laws as expressed by “differential equations for the motion of material systems” should not be identified with “absolute determinism”. Besides the deterministic drift there was “a principle which took charge of driving the systems at bifurcations”, and that principle represented something more than pure chance. He assumed that the action of the guiding principle was subject to “certain laws”, and he imagined “a science” whose wide scope included both the behaviour of inorganic matter and living bodies, “man included”. Two targets appeared within reach of that peculiar science: the inclusion of “physiology into the realm of rational knowledge”, and the possibility of bridging the gulf between mechanics and “social dynamics”. According to Boussinesq, not only was “the guiding principle of life evolution” different from “mechanical actions” but also different from “free causes”: it could fill the gap between the strict causality of “physical-chemical forces” and “the *principle of finality*” which was the hallmark of “wholly conscious life” (Boussinesq 1878a, pp. 133-4 and 140).[10]

[10] After his concluding remarks, Boussinesq devoted almost hundred pages to mathematical specifications, and other ten pages to additional remarks.

2. Mathematical and physical perspectives on differential equations

From the mathematical point of view, two issues were at stake in the context of differential equations: the existence and uniqueness of solutions, and the role played by singular solutions. The two issues were mutually interwoven, and still under scrutiny around 1880: no systematic, conclusive, and universally accepted theory was on the stage. Only around the turn of the century a satisfactory systematisation was achieved.[11] With regard to singular solutions, some mathematicians had already come across them in the eighteenth century: among them Brook Taylor, Alexis Clairaut, and Leonhard Euler. In 1759 Euler devoted a paper to the analysis of "some paradoxes of the integral calculus". From 1776 onwards, Lagrange inquired into the subject matter, and he managed to "create the first systematic theory of singular solutions": his researches were collected in the treatise he published in 1801, *Leçons sur le calcul des fonctions*. In brief, the topic "was studied in detail" in the late eighteenth century, and "some results were taught at the Paris Ecole Polytechnique". From the linguistic point of view, Lagrange labelled "complete solutions" what we call general solution, and "particular integral" what we call singular solutions. The label "particular solution" was referred to "a special case of the complete solution". Unfortunately a plurality of linguistic choices might puzzle the reader: Laplace used the expressions *particular solutions* and *particular integral* "in the converse senses from Lagrange". In 1806 Siméon Denis Poisson devoted a paper to "particular solutions" and some physical applications (Gilain 1994, pp. 444; Grattan-Guinness 1990, vol. 1, pp. 155 and 227).[12] With regard to the theorem of existence and uniqueness, in 1824 and 1835 Cauchy gave the demonstration for a specific class of equations. At that time, Cauchy's researches represented a noteworthy innovation since it had previously been assumed that solutions always did exist. In 1868 Rudolf Lipschitz refined Cauchy's results, and showed that the conditions of existence were weaker than Cauchy's. Only in 1893 Emile Picard offered "the first consistent exposition of the results of existence" (Gilain 1994, p. 446; Grattan-Guinness 1990, vol. 2, p. 759).

As mentioned by Boussinesq in his essay, Poisson hinted at the problem in his *Traité de Mécanique* in 1833. In the context of the differential equations of motion, he analysed the simple case of a body in motion in a viscous medium. More specifically, he imagined that the body did not experience gravity, and that "the resistance of the medium" led to a deceleration φ that was proportional to the squared root of the instantaneous velocity:

$$\varphi = -2g\sqrt{\frac{v}{k}} \,.$$

[11] Twenty years ago the historian of mathematics Christian Gilain stated that "the theory of ordinary differential equations still appears to be one of the most active branches of mathematics" (Gilain 1994, p. 451).

[12] In 1772 Laplace labelled "*solution*" or "intégrale générale" the general solution, and "intégrale particulière" every solution "qui se trouvera de plus comprise dans l'intégrale générale". He labelled "solution particulière, toute solution qui n'y est pas comprise" (Laplace 1772, p. 326).

In order to assure a dimensional consistency, the constant g had the same physical dimension of terrestrial gravity, and the constant k was a given constant velocity. The differential equation of motion was

$$\frac{dv}{dt} = -2g\sqrt{\frac{v}{k}} \, ,$$

and an easy integration led to express time as a function of velocity:

$$-\frac{1}{2}\sqrt{\frac{k}{v}}dv = g \cdot dt \, , \qquad -\frac{1}{2}\int_{v_0}^{v}\sqrt{\frac{k}{v}}dv = \int_0^t g \cdot dt \, , \qquad gt = -\sqrt{k}(\sqrt{v} - \sqrt{v_0}) \, .$$

The algebraic consistence of these expressions required that $v > 0$, $k > 0$, and $v_0 > 0$. Time was a decreasing function of v, and its upper value was

$$t = \frac{\sqrt{kv_0}}{g} \, ,$$

which corresponded to $v = 0$. When Poisson inverted the function, in order to find the time-dependence of v, he simply wrote

$$v = \left(v_0 - \frac{gt}{\sqrt{k}} \right)^2 ,$$

and he did not specified that only one branch of the parabola corresponded to the required inverse function. It is worth remarking that, until then, no reference to singular integrals appeared in the text (Poisson 1833, p. 249).

According to Poisson, the last equation could physically be interpreted in the following way: velocity decreased from v to zero when time increased from zero to $\sqrt{kv_0}/g$, but then "the motion continue in the same sense", and "velocity increases indefinitely". The fact is that, from the physical point of view, this mathematical behaviour makes no sense, because no positive acceleration can occur if no driving force really acts. The mismatch between mathematical and physics can easily be explained by the fact that not only does the last equation correspond to the solution $gt = -\sqrt{k}(\sqrt{v} - \sqrt{v_0})$, which pertains to the physical and mathematical problem, but also to the solution $gt = +\sqrt{k}(\sqrt{v} - \sqrt{v_0})$, which is mathematically and physically extraneous. At that stage, Poisson's singular solution emerged. Physics suggested him that, when velocity vanished, acceleration also vanished, and therefore "in that time the moving body must stop and then stay at rest". The mathematical interpretation suggested that $v = 0$ was a singular solution. However he looked upon the physical example as "purely hypothetical": it had given him the opportunity "to show the necessity of taking into account particular solutions in differential equations of motion". He claimed that the corresponding processes "do not really happen" when we confine ourselves to forces that "really act in nature". In other words, actual forces were mathematical functions which depended "on the instantaneous velocity and distance", and they did not give rise to singular solutions. The complex interplay between mathematics and physics was still waiting for a satisfactory clarification (Poisson 1833, pp. 250-1).[13]

Boussinesq reported that the problem had already been approached by Poisson since 1806, and in more recent time other mathematicians, for instance Duhamel and Cournot, had tackled the subject matter extensively. He mentioned Duhamel's *Course de mécanique* and Cournot's 1841 *Traité élémentaire de la théorie des fonctions et du calcul infinitesimal*. In reality, around the middle of the century, mathematical and physical approaches to singular solutions became different from each other, as we easily realise when we compare the mathematical treatises, which Duhamel and Cournot published in 1847 and 1857 respectively, with the physical treatise Duhamel published in 1853.[14] In Duhamel *Cours d'Analyse* the subject matter was extensively treated in the first sections of the second volume, and in Cournot's *Traité* in the fourth and seventh chapters of part VI (second volume).[15] On the contrary, in Duhamel's physical treatise the subject matter was compressed in a short section of three pages, *Remarque relative aux solutions singulières*, and the physical model he discussed was an improved version of the example Poisson had put forward twenty years before. Four differences between Duhamel and Poisson can be stressed: first, the former spoke of "a point" rather than a body; second, he made use of a more general viscous force of the kind

$$dv / dt = -kv^m ,$$

where $0 < m < 1$; third, he specified that velocity had to be positive, and fourth, no mistake with regard to the inversion of functions appears. The general solution of the differential equation emerged from the simple separations of variables

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[13] Poisson labelled "solution particulière" the singular solution, and "son intégrale" the general solution. I have slightly modified some mathematical steps and symbols, in order to make the whole computation clearer.

[14] After having attended the *Ecole Polytechnique*, in 1830 Duhamel taught mathematics in the same *Ecole*. Cournot was a mathematician and an economist, and he was also interested in inquiring into the foundations of scientific methodology. In 1834, with the help of Poisson's recommendation, he was appointed to a chair of mathematics at Lyon University. Cournot and Duhamel's above-mentioned treatises were the second editions.

[15] In 1841, in the second volume of his *Traité élémentaire de la théorie des fonctions et du calcul infinitésimal,* Cournot had devoted the whole 22 pages of chapter IV of part VI to singular solutions of differential equations. The first section (Cournot 1841, II vol., pp. 271-84) was devoted to differential equations of the first order, and the second to differential equations of higher order (*Ibidem*, pp. 284-92).

$$\frac{dv}{v^m} = -k \cdot dt,$$

and led to the expression

$$v^{1-m} = v_0^{1-m} - (1-m)kt \quad \text{or} \quad v = [v_0^{1-m} - (1-m)kt]^{\frac{1}{1-m}}.$$

The singular solution $v = 0$ had to be added to the general solution in order to obtain "the complete solution of the problem under consideration". The value $v = 0$ corresponded to the time

$$t = \frac{v_0^{1-m}}{(1-m)k},$$

and from that limiting value of t onwards the solution $v = 0$ could not change. From the physical point of view, the point was expected "to remain in the same position where it was found at that time" (Duhamel 1853, pp. 328-30).

No problem emerged on the borderline between mathematics and physics in Duhamel's analysis. It seems that the first mathematician who raised the question of *determinism* in connection with singular solutions of differential equations was really Boussinesq himself, in a brief *Note* ("Sur la conciliation de la liberté morale avec le déterminisme scientifique") he published in the *Comptes Rendus* of the *Académie des sciences* in 1877. He started from his "mathematical definition of *determinism*", which required that second time-derivatives of "the atoms coordinates" were functions of coordinates themselves. It was in reality a definition at the borderline between mathematics and physics. The statement was the mathematical translation of the physical definition of conservative forces: the simplest statement of the conservation of energy required that forces, and therefore accelerations, had to be functions of coordinates. Physical laws were looked upon as "nothing else but specific applications" of mathematics, where the word *mathematics* corresponded specifically to "the differential equations of motions" (Boussinesq 1877b, pp. 362-3).

Besides "the general integrals", which offered the solutions of those differential equations, and gave rise to a family of "particular integrals" corresponding to different "initial states", some equations also admitted "singular solutions". When such solutions appeared, the physical system could pass from "a set of particular integrals to another", and the transition could take place "in infinite ways, and infinite times". Mathematical and physical determinism required that natural phenomena developed "along pathways which never branch off": after having "*completely* translated the problem into equations", only one mathematical solution should emerge. When that requirement was not fulfilled, and bifurcations emerged, the necessity of "a *guiding* principle" also emerged, and it could "repeatedly change … the pathway of natural phenomena" (Boussinesq 1877b, p. 363).

Determinism corresponded to ordinary solutions of differential equations, whereas free will corresponded to the domain of singular solutions. The two different domains, determinism and free will, did not overlap with each other: he stressed that "freedom does not affect determinism", but was complementary to it. Free will came into play when physical laws "did not manage to deduce future from the present", and failed to prescribe "a completely definite pathway for natural phenomena". He dared to imagine a new kind of science which had guiding principles as its objects, and could account for the behaviour of "a *moral* and *responsible* being". He also imagined that "the singular integrals which emerge from the equations of motion for the organ of thought" could concur to set up that new body of knowledge, which was placed "at a higher level than geometry" (Boussinesq 1877b, pp. 363-4).

Free will gave rise to intrinsically unpredictable behaviours: its effects could only be computed by means of statistics. When "*great numbers*" were at stake, single behaviours could undertake any direction, but only slow transformations could be expected in the social domain: more specifically, "in the mean moral state of society", the macroscopic effects could only experience gradual changes (Boussinesq 1877b, p. 364). Boussinesq attempted to bridge the gap between determinism and free will, or between predictable and unpredictable events. The unpredictable behaviour of singular solutions and personal choices stood besides the predictable behaviour of general solutions and collective processes that involved great numbers. Although he did not state it explicitly, it seems that statistics could fill the gap between singular solutions and specific behaviours, on the one hand, and general solutions and collective behaviours, on the other. The fact is that the empirical side of the analogy, namely the relationship between singular and collective behaviours, was consistent with a statistical approach, whereas totally unexplored and still mysterious appeared any statistical relationship between singular and general solutions of differential equations.

Boussinesq's *Note* had been presented by his mentor Barré de Saint-Venant, who did not fail to give support to his protégé. He sent a *Note* with a similar title, *Accord des lois de la Mécanique avec la liberté de l'homme dans son action sur la matière*, to the Comptes Rendus. He started from three physical laws, which he identified with the three laws of conservation for linear momentum, angular momentum, and energy. He specified that in no way could free will and human actions "contradict these laws" which were considered as "firmly established". No contradiction could emerge from the co-existence between "freedom in our visible actions" and "the invariability of physical laws that rule the subsequent motions of bodies" (Saint-Venant 1877, p. 419).

After this specification Saint-Venant took a slightly different pathway. He focused on physics, in particular on explosive processes, where a little quantity of energy triggered off the transformation of huge amounts of energy. He made reference to phenomena like a little spark acting on a gunpowder box, which could make a fortress burst. In those cases, the ratio of "the work which produces the transformation of potential into actual energy" to "the amount of energy thus transformed" might be as negligible as to become zero. Living systems offered other meaningful instances of such processes. In particular, the efforts of our muscles were triggered off by "the impulse of small vibrations in the nervous system which rule our free motions". According to Saint-Venant, nothing prevented us from imagine that physical actions in living systems could take place "without any expenditure of mechanical work" (Saint-Venant 1877, p. 421-2).

