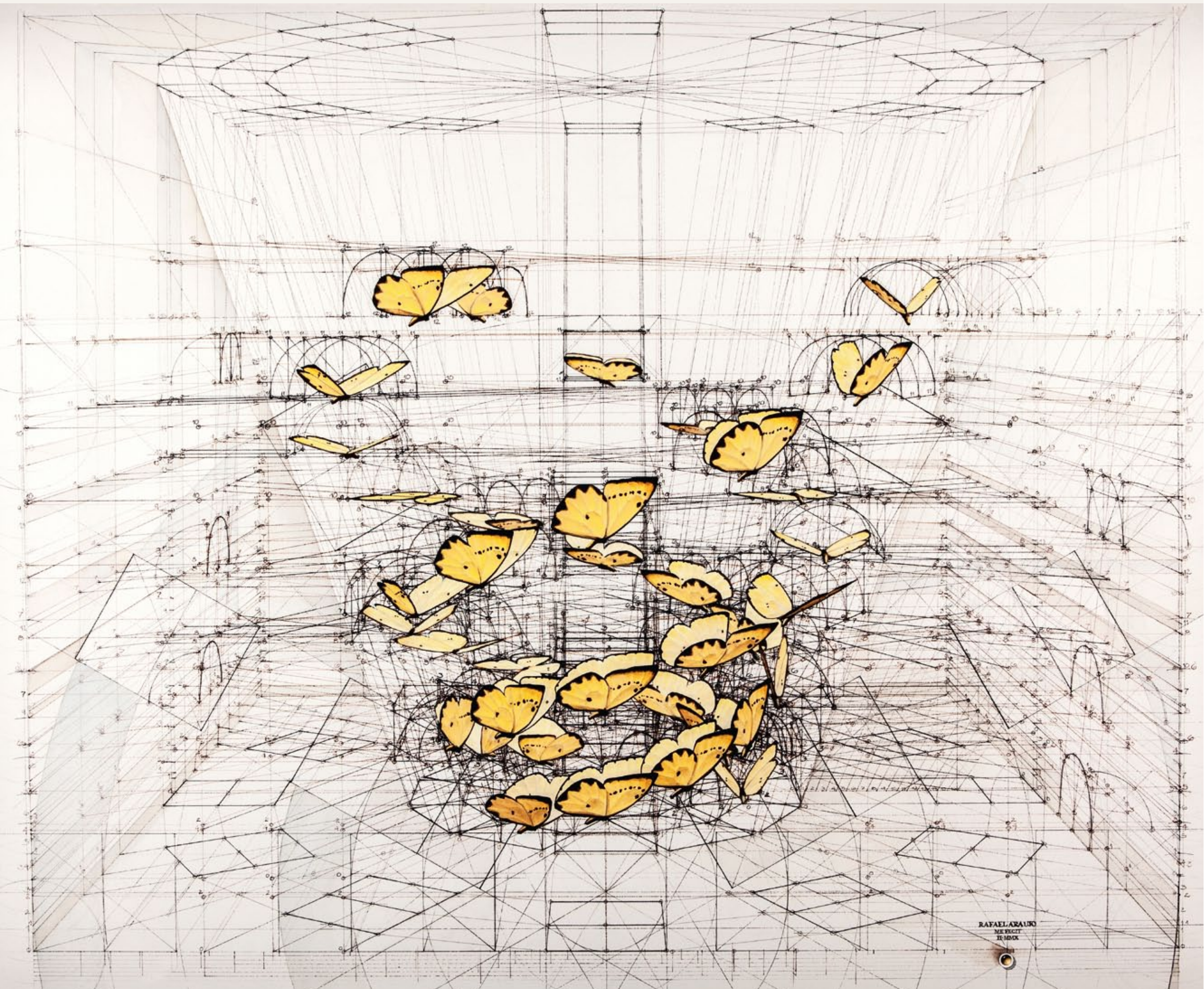
UNIVERSITEIT GENT

Marij van Strien

Continuity, Causality and Determinism in Mathematical Physics

from the late 18th until the early 20th century

Proefschrift voorgelegd tot het behalen van de graad van Doctor in de Wijsbegeerte
Promotor: Prof. dr. Eric Schliesser
Copromotor: Prof. dr. Maarten Van Dyck



Promotor Prof. dr. Eric Schliesser
Vakgroep Wijsbegeerte en Moraalwetenschap
Copromotor Prof. dr. Maarten Van Dyck
Vakgroep Wijsbegeerte en Moraalwetenschap

Decaan Prof. dr. Marc Boone
Rector Prof. dr. Paul Van Cauwenberge

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Introduction

Classical mechanics is strongly associated with determinism. However, there are a few reasons to think that historically, the connection between determinism and classical mechanics may in fact not be so straightforward. The term classical mechanics is often used as synonymous with Newtonian mechanics, but the origin of determinism in mechanics is usually traced to a statement by Laplace from 1814, a full century after Newton. One can thus ask what the exact basis was for Laplace's statement of determinism - did he point out an intrinsic feature of Newton's laws of mechanics, or did he have a different basis for his argument?

Moreover, Earman and Norton have argued that the claim that classical mechanics is deterministic is in fact quite a problematic claim.[1] In contemporary philosophy of physics, especially through the work of Earman and Norton, it has become clear that the question whether classical mechanics is deterministic is a subtle issue. One may suppose that historically, people were unaware of these subtleties, so that mechanics was regarded as unproblematically deterministic at least until the development of quantum mechanics, but it turns out that this is not true. A main example of indeterminism in classical physics that Earman gives are space invaders, and he points out that these were already described by Painlevé in the late nineteenth century;[2] moreover, as the second paper in this thesis shows, the example that Norton gives of indeterminism in classical physics - the Norton dome - was discussed at various points during the nineteenth century, notably by Boussinesq, who used this type of indeterminism as an argument for free will. This gives an indication that also historically, the connection between

[1] See Earman, J. (1986), *A primer on determinism*, Dordrecht: Reidel; Earman, J. (2006), Aspects of determinism in modern physics, in J. Earman and J. Butterfield (ed), *Handbook of the philosophy of science, volume 2: philosophy of physics*, pp. 1369-1434, North Holland: Elsevier; Norton, J. (2008), The dome: an unexpectedly simple failure of determinism, *Philosophy of Science*, 75, 786-98.

[2] See Earman (2006) (footnote 1), p. 1385. Painlevé did not think that this case showed that mechanics is indeterministic, he was in fact a strong proponent of determinism (see paper 6).

determinism and classical mechanics may be more complicated than is usually thought; it leads to the question in how far people thought, before the introduction of quantum mechanics, that determinism could be rigorously derived from the laws of mechanics.

In this PhD thesis, I argue that around the nineteenth century, many physicists, for various reasons, did not regard determinism as a provable fact about physics. Thus, it is not the case that before the introduction of quantum mechanics, determinism was regarded as a straightforward consequence of the laws of mechanics. This is not to say that physicists in this period were not committed to determinism; there were some physicists who argued for fundamental indeterminism, but most were committed to determinism in some sense. However, for them, determinism was often not a provable feature of physical theory, but rather an a priori principle or a methodological presupposition. Determinism was strongly connected with principles of causality and continuity and the principle of sufficient reason; this thesis examines the relevance of these principles in the history of physics.

One can make a distinction between different types of determinism that appear around the nineteenth century:

(1) The principle according to which nothing happens without a cause and causes uniquely determine their effect (this principle is also referred to as the law of causality).
(2) Laplace's statement of determinism: the statement that a perfect intelligence with perfect knowledge of the state of the universe at an instant will also have perfect knowledge of all past and future states of the universe (we will see in paper 1 that Laplace was not the first to argue for this type of determinism).
(3) The principle that there are mathematical laws of nature which give a complete description of natural processes, and through which all processes are uniquely determined, where these laws take the form of differential equations which have a unique solution for given initial conditions.

Note that neither of these types of determinism necessarily imply predictability for a less-than-perfect intelligence with less-than-perfect knowledge, such as a human scientist. In classical mechanics, there can be unstable or chaotic processes which, though deterministic, are highly unpredictable (see paper 3 on instability).

These types of determinism often go together, but they need not go together. There is an ambiguity in the notion of determinism, since one can have determinism both at the level of physical reality and at the level of mathematical descriptions of nature, and a main claim of this thesis is that the two do not always correspond.

Because of the fact that one can argue for or against determinism both on a metaphysical level and on a mathematical level, studying the history of determinism in physics is a way to trace how physicists have conceived of the relation between mathematics and metaphysics, and between the mathematical equations of physics and physical reality. This thesis shows that between the eighteenth and the early twentieth century, these relations radically changed. The metaphysical arguments for determinism made by Laplace and his predecessors in the eighteenth century also provided a foundation for the idea that one could find laws of nature in the form of differential equations which uniquely determine natural processes; thus, there was a direct correspondence between metaphysics and mathematics. During the nineteenth century, rigorous foundations for differential calculus were developed, through which differential calculus was made independent of any empirical and intuitive notions; this development led to concerns regarding the applicability of differential calculus in physics. In the late nineteenth and early twentieth century, a number of physicists considered the possibility of a small mismatch between the mathematical equations of physics and physical reality.

This thesis consists of a collection of six papers, the first five of which are written for independent publication (the sixth, very short one, serves to connect the fourth and fifth paper with the conclusion). The papers discuss different aspects of determinism in classical physics, as well as the related issues of whether all of physics is reducible to mechanics and how the mathematical equations of physics relate to physical reality. Rather than focussing only on the most well-known figures in the history of physics, I develop a broad view on the development of thought on the relevant issues by studying a wide range of authors, including physicists, mathematicians and philosophers. The scope of the papers stretches from the eighteenth until the early twentieth century. However, the six papers included in this thesis do not cover the complete history of determinism, continuity and causality in physics during this period; rather, they each cover a particular issue or episode.

In the first paper, "On the origins and foundations of Laplacian determinism", I argue that Laplace, who is usually pointed out as the first major proponent of scientific determinism, did not derive his statement of determinism directly from the laws of mechanics; rather, his determinism has a background in eighteenth century Leibnizian metaphysics, and is ultimately based on the law of continuity and the principle of sufficient reason. These principles also provided a basis for the idea that one can find laws of nature in the form of differential equations which uniquely determine natural processes.

In "The Norton dome and the nineteenth century foundations of determinism", I give further support to the idea that determinism could not be derived directly from the laws of mechanics by showing that systems similar to the Norton dome were already discussed during the nineteenth century. However, their significance back then was very different from the significance which the Norton dome currently has in philosophy of physics. This is explained by the fact that determinism was conceived of in an essentially different way: in particular, the nineteenth century authors who wrote about Norton dome type indeterminism (Lipschitz indeterminism) regarded determinism as an a priori principle rather than as a property of the equations of physics.

In "Vital instability: life and free will in physics and physiology, 1860-1880", I show how Maxwell, Cournot, Stewart and Boussinesq used the possibility of unstable or indeterministic mechanical systems to argue that the will or a vital principle can intervene in organic processes without violating the laws of physics, and to argue in this way for a strictly dualist account of life and the mind.

In "The nineteenth century conflict between mechanism and irreversibility", I show that in the late nineteenth century, there was a widespread conflict between the aim of reducing physical processes to mechanics and the recognition that certain processes are irreversible. Whereas the so-called reversibility objection is known as an objection that was made to the kinetic theory of gases, it in fact appeared in a wide range of arguments, and was susceptible to very different interpretations. It was only when the project of reducing all of physics to mechanics lost favor, in the late nineteenth century, that the reversibility objection came to be used as an argument against mechanism and against the kinetic theory of gases.

In "Continuity in nature and in mathematics: Boltzmann and Poincaré", I show that the development of rigorous foundations of differential calculus in the nineteenth century led to concerns about its applicability in physics: through this development, differential calculus was made independent of empirical and intuitive notions of continuity and was instead based on mathematical continuity conditions, and for Boltzmann and Poincaré, the applicability of differential calculus in physics depended on whether these continuity conditions could be given a foundation in intuition or experience.

In the final paper, "Determinism around 1900", I briefly discuss the implications of the developments described in the previous two papers for the history of determinism in physics, through a discussion of determinism in Mach, Poincaré and Boltzmann. I show that neither of them regards determinism as a property of the laws of mechanics; rather, for them, determinism is a precondition for science, which can be verified to the extent that science is successful.

Paper 1 On the origins and foundations of Laplacian determinism[1]

In this paper I examine the foundations of Laplace's famous statement of determinism in 1814, and argue that rather than derived from his mechanics, this statement is based on general philosophical principles, namely the principle of sufficient reason and the law of continuity. It is usually supposed that Laplace's statement is based on the fact that each system in classical mechanics has an equation of motion which has a unique solution. But Laplace never proved this result, and in fact he could not have proven it, since it depends on a theorem about uniqueness of solutions to differential equations that was only developed later on. I show that the idea that is at the basis of Laplace's determinism was in fact widespread in enlightenment France, and is ultimately based on a re-interpretation of Leibnizian metaphysics, specifically the principle of sufficient reason and the law of continuity. Since the law of continuity also lies at the basis of the application of differential calculus in physics, one can say that Laplace's determinism and the idea that systems in physics can be described by differential equations with unique solutions have a common foundation.

[1] This paper has been published in *Studies in History and Philosophy of Science*, 45(1), March 2014, pp. 24–31.

1.1 Laplace's statement of determinism

Histories of determinism in science usually start with the following quote, from Laplace's *Essai philosophique sur les probabilités* (1814):

> An intelligence which, for one given instant, would know all the forces by which nature is animated and the respective situation of the entities which compose it, if besides it were sufficiently vast to submit all these data to mathematical analysis, would encompass in the same formula the movements of the largest bodies in the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes. (Laplace, 1814, p. 3-4).[2]

Throughout history, there have been many people arguing for the general idea that everything that happens is necessary or predetermined. The importance of this quote by Laplace lies in the fact that it is not merely an expression of everything in nature being fixed and everything that occurs being necessary, but that it states that prediction is possible through mathematical analysis, on the basis of the forces and the "respective situations of the entities" that are present. It seems that Laplace is stating a fact about physics, namely that the fundamental equations of physics can be solved in principle (although not necessarily in practice) and then give a unique prediction of future states. This suggests that everything is fixed according to *laws of physics* of a mathematical form, and that it is physics that tells us that the world is deterministic. (Laplace, by the way, did not use the word "determinism", which only received its current meaning later on; see Hacking (1983)).

An interpretation of what Laplace had in mind would then be the theorem that for all systems in classical physics, there are equations of motion of the form

$$\frac{d^2r}{dt^2} = F(r) \qquad (1)$$

with r the position of a body and $F(r)$ the force to which it is subjected, and that these equations have a unique solution for given initial conditions $r(t_0) = r_0$ and $\frac{dr}{dt}(t_0) = v_0$. This means that if we know the positions and velocities of all particles at a certain instant, and all the forces that are present, we can uniquely determine the future (as well as past) states of the system. This is nowadays the canonical formulation of

[2] "Une intelligence qui pour un instant donné, connaîtrait toutes les forces dont la nature est animée, et la situation respective des êtres qui la composent, si d'ailleurs elle était assez vaste pour soumettre ces données à l'analyse, embrasserait dans la même formule, les mouvements des plus grands corps de l'univers et ceux du plus léger atome: rien ne serait incertain pour elle, et l'avenir comme le passé, serait présent a ses yeux."

determinism in classical physics (see e.g. Landau and Lifshitz, 1976, pp. 1-2; Arnold, 1989, p. 4, 8).[3] Whether it holds is a different issue: in particular Earman (1986) and Norton (2008) have shown that there are various cases in which this determinism breaks down. But it is usually supposed that this is what Laplace had in mind when he wrote the above statement.

In this paper I examine in how far this interpretation of Laplace's statement holds, and put forward an alternative interpretation, according to which Laplace's determinism is based on general principles, rather than derived from the properties of the equations of mechanics: specifically, it is based on the principle of sufficient reason and the law of continuity. This does not mean that Laplace's determinism is unrelated to developments in physics, but the relation is not as straightforward as one may suppose.

In section 2, I discuss the possible foundations of Laplace's deterministic statement in his mechanics. I show that Laplace could not have proven that the equations of motion in mechanical systems always have a unique solution, since this depends on a theorem about uniqueness of solutions to differential equations that was only developed later on (furthermore, the theorem left a possibility for indeterminism open). In section 3, I discuss the philosophical argument that Laplace provides for his determinism: in fact, the only motivation that he explicitly gives for his determinism is Leibniz' principle of sufficient reason. In section 4, I show that there was a strong eighteenth century background to Laplace's ideas: he was far from the first to argue for determinism, and also far from the first to do so in terms of an intelligence with perfect knowledge and calculating capacities. Tracing out these connections makes it possible, in section 5, to get a better understanding of the foundation of Laplace's determinism and the relation with the possibility of mathematical descriptions of nature.

1.2 The physical foundations of Laplace's determinism

One reason to question the interpretation that Laplace's determinism is based on the idea that there are equations of motion for mechanical systems which always have a unique solution for given initial conditions, is that he never argues for this explicitly; he never makes the argument more explicit than in the above quote. And in this quote, the

[3] One can make a distinction, however, between determinism and predictability: the equation of motion plus initial conditions may uniquely determine future states of the system, but that does not necessarily mean that we can calculate these future states (see Earman, 1986, p. 7). Laplace did not make this distinction; rather, he made a distinction between predictability in principle and in practice.

argument is not formulated very carefully: he does not explicitly say that the equations of mechanics always have a unique solution for given initial conditions, and if this is what he intended to argue, then it is striking that he fails to mention that besides the initial positions of all bodies, one also needs to know their initial velocities in order to solve the equations. Furthermore, from the context of this quote, it appears that he was not trying to say something about the properties of the theory of mechanics. Rather, he was arguing for the importance of probability theory: the above statement appeared in the preface of a work on probability theory, in which Laplace argues that the use of probability theory is not restricted to cases in which there is fundamental chance or randomness. He argues that with perfect knowledge of the present state of the universe, it should be possible to predict future states with certainty; however, in all cases in which our knowledge is less than perfect, we have to rely on probability theory. Thus, it is because of our ignorance that we have to take recourse to probability theory, even though at the bottom of things, there is a solid necessity.[4]

The idea that certain prediction should be possible on the basis of perfect knowledge of the present state of the universe is something that Laplace had considered before. While his 1814 quote has become famous, he already made a similar statement many years earlier, in a lecture in 1773 (printed in 1776), at the very beginning of his career. The statement is made in a similar context, to clarify the notions of necessity and probability, but it is formulated in less physical terms and does not refer to mathematical analysis or calculation, and it is less apparent that it has a foundation in physics:

> ...if we conceive an intelligence which, for a given instant, encompasses all the relations between the beings of this universe, it may determine, for any time in the past or the future, the respective position, the motions, and generally the affections of all those beings. (Laplace, 1773, p. 144).[5]

Despite the fact that Laplace did not explicitly demonstrate that the equations of motion always have a unique solution, one may want to argue that he simply knew that this was the case; one may even want to argue that this was so obvious to him that he did not

[4] As Daston (1992) shows, earlier authors on probability theory such as Jakob Bernoulli and De Moivre also argued for determinism, with a similar aim (but in different terms).

[5] "...si nous concevons une intelligence qui, pour un instant donné, embrasse tous les rapports des êtres de cet Univers, elle pourra déterminer pour un temps quelconque pris dans le passé ou dans l'avenir la position respective, les mouvements, et généralement les affections de tous ces êtres."
The lecture is very mathematical; this statement is placed right in the middle. According to Hahn (2005), this shows that Laplace did not assign an important role to metaphysics and that he wanted to be foremost regarded as a mathematical physicist (Hahn, 2005, p. 53). Hahn also points out the use of the word êtres which suggests that it applies to all beings (also living ones).

think further demonstration was needed. However, it is not that easy to show that the equations of motion always have a unique solution. In fact, such a demonstration depends on a theorem about the existence and uniqueness to differential equations that was only developed later on: therefore, Laplace could have no proof available.

The first person to work on the issue of existence and uniqueness of solutions to differential equations was Cauchy, who showed in the 1820's that an equation like [1] has a unique solution if $F(r)$ is continuously differentiable (Kline, 1972, p. 717). In 1876, Lipschitz gave a more precise analysis, by showing that the function $F(r)$ does not necessarily have to be continuously differentiable in order for an equation like [1] to have a unique solution; it is enough if it fulfils the condition that there is a constant $K > 0$ such that for all r_1 and r_2 in the domain of F,

$$|F(r_1) - F(r_2)| \leq K|r_1 - r_2| \,.$$

(Lipschitz, 1876). This condition came to be known as 'Lipschitz continuity'.

These mathematical results imply that the equations of motion of a classical system can fail to have a unique solution if they involve a force which is not Lipschitz continuous. This fact has recently been used by Norton (2008) to argue that determinism can fail in classical mechanics; but it was already noted a couple of times before, most notably by Boussinesq in (1879) (see Van Strien, 2014). In (1806), several years before Laplace's famous statement of determinism in 1814, Poisson already discussed the possibility that the equations of motion of a system in physics fail to have a unique solution. One of the cases he discussed was that of a body subjected to a force $F(r) = cr^a$ with c and a constants, and $0 < a < 1$, and with initial conditions $r_0 = 0$ and $v_0 = 0$; this force is non-Lipschitz continuous at $r = 0$ and the equation therefore fails to have a unique solution: the body can either remain still at $r = 0$ or it can start to move at an arbitrary time (the same example is used by Malament (2008, p. 801) as an example of indeterminism in classical physics). Poisson argued that in cases in which the equation of motion did not have a unique solution, one had to make sure to pick the *right* solution: in the above case, because the force acting on the body at $t = 0$ is zero, it makes sense to expect the body to stay put (Poisson, 1806, p. 104). Therefore, to know the actual motion of the body, it is in certain cases not enough to study the equations; these have to be supplemented by additional considerations. The answer to the question whether the laws of mechanics always have a unique solution was not all that evident.

Laplace and Poisson knew each other well, Poisson having been a student of Laplace (O'Connor and Robertson, 2002); it is not known whether Laplace read Poisson's (1806) paper, but this could well have been the case, as the subject of the paper was one that interested both. In any case, Laplace knew that it was possible for differential equations to fail to have a unique solution: he himself had worked on so-called singular solutions to differential equations (Laplace, 1772b), which was an issue in pure mathematics that had been explored before him by among others Clairaut, Euler, and D'Alembert (Kline, 1972, p. 477). It may well be that Laplace had an intuition that such singular solutions would not occur in actual systems in physics; however, the question is then where this intuition came from, for Laplace had no mathematical foundation for it.

One may argue that Laplace knew that the equations that he actually encountered and worked with in his physics always had a unique solution; but also this is not entirely clear. Laplace devoted a large part of his life to celestial mechanics, and argued that it was in this domain that the human mind made the closest approach to his hypothetical intelligence:

> The human mind offers, in the perfection which it has been able to give to astronomy, a feeble sketch of this intelligence. Its discoveries in mechanics and geometry, added to that of universal gravity, have brought it within reach to comprehend in the same analytical expressions the past and future states of the system of the world. By applying the same method to some other objects of its knowledge, it has managed to reduce the observed phenomena to general laws, and to foresee those phenomena which given circumstances ought to produce. (Laplace, 1814, p. 4).[6]

In celestial mechanics, it had become possible to make very accurate predictions: as an example, Laplace mentioned Halleys' prediction of the return of a comet in 1759 (Laplace, 1814, p. 5). Therefore, the successes in celestial mechanics supported the idea of determinism. At the same time, it was clear to Laplace that even in the case of celestial mechanics, the gap between the human abilities and those of the Laplacian intelligence could never be closed. In particular, the equations of motion tended to become immensely complicated, and Laplace had to work hard to find approximate solutions to differential equations that could not be solved exactly (on his work in celestial mechanics, see Morando, 1995). Even a simple system such as the three body system could not be solved exactly. In "Recherches sur le calcul intégral et sur le système du monde" (1772a), he introduced some new methods for approximate solutions to the equations for the motion of planets, remarking:

> It is primarily in the application of mathematical analysis to the system of the world that there is a need for simple and convergent methods to integrate differential equations by approximation; in fact, those of the motion of celestial

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[6] "L'esprit humain offre dans la perfection qu'il a su donner à l'astronomie, une faible esquisse de cette intelligence. Ses découvertes en mécanique et en géométrie, jointes à celle de la pesanteur universelle, l'ont mis à portée de comprendre dans les mêmes expressions analytiques, les états passés et futurs du système du monde. En appliquant la même méthode à quelques autres objets de ses connaissances, il est parvenu à ramener à des lois générales, les phénomènes observés, et à prévoir ceux que des circonstances données doivent faire éclore."

> bodies present themselves in such a complicated form that they leave no hope that we will ever succeed in integrating them rigorously... (Laplace, 1772a, p. 369).[7]

But although the differential equations in astronomy can get enormously complicated and can usually not be solved exactly, they are still of a relatively simple kind, at least as long as you treat the celestial bodies as point masses, and it seems reasonable to expect them to have unique solutions. It is in other areas of physics that things get more messy. In particular, it is not evident that the equations of motion have a unique solution when there are discontinuities in the variables. And discontinuities could arise in various problems in physics, for example in collisions of hard bodies, and the analysis of the vibrating string.[8]

Laplace could thus not provide a mathematical proof of the statement that the equations of physics always had a unique solution, and his determinism could not fully be derived from the properties of the equations of physics.

1.3 The philosophical foundations of Laplace's determinism

Whereas Laplace did not provide a mechanical or mathematical derivation of his statement of determinism, he did in fact provide a philosophical argument. In the passage from which the quote at the beginning of this paper is taken, Laplace argues that all occurrences in nature, both on large and small scale, are a necessary consequence of the laws of nature,[9] and then goes on to argue:

> Current events are connected with preceding ones by a tie based upon the evident principle that a thing cannot come to existence without a cause which produces it. This axiom, known as the *principle of sufficient reason*, extends even to the most indifferent acts. The most free will cannot give rise to these indifferent acts

[7] "C'est principalement dans l'application de l'Analyse au système du monde que l'on a besoin de méthodes simples et convergentes, pour intégrer par approximation les équations différentielles; celles du mouvement des corps célestes se présentent, en effet, sous une forme si compliquée, qu'elles ne laissent aucun espoir de réussir jamais à les intégrer rigoureusement..."

[8] In the case of the vibrating string, Laplace agreed with Euler that 'discontinuous' functions had to be admitted in the mathematical treatment of the problem (we would now call these functions continuous with discontinuous derivatives), although it appears from his discussion that these functions did not necessarily have physical significance: he argued that a system in which there are discontinuities is always infinitely close to another system in which there are no discontinuities. See Laplace (1779, pp. 81-83), Truesdell (1960, p. 293).