At this point he was not so far from Boussinesq: in the realm of living being, physical actions could be driven by some kind of guiding principle, which did not correspond to any measurable physical force. According to Saint-Venant, the mathematical key of those processes could be found in Boussinesq's *Note*: singular integrals were "the analytical answer" to "the necessity of a *guiding principle*". That principle could "extend over time the instantaneous state of rest or make the motion restart in accordance with specific values of the general integral". The choice could be performed "arbitrarily and by free will", and "without any mechanical action corresponding to that choice". In conclusion, the supposed incompatibility between free will and "laws of motion" was not rationally founded: deterministic laws and bifurcations were generated by the same mathematical womb. Both general and singular solutions stemmed from differential equations, and no incompatibility was at stake. On the contrary, it was mathematics that assured the possibility of a mutual consistency between "physical laws and the free action of mind [esprit] on matter" (Saint-Venant 1877, p. 422-3).

However, despite the heuristic power of Boussinesq's perspective, the problematic link between singular integrals and free will was still waiting for being clarified. Saint-Venant did not specify whether he was speaking of a structural or more ontological analogy.

The following year Boussinesq published the long essay I discussed in the previous section. It was immediately criticised by the renowned mathematician Joseph Bertrand, who published an aggressive and sarcastic paper in the *Journal des Savants*.[16] He immediately poked fun at "the useless array of scholarly formulae", which could "dazzle a reader who is not an expert in mathematics". According to Bertrand, that expenditure of mathematical scholarship hid a questionable superposition between the mathematical theory of mechanical systems, and concepts like choice, freedom, and will. He asked himself how a mathematician could envisage "a material and inert system which was suddenly endowed with will", and was able "to choose between two possible motions". He ironically remarked that, when equations were "non-determined", Boussinesq found "necessary to compensate for their deficiency" (Bertrand 1878, pp. 517-8).

When Bertrand stopped making very general and sarcastic remarks on the weakness of Boussinesq's analogies, and embarked upon a more specific criticism, he was forced to enter the meta-theoretical ground, where the relationship between mathematics and nature was at stake. He started from two meta-theoretical thesis: first, "the results of equations could not attain absolute precision", and second, "the certainty of equations cannot be greater than the certainty of principles from which they stem". The second statement called into play the hypothetical-deductive structure of mathematics, whereas the first

[16] In 1853 Bertrand had edited the third edition of Lagrange's *Mécanique Analytique*, and in 1874 he had been elected *sécretaire perpétuel* of the *Académie des Sciences*.

made reference to the ancient distinction between the *smoothness* of mathematics and the *roughness* of material world. Afterwards he analysed the case of a body which started from “a state of unstable balance”. In reality, “an infinitely small force would bring about an endless motion”, and from the mathematical point of view, the minimum value of that force was “strictly zero” (Bertrand 1878, pp. 518-9).

Although he had started from a sharp criticism on Boussinesq’s scientific enterprise, he discussed the same physical configuration that Boussinesq had already described. He pointed out the same problems and the same effects, but in the end, he found that nothing really important could be derived from those phenomena. Neither “mechanics appeared worried”, nor “the science of soul had something to gain” from speculations on the possible link between mathematics and free will. He denied any possible connection between the two fields, and sharply concluded that “the mystery of soul remains unattainable”. In reality, the paper did not end here, because he continued to insist on the first meta-theoretical issue he had pointed out two pages before. Among “physical sciences”, mechanics was “the closest to truth”, but it could not reach “a perfect exactness”. In some configurations, equations allowed the physical system to take “two different pathways”, even though physical laws should only lead to “one of them”. In this sense, mathematics and physics had a different nature, where the word *mathematics* here made reference to the mathematical laws of mechanics. In front of the uncertainty of mathematics, physics took the lead: the least amount of force could “make the ambiguity disappear” (Bertrand 1878, pp. 519-20).

The fact is that the supposed uncertainty of mathematics and the deterministic nature of physics opposed the traditional view he had firmly endorsed: the perfection of mathematics against the background of the coarser natural world. In reality, in Bertrand’s line of reasoning, the word *mathematics* had different meanings, because different kinds of mathematics were at stake. The mathematics of “the equations of dynamics” could not attain the “absolute strictness of Euclid’s theorems”. In brief, three bodies of knowledge were involved: classical mathematics, mathematical physics, and physics. Mathematical physics, together with its tool box of differential equations, appeared as a shaky domain when compared to the strictness of pure mathematics and the empirical certainty of physics. On the borderline between physics and pure mathematics, in particular in the mathematical representation of motion, some ambiguities emerged. Alongside the representation of physical forces as continuous entities there was the representation in terms of “subsequent discontinuous impulses acting during a finite time”. Bertrand preferred a discontinuous representation of physical entities: a discrete rather than continuous representation made “multiple solutions disappear”, and “the necessity of a free choice for a dithering molecule“ disappeared as well (Bertrand 1878, p. 520).

According to Bertrand, Boussinesq’s worst fault was his blind trust in mathematical physics: he expected that “inert matter hesitated” when mathematical procedures led to a state of uncertainty. He expected that physical systems loyally followed the differential equations when the latter “refused to decide”. It was just the identification of physical with mathematical entities that had led Boussinesq to “class molecules among living bodies”. In reality, Bertrand’s line of reasoning missed the point, because Boussinesq had not pointed out a material analogy between singular solutions of mathematical physics and processes taking place in living beings. He had confined himself to a structural analogy, where the equations which led to a plurality of pathways in the field of mathematical physics were supposed to be akin to the mathematical structures which might rule the behaviour of living systems. On that structural analogy, on its specific content, and on its global soundness, Bertrand had nothing to say. He was astonished by "the eternal miracle" of "the immaterial soul", which could however influence "the motion of matter" in living bodies (Bertrand 1878, p. 521).

Boussinesq had attempted to outline a scientific approach to that *miracle*, whereas Bertrand found that attempt pointless. Boussinesq had outlined a mathematical approach to complex systems, whereas Bertrand did not dare to undertake a similar step. His last sentence, the most sarcastic indeed, consisted of a rhetoric question, which was at the same time a statement of scientific indifference. He asked: when two pathways are equally probable, "the differential equations dictate nothing", even "the guiding principle refrains from acting", and time elapses, "what can we expect to happen?" (Bertrand 1878, p. 523).

The following month Boussinesq sent a response to the *Journal des Savants*, but the journal refused to publish it: Bertrand was the journal editor, and this might have been the reason for the rejection. As a consequence Boussinesq sent the text to the *Revue Philosophique de la France et de l'Étranger*, which published his paper under the title *Le déterminisme et la liberté* in 1879. He found that some misunderstandings in Bertrand's paper needed to be clarified. He claimed that the main aim of his essay had been the refutation of the deterministic view that had been put forward by "Leibnitz, Laplace, Dubois-Reymond, Huxley, etc.". In positive terms, he aimed at demonstrating that "the equations of motion of a material system, *as they are assumed by classical mechanics*", could not determine "the *complete* series of motions of the system". Since Bertrand seemed in agreement with him on this specific issue, Boussinesq found surprising that the renowned mathematician had forgotten to point out such an important issue. The readers of Bertrand's paper had rather been led to think that he was interested in penetrating "the mystery of immortal soul" or "the action of soul on body", as some quotations from Bertrand's paper testified. He wished to point out that nowhere and in no way had he raised such questions: he had confined himself to criticising the "absolute mechanical determinism" which was supposed to rule "all motions that occur in the universe" (Boussinesq 1879b, pp. 58-60).[17]

Boussinesq discussed Bertrand's meta-theoretical thesis, namely "the mysterious nuances which distinguished abstract from real", and the supposed better reliability of physical-chemical laws when compared to "their mathematical language". He also commented on Bertrand's preference for a discontinuous representation of natural processes: he acknowledged that bifurcations actually disappeared from the equations of discontinuous processes, but could not disappear from "actual facts" (Boussinesq 1879b, pp. 60-1). In the end, Boussinesq focused on the core of his scientific enterprise: he

[17] Bertrand edited the *Journal des Savants* from 1865 to his death in 1900.

preferred a questionable scientific hypothesis, and a rough outline of a scientific theory, to no scientific theory.

> Donc, pour quiconque accepte les principes de la mécanique et rejette les forces vitales de la vieille physiologie, le champ d'action de la vie se trouve forcément aux points de bifurcation qui se présentent quand il y a indétermination athématiques des voies, seule place restée disponible en dehors du domaine incontesté des puissances de la matière brute. Et c'est une bonne fortune, pour le géomètre, que tous les cas d'indétermination mécanique accessibles jusqu'à présent à son analyse correspondent à des états éminemment instables de la matière ; car une instabilité physico-chimique extrême, inimitable, est précisément ce qui, aux yeux du chimiste et du physiologiste, caractérise le mieux les tissus vivants (Boussinesq 1879b, p. 62).

Since he had been sharply criticised by Bertrand because of his supposed pretension to explaining life, he specified that his simplified mathematical models could not account for life in the sense of "conscious life" or life endowed with "intelligence". He had open a field of possibilities: he had also specified that his models could only outline the emergence of "a basic kind of life", probably "a vegetable life". In reality, in his essay Boussinesq had insisted on the structural character of his analogies: the mathematical models he had described did not correspond to specific forms of life, but aimed to offer a mathematical foundation for the creative power of life (Boussinesq 1879b, p. 63).[18]

In the same year Boussinesq published an unsystematic collection of essays on different issues such as "geometrical intuition", and aims and methods of "physical mechanics". One of the essay, "*Complément à une mémoire, publiée en 1878, …*", was intended as a further elaboration of the issues he had raised. However inhomogeneous, the new publication dealt with fundamental issues on the borderline between mathematics, physics and philosophy. The first two essays offered an interesting framework for the subsequent remarks on determinism. He pointed out the gap between mathematics and experience: mathematics consisted of "ideal artefacts", and it made reference to a "peculiar" and "autonomous order of things". Mathematics required "a specific transcendental sight", or a specific "frame of mind", which was quite different from that required in physics and natural sciences. On the other hand, he excluded any actual opposition between mathematics and science. In the context of mathematics, geometry represented a sort of bridge between the realm of formal structures and the realm of human experiences, where "intuition" and "reason" mixed with each other: geometry required "something more than pure deduction" or "pure reason". Geometry contained "unexplored depths" and "infinite dark sides": the development of geometry appeared to him as a demanding task, which deserved to be pursued but could "never be accomplished" (Boussinesq 1879c, pp. 8 and 14-16).[19]

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[18] He made reference to Boussinesq 1878a, pp. 112 and 134.

[19] On the *mixed* character of geometry, see Boussinesq 1879c, pp. 18-19: "Il y a donc tout lieu de croire que, sans le concours apporté au raisonnement par l'intuition géométrique, les mathématiques seraient impossibles. Bien plus, nos connaissances ou notions de toute nature se trouveraient sans doute, de même

In the passage from "the abstract to empirical" Boussinesq found "some irreducibility, or so to speak, some incommensurability": the notion of space and therefore the existence of geometry filled that gap. He refused Leibniz's conception of space as "the order of co-existences", because this definition appeared too broad to him. Space was a specific order of co-existences, or better "the place where a certain order of co-existence is deployed". Once more he stressed that the nature of geometry, the science of space, was neither purely logic nor purely empirical. Even mechanics, or "*la mécanique physique*" had a mixed nature, since it could be placed on the borderline between mathematics and "experience": it required a plurality of "intellectual attitudes", which spanned both speculative and practical ones" (Boussinesq 1879c, pp. 22-3 and 48).

He reported that some "distinguished scholars" had appreciated his 1878 essay, whereas others had sharply criticised him for having involved differential equations in questions which most "men of science" strongly disliked, and for having extended mathematics beyond the boundaries of its "legitimate domain". He acknowledged that the title of the essay had misled some readers, because it had led them to expect a treatise of metaphysics. In his intention, the essay was rather a collection of remarks and applications making reference to a specific field of mathematics. It was a scientific work, "a simple mathematical-physical study" on a specific query of "natural philosophy" which "had involved many minds for two centuries". He had pointed out that "a specific guiding principle" had to be postulated in science besides matter and energy. On that ground he had based a very abstract approach to life sciences, which consisted in putting forward a structural analogy between some mathematical entities and the essential features of living systems (Boussinesq 1879c, pp. 82-3). That structural analogy had led him to conceive the possibility of simplified mathematical models for simplified living structures.

> Or, c'est dans cet ordre d'idées que les solutions dites *singulières* des équations, et, plus généralement, toutes les bifurcations que comportent leurs intégrales, montrent la possibilité, l'existence même, de phénomènes mécaniques où subsiste aux yeux du physicien géomètre une certaine indétermination, de phénomènes dans lesquels il reste de la place pour des causes qui soient distinctes des forces physico-chimiques ou *qui, du moins, aient leur effet absolument distinct de l'effet connu de ces forces*. […]
>
> Il est naturel, vu le caractère spécial de ces effets, de distinguer une telle cause, au moins logiquement, des puissances mécaniques ordinaires : aussi, je l'ai-je appelée *principe directeur* (Boussinesq 1879c, pp. 87-8).

He had assumed a structural analogy between "some kind of indeterminism of integrals" and "the extreme, inimitable, physical-chemical instability of living being". The analogy was based on two essential features of the mathematical models he had put forward: the "extremely weak probability that the physical-chemical conditions for the appearance of life" emerged, and at the same time, the "indefinite persistence of life, *once*

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coup, profondément mutilées, peut-être même anéanties dans ce qu'elle ont de précis, de scientifiques." See also *Ibidem*, pp. 20 and 21.

established". Nevertheless, the confidence in that analogy required three specifications. First, the mathematical model could not account for real living beings, but could only represent some essential features of "very simple systems" of that kind. Second, there could not be any automatic connection between the domain of "computation" and the domain of "facts": the possibility of a meaningful link was based on hypotheses and concepts that did not belong to mathematics. Third, he stressed "the practical impossibility of spontaneous generation" in the context of natural sciences (Boussinesq 1879c, pp. 84-5).

Boussinesq relied on a very strong meta-theoretical belief: natural phenomena required natural explanations, and natural explanations could be translated into mathematical laws. The existence of singular integrals, bifurcations, physical indeterminacy, and guiding principles were embedded into that general meta-theoretical framework. Moreover, he relied on another pillar of scientific tradition: the set of hypotheses and concepts which linked mathematical models to natural phenomena should not be in contradiction with the fundamental laws of physics and chemistry. Since the analogy between mathematical models and natural phenomena was structural and not ontological, Boussinesq did not dare to put forward hypotheses on the nature of guiding principles. They could correspond to either "a higher cause, like *life* and *will*", or something akin to ordinary forces that acted on "inanimate matter". In any case, no "finite amount of force" in the physical sense was required in order to lead matter to choose its way "at the bifurcation points". He could not accept "vital forces" of intensity comparable with "mechanical, physical or chemical forces". Once more he pointed out that the specific feature of living beings was instability, and "ordinary" natural forces could not account for that instability and frailty. At the same time, instability did not prevent living system from preserving a specific kind of equilibrium, which was in reality a homeostasis or dynamical equilibrium. Only open systems could experience that equilibrium: fluxes of energy between the system and the environment had to be "exactly balanced" by "simultaneous fluxes of matter" (Boussinesq 1879c, pp. 90, 94, and 98).

Boussinesq's found that another specific feature of living structures was the influence of past states on present ones. History intrinsically affected natural processes: the sensitivity to history was really one of the hallmarks of life. The natural world could be represented as a hierarchy of three levels or stages: the domain of physical-chemical forces, which depended only on the current state of the system, the intermediate level of "unconscious life", where the whole history of the system was at stake, and "fully conscious life", where a "*principle of finality*" made "the present depend on future". In the second and third level, new causes were at stake: they could not be assimilated to physical forces, even though they "could be represented geometrically". In other words, there was a wide set of phenomena which could not be accounted for by traditional science but could be represented by mathematical procedures (Boussinesq 1879c, pp. 108-9).

The structural or morphological analysis of natural phenomena led Boussinesq to put forward a series of analogies between specific fluid-dynamic effects and well-known processes in life science. The germ of a living being, when placed in a suitable environment, crossed “the transient stages which lead to the adult shape”. The process was not structurally different from a perturbation “of middle dimension” which entered a channel with resting water, when the bottom surface was horizontal, and the width of the channel constant. In this case, the perturbation progressed towards “its limiting shape of *solitary wave*”, which depended on its total energy and the channel width. Another analogy was offered by the phenomenon of metamorphosis, where the whole life span of insects consisted of two subsequent stages, which were quite similar with regard to life time, but “very different as to shapes and ways of life”. The phenomenon showed the same essential features of “a wave of great height” which travelled “upstream along a rushing river”. The wave could overcome the stream, and propagate upwards until it preserved “a reasonable fraction of its original height”, whereas below a given height, the wave transformed into a downstream perturbation (Boussinesq 1879c, pp. 115-6).

In the end, Boussinesq discussed briefly a specific differential equation, where acceleration depended on velocity:

$$\frac{d^2x}{dt^2} = \frac{3}{4}\left(\frac{dx}{dt}\right)^{\frac{2}{3}}.$$

From the structural point of view, the differential equation was not different from those introduced by Poisson and Duhamel, but from the physical point of view, it corresponded to an increasing acceleration rather than a viscous deceleration. The left-hand side of the equation could be slightly transformed in order to perform the first integration:

$$\frac{dv}{dt} = \frac{dv}{dx}\frac{dx}{dt} = v\frac{dv}{dx} = \frac{3}{4}v^{\frac{2}{3}}, \qquad \frac{4}{3}v \cdot v^{-\frac{2}{3}}dv = dx\,, \qquad \frac{4}{3}v^{\frac{1}{3}}dv = dx\,,$$

$$\frac{4}{3}\frac{v^{\frac{4}{3}}}{\frac{4}{3}} + c_1 = x + c_2, \qquad v^{\frac{4}{3}} = x - c\,.$$

At this stage, Boussinesq pointed out the existence of the singular solution

$$v = 0 \quad \text{or} \quad x = c,$$

and then proceeded to a further integration:

$$v^4 = (x-c)^3, \qquad v = \pm\sqrt[4]{(x-c)^3}\,,$$

$$dx = \pm\sqrt[4]{(x-c)^3}\cdot dt\,, \qquad \pm(x-c)^{-\frac{3}{4}}dx = dt\,.$$

In the last equations, $x = c$, and therefore $v = 0$, which corresponded to the already introduced "singular solution", could not be accepted. Under these conditions, ordinary mathematical steps offered the solution of the differential equation:

$$\pm \frac{(x-c)^{\frac{1}{4}}}{\frac{1}{4}} + c_3 = t + c_4\,, \qquad \pm (x-c)^{\frac{1}{4}} = \frac{t-c'}{4}\,,$$

$$x - c = \left(\frac{t-c'}{4}\right)^4, \qquad x = c + \left(\frac{t-c'}{4}\right)^4 \qquad \text{(Boussinesq 1879b, pp. 116-7).}$$

Two results were emphasised by Boussinesq: a stopping point really existed, and its existence did not depend on the choice of the initial conditions. Moreover, general and singular solutions correspond to each other with continuity in $t = c'$, $v = 0$ and $x = c$. After having "descended from $x = \infty$ to $x = c$", the mobile could stop and rest in $x = c$ for an unspecified time interval, and then "regain its accelerated motion". If we consider the dependence of velocity and acceleration on time, we find that

$$\frac{dx}{dt} = \left(\frac{t-c'}{4}\right)^3, \qquad \frac{d^2x}{dt^2} = \frac{3}{4}\left(\frac{t-c'}{4}\right)^2.$$

As a consequence, when $t < c'$, acceleration was positive, velocity negative, and the body moved backwards until $t = c'$. At this point, acceleration and velocity vanished, and the position was $x = c$. This was a stopping point, because in this point $v = 0$ and $a = 0$, and the mobile "could stop for an unspecified time interval". After that, the accelerated motion could go on.

Once more he stressed that the dependence of acceleration on velocity led to stopping points that did not depend on the specific choice of initial conditions, and therefore could not be removed by choosing other conditions. On the contrary, when accelerations depended only on co-ordinates, the stopping points could be removed by changing initial conditions, and those points were nothing else but "the natural positions of unstable equilibrium" (Boussinesq 1879c, pp. 117-8).

3. The second conceptual stream: physics and life sciences

Boussinesq and Saint-Venant explored the problematic link between specific mathematical and physical issues, and long-lasting philosophical problems: in general the boundaries between scientific practice and philosophical commitment remained quite loose until the end of the century, and during the century they could be crossed in both directions.[20] It is known that Laplace published his *Théorie analytique des probabilités* in 1812, and two years later a less demanding *Essai philosophique sur les probabilités*. From the outset he claimed that "the most important problems of life" dealt with "problems of probability". In other words, probability was an essential feature of human knowledge. This meta-theoretical attitude does not appear as a contingent one, because he insisted on the probabilistic nature of scientific knowledge, and specifically mathematical knowledge.

> On peut même dire, à parler en rigueur, que presque toutes nos connaissances ne sont que probables ; et dans le petit nombre des choses que nous pouvons savoir avec certitude, dans les sciences mathématiques elles-mêmes, les principaux moyens de parvenir à la vérité, l'induction et l'analogie se fondent sur les probabilités (Laplace 1825, pp. 1-2).[21]

He stressed the "ignorance of the links" that connected every event "to the whole system of the universe". At the same time the intelligibility of the physical world required the well known "*principe de la raison suffisante*". According to the principle, present events were enchained to past events: in other words, "a thing cannot occur without a triggering cause". We are compelled to represent "the present state of the universe as the effect of a previous state, and as the cause of the following". He hinted at a hypothetical "intelligence" or mighty mind, who "should know all forces acting in nature at every time", and would be able to submit that information to mathematical analysis. A scientist could not attain that kind of cleverness, even though "the perfection" of Astronomy could be looked upon as "a weak outline [faible esquisse]" of it. Laplace acknowledged that mathematical physics aimed at approaching the power of the superior mind he had envisioned. Nevertheless, the human mind would always have been "infinitely distant" from that "intelligence" (Laplace 1825, pp. 2-4).

Laplace remarked that the mathematical laws ruling planetary motions also ruled "the path described by a simple molecule of air", but it was practically impossible to know the huge number of microscopic motions. Probability was a sort of bridge that filled the gap between our body of knowledge and our ignorance (Laplace 1825, pp. 6-7). It seems that

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[20] In reality, the establishment of definite boundaries between science and philosophy was one of the achievements of scientific practice in the late nineteenth century. Even the word *scientist* does not seem suitable for some geographical context. On the process of specialization and professionalization taking place at the end of the nineteenth century, see for instance Ross 1964, p. 66, and Morus 2005, pp. 3, 6-7, 20, and 53.

[21] Laplace's 1814 text is slightly more synthetic, and "l'induction et l'analogie" are not mentioned (Laplace 1814, p. 1). Marij van Strien pointed out that Laplace was not the first scholar to put forward the metaphor of a supernatural mind (van Strien 2014b, pp. 27-8).

Laplace's text does not support a widespread received view, namely Laplace as a champion of strict determinism. The hypothetical mind he described was actually hypothetical: the verbs he employed to describe that hypothetical, astonishing power were conditional verbs, and the sentences had the typical conditional structure *if it were …, then it would …*. As will be discuss in a later section, it seems reasonable to think that the mythology of Laplacian determinism was a late reconstruction, and the physiologist Emile Du Bois-Reymond played an important role in the emergence of that mythology.[22]

This remark allows me to point out another tradition of research that deserves to be explored in this context, namely life sciences: physicians and physiologists opened a debate on sudden release of energy that could take place in living beings. At the same time, some physicists and natural philosophers were interested in inquiring into both the inorganic and organic sides of those processes. The short paper Saint-Venant published in 1877 might be looked upon as an outcome of this conceptual stream.

In 1842 Robert Mayer stressed the two essential features of forces [Kräfte] or causes [Ursachen].[23] Firstly, they could not be destroyed, and second, they could be transformed into each other. The former was a "quantitative" feature, and the second a "qualitative" one. Forces shared the two features with matter, but differently from matter, they were "imponderable": in brief they were "indestructible, transformable, and imponderable entities". Every cause produced a corresponding effect [Wirkung], and the effect had to equal the cause, as in the case of a falling body, where "the distance between the weight and the ground" corresponded to a specific "quantum of motion" gained by the weight. The establishment of equations between gravitational causes and kinetic effects was one of the tasks of "mechanics". In general, a cause could produce more than a single effect, and the equations might involve "gravitational forces, motion and heat" (Mayer 1842, pp. 4-6 and 9).

Two years later, in a letter he sent to the physician and psychiatrist Wilhelm Griesinger, he stressed how questionable the meaning of the words "cause, effect, and transformation" really was. He exhibited a pragmatic attitude: those words and concepts were "means rather than aims [Zweck]" in themselves. Moreover, their meanings could change in the passage from a scientific field to another. In the field of mental processes, might we say that "the cerebral activity" is the "cause" of the book a scholar is writing? The sentence could be accepted in a very general sense, but it would be definitely pointless to say "the cause, namely the cerebral activity, transforms itself into the effect, namely the book". In the field of physiology, it was known that physical activity could "improve breathing, heartbeat, and warmth", and could "accelerate metabolism". The question was, which was exactly the quantitative link "by pounds and ounces" between causes and effects? In the field of physical and chemical processes, the transformation of a cause into an effect was not less problematic. If a spark triggered off an explosion,

[22] For Ernst Cassirer and Ian Hacking's theses on the emergence oft he so called *Laplacian determinism*, see the fifth section of the present paper. With regard to recent appraisals, I agree with Marij van Strien on two specific issues: "Laplace's determinism is based on general principles, rather than derived from the properties of the equations of motion", and "there was a strong eighteenth century background to Laplace's ideas" (van Strien 2014b, p. 25).

[23] In the first page of his paper he had claimed that "forces are causes" (Mayer 1842, p. 1).

might we say that the former is the cause of the latter? In this case, we cannot find an equality between cause and effect: how could the two laws Mayer himself had put forward two years before, namely the conservation of causes and their transformation into effects, be satisfied? (Mayer 1844, pp. 98 and 100-102).

The spark set fire to the gunpowder, the blaze released a certain amount of heat, and finally heat was "in part transformed into the mechanical effect" of explosion. We could say that the spark (a) was "the cause of the gunpowder explosion (b)", and in its turn, the latter was "the cause of the earth blowing-up (c)". Nevertheless, there was a "definite proportion" between (b) and (c) but neither (b) nor (c) could be put into a definite ratio to (a). Identical explosions might be "triggered off by a spark or by a torch". According to Mayer, from a logical point of view, we were "not allowed to label *causal relation* two relationships that are so different" as (a) to (b) and (b) to (c) really are. He found that two alternatives were on hand: we could give up looking upon one of the two connections as a causal connection, or we could give up any demand for "a logically consistent language" (Mayer 1844, pp. 98-99 and 101).

In 1862, in a letter to the Scottish classical scholar Lewis Campbell, James Clerk Maxwell hinted at the problem of "action and reaction between body and soul". He confined himself to remarking that the action was not "of a kind in which energy passes from the one to the other", as some instances could easily show. He briefly stated that "when a man pulls a trigger it is the gun powder that projects the bullet", and "when a pointsman shunts a train it is the rails that bear the thrust" (Maxwell 1862, p. 712). In shorts, Maxwell simply stressed that the transformation of energy should not be confused with the activation energy that triggers off that transformation.

In 1865 the French physician Claude Bernard published a book that was intended as an introduction to "experimental medicine". He stressed the peculiarity of biological processes, and at the same time the necessity of a scientific explanation: both determinism and guiding principles were at stake. From the outset he expressed the optimistic claim that "medicine has permanently undertaken a scientific pathway". According to Bernard, the scientific method was nothing else but "the experimental method", but the adjective experimental did not have a pure empirical meaning. Experiments involved a rational practice: some kind of "*reasoning*" linked "ideas" and "*facts*" to each other. That rational practice in life sciences was not so different from the corresponding practice in sciences dealing with "inanimate matter". In different sciences, different phenomena, and specific "difficulties of investigation" and application were involved. The experimental scientist should be "both a theorist and a practitioner" (Bernard 1865, pp. 6-8).