[9] "Tous les événements, ceux même qui par leur petitesse, semblent ne pas tenir aux grandes lois de la nature, en sont une suite aussi nécessaire que les révolutions du soleil."

> without a determinative motive; for if, in two cases with exactly similar circumstances, the will acted in the one and refrained from acting in the other, its choice would be an effect without a cause. It would then be, says Leibniz, the blind chance of the Epicureans. The contrary opinion is an illusion of the mind which, losing sight of the evasive reasons of the choice of the will among indifferent things, convinces itself that it has determined itself independently and without motives.
> We ought then to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. An intelligence which... (Laplace, 1814, p. 3).[10]

For Laplace, determinism seems to be intimately linked with causation. It is ultimately based on a principle which he calls the principle of sufficient reason: "a thing cannot come to existence without a cause which produces it."[11]

The principle of sufficient reason plays an important role in the work of Leibniz, whom Laplace mentions two sentences later, and who preceded Laplace in arguing for determinism on metaphysical grounds. Leibniz takes this principle to be one of the two principles on which reason is founded (the other being the principle of contradiction) (Leibniz, 2007 [1710], p. 421). Leibniz' principle of sufficient reason states that for everything that happens there must be a reason which determines why it is thus and not otherwise.[12] Laplace uses the word "cause" instead of "reason"; these words could have equivalent meanings, but it is important to note that in Laplace, a cause is *productive* of the event that it brings about, it is an efficient cause in the sense of Aristotle, whereas Leibniz' principle of sufficient reason has more to do with final causation. Thus, there is an important distinction: whereas Leibniz' principle of sufficient reason says that for everything that occurs there must be a reason why it is thus and not otherwise, Laplace's principle of sufficient reason says that for everything there must be a cause that brings it about. In this sense, Laplace's principle of sufficient reason is closer to that of Spinoza: like Laplace, and in contrast with Leibniz, Spinoza

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10 "Les événements actuels ont avec les précédens, une liaison fondée sur le principe évident, qu'une chose ne peut pas commencer d'être, sans une cause qui la produise. Cet axiome connu sous le nom de *principe de la raison suffisante*, s'entend aux actions même les plus indifférentes. La volonté la plus libre ne peut sans un motif déterminant, leur donner naissance; car si toutes les circonstances de deux positions étant exactement les mêmes, elle agissait dans l'une et s'abstenait d'agir dans l'autre, son choix serait un effet sans cause: elle serait alors, dit Leibnitz, le hasard aveugle des épicuriens. L'opinion contraire est une illusion de l'esprit qui perdant de vue, les raisons fugitives du choix de la volonté dans les choses indifférentes, se persuade qu'elle s'est déterminée d'elle-même et sans motifs. Nous devons donc envisager l'état présent de l'univers, comme l'effet de son état antérieur, et comme la cause de celui qui va suivre. Une intelligence qui..."

11 "une chose ne peut pas commencer d'être, sans une cause qui la produise".

12 "nothing happens without its being possible for someone who understands things well enough to provide a reason sufficient to determine why it is as it is and not otherwise" (Leibniz, 1714, p. 262).

does not involve final causation in his discussion of the principle of sufficient reason (Melamed and Lin, 2013).

Laplace thinks of a cause as immediately preceding its effect, and this leads to the conclusion that the present state of the universe should be regarded as the cause of the state of the universe at the next instant. An intelligence with perfect knowledge of the present state of the universe would thus have perfect knowledge of the cause of the universe at a next instant, and under the assumption that causes uniquely determine their effects and it is possible to know the connections between cause and effect (an assumption that Laplace does not state explicitly), this intelligence would have perfect knowledge of the state of the universe at the next instant. By extension, it would have perfect knowledge of all the future and past states of the universe.

This idea, that because of the principle of sufficient reason, perfect knowledge is possible in principle, has parallels with Leibniz' definition of the principle of sufficient reason in the *Theodicy:* "there is no true enunciation whose reason could not be seen by one possessing all the knowledge necessary for its complete understanding" (Leibniz, 2007 [1710], p. 421). In a sense, Laplace gives a temporal dimension to this statement, by stating that the knowledge necessary for the complete understanding of an event can be located at the previous instant (or, through induction, at any other instant in time).

Thus, the argument that Laplace provides for his determinism is based on an interpretation of the principle of sufficient reason, rather than on the equations of mechanics. On this basis, Israel (1992, p. 259) remarks that Laplace's determinism is "a metaphysical causalism à la Leibniz". The fact that Laplace's argument for determinism is not based on mechanics, however, does not mean that it is completely unrelated to uniqueness of solutions to equations in mechanics. As we have seen from the quote at the beginning of section 1, Laplace thinks that perfect prediction is ultimately possible on the basis of mathematical analysis, which implies that ultimately, there are fundamental laws of nature, of a mathematical form, which always have a unique solution for given initial conditions. His argument for determinism gives metaphysical support to this idea, and motivates the search for such laws. Specifically, the idea that the state of the universe is the cause of the state at the next instant supports the idea that differential equations with unique solutions can be found which describe natural processes. However, the connection between these two ideas is not entirely clear, and Laplace does not discuss how they relate. By looking at the context in which Laplace's determinism appeared, we can get a more complete understanding of its foundations, and of the link between the metaphysics of the principle of sufficient reason and the mathematics of differential equations.

1.4 Laplacian determinism before Laplace

Although Laplace is usually pointed out as the first proponent of scientific determinism, he was by far not the first to argue for determinism in a scientific context. Neither was he the first to argue that an intelligence with perfect knowledge and calculating capacities would be able to predict the future states of the universe with certainty. In this section, I show that this idea came up several times before Laplace, and usually in a philosophical context. In fact, this seems to have been an idea that was widespread around the time that Laplace first expressed it in 1773, particularly in France. Variations of this idea can be found in Maupertuis (1756, p. 332), Boscovich (1922 [1763], p. 281), Condorcet (1768, p. 5), D'Holbach (1770, p. 51-52), and an undated fragment in the archives of Diderot (in Diderot, 1951, p. 255).[13] With the exception of Boscovich, the context is philosophical rather than physical, and it appears that the idea did not come out of physics.

For example, D'Holbach, in his *Système de la nature ou des lois du monde physique et du monde moral* (1770), argues that nothing happens without a cause, that causes are necessarily connected with their effects, and that everything that happens is connected through a chain of causation. He writes that even in a storm or in a whirlwind of dust, there is not a single molecule for which there is no sufficient reason why it is at one place rather than at another:

> A geometrician, who would exactly know the different forces which act in these two cases, and the properties of the molecules that are moved, could demonstrate, that, according to given causes, each molecule acts precisely as it ought to act, and could not have acted otherwise than it does. (D'Holbach, 1770, p. 51-52).[14]

There is a great similarity between Laplace's and D'Holbach's statements; whereas Laplace (1814) writes about an intelligence with calculating capacities and perfect knowledge of the forces and the "respective situations of beings", D'Holbach writes about a geometrician with perfect knowledge of the forces and the "properties of

[13] Wolfe (2007, p. 38) already indicates that "Laplacian" determinism already appeared before Laplace, mentioning Condorcet and D'Holbach; Wolfe (2012) in addition mentions the fragment in Diderot (1951). Hahn (2005, p. 58) discusses the connection with Condorcet and writes "The notion of an Intelligence with vast calculating powers was familiar to Leibniz, Maupertuis, and Condorcet. Determinism itself was far from novel, having been expressed in various ways by Leibniz and the Jesuit scientist Boscovich before him." (He gives no references). Furthermore, Stiegler (1974) points to Leibniz and Boscovich as proto-Laplacian determinists.

[14] "Un géometre, qui connoîtroit exactement les différentes forces qui agissent dans ces deux cas, & les propriétés des molécules qui sont mues, démontreroit que, d'après des causes données, chaque molécule agit précisément comme elle doit agir, & ne peut agir autrement qu'elle ne fait."

molecules". D'Holbach, however, does not write that such a geometrician could make perfect predictions, but that it could demonstrate that each molecule acts as it ought to act and that its motion is thus uniquely determined.

However, D'Holbach's determinism is not limited to physics, and does not seem to be based on physics either. In the case of a storm or a whirlwind of dust, knowledge of the forces that are present and of the properties of the molecules suffices to know how each molecule will act, but in general, the laws according to which cause and effect are connected do not have to be laws of physics, but can also be laws of human behaviour, for example.[15] D'Holbach writes that "Nature, in all its phenomena, acts necessarily according to the essence which is proper to it; all the beings which it contains act necessarily according to their particular essences" (D'Holbach, 1770, p. 50).[16] This is a Spinozist view (see Schliesser (forthcoming) on essences in Spinoza). D'Holbach continues to write that, just as in a storm or in a whirlwind of dust, not a single molecule moves by chance, so in a society, the acts of individual men are all determined, even in the whirlwind of a political revolution:

> In the terrible convulsions which sometimes agitate political societies, and which often produce the overthrow of an empire, there is not a single action, a single word, a single thought, a single desire, a single passion in the agents which participate in the revolution as destructors or as victims, which is not necessary, which does not act as it ought to act, which does not infallibly produce the effects that it has to produce, according to the place which these agents occupy in this moral turmoil. This would seem evident for an intelligence that would be in a position to know and appreciate all the actions and reactions of the minds and the bodies of those who contribute to this revolution. (D'Holbach, 1770, p. 52).[17]

It is not clear that this claim, that it is theoretically possible to see how all the details of a revolution hang together in a necessary connection, based on a knowledge of the "minds and bodies" of the people who participate in it, is a consequence of the laws of

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[15] About the necessary laws according to which everything around us happens, D'Holbach writes: "D'après ces loix les corps graves tombent, les corps légers s'élevent, les substances analogues s'attirent, tous les êtres tendent à se conserver, l'homme se chérit lui-même, il aime ce qui lui est avantageux dès qu'il le connoît, & déteste ce qui peut lui être défavorable." (D'Holbach, 1770, p. 51)

[16] "La nature dans tous ses phénomenes agit nécessairement d'après l'essence qui lui est propre; tous les êtres qu'elle renferme agissent nécessairement d'après leurs essences particulieres."

[17] "Dans les convulsions terribles qui agitent quelquefois les sociétés politiques, & qui produisent souvent le renversement d'un empire, il n'y a pas une seule action, une seule parole, une seule pensée, une seule volonté, une seule passion dans les agens qui concourent à la révolution comme destructeurs ou comme victimes, qui ne soit nécessaire, qui n'agisse comme elle doit agir, qui n'opere infailliblement les effets qu'elle doit opérer, suivant la place qu'occupent ces agens dans ce tourbillon moral. Cela paroîtroit évident pour une intelligence qui seroit en état de saisir & d'apprécier toutes les actions & réactions des esprits & des corps de ceux qui contribuent à cette révolution."

physics. D'Holbach argues that all processes are reducible to matter and motion, but this does not necessarily mean that they are reducible to physics, since according to D'Holbach, matter can have different qualities of which he does not claim that they can all be understood in terms of physics. Therefore, he leaves open the possibility that the human body has properties which cannot be understood through the laws of physics: "Holbach is not a naturalist in the stricter sense of attempting to understand human beings in terms of the same laws that explain the rest of nature" (LeBuffe, 2010).[18] Thus, the claim that perfect prediction of human behaviour is possible on the basis of perfect knowledge of mind and body does not seem to be based on determinism in physics, and the motivation for this claim must rather be sought in his philosophy.

The basic idea that there could be an intelligence capable of making perfect predictions can already be found in Leibniz; in the case of Leibniz, it is deeply embedded in his philosophical system. In (1702), Leibniz defends his doctrine of the pre-established harmony against criticism by Bayle, who has argued that it is absurd to think that a body could operate independently of the mind. Leibniz replies:

> There is no doubt that a man could make a machine which was capable of walking around a town for a time, and of turning precisely at the corners of certain streets. And an incomparably more perfect, although still limited, mind could foresee and avoid an incomparably greater number of obstacles. And this being so, if this world were, as some think it is, only a combination of a finite number of atoms which interact in accordance with mechanical laws, it is certain that a finite mind could be sufficiently exalted as to understand and predict with certainty everything that will happen in a given period. This mind could then not only make a ship capable of getting itself to a certain port, by first giving it the route, the direction, and the requisite equipment, but it could also build a body capable of simulating a man. (Leibniz, 1702, p. 243).

However, Leibniz added that the world is *not* a combination of a finite number of atoms, and therefore it takes an infinite mind to foresee all occurrences. [19]

[18] Wolfe (2007, p. 38) makes a distinction between Laplacian determinism and Spinozistic determinism, where the former "emphasizes predictability, based on the laws governing the basic components of the universe and their interactions, and in that sense is purely physicalist", while the latter is a determinism in which "the nature of mind and action requires specific types of explanations." Wolfe classifies D'Holbach as a Laplacian determinist, but although D'Holbach indeed emphasizes predictability, he does so without referring to the reducibility of the processes of the mind to physics, and his view is quite compatible with the possibility that acts of the will, though deterministic, require special explanations and are not reducible to physics.

[19] This infinite mind being God. Leibniz, in (1916 [1765], p. 51): "it belongs only to the supreme Reason, whom nothing escapes, distinctly to comprehend all the infinite and to see all the reasons and all the consequences. All that we can do in regard to infinities is to know them confusedly, and to know at least distinctly that they are such; otherwise we judge very wrongly of the beauty and the grandeur of the universe...."

We have seen that the so-called Laplacian determinism, the idea that it is possible in principle to make perfect predictions on the basis of perfect knowledge of the present state of the universe, did not originate in Laplace, and as this section has shown, the basic idea was not necessarily one that was coming from physics (although in Leibniz there is a strong connection between physics and philosophy).

1.5 The principle of sufficient reason and the law of continuity

We've seen in section 3 that Laplace's determinism is based on the principle of sufficient reason; to see how this principle relates to the possibility of mathematical descriptions of nature, we turn to one of the other authors who already stated something similar to Laplacian determinism before Laplace, namely Condorcet. Laplace was almost certainly influenced by Condorcet's determinism: Condorcet was an important intellectual influence on Laplace, and according to Hahn (2005, p. 50-51), Laplace carefully read the book in which Condorcet's argument for determinism appeared. The context of Condorcet's statement is a discussion about the distinction between necessary and contingent laws of nature.[20] According to Condorcet, the necessary laws of nature are "necessary consequences of the idea that we have of matter" (Condorcet, 1768, p. 4).[21] These laws form mechanics, which at the time was a part of mixed mathematics. The contingent laws of nature are those which God could have willed differently, and we can only know these laws empirically: to discover them, we must closely observe the phenomena in nature and submit the observations to calculation. According to Condorcet, Newton's law of gravitation belongs to the contingent laws of nature and thus does not belong to mechanics properly speaking. The contingent laws of nature constitute the *Système du Monde.* Condorcet argues that although many laws of nature are contingent, there is no contingency in the phenomena, which all follow necessarily from the laws of nature:

> ...if the law of continuity is not violated in the universe, one could regard its state at every instant as the result of what had to happen to matter once arranged in a certain order and then abandoned to itself. An Intelligence that would then know the state of all phenomena at a given instant, the laws to which matter is

[20] There was a debate about this issue at the time; see the preface in D'Alembert (1758).

[21] "des conséquences nécessaires de l'idée que nous avons de la matiere".

> subjected, and their effects after a certain period of time, would have perfect knowledge of the *System of the World*. (Condorcet, 1768, p. 4-5).[22]

Condorcet does not specify which laws the intelligence needs for his perfect knowledge, but in any case, these can not only be the necessary laws which follow directly from our conception of matter, but also have to include contingent laws of nature. Thus, Condorcet's determinism is not based on mechanics, at least not in the narrow 18th century sense in which mechanics is a branch of mixed mathematics; but this still leaves the possibility open that it is based on physics.

To get a better understanding of the foundations of Laplace's determinism, we have to look at the "law of continuity" to which Condorcet refers, and on which according to him the idea that the necessary consequence of the state of the universe at a previous instant depends. A description of this law can be found in the *Encyclopédie* of Diderot and D'Alembert:

> Continuity, (law of) is a principle that we owe to M. Leibniz, and which teaches us that nature takes no leaps, and that a being never passes from one state into another, without passing through all the different states that one can conceive in between them. This law follows, according to M. Leibniz, from the axiom of sufficient reason. (Formey and d'Alembert, 1754, p. 116).[23]

In the *Encyclopédie*, the law of continuity is thus directly linked with Leibniz and the principle of sufficient reason; this supports the idea that there is a common background to Laplace's and Condorcet's determinism.

The law of continuity was a familiar principle in eighteenth century physics, and is usually attributed to Leibniz; it plays an important role in Leibniz' thought. Throughout Leibniz' work, one can find various formulations of the law, in the context of mathematics, physics, and metaphysics. The most familiar formulation of the law of continuity is the statement that "nature makes no leaps", that all transitions in nature are gradual, and that nothing can change from one state to another without passing through all the intermediary stages (Leibniz, 1916 [1765], p. 50). This principle implies that there can be no perfectly hard bodies, because the collision of two hard bodies would involve a discontinuous change in the direction of motion of the bodies (Leibniz,

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[22] "... si la loi de la continuité n'étoit point violée dans l'univers, on pourroit regarder ce qu'il est a chaque instant, comme le résultat de ce qui devoit arriver à la matiere arrangée une fois dans un certain ordre, & abandonnée ensuite à elle-même. Une Intelligence qui connoîtroit alors l'état de tous les phénomenes dans un instant donné, les loix auxquelles la matiere est assujettie, & leur effet au bout d'un tems quelconque, auroit une connoissance parfaite du *Systême du Monde*."

[23] "Continuité, (loi de) c'est un principe que nous devons à M. Leibnitz, & qui nous enseigne que rien ne se fait par saut dans la nature, & qu'un être ne passe point d'un état dans un autre, sans passer par tous les différens états qu'on peut concevoir entr'eux. Cette loi découle, suivant M. Leibnitz, de l'axiome de la raison suffisante."

1695, p. 170). In addition, Leibniz argues that the law of continuity implies that two individual things can never be exactly equal, because there are infinite gradations and variations (Leibniz, 1916 [1765], p. 51; see also Lovejoy, 1936, on the continuity of beings). Another version of the law of continuity that Leibniz gave is the principle that "If any continuous transition is proposed terminating in a certain limit, then it is possible to form a general reasoning, which covers also the final limit" (quoted in Schubring, 2005, p. 174). This is a mathematical principle, and it lies at the basis of Leibniz' calculus, because it guarantees that infinitesimals behave properly: if you take the limit from quantities of a very small but finite size to infinitely small quantities, nothing essential changes. It also implies that rest can be considered as infinitely small velocity; Leibniz demands that the laws of motion have to be consistent with this (Leibniz, 1687; Leibniz, 1695, p. 171).

The law of continuity is thus a general principle in Leibniz' system, with various implications in physics, mathematics, and metaphysics. Schubring (2005, p. 174) argues that although Leibniz gave both metaphysical and purely mathematical formulations of the law of continuity, it was the metaphysical version of the law that was picked up by his contemporaries and discussed throughout the eighteenth century: in particular, it played a central role in the debate about whether there could be perfectly hard bodies.

According to the *Encyclopédie*, Leibniz derived the law of continuity from the principle of sufficient reason. However, for Leibniz, there could only be an indirect connection between the principle of sufficient reason and the law of continuity: it is possible that the law of continuity does not hold, but there is a sufficient reason for it to hold in our world. According to Rutherford, "Leibniz describes the principle of continuity as a 'principle of general order', which obtains in the actual world as a consequence of God's wisdom. He thus explicitly connects it with God's choice of the best of all possible worlds." (Rutherford, 1995, p. 29, and see Leibniz, 1687, p. 52). But others during the eighteenth century drew a more direct relation between the principle of sufficient reason and the law of continuity. For example, Johann Bernoulli, who supported Leibniz' argument that hard body collisions would violate the law of continuity, gave the following justification of the law:

> If nature could pass from one extreme to another, for example from rest to motion, from motion to rest, or from a motion in one direction to a motion in the opposite direction, without passing through all the imperceptible motions that lead from the one to the other, it should be the case that the first state is destroyed, without nature knowing what new state it must conform itself to; for, in the end, what reason could she have to prefer one, of which it could not be asked why this one rather than that one? Since there is no necessary connection

> between these two states, no passage from motion to rest, from rest to motion, or from a motion to a motion in the opposite direction, no reason could determine nature to produce one thing rather than any other (Bernoulli, 1727, p. 9).[24]

Bernoulli thus bases the law of continuity on the principle of sufficient reason in a more direct way than Leibniz: he argues that if there is a discontinuous change between states, there is no sufficient reason for the transition from one state to another. Therefore, the state at a certain instant must continuously determine subsequent states. A similar line of argument can be found in the *Encyclopédie,* where the connection between the principle of sufficient reason and the law of continuity is explained as follows:

> For each state in which a being finds itself, there must be a sufficient reason why this being finds itself in this state rather than in any other; and this reason can only be found in the antecedent state. This antecedent state thus contained something which has given rise to the current state which has followed it; so that these two states are connected in such a way that it is impossible to put another one in between; because if there were a possible state between the current state and that which immediately preceded it, nature would have left the first state without already being determined by the second to abandon the first; there would thus not have been a sufficient reason why it would have passed to this state rather than to any other possible state. Thus no being passes from one state to another, without passing through the intermediary states; just as one does not go from one city to another without going over the road between them (Formey and D'Alembert, 1754, p. 116).[25]

[24] "Si la nature pouvoit passer d'un extrême à l'autre, par exemple, du repos au mouvement, du mouvement au repos, ou d'un mouvement en un sens à un mouvement en sens contraire, sans passer par tous les mouvemens insensibles qui conduisent de l'un à l'autre; il faudroit que le premier état fut détruit, sans que la nature sçût à quel nouvel état elle doit se déterminer; car enfin par quelle raison en choisiroit-elle un par préference, & dont on ne pût demander pourquoi celui-ci plutôt que celui-là? Puisque n'y ayant aucune liaison nécessaire entre ces deux états, point de passage du mouvement au repos, du repos au mouvement, ou d'un mouvement à un mouvement oposé; aucune raison ne la détermineroit à produire une chose plutôt que toute autre."

[25] "Chaque état dans lequel un être se trouve, doit avoir sa raison suffisante pourquoi cet être se trouve dans cet état plûtôt que dans tout autre; & cette raison ne peut se trouver que dans l'état antécédent. Cet état antécédent contenoit donc quelque chose qui a fait naître l'état actuel qui l'a suivi; ensorte que ces deux états sont tellement liés, qu'il est impossible d'en mettre un autre entre deux: car s'il y avoit un état possible entre l'état actuel & celui qui l'a précédé immédiatement, la nature auroit quitté le premier état, sans être encore déterminée par le second à abandonner le premier; il n'y auroit donc point de raison suffisante pourquoi elle passeroit plûtôt à cet état qu'à tout autre état possible. Ainsi aucun être ne passe d'un état à un autre, sans passer par les états intermédiaires; de même que l'on ne va pas d'une ville à une autre, sans parcourir le chemin qui est entre deux."

Like Laplace, the *Encyclopédie* uses a notion of the principle of sufficient reason that deviates from Leibniz' version: it is about how something is brought about, about the efficient cause of the state of a certain being. Laplace had claimed that the idea that the state of the universe is the cause of the state at the next instant followed from the principle of sufficient reason. Here, we see how the argument can be spelled out in more detail with the law of continuity as an intermediary step. There can be no sudden jump or discontinuity between one state and the next, because in that case, there would be no sufficient reason for the transition from one state to the other, there would be an unexplainable gap. Therefore, the state at the universe at a certain instant continuously determines subsequent states, through infinitesimal steps, and in this way, one can say that the state of the universe is the cause of the state at the next instant.

It is important to note that this is a stronger requirement than determinism, in the sense that according to this requirement, it is not enough that there are laws which determine how subsequent states relate to each other: such laws could still allow for discontinuous changes in certain variables. Rather, there has to be a "necessary connection" between states, in the sense that one state is the continuously producing cause of the following state, and states follow each other without gaps. In this view, hard body collisions are excluded: even if these collisions are perfectly deterministic, they still involve a discontinuous change in the direction of motion. This is the reasoning behind Laplace's claim that the principle of sufficient reason implies that the state of the universe is the cause of the state at the next instant, and we see that it is a type of reasoning that is quite foreign to modern physics, in which there is no such rigorous exclusion of discontinuities.

Thus, the argument for determinism that can be reconstructed from historical sources is that the principle of sufficient reason implies that there is a continuity between states, and that this implies that the state of the universe is the cause of the state at the next instant, and in this way it implies determinism. This is a completely general philosophical argument, and the whole story of the background of Laplace's determinism can thus be told without referring to mathematics or the equations of mechanics. However, this does not imply that there is no connection. For Leibniz, the law of continuity plays a central role in the foundations of differential calculus and it lies at the basis of the application of differential calculus to physics: only when it is guaranteed that variables in nature change in a continuous manner, can differential calculus be applied successfully. Thus, the principles which underlie the idea that the state of the universe is the cause of the state at the next instant, also underlie the idea that the evolution of physical systems can be described by differential equations. Cassirer (1956 [1936], p. 5) captures this dual aspect of Laplacian determinism when he writes that "the Laplacian formula is as capable of a scientific as of a purely metaphysical interpretation, and it is precisely this dual character that accounts for the strong influence it exercised".

Furthermore, as we have seen in section (2), it turned out in the course of the nineteenth century that the equation of motion of a system can fail to have a unique solution if it involves a force that is non-Lipschitz-continuous; although this could not yet be spelled out at the time of Laplace (among others because there was not yet a clear notion of continuity of functions in mathematics), it is possible that he had a general, not mathematically precise idea that differential equations could fail to have unique solutions if they involved discontinuities of some sort.

Thus, the law of continuity, which played a role in the general argument for determinism, was also at the basis of the use of differential equations in physics and of the expectation that these equations would have unique solutions.

Conclusion

I have argued that Laplace's determinism was based on a re-interpretation of Leibniz' principle of sufficient reason and the law of continuity, rather than on his mechanics. To say that Laplace's determinism was not based on mechanics, is of course not to say that physics was wholly irrelevant to his idea of determinism. Although it was not possible for Laplace to give a strict mathematical derivation of determinism in the equations of physics, no conflicts with determinism were encountered in physics (except Poisson's theoretical example of an indeterministic system, which was an implausible system to occur in reality, and it is unknown whether Laplace was aware of it), and although it could not be proven that the equations of physics would always have unique solutions, this did seem plausible. It is also not to say that Laplace's determinism was unfounded or based on prejudice. There may be good reasons to appeal to the principle of sufficient reason; but regardless, the law of continuity had independent plausibility and played a significant role in eighteenth century physics. And in a sense, the law of continuity still plays a role in the present discussion of determinism in classical mechanics: it has been argued that the Norton dome may not be a "proper Newtonian system" because it involves a discontinuity in the shape of the dome (Malament, 2008).

The idea that perfect prediction is possible in principle on the basis of calculation and perfect knowledge of the world did not originate in Laplace, and it was not directly derived from the laws of physics. But significantly, the idea did come up in a time when big steps were being made in the application of differential calculus to physics.

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Paper 2: The Norton dome and the nineteenth century foundations of determinism*

Abstract

The recent discovery of an indeterministic system in classical mechanics, the Norton dome, has shown that answering the question whether classical mechanics is deterministic can be a complicated matter. In this paper I show that indeterministic systems similar to the Norton dome were already known in the nineteenth century: I discuss four nineteenth century authors who wrote about such systems, namely Poisson, Duhamel, Boussinesq and Bertrand. However, I argue that their discussion of such systems was very different from the contemporary discussion about the Norton dome, because physicists in the nineteenth century conceived of determinism in essentially different ways: whereas in the contemporary literature on determinism in classical physics, determinism is usually taken to be a property of the equations of physics, in the nineteenth century determinism was primarily taken to be a presupposition of theories in physics, and as such it was not necessarily affected by the possible existence of systems such as the Norton dome.

2.1 Introduction

We usually consider classical mechanics as the prime example of a deterministic theory, but Norton (2003) has shown that there are possible systems in classical mechanics which are not deterministic, notably the system which has become known as the Norton dome. This is regarded as a newly discovered instance of indeterminism in classical physics and it has raised quite some discussion among philosophers of physics in the last few years (see, among others, Norton 2003, 2008; Korolev 2007; Malament 2008; Wilson 2009; Zinkernagel 2010;

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Fletcher 2012). However, the same instance was already discussed during the nineteenth century. In this paper I discuss four nineteenth-century authors who wrote about such systems, namely Siméon Denis Poisson, Jean-Marie Duhamel, Joseph Boussinesq and Joseph Bertrand, all of them prominent French mathematicians and physicists.[1]

I argue that in the nineteenth century, the example did not convince many people that there was indeterminism in physics, and did not even always lead to reflections on the issue of determinism. The reason for this is that the nineteenth century conceptions of determinism were essentially different from the contemporary conception of determinism in classical physics. Contemporary philosophers of physics largely regard determinism as a property of the equations of physics, specifically as the statement that for each system there are equations of motion with unique solutions for given initial conditions. However, I show that in the nineteenth century, this claim was not strongly established, and that the authors that I discuss from this period treated determinism in an essentially different way. Specifically, from their arguments it appears that they thought that determinism could hold even in cases in which the equations of physics did not have a unique solution for given initial conditions. When confronted with systems in physics in which the equations fail to have a unique solution, they did not automatically recognize a violation of determinism; instead, they argued for example that the equation of motion was not rigorously valid, or that it did not reflect all that was to know about a system and that there could be additional determining factors. Apparently, for these nineteenth century authors, whether or not there was determinism in physical reality did not necessarily depend on whether the equations of physics had unique solutions. This indicates that for them, determinism was not an idea based on the properties of the equations of physics, but rather an a priori principle that was possibly based on metaphysical considerations about causality or the principle of sufficient reason; rather than a result derived from science, determinism was a presupposition of science, that had to be upheld even if it was not reflected in the equations.

In section 2, I explain the kind of indeterminism that is involved in the Norton dome. I then turn to the contemporary notion of determinism in classical physics and its foundations (section 3). In the rest of the paper, I chronologically treat the nineteenth century literature on the type of indeterminism involved in Norton's dome, and show how the notions of determinism employed therein differ from the now commonly accepted notion. I end with a brief discussion of the contemporary discussion of the Norton dome, to show how it differs from the nineteenth century treatment of the problem.

[1]Boussinesq (1879a) also mentions Cournot as an author who discussed such indeterministic systems, referring to (Cournot, 1841). But in fact, Cournot only discusses the problem of uniqueness of solutions to differential equations on a mathematical level, without relating it to problems in physics. Therefore his discussion does not tell us anything about his ideas on the issue of determinism in physics; for that reason I will not discuss it here.

2.2 Lipschitz-indeterminism in classical physics

The kind of indeterminism that is at issue in Norton's dome can be called "Lipschitz-indeterminism",[2] and amounts to the fact that differential equations can have non-unique solutions for given initial conditions at points at which they fail to obey a continuity requirement that is called Lipschitz-continuity.

There are different ways in which Lipschitz-indeterminism can arise in physics; below are two basic ways in which it can arise (of which the second is in fact a special case of the first). Both have appeared in recent years as well as in nineteenth century literature.

1) One way in which Lipschitz-indeterminism can arise in physics is through the assumption of a particular force acting on a point particle. Take a point particle which is subjected to a force $F(r) = cr^a$, with r its distance from the origin and c and a constants, with $0 < a < 1$. Newton's equation of motion is then $\frac{d^2r}{dt^2} = cr^a$. Take further $r_0 = 0$ and $v_0 = 0$; thus, the point particle is lying still at the origin for $t = 0$. The trivial solution to this problem is $r(t) = 0$ for all t, but there is also a solution

$$r(t) = \left(\frac{(1-a)^2}{2(1+a)}\right)^{1/(1-a)} (t-T)^{2/(1-a)}.$$

In this equation, T is any time whatsoever. The particle may thus either remain at the origin or start to move at an arbitrary time; both are compatible with the equation of motion and the initial conditions.

This example has been discussed among others by Malament (2008). Malament asks whether the above force is allowed in Newtonian mechanics. The force might be regarded as strange because it is directed outwards from an empty point; and in general, the question is whether any kind of force can simply be posited in a classical system. This is according to him a point for discussion. Therefore, he argues, the case for indeterminism is more convincing if such a force is not simply posited but arises naturally in a Newtonian system.

2) Norton (2003) has designed a system in which the force that is required for Lipschitz-indeterminism arises from Newtonian gravitation. His example involves a dome of a particular shape along which a point particle of unit mass can move. The shape of the dome is given by

$$h(r) = \frac{2}{3g} r^{\frac{3}{2}}$$

with $h(r)$ the decrease in height from the top of the dome as a function of r, the radial distance coordinate defined on the surface of the dome. The point particle is subjected to gravity and placed at the summit of the dome. The equation of motion then becomes $\frac{d^2r}{dt^2} = \sqrt{r}$; in other words, it

[2] This term is introduced by Korolev (2007).

is the same as in the above case of a force $F(r) = cr^a$ acting on a point particle, but with $a = \frac{1}{2}$. The solution can be written as follows:

$$\begin{aligned} r(t) &= \frac{1}{144}(t-T)^4 \text{ for } t \geq T, \\ r(t) &= 0 \text{ for } t \leq T. \end{aligned}$$

Again, T is an arbitrary time. This means that the particle can lie still at the summit for an arbitrary length of time and then start to slide off. There is thus a failure of determinism in this case: there is more than one future time evolution that is compatible with the equations.

There are thus possible systems in classical mechanics for which the equation of motion does not have a unique solution. Lipschitz-indeterminism is a mathematical property of the equation of motion; as we will see, whether this non-uniqueness of solutions of the equations corresponds to indeterminism in an actual physical system depends on how one conceives of the relation between the equations of physics and physical reality.

2.3 The notion of determinism in classical physics

The above systems violate what is currently our standard conception of determinism in classical physics. For convenience, I refer to this notion of determinism as DEM, or Determinism based on the Equation of Motion, and define it as follows:

- [*DEM*]: For each system in classical physics, there are differential equations of motion of the form

$$\frac{d^2r}{dt^2} = F(r). \tag{1}$$

These, together with the initial conditions $r(t_0) = r_0$ and $\frac{dr}{dt}(t_0) = v_0$, uniquely determine the future states of the system.

This is the formulation of determinism in classical physics that is used in most contemporary literature: see for example Landau and Lifshitz (1976, pp. 1-2); Arnold (1989, p. 4, 8); Earman (1986, pp. 29-32); Malament (2008). In this way, determinism can be regarded as a potential theorem in physics. To find out whether DEM holds, one has to study the equations of classical physics; if it holds then determinism can be regarded as a result following from physics, as a property that the equations of classical physics have.

However, Lipschitz-indeterministic systems violate DEM, because these systems are described by differential equations which do *not* have a unique solution. Thus, if there are possible physical systems which are Lipschitz-indeterministic, DEM fails. The fact that Lipschitz-indeterministic systems were already discussed during the nineteenth century as possible physical systems indicates that at least certain authors knew that DEM could fail. However, I argue that they did not necessarily regard this as a failure of determinism; thus, for these authors, DEM was not the definition of determinism. To see what this meant, we first have to examine how well-established DEM was at the time and what its foundations were.

DEM is not necessarily a part of Newtonian mechanics; while (1) equals Newton's second law as it is now understood, Newton did not show that it always had a unique solution, and in fact, he did not even formulate it as a differential equation.

Histories of determinism in science almost always start with a statement from Laplace's *Essai philosophique sur les probabilités* (1814), although in fact, there was little novelty in this statement: Laplace had written something similar as early as the 1770's (see Dahan Dalmedico 1992; Hahn 2005), and Wolfe (2007) has pointed out that similar statements appeared in the decades before in the writings of people like d'Holbach and Condorcet (see also Van Strien (2014)). The famous statement in Laplace (1814) is as follows:

> An intelligence which, at one given instant, knew all the forces by which the natural world is moved and the position of each of its component parts, if as well it had the capacity to submit all these data to Mathematical analysis, would encompass in the same formula the movements of the largest bodies in the universe and those of the lightest atom. [Translation: Deakin (1988). All other translations are my own].

One may ask whether Laplace derived this statement from the fact that for each system in physics there are equations of motion in the form of differential equations which always have a unique solution. Israel (1992) has argued against this, pointing out that at the time that Laplace wrote the above text, there was no mathematical theorem which showed whether these differential equations always had a unique solution. It can be argued that because Laplace did not have a mathematical theorem to support his claim, his determinism was based on metaphysics rather than derived from physics (Israel 1992; Van Strien 2014).

It was known since the eighteenth century that differential equations could have non-unique solutions: people like Clairaut, Euler, D'Alembert and Lagrange had studied so-called singular solutions of differential equations (Kline, 1972, p. 476ff), and in fact, Laplace himself had done important work on this issue (Laplace, 1772; Kline, 1972, p. 477). This was an issue in pure mathematics; possibly it was intuitively clear to Laplace that the differential equations that appeared in problems in physics had to be of a different kind, and always had a unique solution. But this could not be proven mathematically.

According to Kline (1972, p. 717), mathematicians only became interested in the existence and uniqueness of solutions to differential equations in the early nineteenth century, when more and more complicated differential equations appeared in mathematical problems. As mathematicians found themselves more often unable to solve certain equations, they turned to attempts to prove the existence and uniqueness of solutions for certain types of equations. The first person to work on this issue, from the 1820's onwards, was Cauchy. Cauchy showed that an equation like (1) has a unique solution if $F(r)$ is continuously differentiable (Kline, 1972, p. 717). This result apparently did not become well known, for when Lipschitz wrote about the same issue in 1876, he claimed to be unaware of any literature on the topic (Lipschitz, 1876). Lipschitz showed that the function $F(r)$ does not necessarily have to be continuously differentiable in order for an equation like (1) to have a unique solution; it is enough if it fulfils the condition that there is a constant $K > 0$ such that for all r_1 and r_2 in the domain of F,

$$|F(r_1) - F(r_2)| \leq K|r_1 - r_2|.$$

This condition came to be called 'Lipschitz continuity'. Thus, the equations of motion of a classical system can fail to have a unique solution if they involve a force which is not Lipschitz continuous. This is the case with Norton's dome and the other systems that I treat in this paper; hence these are called 'Lipschitz-indeterministic systems'. (But note that this term is an anachronism when applied to the authors that I treat in this paper, who wrote about 'Lipschitz-indeterministic systems' either before Lipschitz's 1876 publication or shortly after but without referring to it).

The conditions under which differential equations have a unique solution, which form the mathematical foundation of DEM, were thus only clarified late in the nineteenth century. Despite this fact, one might suppose that physicists took DEM for granted and accepted it in practice, like Laplace supposedly did. But the way in which Lipschitz-indeterminism was treated shows that at least some authors were aware of the fact that DEM could fail.

2.4 Poisson and Duhamel on singular solutions

While the mathematical study of differential equations with non-unique solutions has continued since the eighteenth century, the question whether such non-unique solutions can appear in problems in physics has only surfaced a couple of times at different moments in the history of physics.

In 1806, Poisson published a mathematical text about differential equations and difference equations with non-unique solutions. He discussed different issues connected to such equations, drawing on earlier work of Euler, Lagrange and other mathematicians. But he also added a paragraph about the possibility for non-unique solutions to occur in dynamics. He wrote that he was probably the first who discussed this issue: "nobody, as far as I know, has yet proposed to determine their usage in issues in dynamics. However, this is a topic of science that deserves to be completely clarified ..." (Poisson, 1806, p. 63).

Poisson wrote that differential equations can have two kinds of solutions: besides the ordinary solutions which can be found by integrating the differential equations, there can be 'solutions particulières', or singular solutions, which also satisfy these equations. Poisson gave two examples of physical systems in which singular solutions could occur, both involving a rectilinear motion of a body subjected to a given force. One of these is equal to the first one we discussed in section (2), namely $F(r) = cr^a$ with $0 < a < 1$. Poisson shows that the equation of motion of a point particle subjected to this force has a singular solution $r = 0$, which predicts that the body will never be put in motion, as well as a regular solution (Poisson, 1806, p. 104). Poisson's other example involves a friction-like force depending on velocity, namely $F(v) = -c\sqrt{v}$, with c a constant (Poisson, 1806, p. 100). Also a particle subjected to this force can fail to have a unique solution: the equation of motion of a particle subjected to this force is

$$\frac{d^2r}{dt^2} = -c\sqrt{\frac{dr}{dt}}$$

If we take $c = -1$ for simplicity, and initial conditions $r(0) = 0$ and $v(0) = 0$, the equation has a singular solution $r(t) = 0$ as well as a regular solution

$$r(t) = \frac{1}{12}(t - T)^3$$

In this equation, T is an arbitrary time.

This example has also reappeared in recent years: it is the same as one that Hutchison independently gave in 1993, a decade before the Norton dome discussion, to show that there could be indeterminism in classical physics. Physically, it is a less interesting case than that of a force depending on distance, because in Newtonian physics, all fundamental forces have to be conservative and therefore a velocity-dependent force like $F(v) = -c\sqrt{v}$ cannot be a fundamental force; for this reason, Hutchison's example was criticized by Callender (1995).

Poisson pointed out that it is important to be aware of the possibility that there is more than one solution to the equation of motion: it is possible that the equations of motion admit singular solutions, and then "one has to know whether one must continue to take the integrals [regular solutions] to represent the motion of the system, or whether one has to resort to the singular solutions" (Poisson, 1806, p. 100). Apparently, it was evident to him that only one of the solutions to the equation of motion could be the 'right' one, and he spent several pages discussing the problem of how to pick out the right solution in different cases; so at first, he did not seem to be worried at all about the issue of determinism. But after a couple of pages, Poisson remarked:

> The motion in space of a body subjected to the action of a given force, and departing from a given position with a velocity that is also given, has to be absolutely determined. It is thus a kind of paradox that the differential equations on which this motion depends can be satisfied by several equations which also satisfy the initial conditions of the motion. It does not seem that this difficulty has ever been noticed, and it would be good to draw the attention of mathematicians to it. (Poisson, 1806, p. 106).

Note that this is one more example of an expression of determinism in physics prior to Laplace (1814). Yet, it is immediately followed by pointing out a problem with this expression.

Poisson did not further elaborate on what was paradoxical about the issue. It was evident to him that physical systems were deterministic in the sense that there could not be more than one possible trajectory for a certain particle; yet he found that the equations of motion alone did not always uniquely determine the trajectory. This had to mean that in these cases, additional considerations were needed to single out the right solution to the equations.

In the case of a point particle subjected to the force $F(r) = cr^a$ with $0 < a < 1$ and initial velocity zero, Poisson argued that it is the singular solution which is the right one, "for it is clear that the particle must remain at the starting point, since at this point its velocity and the force to which it is subjected are equal to zero" (Poisson, 1806, p. 104). Several decades later, Boussinesq (1879a) argued that this solution to the problem depended on an unfounded metaphysical notion of forces as causes of change:

> When saying that a point, currently at a unique position where it is assumed to be placed, without velocity and not solicited by any force, will be kept constantly at rest by its inertia, one goes beyond the bounds of positive science, which only permits to state in such cases that the velocity and the acceleration are *currently* zero. One attributes to the word force a metaphysical meaning of *cause*, which differs from its exact mathematical meaning. (Boussinesq, 1879a, p. 124).

As Boussinesq points out, Poisson's solution can only work if one assumes that force is not simply the product of mass and acceleration, but that force is temporally prior to acceleration, and that velocity does not change continuously with force but in small steps depending on the force experienced immediately before. A similar argument has recently been put forward by Zinkernagel (2010) who uses this understanding of force to argue that there is no true indeterminism in the Norton dome case.[3] Zinkernagel bases this argument on an interpretation of Newton's first law, which says that bodies continue in a state of rest or uniform motion as long as there is no force acting on them. He interprets this as implying that force is prior to acceleration and that for each change in velocity, there needs to be a force causing this change. Thus, the strategy that Poisson seems to employ can be based on a causal interpretation of Newton's first law and of forces. This causal interpretation goes beyond the mere equations, which do not necessarily have to be interpreted causally.

Boussinesq presents Poisson's strategy as the postulation of an extra principle stating what happens at singular points, the principle being that that there is in physical systems a preference for rest over motion, so that the particle will stay at rest (Boussinesq, 1879a, p. 123). But in fact Poisson did not postulate such a general principle. He wrote about the problem how to pick the right solution that it was "a difficulty that can only be resolved in each individual case, through considerations drawn from the nature of the problem" (Poisson, 1806, p. 100): thus, he appealed to additional considerations to decide in specific

[3] The similarity between Poisson and Zinkernagel is also pointed out in Fletcher (2012).

cases which solution to the equations of motion was the right one, and did not attempt to found these considerations on other, new or existing, laws of nature.

In a later text (Poisson, 1833), Poisson argued that singular solutions only occurred in theoretical examples, and never in real physical situations. He gave an example of a system for which the equations of motion have a singular solution, and added:

> This example, purely hypothetical, suffices to demonstrate the necessity to take into account the singular solutions of the differential equations of motion, if there were any singular solutions; which does not happen in reality, as we see from the expressions of forces, as functions of the acquired velocity and the distance travelled, which take place in nature.

It is not clear what his motivation for this statement was, and why he thought that the force functions that he had proposed which led to singular solutions did not occur in nature. But it does fit with his treatment of singular solutions as a practical difficulty that could be encountered when solving problems in physics instead of as a philosophical issue concerning whether there is determinism in physical systems.

Another discussion of the possibility that the equation of motion for a certain system fails to have a unique solution can be found in Duhamel's[4] *Cours de mécanique* (1845). Duhamel's treatment of the issue bears similarity to that of Poisson. He starts his discussion with a seemingly explicit statement of determinism:

> The differential equation of the motion of a point together with the initial conditions completely determines the motion of this point, for an infinite time. But when one integrates this function, one has to take care not to omit any of its solutions, and consider those which are called singular as well those which are known as *general integrals.* (Duhamel, 1845, p. 265).

These two statements—that the equation of motion of a particle plus the initial conditions determine the motion of that particle, and that there are cases in which the equation of motion does not have a unique solution for given initial conditions—seem to be in contradiction with one another. But Duhamel did not seem to be troubled by this and it is not clear from his text whether he perceived it as a contradiction. He just proceeded to discuss the problem how to single out the 'right' solution to the equation of motion, in a way that was very similar to that of Poisson.

The example that Duhamel gave was that of a mass point moving through a fluid with velocity v, with the resistance of the fluid proportional to v^a, $0 < a < 1$. The resulting equation of motion is

$$\frac{dv}{dt} = -cv^a.$$

[4]Duhamel (1797-1872) was at this time professor of mathematics at the École Polytechnique and member of the Académie des Sciences. See O'Connor and Robertson (2005a).

With $a = \frac{1}{2}$, this is equal to the example that Poisson had given of a friction-like force that could lead to a singular solution. Duhamel showed that there were several solutions to this problem, and showed how one could use physical considerations to pick the right one:

> In fact, a point which is placed without velocity in a medium of which the resistance is some function of the velocity, will remain indefinitely in the position where one places it, since no force will be applied to it; it will have no tendency to move in any direction, and will remain forever at rest. (Duhamel, 1845, p. 268).

In other words, if you consider the problem of the mass point moving through the fluid, it is evident that when it comes to rest, it will not start moving again: although a continuation of the motion may also be consistent with the equations, it is not consistent with the physical situation itself.

Apparently, there are cases in which the equations of motion together with the initial conditions do not uniquely determine the future states of the system. How does this relate to Duhamel's statement that the motion of a particle is completely determined by its equation of motion plus initial conditions? A possible interpretation is that in this statement he is not actually talking about determinism, but rather about 'determination' in the sense that the equation of motion, $\frac{d^2r}{dt^2} = F(r)$, plays its role in determining the acceleration of the particle as a function of position or velocity, without implying that the equation always has a unique solution so that also the future positions of the particles are determined. Thus, the equation of motion holds, but does not necessarily have a unique solution.

Duhamel did not further consider the issue of determinism. The fact that he did not see a failure of determinism can be explained by the fact that, like Poisson, he thought that in the cases in which the equation of motion did not have a unique solution, one could take recourse to additional considerations to determine the right solution.

According to DEM, the future states of a system are determined by the equations of motion and initial conditions alone. Poisson and Duhamel recognized that there were theoretical cases in which DEM failed, namely the systems they discussed for which the equation of motion did not have a unique solution; however, they did not conclude that there was indeterminism in these cases. Instead, they argued that in these cases, the future states of the system were determined by the equations of motion plus additional considerations (possibly based on a causal conception of force). Thus, there is not one fixed set of laws in physics which determines the future evolution of each system, and the equations of physics give no guarantee that the future evolution is uniquely determined. From this, we can conclude that Poisson and Duhamel did not regard determinism as being based in the equations of physics; rather, it was an a priori principle, that could hold even when DEM failed.

2.5 Boussinesq's free will theory

The writings by Poisson and Duhamel on the possibility that equations of motion fail to have a unique solution seem not to have caught much attention, for it was not a widely discussed or widely known subject until the French physicist and mathematician Joseph Boussinesq[5] wrote about it in the 1870's (Boussinesq, 1879a), claiming that he had come up with the idea independently of Poisson and Duhamel.

Boussinesq used these singular solutions in an ambitious and elaborate theory about life and free will. According to Boussinesq, whenever there is a singular solution to the equations of motion of a certain system, it is physically undetermined what will happen. However, if the laws of physics do not determine what will happen, there must be something else determining this, and this is what Boussinesq calls the *directive principle*. The directive principle is a principle that does not belong to physics. It might be moral free will in human beings or a physiological, organizing principle in organisms. At singular points, the directive principle can act on the system and change the future course of the system without exerting any force. The equations of motion are never changed or violated by the directive principle, but merely supplemented by it in cases in which the equations themselves leave the future course of the system undecided (Boussinesq, 1879a, pp. 40-41, 53-55).

Boussinesq's theory came at a time at which there was much interest in the relation between physical determinism and free will and his theory was widely discussed by both scientists and philosophers, among others Maxwell, Du Bois-Reymond and Renouvier.[6] It was strongly related to a number of other, more modest theories about life and free will that appeared around the same time, for example those of Kelvin, Maxwell, and Saint-Venant (see Hacking (1983), Porter (1986)). According to these theories, there could be unstable points in physical systems at which a very small act could have a large impact, and at these points, a directive principle may act on the system though exerting a very small, possibly infinitely small force. But even if this force could become infinitely small, it still implied that the will or vital principle must act physically in order to have any impact on the body. The quality of Boussinesq's theory was that according to this theory, the directive principle did not have to exert any force at all to act on the body.

This aspect of Boussinesq's theory has not always been recognized. For example, in his famous lecture "Die sieben Welträthsel", Du Bois-Reymond (1880) criticized Boussinesq's free will theory because he thought that in this theory the physical force needed for mind to act on matter could not actually become zero, although it could become infinitely small. In recent times, Boussinesq has been misunderstood in a similar way by Hacking (1983) and Deakin (1988), who

[5]Boussinesq (1842-1929) was at that time professor of mathematics at the Faculté des Sciences in Lille (Nye, 1976). He is mainly known for his work in hydrodynamics, heat and light.

[6]About Boussinesq's free will theory in its historical context, see Nye (1976), Hacking (1983) and Porter (1986). See also Israel (1992).

mention Boussinesq's theory as an anticipation of modern catastrophe theory because they think that it is about systems in which the exertion of an infinitely small amount of force can have large effects.

But Boussinesq is very explicit about the fact that this is not the idea behind his theory. About the directive principle, he writes:

> This directive principle, very different from the vital principle of the ancient schools, would not have at its service any mechanical force which would enable it to struggle against the forces that it encountered in the world; it would only profit from their insufficiency in the singular cases considered here, to influence the course of phenomena. (Boussinesq, 1879a, pp. 40-41).

Many contemporaries of Boussinesq did not understand the mathematical aspects of his theory very well. Boussinesq's discussion of singular solutions in physics is very extensive and difficult to read, and he seems to have favoured complicated mathematical derivations over the simple examples given by Duhamel and Poisson. It is possibly for this reason that, while Boussinesq's theory has become famous as an example of the concern with free will around the 1870's, the physical and mathematical aspects of his theory have received little attention.

Boussinesq discussed different types of systems in which the equations of motion could have a singular solution. The following are the two most important ones:

(I) The first kind he discussed was the motion of a point particle moving along a perfectly smooth dome-shaped curve and subjected to gravity (Boussinesq, 1879a, p. 67). The equation of motion for this system is $\frac{d^2r}{dt^2} = g\frac{dh(r)}{dr}$ with r the path along the dome and $h(r)$ the decrease in height from the top of the dome. This is similar to the Norton dome, but where $h(r)$ for the Norton dome is given by

$$h(r) = \frac{2}{3g}r^{\frac{3}{2}},$$

Boussinesq puts in a more general equation, namely:

$$h(r) = \frac{1}{2g}K^2\left(\log\frac{a}{r}\right)^{2k}r^{2m}$$

in which K, k, m are constants and a is a constant line. Norton's dome is a special case of Boussinesq's dome; Boussinesq's dome equals it for $K^2 = \frac{4}{3}$, $a = er$ and $m = \frac{3}{4}$. This equation for $h(r)$ leads to the following equation of motion:

$$\frac{d^2r}{dt^2} = K^2\left[m\left(\log\frac{a}{r}\right)^{2k} - k\left(\log\frac{a}{r}\right)^{2k-1}\right]r^{2m-1}.$$

Instead of solving this equation to show that it has a non-unique solution, Boussinesq puts in the conditions under which singular points can occur to derive the values of the constants k and m for which there should be a non-unique solution.