The experimental method called into play determinism, because determinism was nothing else but the possibility of reproducing experiments. More specifically, the experimental method required that, in every specific science, "*the conditions of existence of every phenomenon*" were "*defined in an absolute way*". It was an axiom of science that "in identical conditions, every phenomenon identically happens". Life sciences should not represent an exception: determinism corresponded to the universality of scientific laws. Bernard was swinging between the two poles of a scientific practice whose foundations he was attempting to set up. On the one hand, he put forward a strong process of reduction of life sciences to physics and chemistry, and on the other, he stressed the specific features and "the essence of life". The more demanding task was the clarification of that specific nature or essence. He found that life required a sort of "guiding *idea*" or principle, or "creative idea", which "manifested itself in the organisation" of living beings. In other words, the difference between inanimate and animate matter was self-organisation, and self-organisation required a specific principle or power (Bernard 1865, pp. 116, 119-20, 159, and 161-2).

Life could not be the explanation of anything: in this sense *vitalism* had to be rejected. At the same time, a specific force allowed living beings to sprout and grow: it was "a force that feeds and organises", but in no way could that force "decide the features of living matter". These passages are not completely convincing: they show how narrow and demanding an intermediate way between *mechanism* and *vitalism* really was. His conclusion might be qualified as a sort of mild reductionism: it was wiser "to reduce the features of living beings to physical-chemical features" rather than to reduce the latter to the former. Determinism dealt with necessity, but necessity was intended by Bernard in a very limited way. Determinism called into play "the necessary condition for a phenomenon that is not forced to happen". It seems that Bernard hinted at something like the difference between necessary and sufficient conditions: a sound determinism involved the set of necessary conditions for the appearance of a phenomenon. In no way could those conditions automatically lead to the emergence of that specific phenomenon: the conditions were necessary but not sufficient. We find here three kinds or levels of determinism. There was a stricter determinism, which corresponded to *necessary* and *sufficient* conditions for a given phenomena: it was in tune with a sort of *cosmic* determinism. Bernard's determinism was definitely milder: it was a pragmatic determinism, which corresponded to the existence of *necessary* conditions. At the third level there was the transformation of determinism into fatalism: at that stage, there was "the necessary emergence of a phenomenon independently of its conditions". Bernard depicted his mild determinism and fatalism as two conflicting world-views: in the former, a set of material conditions offered nothing more than the conditions of possibility for a given phenomenon, whereas in the latter a phenomenon could take place independently of whatever condition of possibility (Bernard 1865, pp. 352-4, 359, and 383).

In 1872, in the first part of a lecture he delivered to the German association of scientists and physicians, the German physiologist Emil Du Boi-Reymond took a different way. He claimed that scientific knowledge consisted in "reducing all transformations taking place in the material world to atomic motions". Since mechanical laws could be translated into the mathematical language, they could rely on "the same apodictic certainty of mathematics". The universe was ruled by "mechanical necessity": its present state could be "directly derived from its previous state", and could be looked upon as "the cause of its state in the subsequent infinitesimal time". He mentioned Laplace's Mind [ein Geist], and represented *It* as a powerful entity that would be able to "count the number of hair in our heads". Although "the human mind will always be remotely distant from this perfect scientific knowledge", what he labelled "Laplace's Mind" represented "the highest conceivable stage of our scientific knowledge" (Du Bois Reymond 1872, pp. 441-4 and 446).

According to Du Bois-Reymond, the best implementation of Laplace's *Geist* was astronomy, where "past and future position and motion" of parts of the universe could "be computed with the same accuracy". Astronomical knowledge represented the most perfect attempt to grasp the behaviour of "matter and force" by means of our limited ability. The possibility of reducing "mental processes in nerve fibres and ganglion cells to specific motions of specific atoms" would be "a great triumph". At the same time, that triumph would emphasise "the unsolvable contradiction between the mechanical world view, and free will and ethic". The actual scientific practice was strongly limited by two impossibilities: on the one hand, "the impossibility of grasping matter and force", and on the other, "the inability to reduce mental processes to their material conditions" (Du Bois Reymond 1872, pp. 455-7 and 459-60)

The following year Maxwell wrote a brief essay that was not intended to be published: it was addressed to a club of scholars who had the habit of sharing their reflexions and cogitations. Once more he was interested in the relationship between mind and body, and instabilities and "singular points" were at stake. He found that "the soul of an animal" was not structurally different from the "a steersman of a vessel" whose "function" was "to regulate and direct the animal powers" rather than "to produce" them. The text contained scattered or loosely connected issues that dealt with "(t)he principal developments of physical ideas in modern times", but two main issues were at stake. The first dealt with "the distinction between two kinds of knowledge", which had emerged in the context of "molecular science", and which he labelled "for convenience the Dynamical and Statistical". The second issue dealt with "the consideration of stability and instability", which could shed light on the above-mentioned "questions". In other words, according to Maxwell, the study of instabilities could clarify the comprehension of the problematic connection between dynamical and statistical knowledge. He defined "instability" as a specific "condition of the system" when "an infinitely small variation in the present state may bring about a finite difference in the state of the system in a finite time". The existence of unstable conditions could make difficult or "impossible the prediction of future events", provided that "our knowledge of the present state is only approximate, and not accurate". Here we can find the link between instability and statistical knowledge (Maxwell 1873, pp. 817-9).

Maxwell saw an intrinsic connection between instability and free will: when "we more or less frequently" found ourselves "on a physical or moral watershed", we also found the same features of physical instability. In the moral state that corresponded to physical instability, "an imperceptible deviation" was "sufficient to determine into which of two valleys we shall descend". According to Maxwell, determinism was not automatically in danger in this case: a sort of compatibility or complementarity between determinism and free will could be assumed. They represented nothing else but two different interpretations of the events that took place at a watershed. On the one hand, "(t)he doctrine of free will" required that "the Ego alone" was looked upon as "the determining cause". On the other hand, "(t)he doctrine of determinism" claimed that "without exception" there was an objective explanation: the resulting choice was looked upon as "determined by the previous conditions of the subject". In other words, a predictable chain of "causes and effects" was at stake in the deterministic interpretation. But *which* was the entity that was able to ascertain the chain of causes and effects? If *It* had been a human being, he/she would have been subjected to the already mentioned uncertain knowledge of the effects. If *It* had been "the Deity", Maxwell would have objected to "any argument founded on a supposed acquaintance with the conditions of Divine foreknowledge" (Maxwell 1873, pp. 820-21).

He insisted on the fact that his inquiry into determinism was undertaken from the point of view of physics, and "theology, metaphysics, or mathematics" had not been taken into account. Instability was the key word and the key concept, and physics offered some instances of instability. In optics, more specifically "within a biaxial crystal", when the ray direction was "nearly but not exactly coincident with that of the ray-axis of the crystal", a little change in the ray direction could "produce a great change in direction of the emergent ray". A better known instance was offered by "the conditions under which gun-cotton explodes". In all these cases, "the axiom about like causes producing like effects" could not "be true". What did these different processes have in common? The material system was endowed with "a quantity of potential energy", which could be transformed into motion. Nevertheless, the transformation could take place only when the system had "reached a certain configuration". The "expenditure of work" to attain that configuration might be "infinitesimally small", and moreover it bore "no definite proportion to the energy developed in consequence thereof". When a little spark set fire to a "great forest" or a "little spore" blighted "all the potatoes" or a "little scruple" prevented a man "from doing his will", we dealt with "singular points". In those points "predictions" became "impossible", unless we could rely on "absolutely perfect data", and "the omniscience of contingency" (Maxwell 1873, pp. 821-2).

All singular points shared two essential features: they corresponded to the release of huge amounts of energy, and at the same time they were isolated points, which formed "no appreciable fraction of the continuous course of our existence". Nevertheless, however rare those points might be, "singularities and instabilities" could bring into question "that prejudice in favour of determinism", which was recommended by the other side of our experience, namely "the continuity and stability of things" (Maxwell 1873, pp. 822-3).

In the same year the Scottish physicist Balfour Stewart, Professor of Natural Philosophy at the Owen College, Manchester, published an "elementary treatise on energy and its laws". The book had great success, and it was repeatedly reprinted in the following years. In the last chapter, which was devoted to "the position of life", he discussed physical and chemical instabilities, and some structural analogies with life. The simplest instance of unstable equilibrium was represented by an "egg upon its longer axis". The balance could be suddenly destroyed by a perturbation "so exceedingly small as to be utterly beyond our powers of observation". Other instabilities emerged when "the force at work" was not gravity "but chemical affinity", and "the slightest impulse of any kind" might trigger off a sudden chemical reaction. The best known instance of that process was represented by gunpowder: "the slightest spark" could bring about "the instantaneous and violent generation of a vast volume of heated gas". In brief, both the natural world and scientific practice offered two kinds of "machines or structures": the former were characterised by their stability and "*calculability*", and the latter by their instability and "*incalculability*". Astronomical predictions represented the best instance of calculability whereas explosions, together with their "sudden and violent transmutation of energy", represented the best instance of incalculability (Stewart 1873, pp. 155-9).

According to Stewart, any living being could be looked upon as "a machine of a delicacy that is practically infinite", whose motions "we are utterly unable to predict". Living beings represented the third level of instability and incalculability after the mechanical and the chemical, and their complexity exceeded at length the complexity of first and second level machines. A different kind of action was involved indeed, because "the power of an animal, as far as energy is concerned", was not "creative, but only directive". In living beings, Stewart saw a shower of small perturbations that triggered off a series of energy release. It is worth remarking that he did not expect "to have discovered the true nature of life itself"; he had only confined himself to pointing out a structural analogy. Life could be "associated with machinery of a certain kind", where "an extremely delicate directive touch" was "ultimately magnified into a very considerable transmutation of energy". He insisted on this specific issue: he was not able to offer any mechanical explanation of how a living being worked. He had simply outlined a very general operating principle, which offered a useful analogy rather than a solution for "the problem as to the true nature of life" (Stewart 1873, pp. 161-3).

In brief, life was associated with "delicacy of construction", and in its turn "delicacy of construction" entailed "an unstable arrangement of natural forces". Those forces were nothing else but "chemical forces" acting in the same way as in thermal engines, where explosions took place. Nevertheless, two important differences had to be stressed: differently from physical-chemical machines, living beings could rely on an internal organisation, and were "the subjects of decay". Organisation and decay of living beings were definitely outside the scope of Stewart's elementary treatise, but the insistence on those differences allowed him to stress that he was dealing with analogies rather than explanations (Stewart 1873, pp. 164-5).

In 1844 Mayer had been puzzled by processes involving a sudden release of *force*, and in 1876 he devoted a short paper, "Ueber Auslösung", to the subject. From the outset, the two key words and concepts were "sudden release [Auslösung]" and "triggering action [Anstoß]" or impulse, and both concepts were involved in explosive processes: the latter could be looked upon as the first stage of the former. Mayer's use of the two words, and their semantic scopes indeed, were not so definite, and sometimes it is not easy to ascertain whether *Auslösung* corresponded to the early triggering action or the whole process of sudden energy release. In reality, the distinction does not seem so important in Mayer's text because the processes he described are definitely clear. Making reference to his 1842 paper, he reminded the readers that in those processes "no quantitative relation between cause and effect" could be found, since the cause might be "an infinitely small quantity". Nevertheless, no exception to the sentence "causa aequat effectum" could be admitted: as a consequence, the expression "cause and effect" had to be used "in a completely different way". From the mathematical point of view, cause and effect had no unity of measure in common: triggering processes lay outside the mathematical domain. They were qualitative rather than quantitative processes, and "qualities" could not be "numerically determined" (Mayer 1876, pp. 104-5)[24].

According to Mayer, triggering processes played an important role in life sciences, in particular "in physiology and psychology". Even in organic chemistry, and more specifically in the phenomena of fermentation, the *Auslösung* was at stake. Human life depended on a network of processes of the same kind, and the motions of our limbs represented a meaningful instance. He classified human motions as "instinctive, half-conscious, and conscious": the last ones were "the most evident representation" of the strength and effectiveness of those basic processes. Conscious motions consisted of the contraction of some muscles, and the triggering action took place through the excitement of the correspondent nerves. Mayer's estimation of the nervous impulses speed was "about 30 meters per second", and it led to a very sudden process over the "short distances" of nerves. Human volition travelled through those nerves, and triggered off "the desired action" (Mayer 1876, pp. 105-6).[25]

It is worth remarking that different attitudes towards the possibility of a mathematical representation for life processes emerged in the 1870s. The following year, the Belgian philologist, philosopher, scientist, and psychologist Joseph Delboeuf published a paper on a mathematical model for the propagation of variations in a given biological species.[26] He had decided to tackle some mathematical objections to Darwin's theory: he aimed at supporting mathematically the persistence of unpredictable variations in a species. More specifically, his mathematical model showed that "the number of varied individuals could overtake the number of the unchanged ones". He represented a variation as the possibility of adding or subtracting a specific quantity in a symmetric way. In the model, an individual could generate n unchanged individuals, 1 individual endowed with the specific strengthened feature, and 1 with the weakened one. The number $n+2$ corresponded to the individual "*generative power*". After some generations, the process gave birth to a set of "homogeneous" and two sets of "heterogeneous" individuals (Delboeuf 1877, pp. 672-3).[27]

[24] The historian of science Luca Guzzardi stressed "the influences of metamorphic conceptions (which stemmed from the context of life sciences) on physical sciences", and the role played by the concept of *Auslösung* in the late nineteenth-century physics and chemistry (Guzzardi 2001, pp. 146 and 150). I am indebted to Guzzardi for having drawn my attention to Mayer's *Auslösung*.