First, if the top of the dome lies in the origin at $r = 0$, one needs to have $\frac{dh(r)}{dr} = 0$ at $r = 0$ in order for a singularity to occur (thus the dome has to be horizontal at the summit: in that case the component of the gravitational force along the surface is zero at this point, so that the gravitational force does not put the particle into motion). Since

$$\frac{dh(r)}{dr} = \frac{1}{g}K^2 \left[m \left(\log \frac{a}{r} \right)^{2k} - k \left(\log \frac{a}{r} \right)^{2k-1} \right] r^{2m-1}$$

the latter needs to become zero as r goes to zero. Now, in the limit where r goes to zero, $\frac{dh(r)}{dr} = 0$ if $m > \frac{1}{2}$, and $\frac{dh(r)}{dr} = \infty$ if $m < \frac{1}{2}$. For $m = \frac{1}{2}$, $\frac{dh(r)}{dr}$ goes to zero in case $k < 0$. Thus, in order for a singularity to occur, one needs either $m > \frac{1}{2}$ or $m = \frac{1}{2}$ and $k < 0$.

Second, Boussinesq demands that the particle can arrive at the summit of the dome within a finite time and can slide off within a finite time, so that there is a non-unique time evolution within a finite time. Boussinesq derives from the equation of motion $\frac{d^2r}{dt^2} = g\frac{dh(r)}{dr}$ that

$$\frac{dr}{dt} = \pm\sqrt{2gh(r) + v_0^2}.$$

Taking $v_0 = 0$ and filling in the above equation for $h(r)$ then gives

$$\frac{dr}{dt} = -\sqrt{K^2 \left(\log \frac{a}{r} \right)^{2k} r^{2m}}$$

thus

$$dt = \frac{-r^{-m}dr}{K(log\frac{a}{r})^k}$$

Boussinesq then integrates both sides of the equation and demands that $\int dt$ is finite when r goes from 0 to r'; in this way he is able to derive that $m \leq 1$, and that in the case in which $m = 1$ one has $k \geq 1$.

In this way, Boussinesq argued that there were singular points for $\frac{1}{2} < m < 1$, and possibly also for $m = \frac{1}{2}$ and $m = 1$, depending on the value of k. Norton's dome, having $m = \frac{3}{4}$, falls within this range.

Boussinesq did not directly demonstrate that the equation of motion had more than one solution for these values of m and k. The fact that such a direct demonstration is lacking is a major weakness of his approach, which stems from the fact that he tried to use an equation for the surface of the dome that was as general as possible; this made the mathematics much more complicated than needed, and as a result he could only use indirect means to show that his equation exhibited singularities. What he did show was that for these values of m and k, the second derivative of $h(r)$ is infinite at $r = 0$, and that this in general leads to there being no unique solution (Boussinesq, 1879a, p. 162-167). This is a condition related to Lipschitz continuity: if the second derivative of $h(r)$ is infinite at $r = 0$ then $h(r)$ is not Lipschitz continuous.

In this way, Boussinesq argues that there can be maxima of the curve for which, if the particle lies still at such a point, it can start to move at an undetermined time. But Boussinesq argued that the system described by these equations was not one that could occur in reality: it is an unrealistic situation because it depends on the assumptions that there is no friction and that the weight of the particle does not cause a deformation in the surface of the dome, which in reality is always the case. He argued that therefore, the example of the dome "does not retain another interest, from the point of view of the role of singular integrals in mechanics, than to provide the mind with a simple geometrical representation of an entire caregory of these integrals, and a clear picture of the kind of indetermination that they display" (Boussinesq, 1879a, p. 83).

(II) Boussinesq also argued that singularities could occur in a system of two atoms moving freely in space and acting on each other through some particular force. Boussinesq shows that this situation can be made mathematically equivalent to the one of a particle moving along a dome-shaped curve. It is possible to parameterize the coordinates of the atoms in such a way that atom A is regarded as being fixed in the origin with atom B turning around it; then, for a particular force acting between them, there may be a circular orbit around A at a particular distance from A, in which atom B can turn for an indeterminate time before moving to another distance at an indeterminate moment (Boussinesq, 1879a, p. 97).

Malament (2008) has argued that the dome is more convincing as an example of an indeterministic system in classical physics than a special force function between atoms, but Boussinesq thought that a special force function between atoms was a stronger example. According to Boussinesq, the dome was not a possible physical system, while the kind of force function between atoms that leads to a singularity could occur in reality (Boussinesq, 1879a, p. 99). According to Boussinesq, the action between atoms goes from repulsive to attractive to repulsive again as the distance between them increases. This idea was probably derived from the atomic theory of Boscovich, which dates from 1758 and was one of several atomic theories that were in use during the nineteenth century (see Brush, 1976, vol. 1, p. 277). At the distance where the action goes from attractive to repulsive, there is a point of unstable equilibrium, and Boussinesq argued that it is at such points that singular orbits are to be expected (Boussinesq, 1879a, p. 101).

However, a singularity will only arise if certain parameters have a specific value, and the slightest difference destroys the effect.[7] Furthermore, for the motion of atom B to become undetermined it has to arrive at the singular orbit with its angular velocity relative to atom A being exactly zero. Boussinesq acknowledged that therefore, the probability for singular solutions to occur spontaneously was extremely small (Boussinesq, 1879a, p. 109, 113). This was a problem for him, for the fact that he used singular solutions of differential equations to explain life and free will implied that these singular solutions had

[7]Boussinesq emphasizes that singularies only arise for "les valeurs très-spéciales de A ou par suite de c^2", where $c = r^2 \frac{d\theta}{dt}$ (Boussinesq, 1879a, p. 104).

to be involved in each free act and played an essential role in all organisms.[8] To account for this regular occurrence of singular solutions, Boussinesq developed extensive arguments, arguing that singular solutions were more likely to occur in living organisms due to specially prepared circumstances such as chemical instability, and that these specially prepared circumstances were transmittable through heredity (Boussinesq, 1879a, p. 140).

Thus, Boussinesq thought that singular solutions to the equations of physics could occur in reality, and did in fact occur all the time. But the question whether this implied indeterminism is not that easy to answer because Boussinesq seems to use the word determinism with different meanings.

When Boussinesq defines mechanical determinism, he defines it as the law that for each mechanical system, there are differential equations determining the accelerations of all particles at a certain moment as a function of the positions of the particles. He adds:

> This great law is the expression of *mechanical determinism* [...] It gives for each moment, as a function of the current static state, the second derivative of this same state with respect to time, and only in this restricted manner it connects the future to the present and the past. (Boussinesq, 1879a, p. 46).

This definition is essentially different from DEM, because Boussinesq doesn't specify that these equations must have unique solutions, so that the future positions of the particles are uniquely determined. Because the equations of mechanics are never violated in Lipschitz-indeterministic systems, Boussinesq could argue that

> ... the true mechanical determinism is not limited by anything: it is never in conflict with physiological determinism or with free will. These two superior principles do not prevent it in any case from fully performing its role, which is to regulate at each instant the accelerations of all the material points existing in the universe, according to the laws of composition of their reciprocal actions equal to certain functions of their distances. (Boussinesq, 1879a, p. 59).

One can thus argue that Boussinesq's conception of mechanical determinism differed from DEM and that therefore he did not regard Lipschitz-indeterministic systems as cases in which there was indeterminism. But even though Lipschitz-indeterministic systems involve no violation of mechanical determinism in Boussinesq's strict sense, there is a failure of physical determinism in the sense that the laws of physics do not determine the future states of the system. In fact, elsewhere in the text Boussinesq speaks about determinism in the latter, more familiar sense, and writes that in cases in which there are singular solutions

[8] Fletcher (2012) mentions that Boussinesq used systems similar to the Norton dome as a basis for a theory about free will, and adds in a footnote: "He did not, however, investigate how ubiquitous such systems might be". However, Boussinesq put much effort in investigating exactly this issue, and had detailed ideas about the probability with which such systems can be expected to appear in reality.

there is an indetermination of the position of particles (Boussinesq, 1879a, p. 40).

In these cases in which the equations of physics leave the positions of particles undetermined, however, there is a 'directive principle' that determines what will happen. In purely physiological processes in which no free will is involved, this directive principle acts in a deterministic manner so that although there is no physical determinism, there is still physiological determinism (Boussinesq, 1879a, p. 46). Lipschitz-indeterminism thus does not necessarily correspond with actual indeterminism in the world: it is only when free will is involved that there is actual indeterminism. Whether or not there is determinism depends for Boussinesq not only on the equations of physics but also on the metaphysics of life and free will.

2.6 Bertrand and Boussinesq on the relation between theory and reality

Boussinesq's theory only seems to make sense if the differential equations which make up our laws of nature are rigorously and universally valid. The famous mathematician Joseph Bertrand[9] wrote in a criticism of Boussinesq's theory that Boussinesq was "fearlessly confident in the formulas" (Bertrand, 1878). Bertrand argued that if the laws of mechanics allow for indeterminism there must simply be something wrong with these laws: "Since the laws as expressed by the equations allow for two different paths, while the physical laws can only bring about one, they are, necessarily, distinct."

According to Bertrand, it was evident that there could be no plain indeterminism: something had to determine what would happen. And he could not agree with Boussinesq's analysis according to which a non-physical directive principle was involved at singular points to decide what would happen. Bertrand equated this directive principle with the mind or soul (l'âme) and it was incomprehensible to him how the mind could act on the body without exerting a force. Even though according to the laws of mechanics, the force required for the mind to act on the body at singular points was zero, Bertrand argued that the fact that the mind had an effect on the body implied that some force had to be involved; apparently, he was convinced that there could be no causation without forces. And he thought that the idea that the mind could exert a physical force was highly problematic. But if the mind could not determine what would happen at singular points, there would be an incomprehensible lack of determination:

> In fact, imagine the point to be placed under the indicated conditions, it approaches the critical position, two routes are possible, the differential equations do not prescribe anything, the directive principle abstains,

[9]Bertrand (1822-1900) had been a pupil of Duhamel, and was at this time professor in mathematics at the Collège du France and the École Polytechnique, and secretary of the Académie des Sciences. See O'Connor and Robertson (2005b).

> meanwhile the time is pressing, what will happen?

The indeterminism involved in singular solutions could not be explained; hence something had to be wrong with the equations. Bertrand made clear that this was not contrary to his expectations: "it is neither demonstrated, nor demonstrable, nor probable, nor possible and thus not true that the equations of dynamics objectively have the absolute rigour of Euclid's theorems."
Bertrand argued that what was wrong about our mathematical laws of motion was that they were an abstraction of what happens in reality; this abstraction entered through the assumption that all quantities in physics are perfectly continuous and differentiable. According to Bertrand, forces in fact vary in a discontinuous way, there are successive "impulsions" rather than a continuous force. If one takes this into account, "the allowed laws will be altered without becoming false or uncertain, precisely in the way a circle is altered when it is replaced by a regular polygon with a hundred million sides".[10] Thus, there is a very small difference between what the laws of mechanics say and what really happens in nature, and Bertrand argued that when this small difference is taken into account, it can be shown that the Lipschitz-indeterminism that the equations exhibit is not reflected in reality.

Whereas according to DEM, the differential equations which are the equations of motion for a certain system determine its future states, Bertrand argued that these differential equations are only an idealization, so they do not *exactly* determine what will happen in reality. For Bertrand, this was enough to argue that there was no indeterminism in reality. He had a strong conviction that there could be no indeterminism, and this conviction was apparently not based on the equations of physics but rather had a metaphysical motivation. No matter what the equations said, it was incomprehensible to Bertrand that there could be cases in which there would be nothing to determine what would happen next.

In a reaction to Bertrand's criticism, Boussinesq (1879b) argued that there was no reason to assume that there were such "nuances mystérieuses" making a difference between the abstract, mathematical laws of nature and the 'true' laws of nature. However, this is in contradiction with his original text (1879a), in which Boussinesq also described the laws of mechanics as an idealization of physical reality. Boussinesq's reasoning differed from that of Bertrand but was in the end no less fatal to his own theory.

Boussinesq had argued that physicists can only work with a geometrical representation of phenomena, with atoms as point particles, a continuous space and time and abstract quantities (Boussinesq, 1879a, p. 43). In general this works well, but in certain cases we expect that the geometrical representation that we have of phenomena does not completely conform to reality. In particular:

> The engineer and the physicist are inclined to refuse the infinite divisibility of the abstract magnitude of things, and thus not to attach any importance

[10]That this can make indeterminism disappear is demonstrated in Zinkernagel (2010) who shows that the difference equation for the dome, contrary to the differential equation, does have a unique solution.

> and not even any objective reality to quantities which are below a certain degree of smallness, however, without being able to fix the point where the concrete ends an where the pure and abstract begins. (Boussinesq, 1879a, pp. 43-44).

It is clear that we can measure quantities in physics only to a certain degree of precision, so that very small quantities are beyond our epistemic reach. But Boussinesq's statement also has an ontological component. It is unknown to us whether certain quantities take continuous values, and therefore, there might in reality not be a distinction between two values which are mathematically distinct but very close together (Boussinesq, 1879a, p. 107-8). Also the small differences between the mathematical laws of nature and reality which Bertrand invokes to make singularities disappear are according to Boussinesq too small to be physically significant (Boussinesq, 1879b, footnote p. 66).

Bertrand and Boussinesq both described a gap between mathematics and reality, but whereas Bertrand argued that because of this gap, the singularities in the equations do not correspond to indeterminism in reality, Boussinesq argued that this gap actually increased the probability of there being points in reality at which the future evolution of the system is undetermined. In this way, he hoped to explain why singular solutions play an important role in physical reality, although they have an infinitely small probability according to the mathematics. Boussinesq's argument is as follows: the occurrence of singularities in a physical system requires special circumstances in which certain parameters have a definite value, and according to the equations, circumstances that differ only slightly from the required ones do not lead to Lipschitz-indeterminism. However:

> ... nature does not distinguish between the circumstances in question and circumstances which only differ from these from an abstract point of view, that is, little enough, *analytically*, to be qualified as physically equal, or for the application of supplementary, fictional forces, extremely small for the geometer but in reality devoid of any objective value, to render the bifurcations *mathematically* possible. (Boussinesq, 1879a, p. 107).

Boussinesq argued that therefore, one could say that there was actually an interval of initial conditions rather than one definite initial condition leading to a singularity. Through this argument the probability of singularities could be made finite (though still small). However, there is an important problem with this argument. We've seen in section (5) that the advantage that Boussinesq's theory had over related free will theories of his period was (1) that it involved true physical indeterminism instead of mere instability, and (2) that therefore, the force needed for the will to influence a physical system could be exactly zero, instead of merely very small. The problem is that it becomes difficult to maintain these distinctions in light of the preceding, which seems to make singular points at which there is true indeterminism physically equivalent to highly unstable, but non-singular points, and a very small force physically equivalent to no force at all.

In a footnote in his reaction to Bertrand (Boussinesq, 1879b), Boussinesq admitted this consequence of his ideas. He argued that because his free will theory was empirically equivalent to related theories based on instability and because the distinction between them was so small that it might not correspond to anything in reality, there was little reason to quarrel. However, this implies that by describing the mathematical laws of nature as idealizations of physical reality, Boussinesq had caused his theory to lose the advantage it had over other theories. Although their conclusions were different, for both Bertrand and Boussinesq, there was no one to one correspondence between singular points in the equations of physics and indeterminism in the world. Therefore, whether or not DEM failed was for them not the end of the story regarding the question whether there could be indeterminism in the world; also the relation between the mathematical equations of physics and physical reality came in.

2.7 The contemporary Norton dome discussion

For the nineteenth century authors that have been discussed in the previous sections, whether or not DEM failed was only part of the story of whether there was indeterminism in reality. However, in the contemporary discussion about the Norton dome, almost all authors take for granted that if the equation of motion has more than one solution for given initial conditions, this means that the system that is described by these equations is indeterministic (see for example Norton 2008; Malament 2008; Wilson 2009; Korolev (unpublished)). This means that they accept DEM as a definition of determinism. If the Norton dome is a valid example of a system in classical mechanics then it follows from this definition that classical mechanics is not deterministic. The only way to avoid this conclusion is to argue that the Norton dome is *not* a valid example of a system in classical mechanics, and a number of authors have done exactly that, mainly pointing at idealizations that they think are inadmissible.

A notable exception, however, is Zinkernagel (2010), who has argued against the fact that the Norton dome involves indeterminism on the basis of a causal interpretation of Newton's laws (see section 4). He argues that Newton's first law should be interpreted as saying that a body in uniform motion remains in uniform motion unless it is caused by a force to accelerate: thus, there should always be a first cause for motion. With this interpretation of Newton's first law one can show that the point particle on top of the dome will stay at its place, since there is no force that can cause it to slide off; thus, there is no indeterminism. At the same time, the equation of motion for the Norton dome does not have a unique solution; apparently, according to Zinkernagel this does not imply that there is indeterminism and he thus does not accept DEM. He points out that while the differential equation of motion does not have a unique solution, the difference equation does, and that this is the equation that is relevant in this context. It is no coincidence that his ideas, which have much more in common with the nineteenth century ideas on the topic (especially those of Poisson) than with those of other contemporary authors, are based on an interpretation of Newton's work rather than on modern formulations of classical mechanics.

Other contemporary authors writing on Lipschitz-indeterminism do decide whether classical physics is deterministic on the basis of whether the differential equations of motion always have a unique solution. Therefore, the focus of the debate is on the properties of the theory of classical mechanics and on what is admitted in the theory; the relation between theory and reality does hardly come in.

In the contemporary discussion about the Norton dome, idealizations play an essential role. Whereas Boussinesq and Bertrand were concerned with the idealizations involved in differential equations in general, the contemporary discussion is more concerned with certain specific idealizations on which the indeterminism in Norton's dome depends. For example, the dome has to be perfectly rigid (so that it does not deform under pressure), the particle has to be a point particle and there has to be zero friction. Recently, Korolev (unpublished) has argued that the indeterminism in the Norton dome is no more than an artefact of such idealizations. However, Norton (2008) points out that virtually all textbook examples in Newtonian physics depend on such idealizations, so it is problematic to rule out all examples involving such idealizations. This also holds for the property of the Norton dome that Malament (2008) has pointed out as essential for the indeterminism involved in the dome, namely that at the summit of the dome, there is a discontinuity in its second derivative (it is C^1 but not C^2). This too occurs often in textbook examples; in fact, many classical mechanics textbook examples involve sharp edges that are singular in a much stronger sense (they are not even C^1).

While Korolev has argued that the dome can be dismissed because it is 'unphysical', or in other words impossible in reality, Norton (2008) argues that this argument misses the goal of the Norton dome: "the dome is intended to explore the properties of Newtonian theory, not the actual world." The question then becomes which idealizations can be admitted in the theory of classical mechanics and which not. Malament (2008) and Wilson (2009) have argued that whether or not classical mechanics is deterministic depends on which choices we make on what to allow in classical theories: is it allowed to make up forces? Which idealizations are admissible in the theory and which not? Can there be discontinuity or singularity in the defining constraints of a constraint system?

This attitude, which focuses on the properties of the theory rather than on reality, makes sense because the discussion about Lipschitz-indeterminism is a discussion within classical mechanics, which is by now falsified by quantum mechanics. Specifically, it is clear that in the light of quantum mechanics the Norton dome is not possible, as Norton (2008) points out: for one thing, it is not even possible to place a point particle exactly at the top of a dome and exactly at rest. The introduction of quantum mechanics can thus explain why the contemporary discussion is less concerned with whether there is Lipschitz-indeterminism in reality and more with what it shows about the properties of the physical theory.

Conclusion

We have seen that whether non-uniqueness of solutions to the equations of physics corresponds to indeterminism in a physical system depends on one's notion of determinism. In the contemporary literature on determinism in classical physics, determinism is usually defined as the statement that for each system, there are equations of motion which have a unique solution for given initial conditions. But whereas this is now commonly accepted as a definition of determinism, for the nineteenth century authors that I have discussed,whether the equations of motion had unique solutions did not decide whether there was determinism. Determinism was a presupposition, an a priori truth that had to be upheld even when it turned out that the equations could fail to have unique solutions (except for Boussinesq, who made an exception for cases which involve free will and argued that in these cases there is genuine indeterminism). Determinism held for physical reality, and the relation between the equations of physics and physical reality could be non-trivial. The reason why the possibility of Lipschitz-indeterminism in physics has been forgotten and had to be rediscovered to become the subject of a debate in recent years is that it presently has a completely different relevance than it had in the nineteenth century, when common conceptions of determinism in physics were essentially different.

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Paper 3 Vital instability: life and free will in physics and physiology, 1860-1880[1]

During the period 1860-1880, a number of physicists and mathematicians, including Maxwell, Stewart, Cournot and Boussinesq, used theories formulated in terms of physics to argue that the mind, the soul or a vital principle could have an impact on the body. This paper shows that what was primarily at stake for these authors was a concern about the irreducibility of life and the mind to physics, and that their theories can be regarded primarily as reactions to the law of conservation of energy, which was used among others by Helmholtz and Du Bois-Reymond as an argument against the possibility of vital and mental causes in physiology. In light of this development, Maxwell, Stewart, Cournot and Boussinesq showed that it was still possible to argue for the irreducibility of life and the mind to physics, through an appeal to instability or indeterminism in physics: if the body is an unstable or physically indeterministic system, an immaterial principle can act through triggering or directing motions in the body, without violating the laws of physics.

3.1 Introduction

In 'Nineteenth Century Cracks in the Concept of Determinism', Ian Hacking notes that around the 1870s, there was much anxiety about the issue of scientific determinism and

[1] This paper is forthcoming in *Annals of Science*.

the possibility of free will (Hacking, 1983).[2] In this period, physicists such as Maxwell and Boussinesq made attempts to save free will, and offered explanations in terms of physics of how the will could act in the body, to show that freedom of the will was compatible with physics. Hacking describes this period as a 'silly season in the philosophies of freedom and necessity', and argues that the prevalence of strange ideas about determinism in this period must mean that the very concept of determinism was under pressure: 'When great minds took to a new but crazy idea, we may suspect their very thinking about a concept was undergoing a lot of stress. I contend that the stress in the concept of determinism was as widespread as could be' (Hacking, 1983, p. 465).[3] However, Hacking does not explain what exactly this stress might have consisted in, or what caused it.

In particular, Hacking's account leaves unexplained why it was during this period, roughly 1860–1880, that these explanations of free will in terms of physics appeared. It is not the case that the issue of determinism in physics was especially pressing during this period. Laplace's well-known statement of determinism already dated from half a century earlier, in *Essai philosophique sur les probabilités* (1814), and in fact, Laplacian determinism goes back even further: Laplace had already made a similar claim in 1773, and various earlier authors in eighteenth-century France had argued for determinism in terms very similar to those of Laplace (Van Strien, 2014b). Whereas Laplacian determinism is specifically associated with mechanics,[4] in the course of the nineteenth century there was a rise of other domains of physics such as thermodynamics and electromagnetism; this development had the effect of weakening the idea that all of physics was reducible to the mechanics of point particles and force laws, and if anything, this made it less obvious that all of physics was deterministic. It is thus unclear why determinism would suddenly become such a big issue around 1860–1880.

A number of authors have turned to the theological, moral and cultural contexts to explain the background of these debates about the will.[5] Illuminating though this focus on theological, moral, and cultural contexts may be, there is more to be said about the

[2] See also Hacking (1990), p. 155ff.

[3] Moreover, Hacking shows that it was in fact only during this period that the word 'determinism' received its current meaning, pointing to Renouvier and Du Bois-Reymond as early users of the term. 'Somewhere, let us say between 1854 and 1872, the concept of determinism acquired its modern sense in all the European languages'. (Hacking, 1983, p. 460). Before this period, the central term in debates about science and free will was 'necessity', which played a role that was to some degree analogous to that of the later term 'determinism'. See Harris (2005).

[4] In fact, Laplace's determinism was not a direct consequence of his mechanics: see Van Strien (2014b). However, it was nevertheless associated with mechanics, and there was a natural expectation that in mechanics, the equations of motion generally have unique solutions for given initial conditions so that there is determinism.

[5] See Stanley (2008); Cat (2012); Nye (1976); Smith (1998), p. 249.

role of scientific developments in the debate. Specifically, there was a relevant scientific development which deserves more attention, namely the law of energy conservation and its claimed applicability to physiology. Whereas a number of authors, such as Porter, Smith and Wise, and Harman, have pointed out that the law of conservation of energy played a role in the debate about science and free will in the period 1860-1880, they do not give it a central place, and do not discuss how exactly the law of energy conservation was used as an argument for reductionism in physiology and in how far the arguments for free will by Maxwell, Boussinesq and others can be understood as arguments against reductionism in physiology.[6]

In this paper, I discuss a set of related explanations of free will and life in terms of physics by James Clerk Maxwell, Balfour Stewart (and his co-authors Tait and Lockyer), Antoine Augustin Cournot and Joseph Boussinesq. They were all physicists and/ or mathematicians who published their ideas on life and the will between 1860 and 1880. I argue that what is fundamentally at stake for them is a resistance to the reducibility (in principle) of physiology to physics, and specifically to the claimed implications of the law of energy conservation for physiology. In particular, I show the following:

(1) Their explanations of life and free will can be understood as a reaction to reductionist tendencies in physiology. During this period, roughly 1860-1880 a number of scientists (notably Helmholtz and Du Bois-Reymond) claimed that the law of conservation of energy, as applied to physiology, showed that all physiological processes were completely determined by the laws of physics, and that there was no room for vital or mental agency in physiology. It was this development that the authors I discuss were reacting against.

(2) Their concern was not restricted to the possibility of free will, which they understood exclusively in terms of the possibility for an immaterial mind or soul to act on the body; they were equally concerned about the irreducibility of life to physics, and argued for an irreducible vital principle in physiology. They defended strictly dualist conceptions of life and free will. Specifically, the dualism they defended is a type of substance dualism, since it involves an immaterial entity (mind, soul, or vital principle) that intervenes in the body.

(3) They developed theories of life and free will in terms of mechanics, based on unstable and indeterministic mechanical systems. They argued that in unstable systems, vital and mental causes could intervene with only an extremely small violation of the law of conservation of energy; in indeterministic systems, this violation is reduced to zero. Thus, mechanics provided a resource for showing how there could be vital and mental causes, and for making the law of energy

[6] Porter (1986), p. 202; Smith and Wise (1989), p. 617; Harman (1998), p. 200.

conservation compatible with the intervention of immaterial agency in physiology.

(4) While they were concerned with reductionism in physiology, this did not necessarily imply that they were concerned with physiology being deterministic. In particular, Cournot and Boussinesq explicitly argued that physiology must be deterministic, but emphasized that this physiological determinism was irreducible to physical determinism.

There were others in the respective circles of Maxwell, Stewart, Cournot and Boussinesq with similar ideas, such as William Thomson (Lord Kelvin), Fleeming Jenkin, and Adhémar Barré de Saint-Venant[7]; however, I focus on Maxwell, Stewart, Cournot and Boussinesq, because their ideas are worked out in most detail. Whereas Maxwell was well acquainted with Stewart and Boussinesq was influenced by Cournot, for the rest there is little evidence for contact between them, although Maxwell was aware on the ideas of both Cournot and Boussinesq on the will.[8] Nevertheless, there are strong similarities between their ideas and they fit in the same context, and can therefore be better understood when considered together. The paper focuses on the scientific and metaphysical dimensions of their ideas, and does not discuss moral or religious dimensions, because these have been adequately treated by others (see footnote 5).

In section 2, I provide an introduction to the law of conservation of energy and its claimed implications for physiology; in sections 3 and 4, I discuss the attempts that were made by Maxwell, Stewart, Cournot and Boussinesq to make energy conservation compatible with the intervention of mental and vital causes, through an appeal to instability and indeterminism in physics. In section 5, I show how Cournot and Boussinesq, even though they rejected determinism in physics, did argue for determinism in physiology; and in section 6 I show that the irreducibility of life to physics was as much an issue as free will.

3.2 The law of conservation of energy in physiology

In this section I show how the law of conservation of energy was used as an argument for the reducibility of physiology to physics. From its earliest development, the law of conservation of energy was thought to apply to living beings and to have implications

[7] See Saint-Venant (1877), and Jenkin (1868). On Kelvin, see Smith and Wise (1989).

[8] He mentions them both in a letter to Galton in 1879. See Maxwell (1879a).

for the possibility of vital and mental causes, and was used as an argument against vitalism and mind-matter interaction and for reductionism in physiology.

The law of conservation of energy is a law of physics, but it did not purely originate in the context of physics: the law was famously derived by several scientists around the same time, in several contexts, mainly physics, engineering, and physiology. One of its main developers was Hermann von Helmholtz, who was a physician and a physiologist by training, and it was his work in physiology that led him to the development of the law of conservation of energy (Lenoir, 1982, p. 195ff; Elkana, 1974, p. 97ff). In a lecture in 1861, Helmholtz discusses the application of his law of conservation of force (which later became the law of conservation of energy) to organisms, and argues that an organism can very well be compared to a steam engine: both are machines where fuel or food and oxygen go in and mechanical work and heat come out (Helmholtz, 1861, p. 353; see also Cahan, 2012, p. 60).

Helmholtz writes that physiologists have often supposed that processes in living bodies are determined by a vital principle, which is a directive principle specific to living organisms that can produce changes in the body through suspending or releasing the physical forces in the body. He points out that such variation of the forces that act in the body is in conflict with the law of conservation of force, since this law implies that forces cannot vary independently of physical conditions. He concludes:

> There may be other agents acting in the living body, than those agents which act in the inorganic world; but those forces, as far as they cause chemical and mechanical influences in the body, must be quite of the same character as inorganic forces, in this at least, that their effects must be ruled by necessity, and must be always the same, when acting in the same conditions, and that there cannot exist any arbitrary choice in the direction of their actions (Helmholtz, 1861, p. 357).

It is not always easy to interpret Helmholtz, as he used the word 'force' ('*Kraft*') with different meanings that he did not keep strictly separated; sometimes, the word corresponds to our present notion of energy, sometimes it means Newtonian force, and sometimes it means natural power or agent (Heimann, 1974, p. 214).[9] In the above passage, he equates 'force' with 'agent', implying that all agents acting in the living body are subjected to the law of conservation of force. Helmholtz understood causation in terms of forces,[10] and he describes the law of conservation of force in causal terms:

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[9] Note that P. M. Heimann is the same author as P. M. Harman, referred to earlier; he changed his name from Heimann to Harman.

[10] 'Our desire to *comprehend* natural phenomena, in other words, to ascertain their *laws*, thus takes another form of expression - that is, we have to seek out the *forces* which are the *causes* of the phenomena'. Helmholtz (1869), p. 209. At the time, a widely-debated issue in philosophy of physics was whether forces have an

> We may express the meaning of the law of conservation of force by saying, that every force of nature when it effects any alteration, loses and exhausts its faculty to effect the same alteration a second time. But while, by every alteration in nature, that force which has been the cause of this alteration is exhausted, there is always another force which gains as much power of producing new alterations in nature as the first has lost. Although, therefore, it is the nature of all inorganic forces to become exhausted by their own working, the power of the whole system in which these alterations take place is neither exhausted nor increased in quantity, but only changed in form (Helmholtz, 1861, p. 347-48).

Thus, according to Helmholtz, the law of conservation of force implied that vital and mental agents cannot act independently of physical conditions. If there are any special vital or mental causes, or forces, they have to be subjected to the law of conservation of force; specifically, any such force has to be conservative.[11] This has the consequence that such a force cannot vary independently of physical conditions, and has to be of the same character as physical and chemical forces.

Helmholtz's main target is the possibility of vital forces: he is opposed to the view that there is a vital principle or vital force that is specific to the organic realm, directs processes within the organism, and can explain specific features of living organisms, such as their apparent teleology. He writes that the idea of such a vital principle was still popular in the early nineteenth century, but:

> The present generation, on the contrary, is hard at work to find out the real causes of the processes which go on in the living body. They do not suppose that there is any other difference between the chemical and the mechanical actions in the living body, and out of it, than can be explained by the more complicated circumstances and conditions under which these actions take place ; and we have seen that the law of the conservation of force legitimizes this supposition. This law, moreover, shows the way in which this fundamental question, which has excited so many theoretical speculations, can be really and completely solved by experiment (Helmholtz, 1861, p. 357).

Although Helmholtz's arguments are mainly aimed at vitalism, or the possibility of vital forces, they also imply that mental causes have to be subjected to the law of

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independent ontological status and should be seen as the efficient cause of motions, or whether force is only a relational or derived concept and is not more than the product of mass and acceleration. See Jammer (1957), p. 200-240.

[11] In fact, Helmholtz argued that all forces have to be central, which is a stronger requirement: central forces are conservative forces that are directed towards a point. Helmholtz argued that all occurrences were reducible to matter and central forces. See Helmholtz (1847), p. 5ff.

conservation of force. Yet, Helmholtz does argue that we have free will, but he thinks that free will cannot be comprehended scientifically (Helmholtz, 1861, p. 454).

Thus, Helmholtz used the law of conservation of energy as an argument against vital causes in physiology. This was a step towards reduction of physiology to physics. It is important to note that it was very difficult in practice to apply the law of conservation of energy to physiology, because it was often not possible to make accurate measurements of the relevant quantities, and it was not possible to completely verify experimentally that there was no intervention of vital causes in physiological processes. However, Helmholtz provided experimental support of the law of conservation of energy in physiology to some degree, and he felt confident in arguing that the law was rigorously valid in physiology, and that it excluded the intervention of vital causes.[12] The applicability of the law of conservation of energy to physiology may thus have been limited in the sense that its use in laboratory research in physiology was limited, but it was applicable in the sense that, given the fact that there was a broad range of support for the law in different scientific domains, there were good reasons to think that it applied to all natural processes, including physiological processes. The applicability of the law of conservation of energy in this sense was also acknowledged by others, for example Maxwell wrote in 1879:

> It would be rash to assert that any experiments on living beings have as yet been conducted with such precision as to account for every foot-pound of work done by an animal in terms of the diminution of the intrinsic energy of the body and its contents; but the principle of conservation of energy has acquired so much scientific weight during the last twenty years that no physiologist would feel any confidence in an experiment which shewd a considerable difference between the work done by an animal and the balance of the amount of energy received and spent (Maxwell, 1879b).

Helmholtz was part of the famous physiology laboratory of Johannes Müller, where organic processes were as much as possible explained in physical and chemical terms. Whereas in the work of Johannes Müller himself, there is a mild vitalist element, some of the younger members of the laboratory, among whom was Helmholtz, completely rejected vitalism, and thought that physiology could be completely reduced to physics and chemistry (Lenoir, 1982, p. 195-96). Another member of this group who explicitly used the law of conservation of energy to argue against mental causes and free will was the well-known physiologist Emil Du Bois-Reymond. I will discuss his argument too because it was quite influential: it appeared in a lecture that Du Bois-Reymond gave in

[12] See Elkana (1974), p. 97-111. See also Lenoir (1982), p. 200-214, for Helmholtz' work on muscle contraction and heat production in the body.

1872, titled 'The limits of our knowledge of nature', which became well-known and triggered a large debate about the proper aims of science. Hacking (1983) argues that this lecture contains one of the first explicit arguments for scientific determinism.

On the basis of the law of conservation of energy, Du Bois-Reymond argues that the mind cannot intervene in the physical world: in Du Bois-Reymond's view, the only way to effect a change in nature is to exert a force; thus, if the mind were to intervene in the physical world, it would have to exert a force, and this would disturb the amount of energy in the physical world. Without the possibility of exerting forces, the mind has no causal efficacy, and it is therefore unintelligible:

> In the physical world, no more and no less can happen than this law [of conservation of energy] determines; the mechanical cause passes completely into the mechanical effect. Thus, the mental processes that are associated with the material processes in the brain are for our understanding devoid of a sufficient reason. They lie beyond the law of causation, and are therefore unintelligible, like a *perpetuum mobile.* (Du Bois-Reymond, 1872, p. 41).[13]

Du Bois-Reymond argues that anything that cannot be reduced to motions of atoms is fundamentally unknowable and beyond the realm of causality. The fact that the mind cannot intervene in the physical world, as the law of energy conservation shows, thus makes it non-causal and therefore places it outside of the domain of science.

One way to make mind-matter interaction compatible with the law of conservation of energy would be to argue that there is a 'mental energy' which is convertible into other types of energy and subjected to the law of conservation of energy, so that the mind is included in the system within which energy is conserved. In this way, there can be mental forces (or, by a similar argument, vital forces), that are conservative and thus do not violate the law of conservation of energy. This is an approach that was taken up at the time, for example by Bain (see Bain, 1867). However, it placed strong restrictions on mind-matter interaction and vitalism: it implied that the actions of the mind or the vital principle were determined by the laws of physics, and that vital and mental forces could not vary independently of physical conditions. Thus, the vital and the mental domain would come under the dominion of the laws of physics and would lose their independence.[14]

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[13] 'Mehr als dies Gesetz bestimmt, kann in der Körperwelt nicht geschehen, auch nicht weniger; die mechanische Ursache geht rein auf in der mechanischen Wirkung. Die neben den materiellen Vorgängen im Gehirn einhergehenden geistigen Vorgänge entbehren also für unseren Verstand des zureichenden Grundes. Sie stehen ausserhalb des Causalgesetzes, und schon darum sind sie nicht zu verstehen, so wenig, wie ein Mobile perpetuum es wäre'.

[14] In addition, both Helmholtz and Du Bois-Reymond rejected this possibility because they thought that forces always had to be bound up with matter, and that matter and forces could not be understood separately: they

Thus, Helmholtz and Du Bois-Reymond used the law of conservation of energy as an argument against irregular vital and mental causes, and argued on the basis of this law that all occurrences in the living body were regulated by the laws of physics. In other words, they used the law of conservation of energy as an argument for the completeness of physics, as Papineau defines it in his article "The rise of physicalism": 'All physical effects are fully determined by law by prior physical occurrences' (Papineau, 2001, p. 8).

In his article, Papineau points to the development of the law of conservation of energy as an important step in the acceptance of the idea that physics is complete. He argues that the law of conservation of energy excludes indeterministic forces (Papineau, 2001, p. 25-26). This fits with the use that Helmholtz and Du Bois-Reymond made of the law of conservation of energy. However, as we will see in the next sections, an argument was available and used against this implication; irreducible vital and mental causes could be made compatible with energy conservation if one allowed for unstable and indeterministic mechanical systems.

3.3 Causation without forces

Whereas Helmholtz and Du Bois-Reymond used the law of conservation of energy as an argument against vital and mental causes and for reductionism in physiology, this argument met with resistance from a group of physicists and mathematicians including Maxwell, Stewart, Cournot and Boussinesq. They thought that the domain of physics should be restricted and that the living realm should have some degree of autonomy from the laws of physics. They sought ways to argue for the possibility of genuine vital and/or mental causes which were not determined by the laws of physics, but at the same time did not violate the law of conservation of energy. In this section I show that they could make this argument through an appeal to unstable mechanical systems, on which an immaterial principle can act without exerting a physical force.

Maxwell was well acquainted with Helmholtz (Cahan, 2012), and he discussed Helmholtz's ideas on energy conservation in a letter to Campbell in 1862 (this was probably in reaction to Helmholtz's lecture from 1861, cited above[15]):

both argued that all natural phenomena were reducible to atoms and central forces connected with these atoms. See Helmholtz (1847), p. 5ff; Du Bois-Reymond (1848), p. xxxvi.

[15] According to Harman (1998), p. 202, Maxwell was almost certainly present at Helmholtz's lecture; the letter was written shortly after its publication.

> We see also that the soul is not the direct moving force of the body. If it were, it would only last till it had done a certain amount of work, like the spring of a watch, which works till it is run down. The soul is not the mere mover. Food is the mover, and perishes in the using, which the soul does not. There is action and reaction between body and soul, but it is not of a kind in which energy passes from the one to the other, - as when a man pulls the trigger it is the gunpowder that projects the bullet, or when a pointsman shunts a train it is the rails that bear the trust (Maxwell, 1862).

Harman writes that Maxwell agreed with Helmholtz's anti-vitalism, referring to exactly this paragraph (Harman, 1998, p. 203). Indeed, this statement goes against the idea that there are vital forces which act as the 'motive power of the body' – this would be a clear violation of the law of conservation of energy. However, it seems from the above passage that, according to Maxwell, there are other ways in which a soul may have an effect on the body: it can cause or direct motions without being the motive power. The same idea comes up in an essay in 1873, in which Maxwell writes:

> As the doctrine of the conservation of matter gave a definiteness to statements regarding the immateriality of the soul, so the doctrine of the conservation of energy, when applied to living beings, leads to the conclusion that the soul of an animal is not, like the mainspring of a watch, the motive power of the body, but that its function is rather that of a steersman of a vessel – not to produce, but to regulate and direct the animal powers (Maxwell, 1873, p. 817).

The basic idea we find in Maxwell is that the soul is not a moving force but that it can trigger or direct motions. This possibility of triggering or directing motions depends on instability in the system that is acted upon: there must be certain unstable or singular points in the system 'at which a strictly infinitesimal force may determine the course of the system to any one of a finite number of equally possible paths' (Maxwell, 1879b). In the analogies of pulling a trigger or switching railway tracks, it is the mechanism of the gun or the points lever which makes it possible to act on a large scale through exerting only a very small force.

The analogies Maxwell employs are intended to show that not all change in nature is effected through a direct exertion of energy (pushing and pulling) but that it is also possible to act through triggering or directing motions. Because this involves only a tiny, and possibly infinitely small, amount of energy, it is a big step towards making vital and mental agency compatible with energy conservation. Maxwell argued that the violation could be small enough to be beyond our epistemic reach. Therefore, he claimed, science cannot exclude the possibility of such a directive principle:

> Every existence above a certain rank has its singular points: the higher the rank, the more of them. At these points, influences whose physical magnitude is too

> small to be taken account of by any finite being, may produce results of the greatest importance. (Maxwell, 1873, p. 822).

Here, with 'singular point', Maxwell means a point at which there is strong instability; systems in which there are singular points are unpredictable to such a degree that we can't exclude the possibility that an immaterial directive principle intervenes at such points.

The idea that processes can be caused by an agency acting on a small scale also comes up in Maxwell's work on statistical mechanics, in the form of "Maxwell's demon", a hypothetical entity who can cause entropy decrease by acting on individual molecules. The demon was invented by Maxwell in order to show that violations of the second law of thermodynamics (the law of increase of entropy) are possible in principle. The demon operates through moving a slide to open and close a hole in a wall, and it is assumed that this can be done without friction, so that the demon does not have to exert any force; whereas the complete absence of friction is not a realistic assumption, it approximates a situation in which there is a slide which can move with a very small amount of friction, and this can also be regarded as an unstable mechanical system (see Harman, 1998, p. 134ff).

The physicist Balfour Stewart used the term 'delicacy of construction': he argued that certain machines are delicately constructed, which means that only very little force is needed to operate them.[16] As an example of such a delicately-constructed machine, Stewart mentions a gun or rifle, which can go off through 'the expenditure of a very small amount of energy upon the trigger', and this can have a large effect: 'if well pointed, it may explode a magazine, – nay, even win an empire' (Stewart and Lockyer, 1868, p. 324). However, the gun will not go off spontaneously; the application of a small amount of 'directive energy' to the trigger is required. It is therefore a machine of '*finite* delicacy of construction' – unstable, but deterministic. We only get complete unpredictability in a combined system of man and gun:

> The rifle is delicately constructed, but not surpassingly so; but sportsman and rifle, together, form a machine of surpassing delicacy, ergo the sportsman himself is such a machine. We thus begin to perceive that a human being, or indeed an animal of any kind, is in truth a machine of a delicacy that is practically infinite, the condition or motions of which we are utterly unable to predict. (Stewart, 1875, p. 160-61).

[16] His ideas on the issue can be found mainly in Stewart (1875); Stewart and Lockyer (1868); Stewart and Tait (1875).

Thus, living beings are machines of infinite delicacy of construction. To argue that a living being is no more and no less than a delicately constructed machine would mean that life can be explained in mechanistic terms. But Stewart and his co-author Lockyer explicitly distinguish this materialist conception of life from the view that there is a 'principle in its essence distinct from matter' which can bring about effects by exerting an infinitely small amount of force on the delicate machine that is the organism (Stewart and Lockyer, 1868, p. 326). Stewart and Lockyer claim that no decision between these two options can be made, but at the same time clearly suggest that it is the second that is most attractive to them. Thus they suggest that the difference between animate and inanimate machines lies in the presence of an immaterial vital principle that operates the machine.

Stewart was not concerned with free will; rather, his concern had to do with life's being irreducible to physical processes. Stewart's ideas about a directive principle in living beings were part of a spiritual worldview based on physics. For him, the notion of 'delicacy of construction' and the possibility of directing motions without exerting a force opened up a way to argue that God could operate in the universe without violating the laws of nature. In this way, God could affect the physical world in a way similar to that in which the vital principle ('life') can operate in the body.[17] Also for Maxwell, there was a spiritual element in his ideas about life and the will, although it was less explicit.[18]

With the exception of the spiritual side, Stewart's and Maxwell's ideas about the irreducibility of life to physics are very similar to those of Antoine Augustin Cournot. Cournot was primarily a mathematician, working among others on analysis and probabilities, but he is mainly remembered for his work in mathematical economics and his philosophical writings.[19] To understand how the vital principle can act in a world that is ruled by the laws of physics and chemistry, Cournot proposes starting with the experience that we have of acting in the physical world. He pictures someone rowing a canoe, who has to use his own muscular force to overcome the resistance of the water, wind and stream and tide. But this is not the only way in which we can act: someone who has learned to sail can make use of the force of the wind to overcome the resistance of the water (Cournot, 1875, p. 100). To do this, he needs only a small amount of muscular work to set the sails, but this amount is massively disproportionate to the actual force that is needed to displace the ship. With the invention of steam ships, it became possible to set a ship in motion with even less muscular effort, and future

[17] This view was most explicitly articulated in *The Unseen Universe*, a book published in 1875 by Stewart and Tait of which the stated purpose was 'to endeavour to show that the presumed incompatibility of Science and Religion does not exist' (Stewart and Tait, 1875, p. vii).
[18] See Maxwell (1879b), his review of a book by Stewart and Tait.
[19] On his influence, see Martin (ed.) (2005).

developments may further reduce the required amount of muscular effort. Cournot argues that machines may be invented which can be operated with an infinitely small amount of force:

> Nothing prevents us from imagining (at least in theory) that the physical work that the currently most perfected machine always requires on the part of man, is borrowed from the blind forces of nature by means of an even more perfect machine, so as to infinitely reduce the part of physical work imposed on man, and to increase his power as the master and director of the natural forces. (Cournot, 1861, p. 369-70).[20]

In the same way that man operates a machine, the vital principle can operate the body, which is in fact a superior machine.

The basic principle of approaches such as those of Maxwell, Cournot and Stewart goes back to Descartes, who argued that the mind could change the direction of motion with the quantity of motion being conserved.[21] What is new about the ideas of Maxwell, Cournot and Stewart, however, is that they are developed explicitly as a reaction to the law of conservation of energy and its claimed implications. We can divide the analogies they employed into two groups. Some, such as Maxwell's railway pointsman and Cournot's sailing ship, are directly based on the possibility to direct motions or forces (Cournot, 1861, p. 371). But in the analogy of pulling the trigger of a gun, something else is going on: pulling the trigger causes a transformation of potential energy into kinetic energy. Thus, there are two types of unstable systems on which a change can be effected through exerting a very small force: systems in which a small force suffices to change the direction of motion, and systems in which a small force suffices to cause a transformation of energy.

The concept of energy in particular plays a central role in the ideas of the French physicist Saint-Venant, whose arguments for the possibility of an immaterial directive principle were closely related to those of Maxwell and Boussinesq, and were based

[20] 'Rien n'empêche de concevoir (en théorie du moins) que le travail physique qu'exige toujours, de la part de l'homme, la machine actuellement la plus perfectionnée, soit emprunté aux forces aveugles de la Nature à l'aide d'un mécanisme plus parfait encore, de manière à atténuer indéfiniment la part du travail physique imposé à l'homme, et à accroître sa puissance comme maître et directeur des forces naturelles.' (As an aside, Cournot remarks that, through modern technological developments and the emergence of factories, physical labour is more and more being replaced by the operation of machines, for which forces merely have to be directed.)

[21] Or at least that is the standard view; for a discussion and defense of this standard view, see McLaughlin (1993).

explicitly on the possibility of triggering energy transformations.[22] Saint-Venant argues that when we act in the world it is often through causing a transformation of energy:

> I transform, in the external world, potential energy into actual [kinetic] energy when I open the outlet of a water reservoir, when I pull the trigger of a loaded gun, when I let go of the trigger that releases a pile driver from several meters' height. These effects can be considerable; each of them can constitute a good or bad action, as abundantly spilled water can either fertilize or devastate an area, or as a gunshot can either rid us of a dangerous animal or turn a society upside down by hitting a highly-valued head. To produce these effects, however, takes no more than the barely perceptible effort of one of my fingers. (Saint-Venant, 1877, p. 420).[23]

This is, according to Saint-Venant, comparable to how the will acts in the brain. To cause a transformation of energy, a small force needs to be exerted, but 'this force may be infinitely diminished'.[24] And except for this small force needed to trigger the transformation, the total amount of energy remains constant in such a transformation of energy; thus, it is in almost complete accordance with the law of conservation of energy.

In conclusion: in reaction to the law of conservation of energy and its claimed implications for vitalism and free will, it was argued that a vital or mental agency could cause a transformation of energy or a change in the direction of motion by performing an extremely small switching or triggering act on a mechanical system. However, in the analogies provided, the force needed for pulling the switch or the trigger could be made very small or even infinitely small, but it did not become zero; so although this was a big step towards making vital and mental causes compatible with energy conservation, it did not succeed entirely, as a small violation of the law of conservation of energy was still needed.

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[22] Saint-Venant was also one of the earliest people to work on energy conservation; Darrigol (2001) has shown how Saint-Venant anticipated the law of energy conservation in the 1830s.

[23] 'Je change, hors de moi, de l'énergie potentielle en actuelle si j'ouvre la bonde d'un réservoir d'eau, si je presse la détente d'une arme chargée, si je lâche le déclic retenant élevé de plusieurs mètres un mouton à enfoncer les pieux. Ces effets peuvent être considérables; chacun d'eux peut constituer une bonne ou une mauvaise action, car l'eau abondamment répandue peut ou fertiliser ou dévaster un canton, le coup de feu peut, ou le débarrasser d'une bête nuisible, ou bouleverser la société en frappant une tête précieuse. Il n'a fallu pourtant, pour les produire, que l'effort à peine sensible d'un de mes doigts'.

[24] 'cette force peut être indéfiniment atténuée'.

3.4 The appeal to indeterminism

As we've seen, there was an obvious problem with the attempt to save the possibility of vital and mental causes through an appeal to unstable mechanical systems: in the mechanical systems that we are familiar with, such as a gun or a steam ship, it is possible to cause changes through exerting a very small force, but this does not explain how it is possible to cause changes without exerting any force at all.

Both Cournot and Stewart argued that while man-made machines such as guns always require some triggering work in order to operate them, the case may be different for the body: the body could be regarded as an infinitely perfected machine (or, in the words of Stewart and Lockyer (1868, p. 325-26), a machine of 'infinite delicacy of construction'). In *Matérialisme, Vitalisme, Rationalisme* (1875), Cournot argues that while man can invent sailing boats and steam engines, nature is a far better inventor, and it is conceivable that nature might be able to diminish the work needed further, to the point of 'suppressing this auxiliary or additional expense of mechanical force that we can only reduce'.[25] Thus, it may be possible for a vital principle to act in the body because the body is a perfect machine, whereas man-made machines are always imperfect. However, it remained unclear how such a perfect machine could be possible. In *Traité de l'Enchaînement des Idées Fondamentales dans les Sciences et dans l'Histoire* (1861), Cournot argues that the required force could be made infinitely small, and that a force that is infinitely small according to the mathematics may not correspond to anything at all in reality; however, he did not have a convincing argument for this claim (Cournot, 1861, p. 373-75).

An ingenious solution to this problem was found by the French physicist and mathematician Joseph Boussinesq, in his 'Conciliation du véritable déterminisme mécanique avec l'existence de la vie et de la liberté morale' (1879). Boussinesq, a physicists and mathematician, is mainly known for his work in hydrodynamics. His theory about life and the will was widely discussed by both scientists and philosophers, among others Maxwell, Du Bois-Reymond and Renouvier. The solution he proposed was based on a mathematical theory of singular solutions to differential equations. The equations of motion for a physical system may, at certain points, not have a unique solution, which means that at these so-called singular points, the equations of physics leave the future course of the system undecided. This is a genuine form of indeterminism in classical mechanics: the examples Boussinesq gives of such (physically) indeterministic systems are equivalent to the Norton dome, which was

[25] 'supprimer cette dépense auxiliaire ou accessoire de force mécanique que nous ne pouvons qu'atténuer'. Cournot (1875), p. 53.

described by John Norton in (2003), and which subsequently raised quite some discussion in philosophy of physics about the question whether there can be indeterminism in classical mechanics.[26] Boussinesq was not the first to point out this type of indeterminism in classical mechanics: Poisson and Duhamel pointed out similar cases of indeterminism before him. However, Boussinesq worked out the case in most detail and was the first to use it as an argument for the possibility of vital and mental causes (Van Strien, 2014a).