[25] He remarked that motive nerves and "sensory nerves" had "a common centre", and therefore a healthy "*Auslösung* apparatus" produced a subjective "perception of fitness". A perturbed apparatus could transform "pleasure into pain" (*Ibidem*, p. 106)

[26] In the same year Delboeuf was elected member of the Belgian Royal Academy of Sciences. The official biography of the Académie Royale des Sciences, des Lettres et des Beaux-Arts de Belgique qualifies Delboeuf as "philosopher, psychologist, philologist, naturalist and mathematician". (*Biographie Nationale publiée par l'Académie Royale de Belgique*, p. 164)

[27] Unfortunately, the simplified mathematical model was not in tune with Darwin's theoretical core: Delboeuf's hypothesis that death affected all populations in the same way was obviously in contrast with Darwin's theory.

Delboeuf's computation rested on the symmetry between the increasing and decreasing variations, and on the equal opportunity of life. After the first step, there were only three kinds of individuals, which he labelled A, A+1, and A-1. After the second step, there was the opportunity of a more various descent: A, A+1, A-1, A+2, and A-2. After m steps, a symmetric set of labels were at stake: A+1, A+2, …, A+m, and A-1, A-2, …, A-m.

The first descent contained n individuals A and one individual A+1 and A-1. The second descent contained n^2 individuals A who descended from the previous n and 2 individuals generated by the previous A-1 and A+1. The modified descents A-1 and A+1 could rely on n individuals generated from their immediate ancestor, and on n individuals stemming from the n individuals A in the previous generation. This computation gave rise to n^3+6n individuals A, $3n^2+3$ individuals A-1 and A+1, and 3n individuals A-2 and A+2 (Delboeuf 1877, pp. 673-4).

The Belgian scholar stressed that the ratio between the number of (A+1)-modified and unmodified descents increased over time. The ratio was 1/n after the first step, $2n/(n^2+2)$ after the second step, and $(3n^2+3)/(n^3+6n)$ after the third step. He found that another ratio was as interesting as the above computed one, namely the ratio between the whole of the modified and unmodified descents. This ratio was 2/n after the first step, $(4n+2)/(n^2+2)$ after the second, and $2(3n^2+3+3n+1)/(n^3+6n)$ after the third. Skipping Delboeuf's series of long computations, we can say that the ratio was greater than k/n, where k was the depth of the step or "the number of generations". In brief, the model led to a modified population definitely greater than the unmodified one after a number of steps equal to n. He showed that, for $n=10$ and therefore generative power 12, only 8 steps was required in order to realise that numerical overtaking (Delboeuf 1877, pp. 674-6).

In reality, from the biological point of view, the success of a specific variation was more important than the amount of a generically varied population, and Delboeuf did not miss the point. He briefly claimed that the amount of a population of the kind $A \pm m$ could not reach the original population "even though its relative importance does really increase". A simple computation for the population ratio $(A \pm 1)/A$, where terms inversely proportional to n are disregarded, shows that the value is approximately

$$\frac{k \cdot n}{n^2 + k(k-1)}.$$

If we search for the k value that makes the ratio 1, we find that the corresponding equation $k^2 - (n+1) \cdot k + n^2 = 0$ yields real solutions for only $n \in [-1/3;+1]$, and this is actually inconsistent. However the mathematical model also excluded the opposite outcome, namely the possibility of "recovering the primitive population". As Delboeuf pointed out, the process that led from $A \pm 1$ to A was numerically weaker than the opposite process (Delboeuf 1877, pp. 676-7).

The paper does not contain original mathematical developments or applications, and the only generalisation consisted in writing a very general expression for the term $T_{m,p}$, which represented "the number of individuals of the generation $A \pm m$ after a number p of generations". The biological side of the model suffers from some misunderstandings because of the semantic extension of the word "evolution". He overlapped biological evolution and "an ideal of perfection", and explicitly stated that "evolution and progress are almost synonymous". Moreover he was interested in "the cause that gives birth to an ever more perfection in some species", and thought that "the cause could not be merely found in adaptation". Adaptation could only be "an indefinite cause of variation": in no way could it be "a cause of progress". His mathematical model involved "an indefinite variation" triggered off by "a persistent cause of transformation", but it could not account for "a gradual improvement". He saw an intrinsic limitation of his own mathematical model because he saw progress as more important than variation. In this context he stressed the role of "intelligence" as the real engine of evolution, or more specifically "the first cause of evolution". The contrast with Darwin's theoretical approach is really striking: sensitivity, intelligence, and freedom were not the outcome of variation and evolution but rather causes of evolution. He imagined that "the universe, in its initial state", did contain those three features "at least in its embryonic form", in the same way as it contained matter and motion (Delboeuf 1877, pp. 674, 676, and 678-9).

In December 1878, Maxwell published a three-pages paper in the journal *Nature*, which was formally a review of the book *Paradoxical Philosophy* Balfour Stewart and Peter Guthrie Tait had recently published.[28] On the second page Maxwell went back to his previous cogitations on singular points or "singular phases" where "a strictly infinitesimal force" might "determine the course of the system" towards "any one of the finite number of equally possible states". He mentioned Stewart's *The Conservation of Energy* with reference to the physical side of the problem, and Saint-Venant and Boussinesq, who had analysed "the corresponding phase of some purely mathematical problems". Once more the issue was raised in connection with the difference between living and dead matter. He specified that the difference involved neither matter nor "that more refined entity" called *energy*. A third level was involved, where "the application of energy may be directed without interfering with its amount". It dealt with a directive power, and the power of mind was a suitable instance of that peculiar power. He mentioned "the engine driver, who does not draw the train himself" but rather "directs the course of the steam" in order to "drive the engine forward or backward, or to stop it". It was only "*in general*" that "the present configuration and motion" could determine "the whole course of the system". There could also be "certain isolated and singular phases" where a tiny impulse could change the pathway of a huge amount of energy (Maxwell 1878, pp. 760).

The following year Maxwell mentioned Boussinesq and Saint-Venant once more: the former was informally labelled a scholar "of hydrodynamic reputation" whereas the latter a scholar "of elastic reputation". He credited Boussinesq with having done "the whole business by the theory of the singular solutions of the differential equations of motion" in his 1878 essay. In just a few words Maxwell managed to grasp the essence of Boussinesq's research programme: "when the bifurcation of path occurs" – he wrote – the

[28] The book was intended as a sequel to their successful *The Unseen Universe*, which had been published some years before.

material system "ipso facto invokes some determining principle". That principle was definitely "extra physical" although in no way "extra natural", and allowed the system "to determine which of the two paths it is to follow". Maxwell also managed to grasp the difference between Stewart and Saint Venant's *physical* approaches, and Boussinesq's *mathematical* one. The first two scholars had assumed the existence of "a certain small but finite amount" of force or energy, whereas the third had "managed to reduce this to mathematical zero". Stewart and Saint-Venant made reference to actual physical processes whereas Boussinesq made reference to mathematical models that stemmed from actual physical processes but could be looked upon as a new starting point for further structural analogies. In fact Stewart's "trigger-work" or Saint Venant's "travail décrochant" were conceptually different from Boussinesq's guiding principle. In the end, Maxwell appreciated the philosophical scope of "Boussinesq's method" inasmuch as it was "a very powerful one against metaphysical arguments about cause and effect", and offered a better alternative to "the insinuation that there is something loose about the laws of nature". In other words, Maxwell acknowledged that, however questionable Boussinesq's research programme might be, it had the advantage of relying on mathematics and natural laws, and therefore it could retrieve extraordinary or "singular" events within the domain of a scientific theory (Maxwell 1879, pp. 756-8).

In 1880, Du Bois-Reymond commented on the debate that had been raised by his 1872 lecture. Once more he stressed, on the one hand, the "impossibility of understanding the nature of matter and force", and on the other, the impossibility "of explaining consciousness on mechanical foundations". He deployed seven queries or difficulties [Schwierigkeiten]: the nature of matter and energy, the cause of motion, the origin of life, "the apparently intentional and purposive disposition of nature", the origin of the simple sensorial perception, the existence of rational thought and language, and the existence of free will. The last query was extensively discussed by Du Bois-Reymond: he started from the question "whether human beings were really free to act or were bound and determined by inescapable constraints". After having discussed some philosophical approaches in ancient times and in the Middle Ages, he pointed out what he considered the answer offered by current science. The conservation of matter and energy prescribed that the present state of the world, our brains included, was "the definite mechanical effect" of its state in a previous instant, and was "the definite mechanical cause" of the following state. However astonishing the association might be, he found that the state of "brain molecules [Hirnmolekeln]" was as definite as the fall of dice. According to a "monistic point of view", our volitions are "necessary and undisputed epiphenomena of motions and rearrangements of brain molecules". The universe would be a mechanism, and "in a mechanism there is no place for free will" (Du Bois Reymond 1880, pp. 65, 74-6, 79-80, and 82).

In the last part of his lecture, the renowned physiologist mentioned "the late mathematician Cournot", Boussinesq, and "the Parisien Academic Saint-Venant". They were credited with having claimed that "motion could be produced, or a change in the direction of motion could be realised, without any expenditure of force". In particular, Cournot and Saint-Venant had introduced "the concept of triggering action [Auflösung] or *décrochement*". According to Du Bois-Reymond, Boussinesq had followed a slightly different pathway: he had pointed out that "some differential equations of motion" led tot singular solutions, and "ambiguous or completely undetermined" states of motion followed. It was "a kind of mechanical paradox" that had already been noticed by Poisson. The main aim of the French mathematician was rightly grasped: the possibility of a better comprehension of "the multiplicity and uncertainty of organic processes". At the same time, "the German school of physiologists", he himself included, relied on "a particular kind of mechanism", and could not be satisfied with such an approach. Underneath Boussinesq's guiding principle they saw the old and unreliable vital forces, in spite of Boussinesq's reference to the French physiologist Claude Bernard (Du Bois Reymond 1880, pp. 88-9).

According to Du Bois-Reymond, a theoretical approach to simple forms of life, or "unconscious living beings", could be pursued "without any reference to bifurcations in integrals or guiding principles". He found that the difference between crystals and living beings lay in the conditions of equilibrium: inorganic matter was "matter in a state of steady equilibrium" whereas organic matter was in a state of "completely unstable equilibrium". With regard to great amounts of energy that could be delivered by a little triggering action, he conceded that there was no quantitative relationship between the former and the latter. At the same time he thought that the latter could "not decrease under a given threshold", and the threshold could not be zero. If a zero value for energy was ineffective for triggering off a transformation of energy from potential to actual, "no guiding principle of immaterial nature" could steer "a material point on the top of a knoll". However he specified that his seventh query did "not necessary entail the rejection of free will or its representation as a deception": the query could rather be looked upon as a "transcendental" riddle. In the end, no specific solution was offered, and he let us believe that even his mechanistic meta-theoretical option was *transcendental*. The last word, "*Dubitemus*", seems more the acknowledgement of an intellectual stalemate than an actual answer to very demanding queries (Du Bois Reymond 1880, pp. 89-92).

Boussinesq's mathematical and philosophical approach puzzled the physiologist Du Bois-Reymond no less than the mathematician Bertrand. Only the physicist Maxwell appeared as more sympathetic towards it. However it seems that Du Bois-Reymond managed to grasp the specific weakness of Boussinesq's research programme better than Bertrand. In unstable equilibrium, when the transition from ordinary to singular solutions took place, mathematics was silent, and philosophy even too talkative, but physics imposed a definite answer: a negligible but non-zero amount of energy was in order. Indeed the link between the mental activity and its physical effects really appeared more problematic. A nervous impulse carried energy, and could trigger off a much greater amount of energy, but it was doubtful whether the purely mental act that prompted the nervous impulse did really require energy. As Maxwell had pointed out, the connection between mind and body could probably find a suitable representation in Boussinesq's mathematical and philosophical interpretation of singular solutions.

4. The debate was taken over by philosophers

Structural analogies between physical and biological processes, the questionable link between mathematical physics and free will, and determinism in general attracted the attention of philosophers after having raised some debates in the scientific context. Philosophers made use of words and concepts very different from the words and concepts belonging to the tradition of mathematics and mathematical physics. Boussinesq, Barré de Saint-Venant, and Bertrand had spoken of "singular solutions" of differential equations, bifurcations and determinism with reference to a specific field of mathematics, whereas philosophers widened the scope of the debate, and focused on very general themes. In general it does not seem that those scientific debates managed to open new perspectives in philosophy, even though sometimes mathematical and scientific issues became instrumental in criticising or upholding traditional philosophical thesis. Different attitudes towards science emerged, and two main attitudes can be singled out. On the one hand we find philosophers who were acquainted with recent developments in science, and acknowledged the philosophical meaningfulness of specific contents and theories. On the other hand, we find philosophers who underrated the scope of scientific enterprise, and expected that no scientific achievement or research could be philosophically meaningful.

In reality philosophical debates on determinism had already begun. In 1872 a French philosopher with interests in politics and sociology, Alfred Fouillée, had published a book on "freedom and determinism", with the explicit aim of "conciliation" between them. He found that "the method of conciliation" was better than "the method of refutation" in the same way that "liberalism in the social context" was better than "repressive means". No philosophical system could encompass the whole "truth", which did not stem from the exclusion but rather the inclusion of different, even "opposite" contents. According to Fouillée, "the present state of science" was not consistent with a radical determinism: "the structured net" of "causes" and "effects", which was one of the hallmarks of scientific practice, called into play a complex interaction between "reason" and "experience" (Fouillée 1872, pp. v-vi, 5-6, and 8-10)[29].

In the second chapter of his book, Fouillée stressed the difference between a sound determinism, which was consistent with contemporary science, and a blind determinism that was not so different from "the lazy sophism of oriental fatalism". The latter led human beings to believe that "phenomena occur despite causes", whereas the former implied that phenomena occur "according to causes". In a sound determinism, the subject and his/her thought entered on the scene, and they played an important role in the chain of rational connections between causes and effects. For instance, "our knowledge about blood flow" did not affect "directly the flow itself", but beliefs "in the necessity of preventing us from danger" were among "the causes of the motion of our limbs". There was an interaction between consciousness and free will, on the one hand, and material processes, on the other: that interaction assured that no incompatibility could exist

[29] In the same year Fouillée became *maitre de conférences* at the *École Normale Supérieure* in Paris.

between the freedom of human beings and the necessity of natural laws (Fouillée 1872, pp. 11-12).

In 1878 Charles Renouvier, a philosopher who had founded the French journal *La critique philosophique* in 1872, and never held an academic position, sharply criticised Balfour Stewart. He focused on the transformations of energy, in particular the application of "the principle of living forces" to "some vague, indefinite forces, which cannot be measured". He made reference to a French translation of Stewart's 1873 booklet, and his criticism was without appeal. He had nothing to say about "the quantitative transformations and conservation of that energy that was able to produce work", but he found that "an obscure metaphysics superposed to physics" could not be accepted. He did not enter into details but confined himself to blaming Stewart for having confused "the real object of scientific researches" with "indefinite concepts" (Renouvier 1878, pp. 168-70).

In 1879 the French-speaking Swiss philosopher Ernest Naville published a paper under the title "La physique et la morale" in the French journal *Revue Philosophique de la France et de l'Étranger*. In the first three passages of the paper we find many occurrences of words like "*la pensée*", "la morale", "*l'ordre spirituel*", "*faits spirituels*", *phénomènes spirituels*", and "*phénomènes psychiques*" (Naville 1879, pp. 265-6).[30] Nevertheless he focused on science, and pointed out two opposite trends in recent scientific practice. On the one hand, he found deep connections among different sciences, and the explicit acknowledgment of those connections, in spite of the process of professionalization and specialisation that had taken place in the second half of the century. He mentioned the tight links between physics and physiology, and between physiology and psychology. On the other hand, since the emergence of modern science, natural philosophers and scientists had stressed "the sharp difference between material facts as experienced by senses, and mental facts as experienced by mind". He took an intermediate way, where a mutual influence between matter and mind could not be excluded, but any kind of sharp reductionism was definitely rejected. Mind and thoughts could not be imagined as the results of mechanical processes, even though "molecular motions, or waves" could offer "the condition of thinking". In particular, he found unacceptable Spencer's radical reductionism. Although human perceptions could be looked upon as the effect of a chain of mechanical processes, in no way the act of thinking and the content of thought could automatically be reduced to matter and motion (Naville 1879, pp. 265-7).[31]

A radical answer to the most radical determinism had already been given by Renouvier: physical principles of conservation might not be universally applied to all kinds of processes. Nevertheless, Naville was unsatisfied with this perspective, and was to take

[30] The English translations of these words could be: thought, ethics, mental/spiritual domain, mental/spiritual facts, mental/spiritual phenomena, psychic phenomena. Naville was professor of philosophy at the University of Genève, and theologian and minister of the Evangelical Protestant Church. He aimed at the reconciliation between science and religion, and in 1883 he published *La physique moderne: études historiques et philosophiques* (in 1884 the book was translated into English).

[31] He insisted on the problematic link between the physical bases of life and the activity of mind (*Ibidem*, pp. 272-3). He mentioned and widely quoted from the French edition of Spencer's *The First Priciples*, which had first been published in 1862.

another pathway. He did not find good reasons to give up the principle of conservation of energy: he was rather more interested in showing that the existence of specific principles of conservation did not collide with "the commitment to defend moral freedom". In the context of "a positive and cautious" scientific practice, it could be assumed that both "physical laws, and specific laws for living beings" were at stake in life processes. He reported Claude Bernard's conception of a specific "living force" which did not oppose ordinary physical laws: the renowned physician had specifically spoken of "a *legislative* living force, which was not *executive*". In other words, living beings could be looked upon as the seat of specific actions, but those actions stemmed from a "*driving*" rather than "*creative*" power. Those "plastic forces" could not oppose the physical laws of conservation, because they acted at a different level (Naville 1879, pp. 273-7 and 281-2).[32]

In the end, Naville found "no conflict between physics and ethics". Human beings could not "create energy [force]", but could make use of the amount of energy at their disposal in accordance with the laws of physics. Human beings could freely decide how to make use of their energies: they could choose "for good or for evil". Ethic principles, or the guiding principles which acted in accordance with free will, were placed "outside the domain of mechanics". He claimed that "the laws of mechanics" could be applied "to everything", but they "do not explain everything". Nevertheless the existence of some kind of logical or philosophical link between mechanical effects and free actions deserved to be explored. According to Naville, his balanced option could save both the determinism of physical laws and the freedom of "moral judgment". In the last passage of his paper, he claimed that his perspective was not so different from Boussinesq's, who had arrived at similar conclusions "by means of mathematical considerations" (Naville 1879, pp. 284-6).

Two years later, Delbeouf published a paper on the same subject in the *Revue Philosophique de la France et de l'Étranger*. The paper, "Determinisme et liberté – la liberté démontrée par la mécanique", appeared in three different parts, in two subsequent volumes of the journal. Probably because of his manifold training as a philologist, mathematician, and psychologist, he was able to grasp both the core of Boussinesq's research programme, and its scientific and philosophical frailty. On the borderline between science and philosophy, he attempted to cope with the problem of freedom, which he found at the same time "fascinating and discouraging". From the outset, he pointed out some paradoxes that stemmed from the supposed opposition between freedom and determinism. He found that, on the track of a sharp determinism, "a determinist who strove to uphold his thesis was nothing else but a puppet in the hands of fate". In other words, a coherent determinist should be aware that her/his determinism is not a free belief, but a consequence of determinism itself. If determinists applied determinism to themselves, they would fall either into a contradiction or a vicious circle.

[32] He mentioned Claude Bernard's *Leçons sur les phénomènes de la vie communs aux animaux et aux végétaux*, and *Rapport sur le progrès et la marche de la phisiologie générale*. The former was published in the same year (1879), whereas the latter had been published in 1867. In the scientific context, the word "force" and the principle of conservation of *force* had already been replaced by the word *energy*, and by the principle of conservation of *energy*.

On the other hand, a naïf conception of freedom would be pointless, since it would allow ourselves to "subvert every computation, … and would transform axioms into a mere illusion" (Delboeuf 1882a, pp. 453-5).

Delboeuf was aware of the debate on determinism which had emerged from the scientific and mathematical context: he mentioned Cournot, Boussinesq, Janet, Saint-Venant, Bertrand, and the German physiologist Emile Du Bois-Reymond. In particular, he found that Boussinesq's essay on the mathematical roots of indeterminism was "rich in scholarly formulae and clever metaphysical remarks", but was "quite confused". He qualified Boussinesq's guiding principle as a "*deus ex machina*", and criticised him for having looked upon "specific mathematical abstractions" as "facts". Moreover he found unconvincing that a material sphere in unstable equilibrium could be put in motion without any force: however little or negligible it could be, only a force rather than a guiding principle could put it in motion. The concept of freedom was not suitable for mechanical processes, where a sphere could fall towards equally probable directions, because those directions were "*totally independent* from each other". From the mathematical point of view, there was a perfect symmetry; from the ethical point of view, there was a sort of *indifference*. Freedom called into play something more complex: when freedom was practised, it transformed the landscape of facts and feelings. In the end, Delboeuf did not trust in the "ingenious" attempts Cournot, Saint-Venant and Boussinesq had put forward, because they were considered as "artificial and unworkable" (Delboeuf 1882a, pp. 467, 469-70, 475, and 477-8).[33]

In the second part of his paper, Delboeuf tackled the conceptual link between freedom and time: freedom depended on the possibility of making use of time. If a person could delay some actions, he/she were free, and "any forecast" would become "impossible". The explosion of a gunpowder box produced a given amount of energy, but a person could choose whether to burn it "today or tomorrow". The effects of the choice between two different times might be quite different: for instance, "a useful work today, and the death of centuries of people tomorrow". The same thing might happen in the case of a gun trigger. According to Delboeuf, the freedom of deciding when a given action should be performed could really overcome Laplace and Du Bois-Reymond's determinism (Delboeuf 1882a, pp. 613-16 and 623).

He addressed specific physical contents with a certain degree of competence, but the flux of thoughts and words followed a tangled pathway. He wandered through a crowd of instances, which encompassed physics, astronomy, and ordinary life: a stone which broke away from a mountain, and then rolled, leapt, and hit various hurdles, …, or a little ball in motion in the reference frame of a moving ship, or an asteroid in motion around the Sun, ... In the end, he insisted on the specific feature of intentional motions in living beings, which he identified with "discontinuity". As an instance of that kind of discontinuity he mentioned the sudden and intentional attack of a wild animal on its prey. In conclusion, a specific "kind of power" was at stake in non-deterministic processes. That power was

[33] He criticised both Boussinesq and Cournot. See, in particular, Delboeuf 1882a, p. 478: "L'être libre n'est pas dans un monde à lui, il est dans le monde, et, au moment où sa liberté se déploie, il donne une physionomie nouvelle à la scène qui s'y jouait".

involved in volitions of living beings, and it was different from the "initial forces that triggered off and maintain the motion of the universe". Boussinesq' guiding principle could suitably be applied to intentional actions rather than biological processes in general or physical processes (Delboeuf 1882a, pp. 626-9 and 632).[34]

In the same year, in the same journal, and in the same volume, Fouillée published a paper on the same subject. If the title mentioned "the new contrivances in favour of free will", the subtitle specified that he was to criticise "logical and mechanical" aspects. He criticised Naville, "the learned mathematician and psychologist Delboeuf", and reported Bertrand's criticism to Boussinesq, but his physical remarks are quite vague. He also criticised Saint-Venant's interpretation of explosive processes: he stressed that "*an increasingly vanishing* [*aussi petite qu'on veut*] force is different from *no* force" at all. In Fouillée's reconstruction, the gun trigger represented an analogy for Boussinesq's bifurcations, but the analogy seems intrinsically misleading. He analysed the concept of "guiding principle", and concluded that it led to "improbable" consequences. The principle would be "in contrast with the principle of action and reaction": as a consequence, "the conservation of the centre-of-gravity motion", "the conservation of the linear momentum", and "the principle of areas" were in danger. In this context he mentioned "the possibility of a *clinamen*", which he attributed to Epicure and Descartes. The *clinamen* violated the principle of conservation of energy, and allowed men to create "motive force", but Boussinesq's guiding principle had nothing to do with Epicure's *clinamen* (Fouillée 1882, pp. 585, 600-2, 604, and 608-9).

In the next issue (January-June 1883) of the same philosophical journal Fouillée published a three-pages *Note* which followed another three-pages *Note* by the French *polytechnicien* and historian of mathematics Paul Tannery. The two *Notes* were published under the common title of "*Le libre arbitre et le temps*", and were specifically devoted to criticise Boussinesq, Naville and Delboeuf. Although the content of Tannery's text was in general agreement with Fouillée's theses, his language and his general style appeared as definitely less radical. Tannery acknowledged that, "in accordance with the principle of causality" as it was "intended nowadays", determinism could be considered as "its preferred logical consequence". He stated that he was "in perfect agreement with Fouillée", and he was satisfied with "the Fouillée conception of free will", but he found that "future could not be exactly foreseen", and it was not the consequence "of a mere mechanism" (Tannery 1883, p. 85).

On the contrary, Fouillée found that "logic, physics, and psychology" imposed strict constraints to mathematical models, and the evolution of the world was more deterministic than Tannery was willing to acknowledge. To explore the boundaries of mechanics in order to account for the existence of free will, appeared to him as pointless, because mechanics was "the domain of the utmost determinism and *passivity*". Neither bifurcations nor time could be called into play, and Delboeuf's remarks on the possibility of time delays were nothing else but an act of faith in "a sort of mechanical miracle" (Fouillée 1883, pp. 86-8).

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[34] He labelled "theorem" or "axiom" the definition of determinism, and "corollary" the intrinsic link between determinism and continuity (*Ibidem*, pp. 627 and 630).

In 1883 Renouvier went back to the question of free will, and criticised the theses Fouillé had put forward the year before. He opposed Fouillé to Cournot, Saint Venant, Boussinesq, Boutroux, Naville, Secretan, Delboeuf and he himself, a list of scientists and philosophers who have attempted to "explain the possible existence of a certain degree of indetermination in the natural order". He put scientists and philosopher together but this must not deceive us: with regard to some specific issues such as free will, he found that only philosophers were allowed to speak. With regard to "some important questions", which had been debated for "two thousand and five hundred years, and even today", he claimed that science really did "*know nothing*". Some typical philosophical issues were not suitable for "being tested by experience"; nor could they be changed by a series of better observations. Some facts or events could be submitted to experiments and computations, but other entities could not, because they were not "observable facts" but general laws "encompassing the totality of facts of a given kind" (Renouivier 1883a, pp. 371-2 and 375).[35]

In a following paper Renouvier focused on a more specific issue, namely the breakdown of equilibrium or explosive processes. Those processes offered a mechanical representation of "the sudden transformations of energy that actually take place in living beings". The question was whether those discontinuous transformations could be triggered off by "a merely mental action", without any expenditure of mechanical energy. He leant towards a positive answer. Despite the incommensurability between "the mental force" and "the transformation of mechanical forces that follows the breakdown of the equilibrium", a "*law of correspondence*" between them had to be assumed. Nevertheless, that correspondence remained quite mysterious because quite mysterious the foundations of science really were. Not only were "the fundamental natural laws unexplainable" but also the relation of causality was unexplainable. In the end, a sceptical point of view emerged. He did not claim that mental acts did not require any expenditure of physical energy; he confined himself to "showing that it might be so". He found that the assumption of a mere "*possibility*" did not distance him from Cournot, Saint-Venant and Boussinesq, but the relationship between his mental force and Boussinesq's guiding principle was not further clarified (Renouvier 1883b, pp. 389, 391, 396, and 398-400).