Boussinesq argues that if the laws of physics do not uniquely determine the future course of a system, there must be a directive principle to determine what will happen. This directive principle can be either free will or a vital principle. At singular points, the directive principle can act on the system and change the future course of the system without exerting any force at all: not even an infinitely small force is needed. The equations of motion are never changed or violated by the directive principle, but merely supplemented by it in cases in which the equations themselves leave the future course of the system undecided. Thus, the possibility of genuinely indeterministic systems in physics opens the way for vitalism and free will.

Boussinesq could allow that the laws of physics are universally valid and hold without restriction for living organisms, and at the same time resist a materialist conception of life. These two positions could be reconciled through singular points, at which changes in the system could be effected by a non-physical principle, without a violation of the laws of physics.

The main practical challenge for Boussinesq was to make plausible that singular solutions to equations of motion occur specifically in the equations describing organic systems and that they can occur sufficiently regularly to form a basis for a theory of life and free will. Boussinesq proves mathematically that singular solutions are theoretically possible in a system of two atoms acting on each other, but it also turns out that the probability of their occurrence in such a system is infinitely small (Boussinesq, 1879, p. 109, 113). He argues that for there to be a finite chance that singularities occur, specially prepared circumstances are required which accommodate them (Boussinesq, 1879, p. 109), and that there are such prepared circumstances in living organisms. Life depends on the maintenance of a special interior environment, through the organization of the organs and the availability of nutrition, which is ultimately characterized by a physico-chemical instability beyond which 'there is only death, that is to say, the reign of mechanical laws only'.[27] It is probably the action of the directive principle itself which causes these special interior conditions to be maintained; thus, the directive principle makes the conditions for its enduring influence possible (Boussinesq, 1879, p. 116).

[26] On the similarities between Norton's dome and Boussinesq's dome, see Van Strien (2014a).

[27] 'il n'y a que la mort, c'est-à-dire le règne des lois mécaniques seules'. Boussinesq (1879), p. 65.

For Boussinesq, the connection between singular points and life was so strong that he defined life mathematically. According to his definition, a living being is a system for which the equations of motion allow for singular points, at which the intervention of a directive principle, that is, an extra-physical cause, is necessary (Boussinesq, 1879, p. 40, 113). Boussinesq thought that it was desirable to have such an exact definition of the word *life*, which he thought to be 'a bit vague' (Boussinesq, 1879, p. 112).

Boussinesq's approach to the problem of the will was taken up by Maxwell, who thought that this approach was interesting, even if it was also problematic. In a letter to Galton, he wrote:

> In most of the former methods Dr Balfour Stewart's &c. there was a certain small but finite amount of travail decrochant or trigger-work for the Will to do. Boussinesq has managed to reduce this to mathematical zero, but at the expense of having to restrict certain of the arbitrary constants of the motion to mathematically definite values, and this I think will be found in the long run, very expensive. But I think Boussinesq's method is a very powerful one against metaphysical arguments about cause and effect and much better than the insinuation that there is something loose about the laws of nature, not of sensible magnitude but enough to bring her round in time (Maxwell, 1879a).

Maxwell did realize that a central problem for Boussinesq's theory is that singular solutions, although possible in theory, are extremely unlikely to occur in actual systems. This is because they will only occur if certain quantities (such as forces and particle positions) have a specific value, and the effect disappears if the value is only slightly different. Nevertheless, he thought it was a promising approach, especially because it avoids any need for the mind to exert even a small force that is not regulated by the laws of physics; there is therefore no violation of the laws of physics involved at any scale.

This was an improvement over Maxwell's own approach, a couple of years earlier: in his essay from 1873 on science and free will, Maxwell had suggested that there could be an immaterial directive principle capable of triggering or directing motions in unstable mechanical systems. This would involve indeterminism through a violation of the laws of physics on a small scale, small enough to be beyond our epistemic reach. Maxwell explicitly rejected the view that the principle of determinism should be taken to be absolutely valid to the exclusion of any possible influence of the mind. For us as human beings, with our limited capacities of observation, determinism cannot be confirmed by observation: Maxwell argues that there are many cases in which we are not able to make predictions. This is especially the case in those processes which involve instabilities. He concludes:

> If, therefore, those cultivators of physical science from whom the intelligent public deduce their conception of the physicist, and whose style is recognised as

> marking with a scientific stamp the doctrines they promulgate, are led in pursuit of the arcana of science to the study of the singularities and instabilities, rather than the continuities and stabilities of things, the promotion of natural knowledge may tend to remove that prejudice in favour of determinism which seems to arise from assuming that the physical science of the future is a mere magnified image of that of the past (Maxwell, 1873, p. 823).

As the occurrence of singularities and instabilities is, according to Maxwell, a characteristic of living beings, we should not expect to find determinism when studying living beings.

In the analogies that Maxwell gave of unstable mechanical systems, there could be physical indeterminism through violations of the laws of physics on a small scale, through the exertion of a very small force by an immaterial principle, without there ever being any possibility of observing these violations. One reason why Maxwell might think that the laws of physics could be violated on a small scale is his work in statistical mechanics, which led him to argue that violations of the second law of thermodynamics were theoretically possible (e.g. through Maxwell's demon). According to Maxwell, the second law of thermodynamics is a statistical law of nature, that holds on average on a macroscopic scale, but does not hold rigorously on a small scale. The same might hold for other laws of physics as well: they may also be statistical laws of which violations are possible in principle. Nevertheless, Maxwell apparently thought that this assumption of there being 'something loose' about the laws was not the most elegant approach to the problem of free will, and he therefore approved of Boussinesq's approach in which there is indeterminism in the mechanical system itself and in which there is no violation of the laws of physics involved at any level.

We have seen that if one wants to argue that an immaterial principle can act through directing motions or triggering energy transformations, there are two possibilities: one can argue either that this directive principle can act through exerting a very small (possibly infinitely small) force, or that it can act without exerting any force. The first possibility requires physical systems that are unstable, and involves a small violation of the laws of physics, in particular the law of conservation of energy; moreover, there is the metaphysical problem of how an immaterial principle can exert a physical force, however small. The second possibility requires genuine indeterminism in the equations of physics. Arguing for indeterminism in physics was thus a way to make vital and mental causes compatible with the law of energy conservation.

3.5 Physical determinism versus physiological determinism

The explanations of life and free will by Maxwell, Stewart, Cournot and Boussinesq were primarily an attempt to make vital and mental causes in physiology compatible with the law of conservation of energy. While indeterminism in mechanics provided a resource to make their arguments work, the debate was not primarily triggered by a general concern over determinism in science, but rather by the claimed applicability of the law of conservation of energy to physiology. This can also be seen by the fact that although Maxwell, Stewart, Cournot and Boussinesq were all concerned about the implications of the law of energy conservation in physiology, not all of them were concerned about determinism in the physiological realm. In particular, Cournot as well as Boussinesq accepted physiology as a deterministic science, as long as it was not reducible to physics and as long as there was a role for genuine vital causes in physiology.

One can make a distinction between physical determinism and physiological determinism:

(1) *Physical* determinism says that the future course of a system is uniquely determined by the laws of physics. Laplacian determinism, according to which perfect prediction of the future states of the universe is possible on the basis of perfect knowledge of the positions, velocities, and forces on all the particles in the universe at an instant, can be counted as a type of physical determinism.[28]

(2) *Physiological* determinism says that the future course of a system is uniquely determined by laws of physiology, which may involve an irreducible vital principle or be otherwise irreducible to physics. A well-known proponent of physiological determinism is Claude Bernard, who argued for determinism in physiology in his *Introduction à l'étude de la médecine expérimentale* (1865), while also arguing that physiological processes are irreducible to physical and chemical processes.[29]

In these terms, Cournot and Boussinesq specifically argue against physical determinism, because they fear that it would lead to an exclusion of non-physical causes and to physical reductionism. Despite their objections against physical determinism, Cournot and Boussinesq do argue for determinism in the physiological realm. Both Cournot and Boussinesq argue that, as long as no free will is involved, physiological processes take place according to deterministic laws. However, this is a physiological determinism that

[28] In Van Strien (2014b), I argue that Laplace's determinism was not directly derived from his physics; nevertheless it is closely related to physics and can be formulated in terms of laws of mechanics.

[29] Bernard argues that there is a kind of vital principle involved in organic processes that directs processes without producing them, but he does not give a detailed account of how this could work. See Bernard (1865), p. 51. On the contrast between Bernard's determinism and Laplacian determinism, see Gayon (2009); Israel (1992).

is irreducible to physical determinism, and compatible with the intervention of non-physical causes. In this way they can argue, like Claude Bernard, for lawfulness of physiological processes, so that physiology can be a proper science even though it is not reducible to physics.

Both Cournot and Boussinesq specify that physiological processes are deterministic in a non-Laplacian way. According to Laplacian determinism, all future (and past) states of a closed system are fully determined by a specification of the state of the system at the present instant. But both Cournot and Boussinesq argue that in physiology, perfect knowledge of the present state of a system does not suffice to determine the future states; in order to determine the future states of the system, one needs knowledge of past states as well. The difference has to do with heredity: Cournot and Boussinesq do not think that all the information needed for the future development of an organism is present in the seed or embryo. Cournot argues that to determine what will come of a seed, one has to study not only its present state but the past as well:

> That which we lack for predicting the fate of the future plant, when taking into account the actual data of the surrounding environment and hence the variations that it will undergo under the influence of physical forces, is not so much a descriptive anatomy of the germ, pushed far enough, than a genealogy, a history of ancestors, sufficiently detailed and going back far enough. (Cournot, 1875, p. 115-16).[30]

Boussinesq thinks that heredity must be explained through the assumption of a direct influence of the past states, or 'anterior evolutions', on current physiological processes (Boussinesq, 1879, p. 134). Such influence is possible, in his theory, through the directive principle which acts in living beings at singular points. In processes in which no free will is involved, this principle acts according to deterministic laws of physiology. There is thus a 'special', irreducible explanation of heredity, and this explanation depends on the possibility of vital causes that act at unstable or singular points in the system.

However, both Cournot and Boussinesq did think that although processes in physiology are typically deterministic, indeterminism had a limited role to play in the organic world. Cournot argues that occasionally the laws of physiology can allow for indeterminism (just as the laws of physics can allow for physical indeterminism at certain points) (Cournot, 1875, p. 118ff), and that this occasional indeterminism in

[30] 'Ce qui nous manque pour prédire les destinées de la future plante, en tenant compte comme de raison des données actuelles du milieu ambiant et par suite des variations qu'il subira sous l'influence des forces physiques, c'est bien moins une anatomie descriptive du germe, poussée assez loin, qu'une généalogie, une histoire des ancêtres, suffisamment détaillée et remontant assez haut'.

physiology is needed to account for novelty in the organic realm, such as the development of new species. Boussinesq limits pure indeterminism to the realm of free will (that is to say, the acts of the will are determined in the sense that there is an agency that determines what will happen, but undetermined in the sense that they are not predictable in any way) (Boussinesq, 1879, p. 57).

Thus, although both Cournot and Boussinesq left some room for indeterminism, they both argued that, usually, physiological processes are completely deterministic. Their concern was thus not with determinism in the physiological realm; what they wanted to ensure was that the law of conservation of energy was not taken to lead to a reduction of physiology to physics, and that there could be genuine vital causes.

3.6 Free will and life

When Maxwell and Boussinesq wrote about free will, they understood it strictly in terms of mind–matter interaction. They argued for an immaterial mind or soul that could cause changes in the body (where the body is treated as a mechanical system), and thus regarded an antimaterialist and dualistic metaphysics as essential for free will. As we've seen, these ideas about the will were very much connected to ideas about life, and about the possibility of non-materialistic explanations of organic processes. These special explanations of life could account e.g. for holistic and teleological features of organisms. For Boussinesq, the problem of how to account for organic processes was just as important as that of free will: he was quite critical of materialist physiology that fully reduced organic processes to the laws of physics and chemistry (Boussinesq, 1879, p. 38-39). Maxwell's primary concern was with free will, but his ideas about the possibility of free will are also connected to the issue of whether life can be explained in a materialist way. This becomes clear in the following passage from 1879 about the very definition of life:

> Science has thus compelled us to admit that that which distinguishes a living body from a dead one is neither a material thing, nor that more refined entity, a 'form of energy'. There are methods, however, by which the application of energy may be directed without interfering with its amount. Is the soul like the engine-driver, who does not draw the train himself, but, by means of certain valves, directs the course of the steam so as to drive the engine forward or backward, or to stop it? (Maxwell, 1879b).

Thus, according to Maxwell, there is no vital matter or vital energy, but there can be a soul which directs motions in living beings.

Finally, Cournot and Stewart were not at all concerned with the issue of free will, with Cournot going so far as to deny free will. According to Cournot, an act is always determined by something, by physical circumstances, physiological or psychological factors, reason, superstition, or by past experiences (Cournot, 1875, p. 239). The French philosopher Renouvier argued that Cournot's ideas could provide a basis for a theory of free will, even if Cournot himself was not interested in the problem of free will but only in the problem of life. But, as Renouvier remarked, 'the question is the same, as it concerns finding a conciliation between a mechanical order and actions that are exerted on this order without belonging to it'.[31]

Cournot and Boussinesq can both be counted as vitalists; although neither held the position that there are vital 'forces' of the same order as physical forces, they did have a dualistic conception of life, and their ideas seem to fit under the label of ontological vitalism or substance vitalism, with an immaterial principle directing motions at the microlevel.[32] The authors discussed in this paper were thus primarily interested in the possibility for a non-physical cause to intervene in living beings, whether the mind or the soul or a vital principle (or even God). They argued for a strict dualism between the body as machine and an immaterial entity that operates the machine. However, they did not go into details about the nature of this immaterial entity or exactly how it was to relate to the body.[33]

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[31] 'la question est la même, en tant qu'elle a trait à une conciliation à trouver entre un ordre mécanique et des actions qui s'exercent sur lui sans lui appartenir'. Renouvier (1882).

[32] On types of vitalism, see Wolfe (2011). On vitalism in Cournot, see Martin (2011), and Vatin (2007).

[33] The argument that the possibility of vital and mental causes could be saved through an appeal to instability or indeterminism in physics had a kind of afterlife in Bergson, who, in a lecture in 1911, explained how vitalism could work in terms that are very reminiscent of Maxwell, Stewart and Cournot: 'When we investigate the way in which a living body goes to execute movements, we find that the method it employs is always the same. This consists in utilizing certain unstable substances which, like gunpowder, need only a spark to explode them. I refer to foodstuff, especially to ternary substances, carbohydrates and fat. A considerable sum of potential energy, accumulated in them, is ready to be converted into movement. That energy has been slowly and gradually borrowed from the sun by plants; and the animal which feeds on a plant, or on an animal which has been fed on a plant, and so on, simply receives into its body an explosive which life has fabricated by storing solar energy. To execute a movement, the imprisoned energy is liberated. All that is required is, as it were, to press a button, touch a hair trigger, apply a spark; the explosion occurs, and the movement in the chosen direction is accomplished.' (Bergson (1911), p. 18). But Bergson did not think there was an immaterial directive principle that could pull the trigger; instead there was indeterminacy originating in a difference in temporal span between the mental and the physical.

Conclusion

The debate discussed in this paper about the possibility of free will or a vital principle that is not regulated by the laws of physics, is very much about the same issue with which Descartes wrestled (notably in the correspondence with Princess Elisabeth; see Shapiro, 1999), of how the mind could act on the body, or, more generally, how a non-physical cause could have an impact in the physical world. Around the 1870s, this issue was again brought to the foreground through the development of the law of conservation of energy, which Helmholtz and Du Bois-Reymond applied to physiology in order to exclude the intervention of non-physical causes. In response, a number of physicists and mathematicians such as Maxwell, Boussinesq, Cournot and Stewart made attempts to defend strictly dualist conceptions of life and the will, through an appeal to unstable and indeterministic systems in mechanics. The law of conservation of energy thus posed a problem for dualism with regard to life and the will, but did not bring an end to dualist theories.

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Paper 4 The nineteenth century conflict between mechanism and irreversibility[1]

The reversibility problem (better known as the reversibility objection) is usually taken to be an internal problem in the kinetic theory of gases, namely the problem of how to account for the second law of thermodynamics within this theory. Historically, it is seen as an objection that was raised against Boltzmann's kinetic theory of gases, which led Boltzmann to a statistical approach to the kinetic theory, culminating in the development of statistical mechanics. In this paper, I show that in the late nineteenth century, the reversibility problem had a much broader significance - it was widely discussed and certainly not only as an objection to Boltzmann's kinetic theory of gases. In this period, there was a conflict between mechanism and irreversibility in physics which was tied up with central issues in philosophy of science such as materialism, empiricism and the need for mechanistic foundations of physical theories, as well as with concerns about the heat death of the universe. I discuss how this conflict was handled by the major physicists of the period, such as Maxwell, Kelvin, Duhem, Poincaré, Mach and Planck, as well as by a number of lesser-known authors.

[1] This paper has been published in *Studies in History and Philosophy of Modern Physics*, 44(3), August 2013, pp. 191–205.

4.1 Introduction

The reversibility problem – which has become known as the reversibility *objection* (or *Umkehreinwand*) – is well known in the literature as an objection that was raised against Boltzmann's kinetic theory of gases, an objection with which Boltzmann wrestled for many years.[2] It is usually presented along with the recurrence objection (or *Wiederkehreinwand*) as physical theorems which show that, within the kinetic theory of gases, there is a problem with accounting for the second law of thermodynamics. However, when the reversibility problem first appeared, in the last decades of the nineteenth century, it was the subject of a much broader discussion. For that reason I will speak of the reversibility *problem* rather than the reversibility objection.

The problem lies in the difficulty with giving a mechanical account of irreversible processes. The laws of mechanics are reversible, which means that if you exactly reverse the velocities of all particles in a closed mechanical system, all processes will subsequently run backwards. In a mechanical theory such as the kinetic theory of gases, the reversal of each process is thus a theoretical possibility. There is therefore a conflict between mechanical accounts of nature and the irreversibility that we experience in many daily phenomena and which is reflected in the second law of thermodynamics. This conflict was a much debated issue in late-nineteenth-century physics because it was connected with the most pressing debates of that time: the debate over the need for mechanical foundations in physics versus empirical approaches; materialism; the question whether the laws of nature should be regarded as strictly and universally valid or could also have statistical validity; and the concerns about the heat death of the universe which the second law of thermodynamics seemed to imply. In this paper I show how the reversibility problem served as an argument in each of these debates. When discussing this problem, authors were confronted with conflicting values in physics, such as empiricism versus the need for mechanical foundations, and therefore studying the way in which different authors dealt with the issue of reversibility provides an interesting opportunity to trace developments in the philosophy of physics.

In the first two sections of this paper, the main concepts which play a role in the reversibility problem are clarified, namely irreversibility and mechanism. The rest of the paper is a roughly chronological study of the treatments of the reversibility problem in the second half of the nineteenth century. It is intended to be more or less exhaustive, involving the work of many of the leading physicists of the period, including Maxwell, Planck, Mach, Poincaré and Duhem, as well as lesser-known scientists whose

[2] See for example Kuhn (1978, p. 46ff), Torretti (2007, p. 745ff), Uffink (2007, p. 929ff).

work on the reversibility problem has not previously been studied.[3] By including not only well-known physicists in the story but also relatively unknown authors, I intend to give a view of the full dimensions of the debate. In this way I show that the reversibility problem was much more than an internal problem within the kinetic theory of gases.

4.2 Irreversibility and the second law of thermodynamics

Irreversible processes in physics are those processes which are not not physically possible to reverse, or for which a reversal is not allowed by the laws of nature. Here, for any process P which takes a system from an initial state s_1 to a final state s_2, the reverse process is that which takes the system from s_2 to s_1.[4] Examples of processes thought to be irreversible are the melting of an ice cube in a closed system, a body losing speed through friction, and processes of decay and ageing.

During the second half of the nineteenth century, the term "irreversibility" was introduced in physics mainly in the context of the second law of thermodynamics. But the exact relation between irreversibility and the second law of thermodynamics was not always clear, mainly because during the period, it was never entirely clear what exactly the second law of thermodynamics was. The law became established in the 1850's, soon after the first law of thermodynamics (the law of conservation of energy). Although the importance of the second law as a law of nature was soon widely acknowledged, physicists did sometimes remark that it was less well established and received less recognition than the first law of thermodynamics, because it was more complicated and had less clear foundations.[5] The fact that there were actually many different versions of the second law of thermodynamics was both a factor in and a

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[3] A notable omission is Gibbs, who, despite the fact that he made important contributions to both thermodynamics and statistical mechanics, does not seem to have directly discussed the problem of reversibility of mechanical motion versus irreversibility in thermodynamics. Gibbs did point out a problem with the foundations of the second law within the kinetic theory of gases, namely the diffusion paradox that is named after him, and concluded: "In other words, the impossibility of an uncompensated decrease of entropy seems to be reduced to improbability". But this has little to do with the reversibility problem otherwise. See Gibbs (1876-1878, p. 167).

[4] In treatises of thermodynamics, the term 'reversible' can also be used to denote a slow and gentle process, during which the system remains close to equilibrium (also called a *quasi-static* process); a process is then 'irreversible' if this does not hold. Note that this definition is not fully equivalent with the above sense of irreversibility. For a thorough treatment of the different notions of irreversibility in physics, see Uffink (2001, p. 315ff).

[5] See for example Loschmidt (1869, p. 395), Wald (1889, p. 2).

consequence of this confusion. The main versions of the second law that were used at the time are the following:

1) The statement that there is an entropy function in thermodynamics which can never decrease (that is, in adiabatically isolated processes). This implies that all processes in which entropy increases are irreversible.
2) The principles of Thomson (the later Lord Kelvin) and Clausius (cited in Uffink, 2001, p. 327):
   - [Thomson] It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects.
   - [Clausius] It is impossible for a self-acting machine, unaided by any external agency, to convey heat from one body to another at a higher temperature.

   These principles are intended to apply to heat engines working in a cycle. They are empirical principles based on experiences of the behaviour of heat: heat always tends to flow from hot bodies to cold bodies, and it takes energy to cool an object below the temperature of its surroundings. These principles, too, forbid the reversal of certain processes. For example, a process in which heat is transferred from a higher temperature to a lower temperature is perfectly possible, but its reversal is not (that is, as long as it is a cyclic process which has the transfer of heat as its only result).
3) The statement that mechanical energy can be completely converted into heat, but heat cannot be completely converted into mechanical energy. Thus whenever mechanical energy is converted into heat, for example through friction, there is a decrease of the amount of energy that can be converted into mechanical energy: a decrease in "available" energy, also called "dissipation" or "degradation" of energy. This version of the second law was often used by Thomson.[6] It clearly involves irreversibility.
4) The statement that there is a tendency toward thermal equilibrium: in isolated systems, differences in heat and temperature always tend to disappear. This statement also clearly involves irreversibility, but it is not clear whether it can be regarded as a formulation of the second law of thermodynamics. It can partly be traced back to Fourier's theory of heat conduction from 1822. Brown and Uffink (2001) have called this principle the "minus first law" of thermodynamics, and argue that it is actually through this principle that time asymmetry enters thermodynamics. This would mean that not all irreversibility in nature is connected to the second law of thermodynamics.

[6] He introduced this principle in his 1852 article titled “On a universal tendency in nature to the dissipation of mechanical energy” (Thomson, 1852).

5) The statement that there is an entropy function S such that $d'Q/T = dS$, with $d'Q$ an inexact differential of the heat Q that a system exchanges with a reservoir, and T its temperature. This version of the second law is not connected to irreversibility; the validity of this equation is even limited to processes which are "reversible" in Clausius' sense, which is close to quasi-static (Uffink, 2001, p. 337).

The above versions of the second law are closely related but not exactly equivalent; for the relations between them, see Uffink (2001). Except for 5), all of these statements involve irreversible processes. But although, in the period that I discuss, irreversibility in physics was mainly discussed in the context of the second law, it was not clear whether all irreversible processes were connected to it. In 1879, Max Planck argued that the second law was the unique law that governed all irreversible processes and was therefore the key to understanding irreversibility in nature. Specifically, he argued that all irreversible processes involved an increase of entropy (Planck, 1879). But this view was by no means commonly accepted at the time.

4.3 Mechanism and the reversibility of mechanical processes

With "mechanism" I mean the attempt to reduce all physical theories to mechanistic theories. By "mechanistic theory" I mean a theory in which all entities are defined in terms of matter, motion, and central forces (forces depending only on distance). Mechanistic theories are often, but not necessarily, atomistic. Alternatively, we might think of a mechanistic theory as a theory which explains natural phenomena in terms of springs, wheels, pulleys, etc., in order to show the "mechanism" behind the phenomenon. However, such a theory does not always need to be mechanistic in the strict sense defined above, and in particular, actual mechanisms usually involve a certain amount of friction.

It is an ancient ideal in physics to give mechanical explanations of all phenomena: physicists have often felt that one can only truly understand something after reducing it to understandable, mechanical terms. In the nineteenth century mechanism played an important role in physics, so for example when electromagnetism was developed, physicists like Maxwell, William Thomson and J. J. Thomson attempted to give it a mechanical foundation. According to the latter, the belief in the possibility of mechanical explanation of natural phenomena was "the axiom on which all Modern Physics is founded" (J. J. Thomson, 1888, p. 1). Also the attempt to reduce thermodynamics to mechanics was an important project in nineteenth century physics. Thermodynamics itself was evidently not mechanistic, since it employed entities that were not reduced to mechanical entities, such as heat. It was based on empirical principles, for example that heat always tends to flow from hot to cold. To reduce thermodynamics to a mechanistic theory, Clausius, Maxwell, Boltzmann and others developed the kinetic theory of gases, which described the properties of gases in terms of the motion of its atoms or molecules.

A property of such a mechanistic theory is that its laws are time symmetric; therefore, strictly speaking, it does not allow for irreversible processes. Take a mechanical system, and a process which takes the system from its initial state s_1 to a state s_2 within a certain interval of time. Now take the state s_2 and instantaneously reverse all velocities to obtain the reversed state $R(s_2)$. Then subsequently, a reversed process takes place which takes the system from state $R(s_2)$ to state $R(s_1)$. If the original process showed an increase in entropy, the reverse process shows a decrease in entropy. This reversal of all velocities at an instant may not actually be practicable, but nevertheless the thought experiment shows that for each process $P: s_1 \rightarrow s_2$ that can occur within a certain mechanical system, initial conditions can be found which lead to a reversed process $P_R: R(s_2) \rightarrow R(s_1)$: thus the reversed process is always *physically possible,* that is, it is allowed by the laws of nature.