In 1884 the American philosopher and psychologist William James addressed the students of the Harvard Divinity School delivering a lecture on "The Dilemma of Determinism". From the outset he credited Renouvier, Fouillée, and Delboeuf with having "completely changed and refreshed … the form of all the old disputes". He focused on the pliability of the world towards our attempts of interpretation. The world had "shown itself, to a great extent, plastic to this demand of ours for rationality", and that pliability bridged the gap between the ontological and epistemic level. At the same time, he underlined the mythological nature of every belief, rational beliefs included. Both scientific and philosophical "ideals" were nothing else but "altars to unknown gods". He took care to separate the "[o]ld-fashioned determinism", which he labelled "*hard* determinism", from the recent or "*soft* determinism", which repudiated "fatality,

[35] It is not easy to follow Renouvier's line of reasoning because of the fragmented and branched structure of his texts, and the exceedingly argumentative style: sometimes his criticism blurred into mockery.

necessity, and even predetermination". The latter could easily be identified with "true freedom" (James 1884, pp. 145, 147, and 149).

The debate continued in the philosophical journals but the specific issues raised by the debate between Boussinesq and Bertrand slowly faded into the background. In 1890 Naville published a book on free will (*Le libre arbitre. Études philosophiques*), but only the third chapter was devoted to determinism, and references were made mainly to Pierre Bayle, Baruch Spinoza, Étienne Bonnot de Condillac, Herbert Spencer, Schopenauer, Fouillé, and Hegel's philosophical theses. The processes of professionalisation and specialisation made the pursuit of cross-fertilisations between different bodies of knowledge ever more difficult, and discouraged further attempts in this direction.

5. Later debates and rediscoveries

In 1914 the physical chemist Wilhelm Ostwald wrote down some remarks on Mayer's 1876 essay, and developed the concept of *Auslösung* in the context of the second principle of thermodynamics. The manuscript remained unpublished until 1953, when the chemist and historian of science Alwin Mittasch published it with the sponsorship of the *German Association of Chemists*. Ostwald was actually aware of Mayer's 1876 paper, and the letter Mayer had sent to Griesinger, where the renowned physician had managed "to explain the question at issue better" than he himself would have been able to do. At the same time, Mayer "had not been able to completely solve the problem", because he had confined himself to "triggering processes in the organic world" (Ostwald 1953 [1914], pp. 19 and 21-2).

In 1922 a new edition of Boussinesq's essay appeared, and the following year a review in the journal *Isis* also appeared. The reviewer qualified Boussinesq as "professor of experimental and physical mechanics" and "thoughtful philosopher", and guarded the reader against the risk of looking upon the booklet as "a piece of metaphysics". Boussinesq's main thesis was synthesised into the claim that physical laws and differential equations did not have to be identified with "absolute determinism", and could not be in contradiction with "free will and responsibility". Philosophers continued to debate on determinism, and Laplace and his powerful metaphor played an important role in it, but it seems that they had forgotten Boussinesq's research programme. Nevertheless a short passage can be found in a lecture the mathematician (with wide cultural interests) Karl Pearson gave on the early history of statistics presumably in the first term of the academic session 1925-6. He remarked that, however "remote from morality" the singular solutions of differential equations might appear, Saint-Venant and Boussinesq's researches would not be grasped unless we realise that "they viewed Singular Solutions as the great solutions of the problem of Freewill". He made also reference to "a letter of Clerk-Maxwell in which he states that their work on Singular Solutions is epoch-making ", which is probably the letter Maxwell sent to Galton. (Guinet 1923, pp. 483-4; Pearson 1978 [1925], pp. viii, xiv, and 360).[36]

In a manuscript written in 1932 but which remained unpublished until 1990 (after the author's death in 1968), the French philosopher Alexandre Kojeve stressed the difference between "causality" and "legality", and claimed that the latter deserved more attention in the debate on "determinism". He focused on that "*minimum* of determinism" which lay at the foundations of classical physics, and that minimum entailed "the idea of legality" or the necessity of a physical law. He found that "the notions of cause and effect are not involved (at least explicitly)" in "the principle of determinism of classical physics", and he quoted from Kant's *Critique of pure reason* in order to uphold his philosophical and historiographical view: scientific practice was based on the necessity of rules or laws. The metaphor that Laplace had put forward corresponded to "the mathematical expression of this principle": the past and the future of "a physical phenomenon" could be derived from

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[36] As far as I know, Ian Hacking was the first to point out Pearson's remark (Hacking 1983, pp. 464-5).

blending "the differential equations which rule the evolution" with their initial conditions. In its "hypothetical version" the metaphor was in some sense "a *tautology*", whereas the demand for "a universal application" of that principle corresponded to a stronger kind of determinism, or "the doctrine of causal determinism". The latter maintained that "*in principle*" the prediction and retro-diction could affect both "the world as a whole" and "every isolated section of the world". In other words, Laplace's *Intelligence* was "nothing else but a fictive mind", and two different interpretations were at stake: prediction as "infinitely far away or infinitely accessible target". They led to two opposite meta-theoretical options: an asymptotic impossibility on the one hand, and an asymptotic possibility on the other. He found the second interpretation more suitable, and claimed that "Laplace's ideal" corresponded to the possibility of "approaching indefinitely the ideal of a definite, detailed, and universal prediction". (Kojeve 1990 [1932], pp. 27-8, 44-5, and 47-9).

In 1937, in the book *Determinismus und Indeterminismus in der modernen Physik*, Ernst Cassirer addressed indeterminism in the context of classical and quantum physics. The first chapter was devoted to "Laplace's Mind" because he acknowledged that Laplace's figure of speech had played an important role in subsequent debates. Nevertheless he stressed that the expression was "slightly more than a ingenious metaphor": it had been devised in order to "clarify and highlight the difference between the concept of probability and that of certainty". According to Cassirer, Emil Du Bois-Reymond had been the first scholar to emphasise that metaphor: he had given it "prominence in the contexts of science and theory of knowledge". He had put forward a sort of "ideal" or mythology of omniscience [Allwissenheit], where "the past and future course of the world would be perfectly clear in all details". That mythology stemmed from the "identity between scientific and materialistic-mechanistic knowledge". The imaginary pattern of knowledge, which Laplace had outlined and "Du Bois-Reymond had subsequently developed and amplified", was nothing else but an "idol". It was "an empty concept", devoid of any empirical content, and it was useless even as "a methodological precept or guideline for our knowledge". In the book postumhously published in 1950, *The Problem of Knowledge - Philosophy, Science and History since Hegel*, which can be looked upon as the last volume of the series *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit,* Cassirer commented on Du Bois-Reymond once more. In the talks the authoritative physiologist had addressed to the German association of scientists and physicians in the 1870s and 1880s, he saw a mixture of "skepticism and dogmatism". In the end, Du Bois-Reymond's identified the impossibility of a mechanical explanation with the impossibility of any explanation (Cassirer 1937, pp. 7-9, 11, 16, and 32; Cassirer 1950, p. 87).

After the Second world war Karl Popper remarked that, differently from quantum physics, which was "generally admitted to be indeterministic", classical physics was "usually "taken to be determininistic", but that "opposition" was "misleading". He found that "most systems of physics, including classical physics, were indeterministic "in perhaps an even more fundamental sense than the one usually ascribed to the indeterminism of quantum physics". As the more extreme instance of physical determinism he considered the "superhuman intelligence" of "Laplacean demon", which failed to escape the indeterminism that he labelled "interference from within". From a logical and computational point of view, there were "finite specified classical mechanical predictions tasks which no classical mechanical predictor can perform". According to Popper, "the computing part of a predictor" was not able to give "an accurate answer to every question concerning its own future physical states". No predictor could "fully predict its own closer environment". In the end, classical mechanics was not deterministic, and was forced to "admit the existence of unpredictable events". A mythological or "theological view of science" had led to "the tendency of attributing to Science (with a capital S) a kind of omniscience". What was labelled as "Laplace's determinism" was nothing else but "a misinterpretation" (Popper 1950a, pp. 117, 122, and 128; Popper 1950b, pp. 173 and 193).

In 1953 Mittasch published Ostwald's essay together with a new layout of Mayer's 1876 paper. He pointed out some analogies between Mayer and Ostwald's concepts of *Auslösung*: Ostwald interpreted it as "the release of the available free energy" by an exciting action. It does not seem that the booklet reached a wide audience.[37] In reality Mittasch had already published a book in 1940, *Julius Robert Mayers Kausalbegriff*, and two papers in 1942, in the volume that celebrated the centenary of "the discovery of the energy principle". In his book he credited Mayer with "having begun to transform the essentially limited *mechanical* physics of his time" into a wide-scope "physics of energy". In Schopenauer and Lotze he found some philosophical roots of "Mayer's principle of causality in trigger processes". He mentioned Saint Venant's "travail décrochant" and Boussinesq's "principe directeur" in the context of "*Analytical mechanics*". In his simplified report, Boussinesq had associated the domain of "inanimate entities" with general solutions whereas "the domain of living beings" was ruled by "singular solutions". In the first paper he published in the centenary volume, he focused on the "meaning of Mayer's *Auslösung* for chemistry", and interpreted that concept as "a noteworthy complement" to the principle of energy", however "merely formal, essentially elementary, and non-mathematical" it might be (Mittasch 1940, pp. 55 and 126; Mittasch 1942a, p. 281; Mittasch 1953, p. 7).

In the 1970s the historian of science Mary Jo Nye reminded the readers of the existence of Boussinesq and his research programme, and framed his programme into a conceptual stream that crossed French-speaking countries between the 1870s and the 1890s. She also acknowledged the influence "the mathematician A.A. Cournot and physiologist Claude Bernard" had had on him. From the philosophical point of view, she found meaningful connections between Boussinesq's mathematical and philosophical inquiry into determinism and free will, and the line of research that went from Emile Boutroux to Maurice Blondel. In her historical perspective, Boussinesq's researches were part of "a renaissance in French science", which paralleled a contemporary philosophical

[37] Only three years later a short review appeared, where the physicist and historian of science Hans Schimank qualified the booklet as "excellent for a seminar of the history of science or medicine" (Schimank 1956, p. 190)

renaissance in the last decades of the nineteenth century (Nye 1976, pp. 274, 276, and 289-90; Nye 1979, pp. 107, 110, and 117-9).[38]

In 1983 Ian Hacking endorsed, at least in part, Cassirer's thesis on Du Bois-Reymond's role in the emergence of "Laplace's Mind" as a modern mythology of determinism. At the same time he traced back the origin of the word and concept of determinism to the seventeenth and eighteenth century: in particular, he mentioned the uses and meanings of the word in Kant, the Scottish philosopher William Hamilton, the French Renouvier, and the Americam James. However Hacking thought that "Laplace's celebrated determinist dictum" was not "only a metaphor". He found that Laplace was involved in "a kind of dualism", in the sense that "[i]t was possible for Laplace, like Kant and Hume, to be a determinist about external bodies but not about the mind". Hacking interpreted Du Bois-Reymond' 1872 lecture as "a confession of doubt, of limitation" about his mechanistic monism: he belonged to a German community of physiologists who "proclaimed it would never allow merely mental causation in the study of the brain". He found in Saint-Venant, his former pupil Boussinesq, and Maxwell a common attitude towards the query of determinism, and the emergence of "a completely new idea", which he qualified as "completely crazy" even though it was "embraced by some of the powerful minds of the age". The attractive power of the idea lay in the possibility of encompassing "mind and matter in a new and intimate way": human freedom could be placed exactly at "the empty spot" created by singular solutions (Hacking 1983, pp. 455-60 and 462-5).

From the 1980s onwards, a new trend in philosophy of science led to overlook the tradition of the discipline, and history of science and philosophy in general. A new generation of philosophers skilfully focused on the logical foundations of science, and lost interest in the branched and tangled developments in science and philosophy. In 1986, John Earman, in his *introductory* but authoritative treatise on determinism, depicted determinism as "a perennial topic of philosophical discussion" but claimed that classical physics was looked upon "by philosophers" as "a largely deterministic affair". In reality, it is doubtful whether the supposed point of view should be attributed to philosophers or physicists. However he found the supposition "out of focus": on the contrary, "classical worlds prove to be an unfriendly environment for any form of Laplacian determinism". With regard to Laplace, he remarked that "we get different senses of determinism" depending on "what powers we endow the demon with". He focused on specific physical issues in the context of classical physics, as for instance the possibility of an unbounded speed for particles and heat fluxes. In general, he claimed that ontology and epistemology should be carefully disentangled. He found that "an epistemological sense of determinism" was really misleading, because "we already have a perfectly adequate and more accurate term", and the term was "prediction". In other words, determinism should not be confused with the possibility of prediction. In particular, he found that the "determinism-free will controversy" had "all of the earmarks

[38] She pointed out the existence of a generation of talented scientists and mathematicians who were sensitive to historical and philosophical issues: among them she mentioned Paul Tannery, Pierre Duhem, and Henri Poincaré. She listed Joseph Boussinesq in the number of prominent Catholic members of the *Brussels Scientific Society*, which had been founded in 1875, and in 1877 gave birth to the scientific journal *Revue des questions scientifiques*.

of a dead problem", and that following "the moves in the determinism-free will controversy is an exercise in frustration" (Earman 1986, pp. 1-2, 7-8, 24, 34-5, 41-2, 235, and 243).

In 1987 the mathematician and historian of science Giorgio Israel qualified Laplace's research programme as an "ideal Newtonian programme", namely "the reduction of every phenomenon to the motion of a set of material points". Two steps were in order: first, the experimental determination "of the force acting on each point", and then "the integration of the corresponding system of differential equations". The general programme could not actually be pursued, because neither the determination of all forces nor the integration of all equations could be performed in general. Moreover the whole strategy was based on a mismatch between continuous and discrete structures. On the one hand there are "interactions between individual entities" like single masses, and on the other we have "*the continuity of real numbers*" by which the mathematical toolbox of differential equations operates. He also stressed the difference between determinism and predictability, which involved the difference between ontological and epistemological levels. A physical system can be ruled by definite mathematical procedures but accurate predictions might be problematic. (Israel 1987, pp. 84, 86, and 93-4).