In practice, macroscopic motion always involves a certain amount of friction, which causes it to be irreversible. However, mechanistic theories do not allow for friction on the fundamental level. The kinetic theory of heat made it possible to explain friction as the transfer of kinetic energy from the moving body to small particles in its surroundings, whose motion constitutes heat. It follows from this explanation of friction that if the velocity of all particles, including the particles in the surrounding environment, are exactly reversed then even a process involving friction becomes reversed.

4.4 Reversibility and materialism

An early appearance of the principle of reversibility of mechanical motion can be found in the correspondence between James Clerk Maxwell and William Thomson (the later Lord Kelvin). In a letter from 1857, in which Maxwell discussed the properties of motion in a perfect fluid (one without viscosity or fluid friction), he remarked:

> If you pour a perfect fluid from any height into a perfectly hard or perfectly elastic basin its motion will break up into eddies innumerable forming on the whole one large eddy in the basin depending on the total moments of momenta for the mass.

> If after a given time say 1 hour you reverse every motion of every particle, the eddies will all unwind themselves till at the end of another hour there is a great commotion in the basin, and the water flies up in a fountain to the vessel above. But all this depends on the *exact* reversal for the motions are *unstable* and an approximate reversal would only produce *a new set of eddies multiplying by division* (Maxwell, 1857).

Although Maxwell emphasized that the reversal of the process of pouring a perfect fluid is virtually impossible in practice (he added: "I do not see why the unstable motion of a perfect fluid should not produce eddies which can never be gathered up again except by miracle"), he did recognize that the process is reversible in principle. However, this did not mean that he thought that the reversal of every process was possible. In a letter that was published in the *Saturday Review* (Maxwell, 1868), he attributed such a view to unnamed materialists. A materialist, believing that everything can be reduced to matter and motion, must accept the idea that every process can also occur in reverse:

> ...one thing in which the materialist (fortified with dynamical knowledge) believes is that if every motion great & small were accurately reversed, and the world left to itself again, everything would happen backwards: the fresh water would collect out of the sea and run up the rivers and finally fly up to the clouds in drops which would extract heat from the air and evaporate and afterwards in condensing would shoot out rays of light to the sun and so on. Of course all living things would regrede from the grave to the cradle and we should have a memory of the future but not of the past (Maxwell, 1868).

Maxwell thus argued that materialism leads to consequences which are evidently in conflict with common sense. He was committed to mechanism and was one of the main physicists involved in the attempt to reduce all physical phenomena to mechanics, and he therefore had to acknowledge that all purely physical processes are reversible, although their actual reversal may have a probability close to zero. But Maxwell was no materialist; a deeply religious man, he thought that the ultimate nature of things was fundamentally unknowable, and he was opposed to the scientific materialism of John Tyndall and others (Harman, 1998, pp. 197-208). He did not think that processes involving life could be fully explained mechanically; in an essay from 1873 he argued that the laws of physics could allow for the possibility of a soul to act in living beings, thus introducing indeterminism (Maxwell, 1873, p. 817ff). If a living system could not fully be described in a mechanistic, deterministic manner, it seems likely that it would not be subject to reversibility. It is likely that Maxwell therefore rejected the conclusion that "all living things would regrede from the grave to the cradle", although he did not make this line of thought explicit.

The argument for the non-reversibility of processes involving life can be found in explicit form in an article about the reversibility problem by Maxwell's friend Thomson, published in 1874. Thomson argues that if at a certain moment the velocities of all the particles in the universe were exactly reversed, the result would be a complete reversal of "the course of nature":

> The bursting bubble of foam at the foot of a waterfall would reunite and descend into the water; the thermal motions would reconcentrate their energy, and throw the mass up the fall in drops re-forming into a close column of ascending water. Heat which had been generated by the friction of solids and dissipated by conduction, and radiation with absorption, would come again to the place of contact, and throw the moving body back against the force to which it had previously yielded. Boulders would recover from the mud the materials required to rebuild them into their previous jagged forms, and they would become reunited to the mountain peak from which they had formerly broken away. And *if also the materialistic hypothesis of life were true*, living creatures would grow backwards, with conscious knowledge of the future, but no memory of the past, and would become again unborn. (Thomson, 1874, pp. 351-352) [my italics].

However, Thomson did not think that the materialistic hypothesis of life was true. He continued: "the real phenomena of life infinitely transcend human science; and speculation regarding consequences of their imagined reversal is utterly unprofitable" (Thomson, 1874, p. 352). As he wrote many years later:

> The considerations of ideal reversibility, by which Carnot was led to his theory, and the true reversibility of every motion in pure dynamics have no place in the world of life. Even to think of it (and on the merely dynamical hypothesis of life we can think of it as understandingly as of the origination of life and evolution of living beings without creative power), we must imagine men, with conscious knowledge of the future but with no memory of the past, growing backward and becoming again unborn; and plants growing downwards into the seeds from which they sprang (Thomson, 1892, p. 464).

A similar argument can be found in the writings of the Belgian Jesuit Ignace Carbonnelle, mathematician and philosopher, who argued against materialism from a Catholic point of view. Like Maxwell and Thomson, he argued that the reversal of purely material processes was theoretically possible, though very improbable. But it would be absurd to think that "the phenomena of the intellectual and moral order"[7] are reversible, and thus these phenomena cannot be purely material (Carbonnelle, 1881, p. 336).

In an 1875 article about the reversibility problem by Philippe Breton, a little-known engineer from Grenoble with an interest in philosophy, we also find the argument

[7] "les phénomènes de l'ordre intellectuel et moral".

against materialism (Breton, 1875). In this article, Breton points out that the fact that processes generally take place in a definite direction and the fact that causes always precede their effects are not reflected in the equations of mechanics, which are time symmetric. Therefore, according to the laws of mechanics, the original and the reversed processes are equally possible. In the case of the study of comets, this leads to the valuable insight that comets which are caught in the solar system are not necessarily caught forever but might move away after a certain time. But Breton finds other examples more disturbing: according to the "materialistic world view", the process of a stone breaking to pieces must also be reversible, as well as the process of ripening and rotting of fruit, the process of ageing... Breton writes about reversed sensations, reversed memories, reversed will and morality, and even about reversed Darwinism—an important theory for the true materialist, according to which the environment adapts itself to the changing animal. Furthermore, he argues that when the possibility of the reversal of all processes is taken into account, it is no longer possible to distinguish causes from effects.

Breton's conclusion is clear: “It is thus evident that mechanics is not the universal science”.[8] To avoid absurdity one has to conclude that not everything is reducible to matter and motion. The laws of mechanics were derived from a limited set of phenomena and are thus only applicable within a limited domain. Breton also argues that mechanics is ultimately based on common sense, so if it leads to results that are in conflict with common sense, then one should suspect there is something wrong with its foundations instead of uncritically accepting the results.

According to Maxwell, Thomson, Carbonnelle and Breton, there is thus a limit on which processes should be thought to be reversible. The physicist may have to admit that it is theoretically possible that processes such as the falling of an object or the melting of ice can also occur in reverse, but presumably, for these authors the thought of people growing younger is simply absurd and should not be accepted. Their motivation for arguing that materialism ultimately leads to such absurd consequences was to make materialism itself seem absurd.

[8]"Il est donc évident que la mécanique n'est pas la science universelle"

4.5 Reversibility and statistical laws of nature

Both Thomson and Maxwell did accept the reversibility (at least in theory) of those processes which did not involve life or the mind. The idea that appears from Maxwell's writings is that there are physical processes which may be called irreversible, but only in the sense that the reversed process is exceedingly improbable, a fact that depends on contingent properties of the world. Theoretically, each physical process is reversible. This reversibility entails that violations of the second law of thermodynamics (or at least most versions of it) are theoretically possible, although the probability that the second law is violated on an observable scale is extremely small (see Maxwell, 1878, p. 285ff). For Maxwell, the reversibility problem showed that the second law was a *statistical* law of nature (see Porter, 1986, p. 111ff, about the role of the statistical method in Maxwell's thought).

In addition to the reversibility thought experiment, Maxwell came up with a second thought experiment which showed that violations of the second law of thermodynamics were theoretically possible, which is now known as "Maxwell's demon". Maxwell's demon is a small being that can observe and manipulate individual atoms. By moving a frictionless slide to open and close a hole in a wall between two vessels, this being can let slowly moving atoms pass through only to one side and quickly moving atoms only to the other side. In this way, he can make heat flow from cold to hot without performing work; this means a violation of the second law of thermodynamics. Maxwell concluded that the validity of the second law depends on the assumption that we as human beings cannot perform this operation: the particles whose motion is experienced by us as heat just happen to be too small for us to observe and manipulate (Maxwell, [1871] 1970, pp. 153-154). In a letter to John William Strutt (the later Third Baron Rayleigh), Maxwell explained both the reversibility problem and the demon, and concluded:

> *Moral*. The 2nd law of thermodynamics has the same degree of truth as the statement that if you throw a tumblerful of water into the sea you cannot get the same tumblerful of water out again (Maxwell, 1870b).

Despite the significance of the second law in Maxwell's thought, he did not seem to regret the fact that, according to the kinetic theory of gases, the second law was not universally valid. This has much to do with his conviction that, although the truth of the second law was "not of the same order as that of the first law", it was still a reliable statement, just as the statement that "if you throw a tumblerful of water into the sea, you cannot get the same tumblerful of water out again" was reliable. The relevant point was not whether exceptions to the second law are theoretically possible, but whether it holds in practice in the world as we experience it; and one may say that it does.

Thomson also had no difficulty in allowing that the second law could theoretically be violated. He did not even perceive a tension between mechanism and irreversibility: in his 1874 article he wrote that "a very elementary consideration of [the reversibility of mechanical motion] leads to the full explanation of the theory of dissipation of energy" (Thomson, 1874, p. 352). Apparently, according to Thomson, the reversibility problem not only did not threaten the principle of energy dissipation (version (3) of the second law) but even clarified it. As an example he mentioned a process of temperature equalization in a body composed of freely moving atoms, and argued that a spontaneous reversal of this process is a statistical possibility, but that one can explain why such a process of disequalization is never observed in practice by calculating how extremely small its probability is.

The reversibility problem, together with Maxwell's demon, played a central role in the development of the idea of statistical laws of nature, and the change of the second law of thermodynamics from an absolute to a statistical law of nature was a crucial step in the development of statistical mechanics.

4.6 Reversibility and the heat death of the universe

It is somewhat surprising that Maxwell and Thomson had no difficulty giving up on the absolute validity of the second law, given that both attached great importance to the law. According to Thomson, who was one of the physicists who developed the second law, the law predicts that our world is inevitably evolving towards a final state, in which it will be "unfit for the habitation of man as presently constituted" (Thomson, 1852). This final state has come to be known as the heat death, a state in which all energy is converted into heat, uniformly distributed through space, and all motion has ceased. But not only did the second law predict a heat death, it also pointed at a origin in time of the universe, as Kragh (2008) has shown. Temperature differences cannot have been diminishing forever, mechanical energy cannot have been irrecoverably converted into heat forever. Thus, there must have been a temporal origin for these processes, which could be connected with divine creation; Maxwell and Thomson both connected the second law (or the law of dissipation of energy) with such a temporal origin (Maxwell 1870a, p. 226, and Thomson, 1852). Smith and Wise (1989, p. 330ff) have argued that the idea of directionality in nature deeply influenced Thomson's work.

However, not everyone was pleased with these cosmic implications of the second law. The German chemist and physicist Joseph Loschmidt was greatly troubled by the prospect of the heat death of the universe, and these concerns led him to use the reversibility problem to argue that the second law had to be rejected altogether, or at least the principles of Thomson and Clausius (version (2) of the second law) and the principle of dissipation of energy (version (3) of the second law).

Loschmidt was a materialist (Loschmidt, 1867, p. 81) and an active proponent of molecular science, famous for his estimation of the size of molecules. He believed that the success of the kinetic theory of gases greatly supported atomism (Loschmidt, 1866, p. 646). His enthusiasm for the second law of thermodynamics seems to have been more limited from the start. In a popular lecture in 1867, he mentioned Clausius' principle (version (2) of the second law) and the fact that a consequence of this principle was the heat death of the universe. He concluded:

> We then have complete rest in all worlds, the eternal rest of overall death.
> The greatest efforts have been made to find a way to escape these not very edifying inferences, so far in vain, and little hope of success remains. (Loschmidt, 1867, p. 66).[9]

Despite the fact that Loschmidt's hopes for disproving the heat death of the universe were initially low, he set out to examine the foundations of the second law. In 1869, he argued that this law was still lacking a solid foundation and a clear exposition of its meaning, though "far-reaching deductions"[10] were generally drawn from it (Loschmidt, 1869, p. 395). It appears that Loschmidt thought that basing a law of nature on an empirical principle was not rigorous enough. He argued that one cannot be sure that Clausius' principle that heat can never flow from a cold to a hot body without compensation is universally valid, even though we never observe a violation of it: there may be circumstances which prevent us from observing the transition of heat from a cold to a hot body. Loschmidt continues:

> It is therefore in any case advisable to see if in the nature of molecular thermal motion itself there cannot be found grounds on which one can decide for or against the possibility of such a transition. (Loschmidt, 1869, p. 399).[11]

His subsequent considerations of molecular motion led him to a thought experiment similar to that of Maxwell's demon. Take a space with a large amount of gas molecules, in which the average velocity of the molecules remains constant but the individual velocities vary in time and amongst each other. Take a second, smaller space separated

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[9] "Wir haben dann vollkommene Ruhe in allen Welten, die ewige Ruhe des allgemeinen Todes.
Mann hat die grössten Anstrengungen gemacht, einen Ausweg zu finden, diesen wenig erbaulichen Folgerungen zu entgehen, bisher vergeblich, und es ist wenig Aussicht vorhanden, dass es noch gelingen werde."

[10] "weitgreifenden Deductionen"

[11] "Es ist daher jedenfalls gerathen zuzusehen, ob nicht im Wesen der molecularen Wärmebewegung selber Gründe aufzufinden seien, welche für oder gegen die Möglichkeit eines solchen Überganges sprechen."

from the first by a wall with a hole in it. If the initial condition of the gas is known at the molecular level, then, since the system is deterministic, it is possible to calculate at which moments and with what velocities the molecules will hit the wall at a given place. Thus one can calculate in advance at which moments to open or close the hole in the wall in order to let through only molecules with velocities above the average. In this way heat can be made to flow from the large space to the smaller one, also when this entails a heat flow from a cold to a hot space. It is even possible to make the density of the gas in the smaller space rise above that in the larger space.

No demon-like being is employed here, but to perform this experiment one does need to be able to observe the behaviour of individual molecules (in order to know the initial positions and velocities of all molecules) and to be able to open and close a hole in the wall quickly enough to let individual molecules through: these are exactly the faculties that Maxwell proposed for his demon. So the only difference between Maxwell's and Loschmidt's thought experiments is that Loschmidt employs the determinism of the system to relieve the door-handler of making his decisions at the moment a molecule approaches. And, most significantly, Loschmidt uses the thought experiment for a different end: he does not use it in order to argue that the second law depends on the assumption that one cannot observe and manipulate individual particles, as Maxwell did, but to argue that it is problematic to give a mechanistic account of the second law. This, however, did not lead him immediately to renounce the second law:

> Nevertheless, the second law may keep its validity here as well, and it is possibly only its proof that must be derived in a different way. (Loschmidt, 1869, p. 406).[12]

But, in an article from 1876, Loschmidt brought up more problems for the possibility of giving a mechanistic account of the second law. The article dealt with the temperature of a column of gas in a gravitational force field, a problem about which there was a debate at the time: in 1866, Maxwell had derived on the basis of the kinetic theory of gases that the temperature in a column of gas subjected to gravity is uniform (Brush, 1976, p. 149), but Loschmidt thought that Maxwell's derivation was incorrect and put forward a (not very convincing) argument to show that the temperature actually decreases with height. He also showed that if this is the case, a system can be devised in which there is a continuous heat flow, which means that the empirical principles of Thomson and Clausius (version (2) of the second law) are violated. This violation does not mean that the second law itself is overturned: version (5) of the

[12] "Nichtsdestoweniger mag auch hier der zweite Hauptsatz seine Giltigkeit behaupten, und es ist vielleicht nur der Beweis, welcher da auf eine andere Weise geführt werden müsste."

second law still holds. But Loschmidt was glad with the result that Thomson's and Clausius empirical principles can be violated:

> Thereby, also the terroristic nimbus of the second law would be destroyed, which makes it appear to be an annihilating principle for all life in the universe; and at the same time the comforting prospect would be opened up that mankind is not only dependent on mineral coal or the sun for transforming heat into work, but rather may have an inexhaustible supply of transformable heat available for all times. (Loschmidt, 1876, p. 135).[13]

More support for this claim was given by the reversibility problem. Loschmidt argued that one cannot be sure that a "so-called stationary state" of a gaseous system, when left undisturbed, will last forever. The case he considered was that of a container filled with gas in a homogeneous gravitational field, in which initially one atom is set in motion while all the others are at rest at the bottom of the container. The moving atom disturbs the other atoms until all are in motion in what appears to be a stable state of the gas. Suppose now that the velocities of all atoms are reversed at an instant. At first the gas will appear to remain close to its stationary state, but after a certain time, one single atom will have absorbed all kinetic energy while the other atoms lie still at the bottom.

> Clearly, in general the course of events must in any arbitrary system become reversed, if the velocities of all its elements are reversed at an instant. (Loschmidt, 1876, p. 139).[14]

Ironically, this remark has become famous as an objection to the kinetic theory of gases, but what Loschmidt intended to show with it was quite the opposite: he intended to support his case against the versions of the second law which involved irreversibility, notably against version (3) of the second law, according to which there is an irreversible transformation of mechanical energy into heat, which leads to energy sources running out and ultimately to heat death. While other physicists based their derivations of the second law on the assumption of the impossibility of endless supplies of transformable heat, Loschmidt used the kinetic theory of gases to undermine this assumption. Motivated by a dislike of the prospect of heat death, he made an extreme choice in the conflict between mechanism and irreversibility.

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[13] "Damit wäre auch der terroristische Nimbus des zweiten Hauptsatzes zerstört, welcher ihn als vernichtendes Princip des gesammten Lebens des Universums erscheinen lässt, und zugleich würde die tröstliche Perspective eröffnet, dass das Menschengeschlecht betreffs der Umsetzung von Wärme in Arbeit nicht einzig auf die Intervention der Steinkohle oder der Sonne angewiesen ist, sondern für alle Zeiten einen unerschöpflichen Vorrath verwandelbarer Wärme zur Verfügung haben werde."

[14] "Offenbar muss ganz allgemein, in jedem beliebigen System, der gesammte Verlauf der Begebenheiten rückläufig werden, wenn momentan die Geschwindigkeiten aller seiner Elemente ungekehrt werden."

It is remarkable that both Maxwell's demon and the reversibility problem have appeared seemingly independently in the work of Maxwell and Loschmidt. Porter (1986, p. 211) remarks that Loschmidt invented the reversibility argument independently of Maxwell and Thomson; and while Maxwell first conceived his demon in 1867 and only published about it in his *Theory of Heat* (1871), a similar thought experiment already appeared in the Loschmidt's work in 1869. There are no indications of any contact between them.

4.7 Reversibility and the nature of time

Not all physicists who discussed the reversibility of mechanical processes recognized a conflict between mechanism and irreversibility. G. Johnstone Stoney was an Irish physicist who had some influence in the kinetic theory of gases and worked on atomic structure; he is best known for introducing the name "electron" and estimating its magnitude in 1874 (Brush, 1976, p. 199). In 1887, he published an article about reversibility of mechanical processes, in which he gave a vivid description of the kind of phenomena that could be observed if the velocities of all the particles in the universe were reversed at an instant, if it were possible to observe these phenomena as it were from a standpoint outside of the system of the universe:

> The bird which was shot to-day by the sportsman, and which is now lying in his kitchen, will, if the reversal of the universe were to take place at this instant, be restored by the keeper to his gamebag, will be carried by him, walking backwards, to the place where the pointer had fetched it in, where he will take it out, and lay it on the ground. Thence the dog will lift it in his mouth, and, trotting backwards, will reach the spot where the bird fell, where, however, it will now rise to the height at which it was shot, from which it will fly away backwards unharmed. Meanwhile, the vapours into which the powder had been dissipated will stream back into the barrel of the fowling-piece, and condense themselves again into gunpowder, while the grains of shot will rush towards the muzzle of the gun, and crowd into its breach (Stoney, 1887, p. 544).

Stoney worked out the thought experiment in order to clarify some physical concepts. The contemplation of reversed processes makes clear that there is a distinction between two types of laws of nature: the truly dynamical laws which remain valid in their original form in the reversed world, for example the law of conservation of energy, and the "quasi-dynamical" laws which do not remain valid, such as the second law of thermodynamics. Furthermore, a distinction can be made between true causes and quasi-causes: true causes are those that instantaneously produce their effect and thus occur simultaneously with it, so that reversal leaves the causal relation intact, while for quasi-causes, cause and effect become reversed through the reversal of processes.

For a further insight, Stoney argues that our thoughts are in fact in the same way susceptible to reversal as any physical event is. Thus a complete reversal of all processes in the universe would reverse the observer's thoughts and memories as well. While watching the reversed universe, one would see nothing special—one would not even notice that all processes occur in the wrong time direction, Stoney argues. Thus, according to him, the direction in which we experience time to pass depends on the direction in which processes take place (Stoney, p. 546). This idea was later expressed by Boltzmann (1897, p. 416), probably independently, and has since then been widely discussed; it has always been attributed to Boltzmann.

According to Stoney, the thought experiment of reversal gives an important insight in the nature of time, namely that time is only an abstraction and does not exist independently of the individual time-relations between thoughts and between events. These time-relations remain unaffected by the reversal, so it is not only impossible to observe a difference between the actual and the reversed world, but in fact, there is no difference.

So Stoney used the thought experiment of reversal to obtain a clarification of the concepts of true dynamical laws and true causes, and to argue that time is only an abstraction. In his discussion of dynamical laws, he mentioned the second law of thermodynamics as a "quasi-dynamical" law, but did not draw any conclusions about its validity. He did not describe the reversibility problem as problematic for either the second law or the kinetic theory of gases; instead he used it to develop some interesting new ideas in philosophy of physics.

4.8 Attempts at a mechanical derivation of the second law

The reversibility problem showed that, within the kinetic theory of gases, the reversal of each process is possible and thus the second law can theoretically be violated. Maxwell and Thomson had drawn the conclusion that the second law could only have statistical validity; Loschmidt argued that it should be rejected altogether. In any case, these considerations seemed to make clear that it was not possible to give a mechanical derivation of the second law. Yet, attempts in this direction were made during the period. In this section, I give a very brief discussion of the attempts to derive the second law within the kinetic theory of gases. This is a topic that has already been well described in the literature; for a fuller treatment, see Klein (1973), Kuhn (1978), Cercignani (1998), and Uffink (2007).

There had been attempts at giving a mechanical derivation of the second law of thermodynamics since the 1860's. Some of the earliest attempts, by Boltzmann, Clausius and Szily, concentrated on version (5) of the second law, which did not involve irreversibility, and for which the reversibility problem thus posed no problem (see Klein, 1972; Bierhalter, 1992). In 1872, Boltzmann made a first attempt to give a mechanical derivation of irreversible processes in thermodynamics (Boltzmann, 1872). He derived an expression which he thought to be analogous to the law of increase of entropy (version (1) of the second law), namely that there is a function, later to be named *H*, for which

$$\frac{dH(f_t)}{dt} \leq 0$$

That is, *H* may decrease but cannot increase in time. This inequality becomes an equality if and only if an equilibrium distribution is reached. By identifying *H* with negative entropy, it appears that when a system approaches equilibrium, this approach is accompanied by an increase of entropy, and that this is an irreversible process.

Boltzmann believed he had given a rigorous derivation of his *H*-theorem; the question remains whether this derivation was thoroughly mechanical. One reason we may doubt that the derivation was mechanical is that it depended on probability considerations. But Uffink has pointed out that Boltzmann used a frequentist conception of probability, defining the probability that the state of a particle lies within a certain range simply as the relative number of particles whose state lies within that range, and that therefore, the probabilities that are used "can be fully expressed in mechanical terms" (Uffink, 2007, p. 967).

Boltzmann was confronted with the reversibility problem when his good friend Loschmidt discussed the reversibility of mechanical processes in his 1876 article. Boltzmann immediately realized how problematic this issue was for his *H*-theorem. He wrote a reaction (Boltzmann, 1877) in which he gave a clear formulation of the reversibility problem, and pointed out that it leads to the conclusion that any attempt to give a general mechanical derivation, independent of initial conditions, of the fact that entropy can only increase "must necessarily be futile" (Boltzmann, 1877, p. 365). Yet he immediately added: "One sees that this conclusion has great seductiveness and that one must call it an interesting sophism", and subsequently set out to find the "source of the fallacy in this argument". This he did by arguing, like Maxwell and Thomson, that the second law of thermodynamics has a statistical validity, and that although processes in which entropy decreases are theoretically possible, increase of entropy is much more likely than decrease. And, like Maxwell and Thomson, Boltzmann did not seem to perceive this new interpretation of the second law as a weakening of it:

it was still a reliable statement, though it was a statistical instead of absolute law of nature (Boltzmann, 1877, p. 366).

However, giving a mechanical derivation of the statement that entropy is more likely to increase than to decrease also turned out to be problematic. The reversibility problem shows that for all initial conditions leading to a process of increase of entropy, other initial conditions can be found which lead to a decrease of entropy; thus, to derive a statistical version of the second law one had either to show or to assume that certain initial conditions, although physically possible, were less probable than others. It was disputable whether one could still claim to have given a mechanical derivation of the second law if the derivation depended on such assumptions. The German mathematician Ernst Zermelo argued against Boltzmann that as long as you cannot explain *why* certain initial conditions are less probable than others, you have actually proven nothing: "as long as one cannot make comprehensible the *physical origin* of the initial state, one must merely assume what one wants to prove" (Zermelo, 1896b, p. 409). To give some plausibility to his claim that certain initial conditions were more probable than others, Boltzmann put forward the argument that the universe as a whole was initially in a very exceptional low-entropy condition and is now still in a process of increase of entropy, and that systems which at a certain moment become isolated from the rest of the universe usually have an exceptionally low-entropy initial condition as well, so that their entropy is more likely to increase than to decrease. Through this assumption the second law can be "explained mechanically". The low-entropy initial state of the universe then remains unexplained, but Boltzmann argued that "one can never expect that the explanatory principle must itself be explained" (Boltzmann, 1897, p. 413). Whether this was a satisfactory mechanical foundation of the second law still remained a point for discussion.