In a certain sense, here we are at the core of Boussinesq's intellectual pathway: mathematical-mechanical determinism can give rise to unpredictable processes. The following year, the mathematician and historian of mathematics Michael Deakin explored Maxwell, Saint-Venant and Boussinesq's approaches to an "apparent paradox", and devoted two pages to Boussinesq's "Singular Integrals". The paradox was nothing else but the "contradiction between the laws of physics and the freedom of the human will". His short paper, "Nineteenth Century Anticipations of Modern Theory of Dynamical Systems", dealt with "Laplace demon" and the above-mentioned paradox from a modern point of view, as the expression "anticipation" let us understand. He found that "Laplace's intelligence" could be looked upon "as anything other than a figure of speech". He also found that the conception of physical law as something that deals with the "human knowledge of the external world" rather than the "world *per se*" was "a relatively modern one, owing much to the development of the quantum theory". In reality, that awareness might be considered as a specific heritage of Kantian tradition, and it was not uncommon in the second half of the nineteenth century, when a more mature attitude toward science emerged both in philosophical and scientific fields. In accordance with his *whig* perspective, Deakin claimed that Maxwell was "distinctly modern", while Saint-Venant and Boussinesq were definitely not (Deakin 1988, pp. 183 and 186-9).

In 1991 Israel undertook a historical research on determinism and its "translation in mathematical language", namely "the existence and uniqueness theorem for a system of ordinary differential equations". He found that "Laplace's paradigm" did not cast light on that meaningful link because "it rather consisted of a philosophical program". Before realising a fruitful interaction, "the history of determinism and the history of the existence and uniqueness theorem" had followed different pathways. Israel suitably remarked that the first explicit statement of determinism "does not belong to the history of mathematical physics … but to the history of medical science". It had been the French physiologist

Claude Bernard "to first introduce this term in the scientific language, at least the French one". He pointed out Laplace's influence on Bernard, but stressed that Bernard's determinism was devoid of "any metaphysical interpretation" and "any materialistic hypothesis". In Boussinesq's 1878 essay, Israel found "the widest analysis of the relationship between determinism and the theory of ordinary differential equations". The French mathematician had questioned "the biunivocal correspondence between actual motions and differential equations submitted to the existence and uniqueness conditions". Boussinesq had managed to rightly point out the problem but his solution was "technically weak". Neither Cournot nor Saint-Venant had managed to do better: they had put forward "metaphysical hypotheses" rather than "scientific theories" (Israel 1991, pp. 305, 307, 313, 331-3, 339, 344-5, and 352).[39]

In 2003 the philosopher of science John Norton focused on causation and determinism in science, and on what he looked upon as the received view on the subject matter. He claimed that a principle of causality was only consistent with specific and simplified implementations of scientific theories. He discussed a simple but interesting case: a point-like mass at rest upon the top of a rotationally symmetric surface. He showed that even "quite simple Newtonian systems can harbor uncaused events", and "the mass on the dome" offered a meaningful instance. When put on the top of the dome, the mass might stay at rest or move off "in an arbitrary direction, with the theory supplying no probabilities for the time or direction of the motion" (Norton 2003, pp. 1-5, 8-9, 13-14, and 19-21).[40] The mass upon the dome was looked upon as a recent issue, a problem without any history. In the subsequent *philosophical* literature, it was addressed as "Norton's dome". Boussinesq and the debate that involved philosophers, scientists, and mathematicians in the second half of the nineteenth century were definitely out of reach.

In 2008, in the context of a debate that was hosted by the journal *Philosophy of Science*, Norton went back to his dome and the corresponding indeterminism. He focused on David Malament's criticism, which was hosted by the same journal, in the same issue (Norton 2008, pp. 789-90 and 792-3). Malament's purpose was "to decide whether Newtonian particle mechanics qualifies as a deterministic theory"; at the same time he looked upon his paper as "a minitutorial on the geometry and dynamical properties of Norton's dome surface". Norton had focused on the dynamical aspect of what should happen at the top of the dome whereas Malament focused on the geometrical aspect, and claimed that "Norton's indeterminism" should be "traced to it", namely "the singularity at the summit". The singularity corresponded to the fact that the dome surface was wherever infinitely differentiable apart from the apex, where it was only once differentiable. In the end, he acknowledged that he was still asking himself what a "Newtonian system" really was, or what the "domain of application" of Newtonian theory was. However he found that both questions and answers were not as interesting as "the rich set of issues" that Norton's dome raised (Malament 2008, pp. 799, 808, and 815).

[39] Israel acknowledged that the French philosopher and historian of science Georges Canguilhem had been the first to stress the link between "the doctrine" of determinism and Claude Bernard's scientific work. See, for instance, Canguilhem 1998, pp. 64-5.

[40] Norton mentioned Russell's similar approach, in particular the statement that "The law of causality, …, is a relic of a bygone age" (Norton 2003, p. 3; Russell 1917, p. 132).

The philosopher of science Alexandre Korolev also devoted his paper to geometrical and physical technicalities, and focused on "the initiating cause that sets the mass in motion". In reality he found that the time $t = t_0$ was not "the first instant at which the mass moves" but "the *last* instant at which the mass does *not* move", and therefore there was "no first instant at which the mass moves". As a consequence, there was "no first instant at which to seek the initiating cause". Another problem arose in connection with the dome curvature: the detachment of the mass from the surface depended not only on the initial conditions but also on the mathematical equation of the surface: the point-mass might refuse "to follow its curvature" (Korolev 2008, pp. 945 and 949-51).

John Earman took part in the debate on the so-called Norton dome, and focused on the problematic link between mathematics and physics. He started from the tight connection between determinism and the mathematical existence and uniqueness of solutions for a differential equation, which he identified with "Lipschitz condition" or Lipschitz continuity. Nevertheless he did not completely rely on the perfect identity between physical determinism and Lipschitz condition, because "the standard existence and uniqueness theorems" were only "local in time". The unique solution associated with the initial conditions might "break down after a finite time", and this put in danger "the fortunes of determinism for all future (or past) times". He remarked that, even in the case of the traditional gravitational force of the kind $1/r^2$, a material body might suffer from the emergence of "collision singularities" or other processes "where the particles run off to spatial infinity in a finite time". Furthermore, classical mechanics was consistent with more exotic systems, for instance "an infinite system of particles that interact via elastic collisions". In the end, 130 years after Boussinesq's essay, a renowned philosopher of science acknowledged that determinism in the context of physical sciences still managed "to spring surprise on us" (Earman 2008, pp. 817-9 and 828).

The philosophers of science Mark Wilson showed a more historical sensitivity. He remarked that the expression "classical mechanics" or "the Newtonian picture" could be "readily applied to deeply incompatible doctrines". Moreover, although he did not make specific reference to the existing literature, he noticed that Norton's dome represented "an improved recasting of a circumstance that has been long familiar in the physical literature". He singled out "three basis families of foundational approach" to mechanics, which were not "strictly compatible with one another" from "a foundational point of view". In brief, he analysed the physics of "*mass point*" particles, the physics of "*rigid bodies and perfect constraints*", and the physics of "*continuum mechanics*". In the end he claimed that the supposed indeterminism stemmed from a sort of incompleteness of the different approaches to mechanics, an incompleteness which he variously labelled "missing physics", "foundational gaps", "*descriptive holes*", and "strange descriptive gaps" (Wilson 2009, pp. 174-7 and 181-2).

Recently Marij van Strien discussed Boussinesq's research programme in connection with the debates that took place on "Norton's dome" around 2008. Her paper, "The Norton Dome and the Nineteenth Century Foundations of Determinism" was primarily directed at a comparison between the nineteenth-century notion of determinism and the recent one. More specifically she focused on what she considered the standard view on determinism, namely "Determinism based on the Equation of Motion" or "DEM". From the outset she stressed that Poisson, Duhamel, Boussinesq and Bertrand's approach to "indeterministic systems similar to the Norton dome" were "very different from the contemporary discussion" (van Strien 2014a, pp. 167 and 170).[41] This sounds like an obvious statement for historians, and in some way as something that is taken for granted. Nevertheless, Poisson, Duhamel, Boussinesq and Bertrand did not share the same view on determinism.

When van Strien stated that "for them, determinism was not an idea based on the properties of the equations of physics", it seems that Boussinesq was overturned, because he looked upon indeterminism as a result of a mathematical procedure. According to Boussinesq, the non-uniqueness of solutions let two complementary domains emerge: the domain of determinism, which dealt with ordinary solutions, and the domain of life and free will, which dealt with singular solutions. A complementary representation of the natural world also emerged, where both dead and living beings found room. In this context, I found questionable that "for these authors, … whether or not there was determinism in physical reality did not necessarily depend on whether the equations of physics had unique solutions". At least for Boussinesq, I could say that indeterminism did depend on the non-uniqueness indeed, but depended in a way that is different from that expected by twenty-first-century philosophers of science. It is true that Boussinesq did not "specify that these equations must have unique solutions" but he was obviously aware of the problem of non-uniqueness. The existence of both ordinary and singular solutions was an instance of that non-uniqueness (van Strien 2014a, pp. 168 and 179).

More specifically, with regard to Poisson and Duhamel, who explored the mathematical side of what we nowadays call determinism, it is worth stressing that in no way were they interested in *determinism*. With regard to the comparison between Boussinesq and Bertrand's views on the relationship between mathematics and the natural world, I find a subtle difference. Only as a rough approximation can we accept that they had in common the concept of "idealisation", in the sense that mathematics in general, and differential equations in particular, represented an idealisation of material processes.[42] While Bertrand can really be associated with this concept, Boussinesq might more conveniently be associated with the concept of structural analogy. He was not so philosophically naïve to trust in the automatic correspondence between mathematical structures and observed or perceived phenomena. He trusted in the possibility of a structural correspondence between the essential features of singular solutions of differential equations, on the one hand, and some essential features of life and moral processes, on the other. The correspondence was not an explanation. More specifically, he did not expect to be able to explain life, but was confident in the possibility of describing the simplified structure of some processes in suitable mathematical terms. A structural analogy might be looked upon as something weaker than an idealisation and at the same time something stronger,

[41] The comparison and the differences were repeatedly pointed out by the author (van Strien 2014, pp. 168, 179, and 184).

[42] According to van Strien, "Bertrand argued that these differential equations are only an idealisation" and "Boussinesq also described the laws of mechanic as an idealisation of physical reality" (van Strien 2014, pp. 181).

depending on the specific point of view. In this comparison, both mathematics and philosophy are at stake. A structural analogy is weaker than an idealisation in the sense that it does not require faith in the existence of an idealisation: idealisation could not exist or could not be considered admissible. On the other hand, a structural analogy requires confidence in the reliability of formal or mathematical representations. Mathematical language manages to catch some essential features of natural processes. While idealisation involves a stronger confidence in philosophy, a structural analogy involves a stronger confidence in mathematics.

With regard to the logical connection between determinism and directive principles, Boussinesq did not state that "this directive principle acts in a deterministic manner", or that "although there is no physical determinism, there is still physiological determinism". He simply confined himself to hinting at the possibility of a more sophisticated kind of "science", which might also take into account "*a higher dynamics or dynamics of directive principles*". That science should have encompassed both the ordinary mechanics and the science of directive principles. Both deterministic mechanics and indeterministic processes like the emergence of life and free will could have been hosted by that merely imagined science. He hinted at a mere possibility rather than to a definite theory or some kind of "deterministic" science of life (van Strien 2014a, pp. 179; Boussinesq 1978a, p. 133)

With regard to Van Strien's appraisal of Laplace's celebrated determinism, I find that Laplace's approach was deeply rooted in his physics, and it might be qualified as "metaphysical" only in a very broad sense, in the same sense as we label metaphysical the hypotheses that lay at the basis of his physics. Instead of the statement "his determinism was based on metaphysics rather than derived from physics", I would prefer to say that his determinism stemmed from the metaphysical foundations of his physics. I obviously agree that Laplace did not merge his physical determinism with the mathematical issue of the existence and uniqueness of solutions in differential equation. We know that the issue was to be clarified "late in the nineteenth century" (van Strien 2014a, pp. 171). However, as I have already stated, I find that Laplace's determinism was more pragmatic and milder than its subsequent idealisations.

Concluding remarks

In the context of Boussinesq's researches, both determinism and indeterminism played an important role in the natural world, even though he preferred not to have recourse to the word "indeterminism". He put mechanics, or deterministic mechanics, on the one hand, and life and free will, rather than indeterminism, on the other. Determinism corresponded to predictable and stable trajectories: physical stability had its mathematical counterpart in ordinary solutions of differential equations. Life and free will corresponded to mechanically unpredictable and unstable trajectories: physical instability corresponded to singular solutions. In the case of life and free will, the correspondence was formal or structural: he never spoke of or hinted at something like the equations of living processes or free actions. The correspondence consisted of a structural analogy, which was not based on specific material similarities but on wide-scope mathematical structures. He was not interested in defining what determinism was: determinism corresponded to ordinary mechanics, and ordinary mechanics corresponded to ordinary solutions of differential equations. Boussinesq's research programme realised a convergence among different traditions of research. A set of different problems that emerged from mathematics, physics, physiology, and philosophy found an original synthesis and a provisional equilibrium.

A direct confrontation between Boussinesq's research programme, on the one hand, and recent debates on "Norton's dome" and determinism in the context of classical physics, on the other, might be misleading. The question as to whether classical mechanics is deterministic or not makes sense for us probably because we are acquainted with quantum physics and its history. Boussinesq was not interested in exploring the possible indeterminism of classical mechanics because he looked upon classical mechanics as the best implementation of determinism. Indeterminism was mainly a mathematical issue: it involved a scientific field of research that lay outside the domain of classical mechanics. Exploring indeterminism corresponded to exploring a wider domain of science whose extension and scope was complementary to the extension and scope of mechanics.

Although Boussinesq's research programme was short-lived, and apparently faded away within a few years, the re-emergence of themes and contents that Boussinesq developed more than a century ago shows us how fruitful his heritage has been, and how interesting the scientific and philosophical context of the late nineteenth century really was.

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