After Boltzmann's claim that the second law had only statistical validity in (1877), the status of his *H*-theorem remained uncertain for some period. It was supposed to be a purely mechanical and rigorous derivation of the law of increase of entropy, but this was something that Boltzmann no longer thought to be possible. Yet, at the beginning of the 1890's, the *H*-theorem was still sometimes used without reservations, for example in Burbury (1890, p. 299ff) and Watson (1893, p. 42ff). For those who did realize that there could be no strictly mechanical derivation of the *H*-theorem, it was unclear what could be wrong with the derivation that Boltzmann had given.

This matter was resolved in the 1890's in a debate in *Nature* between a number of physicists, namely Burbury, Bryan, Culverwell, Larmor, Watson, and Boltzmann (see Dias, 1994). It became clear that Boltzmann's derivation of the *H*-theorem depended on an assumption about collisions that was not time symmetric, an assumption that is now known as the Stosszahlansatz. It says that for any two colliding molecules the initial velocities are independent, while the velocities after the collision may be correlated. One has to assume that this holds in order to prove the *H*-theorem, but if it holds for a certain process, then it fails for the reversed process. The question of where time asymmetry entered in the derivation of the *H*-theorem was now solved. Moreover, one could argue that the Stosszahlansatz does hold in ordinary gases, and under this assumption, it is possible to give a strict and rigorous derivation of the *H*-theorem. Boltzmann did exactly this in his *Vorlesungen über Gastheorie* (1896, p. 38): he argued that the Stosszahlansatz holds in any system that is "molecularly disordered", and that molecular disorder is the natural state of gases (see also Kuhn, 1978, p.66).

One may think that the assumption of molecular disorder amounts to the introduction of a fundamental randomness, but Kuhn (1978, pp. 66-67) points out that this is not correct. If a molecular distribution were picked completely at random, there would be a non-zero probability that it would accidentally be "ordered" and in this case the Stosszahlansatz would not hold. The assumption of molecular disorder instead amounts to the assumption that "ordered" states do not occur at all. We can see that this assumption is quite unnatural. Shortly after its introduction, the assumption was criticized by Zermelo, who argued that while molecular disorder may be acceptable as an assumption about the initial state of a system, one cannot simply assume that it also holds for later states because these later states are determined by the initial state, so whether it holds or not needs to be calculated (Zermelo, 1896b, p. 410).

Thus, it turned out that one could derive either a statistical version of the second law through assumptions about low-entropy initial conditions, or an absolute version of the second law through the assumption of molecular disorder. Both types of assumption were controversial and, in both cases, it was unclear whether the result was a genuine mechanical derivation of the second law. Reconciling mechanism and irreversibility therefore remained problematic.

4.9 Reversibility and the decline of the mechanistic world view

In Britain, the discussion about the reversibility problem centred on the question of what kind of assumptions should be adopted in order to account for the second law within the kinetic theory of gases: statistical considerations, assumptions about molecular disorder or assumptions about the aether, the latter being proposed by E. P. Culverwell in 1890 as a possible way to account for irreversibility (Culverwell, 1890). Though these different possibilities could have consequences for whether the second law should be regarded as an absolute or statistical law of nature, and for the extent to which the kinetic theory of gases remained mechanistic, neither the second law nor mechanism were questioned in these discussions. When a committee that was appointed by the British Association to investigate "the present state of our knowledge of Thermodynamics, specially with regard to the Second Law" delivered its first report, in 1891, it expressed satisfaction with Boltzmann's statistical explanation of the second law (Bryan, 1892).

Meanwhile, the developments on the continent were rather different. There, in the 1890's, a number of physicists used the reversibility problem to argue against mechanism in physics. In the remainder of this paper, I will focus on their arguments.

Towards the end of the nineteenth century, mechanism in physics received increasing criticism. Mechanical models had played a major role in physics throughout the nineteenth century, but now many physicists such as Duhem and Poincaré felt that certain mechanistic theories had become too complex, involving purely hypothetical yet overly-detailed mechanical models, which were not as fruitful as one would wish. Critics of the mechanistic world view argued that it was better to base science on empirical principles than on speculative mechanistic hypotheses. A key influence was Ernst Mach, who had argued in 1872 that physics should limit itself to describing the "knowledge of the connections between observable phenomena" (Mach, [1872] 1909, pp. 25-26).[15]

As an example of a mechanical theory that had become too speculative and too little fruitful, the kinetic theory of gases was often brought up. In a lecture he held in 1891, Max Planck said of the kinetic theory:

> With every attempt to build up the [kinetic] theory more elaborately, the difficulties have mounted in a serious way. Everyone who studies the works of the two investigators who probably have penetrated most deeply into the analysis of molecular motions, namely Maxwell and Boltzmann, will be unable to avoid the impression that the admirable expenditure of physical ingenuity and mathematical skill that they have shown in their attempts to master these problems are not in proportion to the fruitfulness of the results achieved. (Planck, 1891a, p. 373).[16]

In 1895 the French physicist and philosopher of science Pierre Duhem wrote that while the kinetic theory of gases had originally been received with high expectations,

[15] "Erkenntniss des Zusammenhanges der Erscheinungen"

[16] "...bei jedem Versuch, diese Theorie sorgfältiger auszubauen, haben sich die Schwierigkeiten in bedenklicher Weise gehäuft. Jeder, der die Arbeiten derjenigen beiden Forscher studiert, die wohl am tiefsten in die Analyse der Molekularbewegungen eingedrungen sind: Maxwell und Boltzmann, wird sich des Eindrucks nicht erwehren können, dass der bei der Bewältigung dieser Probleme zu Tage getretene bewunderungswürdige Aufwand von physikalischem Scharfsinn und mathematischer Geschicklichkeit nichts im wünschenswerten Verhältnis steht zu der Fruchtbarkeit der gewonnenen Resultate."

currently disappointment dominated (Duhem, 1895, pp. 852-853). The kinetic theory of gases had started from a quite general idea, namely that heat is a form of motion, but it had grown into a detailed theory to which had been added many assumptions about things that could not be known. Duhem complained that the kinetic theory of gases led to few new results that couldn't have been derived in pure thermodynamics.

In addition, Duhem argued that there were problems with the kinetic theory of gases that still remained unsolved. One was that the theory led to a prediction for the ratio between the specific heat of a gas under constant pressure and the specific heat under constant volume, and that this value was different from the value that was found experimentally (Duhem 1895, p. 862). This "specific heats problem" had appeared already in an early stage of the kinetic theory: Maxwell had mentioned it in 1860 and had said that it "overturns the whole hypothesis" of the mechanical nature of heat (Maxwell, 1860, p. 660). In the following years various molecular models had been proposed to account for the experimentally obtained ratio of specific heats, but none had become generally accepted, and the problem remained unsolved until the 20th century.

Another problem with the kinetic theory of gases was the problem with accounting for the second law of thermodynamics. In 1892, Duhem had argued that it may well turn out to be impossible to reduce all physical notions and laws to mechanical concepts. He gives an analogy of an artist who can only make pencil sketches: for such an artist it is impossible to represent colour. "Is it not for an analogous reason that the most complex mechanical theories have not been able, up to now, to give a satisfactory account of Carnot's principle?" (Duhem, 1892, pp. 156-157).[17]

In 1895, Duhem wrote that despite the efforts that had been made in the kinetic theory of gases by among others Clausius and Boltzmann, the theory was ultimately not very successful exactly because it could not give an account of irreversibility:

> ...without entering into technical details, which would be out of place in this study, let us acknowledge that they were able to connect the laws of dynamics to the properties of reversible transformations, though not without giving rise to certain criticisms and certain objections; but let us admit that so far, they have failed to account for the properties of irreversible processes – that is, all actual processes. (Duhem, 1895, p. 852).[18]

[17] "N'est-ce pas pour une raison analogue que les théories mécaniques les plus complexes n'ont pu, jusqu'ici, rendre un compte satisfaisant du principe de Carnot?" Translation: Duhem (1996), pp. 13-14.

[18] "sans entrer dans des détails techniques qui ne seraient pas de mise en cette étude, reconnaissons qu'elles sont parvenues à rattacher aux lois de la dynamique les propriétés des transformations réversibles, non sans

Duhem contrasted the general disappointment about the kinetic theory of gases with the success of thermodynamics. Thermodynamics became popular around this time as a phenomenological domain of physics, based solidly on observable phenomena and empirical principles. According to Duhem, thermodynamics "had reached maturity and constitutional vigor when mechanical models and kinetic hypotheses came to give it assistance for which it did not ask, with which it had nothing to do, and to which it owed nothing" (Duhem, [1906] 1997, p. 139).[19]

But one has to note that most physicists did acknowledge that the kinetic theory of gases had been very successful in the recent past, and that despite the negative statements made about it, its popularity in practice was probably less damaged than it now seems. Work on the kinetic theory of gases continued throughout the 1890's. Boltzmann, the main proponent of the kinetic theory, greatly complained about the lack of recognition he received during these years; but Uffink (2004) has pointed out that he was in fact a well-respected and much-honoured theoretical physicist. And, in general, those who criticized mechanism often didn't completely condemn it but acknowledged that mechanical pictures and analogies could be useful as a means of research, though one should be careful not to adopt them as truths.[20]

4.10 Reversibility and recurrence

In (1893), Henri Poincaré argued that reversibility, which is "a necessary consequence of all mechanistic hypotheses", is in contradiction with our daily experience of irreversible phenomena. If this difficulty is not overcome, he argued, it means a definite condemnation of the mechanistic world view. And to strengthen this point he put forward a second, completely new, argument for the incompatibility between mechanism and irreversibility, now known as the recurrence objection. The argument is based on a theorem that he had derived a few years earlier, which says that any bounded dynamical system will, after a certain time (which may be extremely long), almost certainly return to a state that is arbitrarily close to its initial state. Poincaré had

donner prise à quelques critiques et à quelques objections; mais avouons qu'elles n'ont pu, jusqu'ici, rendre compte des propriétés des modifications non réversibles – c'est-à-dire de toutes les modifications réelles."

[19] "était adulte et vigoureusement constituée lorsque les modèles mécaniques et les hypothèses cinétiques sont venues lui apporter un concours qu'elle ne réclamait point, dont elle n'avait que faire et dont elle n'a tiré aucun parti."

[20] See for example Mach (1896, p. 362), Poincaré ([1905] 1952, p. xvi).

derived this theorem in order to prove that our solar system is stable: he could show with this theorem that it is practically certain that the sun, earth and moon will return to positions close to their current position for infinitely many times to come. But he later realized that the theorem also applies to a gas conceived as a mechanical system consisting of molecules: it then says that this system will practically always return to its initial state, which is contrary to the expectation that it will evolve towards a stable and permanent equilibrium state. And if you regard the universe as a whole as a mechanical system, it follows from the recurrence theorem that the universe cannot be irreversibly approaching a state of heat death.

In his *Thermodynamique* (1892a), Poincaré had deliberately ignored Boltzmann's statistical explanation of the second law, and when Peter Guthrie Tait criticized him in a review for this omission (Tait, 1892), Poincaré responded that the omission was made on purpose:

> I have not spoken of this explanation, which by the way seems to me hardly satisfactory, because I wanted to stay completely outside of all molecular hypotheses however ingenious they might be; and in particular I passed over the kinetic theory of gases in silence. (Poincaré, 1892b).[21]

But in (1893), Poincaré did mention the statistical interpretation of the second law that had been proposed by "the English", calling it the most serious attempt so far to reconcile mechanism and experience (Poincaré, 1893, p. 379). Poincaré did not seem to expect that "the English" would be impressed by the recurrence theorem: they can still argue that the universe is approaching a state of heat death, as long as they admit that this state is not an everlasting final state, but "a sort of slumber, from which it will awake after millions of millions of centuries." Poincaré admitted that this reasoning would be consistent with both the recurrence theorem and experience. He continues:

> According to this theory, to see heat pass from a cold body to a warm one, it will not be necessary to have the acute vision, the intelligence, and the dexterity of Maxwell's demon; it will suffice to have a little patience.
> One would like to be able to stop at this point and hope that some day the telescope will show us a world in the process of waking up, where the laws of thermodynamics are reversed (Poincaré, 1893, p. 380).

As Brown, Myrvold and Uffink (2009, p. 178) remark, "It is hard not to wonder whether there is a hint of irony here on Poincaré's part". Poincaré did not think he had a

[21] "Je n'ai pas parlé de cette explication, qui me parâit d'ailleurs assez peu satisfaisante, parce que je désirais rester complètement en dehors de toutes les hypothèses moléculaires quelque ingénieuses qu'elles puissent être; et en particulier j'ai passé sous silence la théorie cinétique des gaz".

definitive answer to "the English", a definitive way to show that mechanism could not account for the irreversibility that we observe. Nevertheless, in the conflict between mechanism and irreversibility he clearly chose in favour of the latter. Whether or not his argument was definitive, it was clear to Poincaré what the most logical conclusion was:

> ...there is no need for a long discussion in order to challenge an argument of which the premises are apparently in contradiction with the conclusion, where one finds in effect reversibility in the premises and irreversibility in the conclusion (Poincaré, 1893, p. 380).

A few years later the German mathematician Ernst Zermelo, famous for his work on set theory, also applied Poincaré's recurrence theorem to the issue of mechanism and irreversibility. At this time he was an assistant of Planck, and he agreed with Planck that the second law needed to have an absolute validity rather than a mere statistical validity. While Zermelo had read about Poincaré's theorem in relation to the stability of the solar system, he was actually unaware of the fact that Poincaré had also applied it to the kinetic theory of gases, and even remarked that although Poincaré must certainly be interested in this issue he "does not seem to have noticed [the applicability of his theorem] to systems of molecules or atoms and thus to the mechanical theory of heat" (Zermelo, 1896a, p. 383).

Zermelo's treatment of the recurrence theorem in relation to the kinetic theory of gases was more thorough than that of Poincaré. He noted that the recurrence theorem showed that for almost all initial states of a mechanical system, the system will return, after a certain time, to a state that is arbitrarily close to its initial state (provided that positions and velocities cannot extend to infinity), and there can therefore be no irreversible processes and no monotonically increasing entropy function. There are a few initial states for which the system does not return to its initial state and for which irreversible processes are possible, but these are singular states whose number is "vanishingly small" compared to the others; in modern terminology, they form a set with measure zero. Zermelo did mention the possibility that only these singular states are "actually realized in nature", but he did not think that this assumption was justified. As we have seen in section 8, Zermelo thought that one could not simply assume that certain initial conditions did not occur as long as one could not explain *why* they did not occur.

> Though such an assumption would be irrefutable, it would hardly correspond to our requirement for causality; and in any case the spirit of the mechanical view of nature itself requires that we should always assume that all *imaginable* mechanical initial states are physically *possible*, at least within certain limits, and certainly we must allow those states that constitute an overwhelming majority and deviate by

> an arbitrarily small amount from the ones that actually occur (Zermelo, 1896a, p. 389).

According to Zermelo, Poincaré's recurrence theorem showed more convincingly than the reversibility argument that a mechanical derivation of the second law was impossible.[22] He concluded:

> It is now necessary to formulate either the Carnot-Clausius principle or the mechanical theory in an essentially different way, or else give up the latter theory altogether (Zermelo, 1896a, p. 390).

Apparently, giving up the former, the second law, was not an option. Just like Poincaré, he trusted the second law more than the kinetic theory of gases. In fact, like Poincaré, he already had an antipathy of mechanical explanations before he wrote about the recurrence theorem. Ebbinghaus (2007, p. 8) mentions that two years before Zermelo's article about the recurrence theorem was published, in 1894, he had obtained a Ph. D. in mathematics at the Friedrich Wilhelm University in Berlin and, as a part of the procedure, had to defend three theses of his own choice in an oral examination. The second thesis he chose was: "It is not justified to confront physics with the task of reducing all natural phenomena to the mechanics of atoms".[23] With the recurrence theorem, he found a new way to argue for this position.

4.11 Reversibility, the second law, and empirical principles

In the 1890's, there was still a certain degree of ambiguity about what exactly the second law of thermodynamics was, and thus the relevance of the reversibility problem to the second law was similarly unclear. Many of those who used the reversibility principle to argue against mechanism tended to claim, as did Zermelo, that mechanism failed because it couldn't account for the second law. Another example is František Wald, a relatively unknown and largely uninfluential chemist who had studied technical chemistry in Prague around 1880 and subsequently worked in industrial chemistry. In 1907 he became a professor at the Czech Technical University (Ruthenberg, 2007). In

[22] A comparison of the strength of the reversibility and recurrence objections is given in Brown, Myrvold and Uffink (2009, p. 181).

[23] "Mit Unrecht wird der Physik die Aufgabe gestellt, alle Naturerscheinungen auf Mechanik der Atome zurückzuführen".

1889, he published a book about the second law of thermodynamics, a law which according to him received too little recognition (Wald, 1889). Just like Loschmidt, Wald thought that the second law was counterintuitive in the context of the kinetic theory of gases, since, if heat is nothing more than a kind of motion, the only difference between heat and other kinds of motion is that the former is the unordered motion of molecules. This makes the second law equivalent to the proposition that unordered motion cannot be converted into ordered motion without compensation. But Wald suspected that there were many ways in which this proposition could be violated, for example through Maxwell's demon, who is able to convert unordered motion into ordered motion by manipulating individual molecules and can therefore convert heat into mechanical motion. According to Wald, this possibility would have enormous implications:

> A stone lying on the ground has molecular motion – ergo, it could go up in the air without external work being applied to it. Every train also contains enough heat without a heat engine – for what do we need heat engines? For what do we need machines or coal mines? We can find heat everywhere. (Wald, 1889, p. 104).[24]

Wald thought that the possibility of Maxwell's demon was fatal to the attempt to bring the second law into accordance with the kinetic theory of gases. Maxwell had argued that the demon could only violate the second law on a microscopic scale and that it is not possible for us to cause such a violation because we cannot observe and manipulate individual molecules. But Wald argued that it was rather ad hoc to introduce assumptions explicitly in order to make sure that Maxwell's demon cannot operate on an observable scale. Someone might come up with a new thought experiment in which the second law was violated, and then we can do nothing but putting forward the hypothesis that such a violation is not possible in practice. Such ad hoc assumptions were inadequate for the foundation of the second law, Wald thought. Thus, he criticized the kinetic theory of gases because it could not give a strong foundation for the second law of thermodynamics, which he valued highly as a law of nature.

But in the 1890's there were still treatments of the second law according to which it was inapplicable to irreversible processes. One example is the version presented in Mach (1892). Mach proposed generalizing the second law to the statement that "every conversion of a form of energy A is connected to a drop in potential of that form of energy" (p. 1598).[25] This statement does not involve irreversibility, and it is exactly for

[24] "Ein am Boden liegender Stein hat Molekularbewegung – ergo könnte er ohne äussere Arbeitsleistung in die Luft hinaufsteigen. Jeder Eisenbahnzug hat auch ohne Lokomotive genug Wärme – wozu brauchten wir Lokomotiven? Wozu Maschinen, wozu Kohlenwerke? Wärme finden wir überall".

[25] "jede Umwandlung einer Energieart A ist an einen Potentialfall dieser Energieart gebunden"

this reason that Mach thought that the second law could be generalized to be applicable to other domains of physics.

A few years later, in *Principien der Wärmelehre* (1896), Mach gave a different treatment of the second law, and this time he did connect it to irreversible increase of entropy. In a chapter titled "The opposition between mechanical and phenomenological physics",[26] Mach discussed the mechanical foundations for the second law of thermodynamics. He wrote:

> If one realizes that a real analogy of entropy increase in a purely mechanical system, consisting of absolutely elastic atoms, does not exist, one can hardly help thinking that a violation of the second law would have to be possible, also without the help of demons, if such a mechanical system were the real basis of thermal processes. (Mach, 1896, p.364).[27]

Mach made clear that the problem with accounting for entropy increase in mechanistic theories was a problem for mechanism, while the principle of increase of entropy remained well-established as an empirical principle. According to Mach, the existence of irreversible processes was essential for our notion of time. Suppose that all energy conversions could also occur in the reverse direction and all processes could be reversed, "Then time itself would be reversible, or rather, the notion of time could not have arisen" (Mach, 1896, p. 338).[28] It was not the second law specifically that was important for Mach, but empirical principles in general; empirical principles should form the foundations of physics.

4.12 Reversibility and energetics

Energetics was a movement that emerged in the 1890's with the goal of unifying natural science by founding it upon the energy concept. It was meant to provide an alternative to the mechanistic world view and intended to be a phenomenological and anti-hypothetical science (Nyhof, 1988, p. 90ff; Deltete, 2007a, p. 6ff). For Wilhelm Ostwald,

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[26] "Der Gegensatz zwischen der mechanischen und der phänomenologischen Physik"

[27] "Bedenkt man, dass ein wirkliches Analogon der Entropievermehrung in einem rein mechanischen System aus absolute elastischen Atomen nicht existirt, so kann man sich kaum des Gedankens erwehren, dass eine Durchbrechung des zweiten Hauptsatzes - auch ohne Hülfe von Dämonen - möglich sein müsste, wenn ein solches mechanisches System die wirkliche Grundlage der Wärmevorgänge wäre."

[28] "Dann wäre die Zeit selbst umkehrbar, oder vielmehr, die Vorstellung der Zeit hätte gar nicht entstehen können".

the main proponent of energetics, the concept of energy was far more important than the concept of entropy, and this led him to devise his own versions of the second law in terms of energy. In the course of time, he gave various accounts of the second law of thermodynamics, which all centred on the question of under which conditions energy changes or conversions occur but were very different in other respects, as Deltete (2007b, p. 303) shows. One can well argue that Ostwald misunderstood the second law of thermodynamics. In a letter to Boltzmann from 1892, Ostwald wrote that the second law had nothing to do with dissipation of energy or with increase of entropy (Ostwald, 1892). He didn't think that there was a relevant principle of increase of entropy, and for Ostwald the second law was not even connected to irreversibility.

In a personal letter, Max Planck expressed his worries to Ostwald about the minor place that he gave to irreversibility in his theory of energetics:

> I look forward with great interest to the continuation of your science of energetics, but I cannot conceal my firm conviction that a comprehensive description of all natural processes cannot be made without the fundamental distinction between reversible and irreversible processes. (Planck, 1891b).[29]

Planck gradually lost patience with energetics, which, despite its strong claims of reforming natural science, did not lead to theoretical progress. In 1896 Planck published a firm critique titled "Against the more recent energetics"[30] (Planck, 1896; cf. Hiebert, 1971), in which he again emphasized that Ostwald's energetics failed to give an account of irreversibility. Therefore, he argued, the domain of energetics is limited to the study of reversible processes, which includes important parts of mechanics, electrodynamics and optics, but does not include chemistry or thermodynamics (ironically, since energetics was supposed to be anti-mechanistic and close to thermodynamics).

But while irreversibility did not play a central role in Ostwald's physics and specifically played no role in his versions of the second law, this did not mean that irreversibility was not an issue for him. In September 1895, in a lecture titled "The overcoming of scientific materialism"[31], Ostwald had even used the reversibility problem as an argument against mechanism. He emphasized that the mechanistic world view was incompatible with our daily experience of irreversible processes:

> ...the theoretically completely mechanical processes can equally well take place forwards as backwards. Therefore, in a completely mechanical world there can be

[29] "Der Fortsetzung Ihrer Energetik sehe ich mit großem Interesse entgegen, kann Ihnen aber meine feste Ueberzeugung nicht verhehlen, daß eine alle Naturprozesse umfassende Darstellung sich nicht ausführen lassen wird ohne die principielle Unterscheidung zwischen reversibeln u. irreversibeln Prozessen."

[30] "Gegen die neuere Energetik"

[31] "Die Überwindung des wissenschaftlichen Materialismus"

> no earlier or later in the sense of our world; a tree might turn back into a twig and into a seed, a butterfly might change into a caterpillar, an old man into a child. (Ostwald, 1895, p. 230).[32]

Ostwald drew a strong conclusion:

> Thus, the actual irreversibility of real natural phenomena proofs the existence of processes which cannot be derived from mechanical equations, and thereby the verdict of scientific materialism is spoken. (Ostwald, 1895, p. 230).[33]

This being said, Ostwald went further to discuss the more promising prospect of energetics. So he used the reversibility problem as a decisive argument against the mechanistic and materialistic world view. He did not argue that the reversibility of mechanical motion was in conflict with the second law; instead he argued that it was in conflict with our direct experience of irreversible processes, of growth and decay.

Ostwald's fellow energeticist Georg Helm was, however, more nuanced. Helm was a German physicist and mathematician, mainly known for being a proponent of energetics but otherwise little influential. Like many other physicists in the 1890's, Helm expressed a dislike for mechanism and the kinetic theory of gases. In his book *The historical development of energetics* (Helm, [1898] 2000), Helm argues that one should not strive for mechanical explanation of all physical phenomena. He adds that the use of mechanical analogies in science can, in certain cases, be quite fruitful, but one should realize that they are based on speculation and should not suppose them to be literally true. Helm complains that "It just seems everywhere to be the fate of mechanical hypotheses that they require too many accessories". In the end, the thermodynamic approach is "more perfect and consistent" than the mechanistic one, for this approach is simpler and needs fewer assumptions. (Helm, [1898] 2000, p. 381, 400; cf. Deltete, 1999).

The very last section of his book is titled "The limits of description by means of mechanical pictures" and begins with the statement that it is problematic to account for the second law of thermodynamics within a mechanistic theory, mainly because it is problematic to account for irreversible processes. However, Helm argues that this problem can be solved in a satisfactory manner through statistical considerations, and

[32] "...die theoretisch vollkommenen mechanischen Vorgänge können ebenso gut vorwärts wie rückwärts verlaufen. In einer rein mechanischen Welt gäbe es daher kein Früher oder Später im Sinne unserer Welt; es könnte der Baum wieder zum Reis und zum Samenkorn werden, der Schmetterling sich in die Raupe, der Greis in ein Kind verwandeln"

[33] "Die tatsächliche Nichtumkehrbarkeit der wirklichen Naturerscheinungen beweist also das Vorhandensein von Vorgängen, welche durch mechanische Gleichungen nicht darstellbar sind, und damit ist das Urteil des wissenschaftlichen Materialismus gesprochen"

he seems to be quite convinced by Boltzmann's statistical approach, even if it entails that the spontaneous reversal of an "irreversible" process is not impossible but merely highly unlikely:

> ...for the sake of the mechanical hypothesis, one must also accept into the bargain that the course of the world is occasionally reversed. And one must therefore also accept that, in the fullness of time, children will one day return to their mothers' wombs – if one wishes to have the proud feeling that child-bearing follows from conservative forces in accordance with Lagrange's differential equations.
> It would certainly be foolish and unjust to want to prove, with this absurdity, that the mechanical world-view is simply a failure. (Helm, [1898] 2000, p. 398).

It might be "foolish and unjust" to argue that mechanism was a failure because it could not account for irreversible processes; meanwhile, this was exactly what Ostwald had done only three years earlier and what also other authors such as Duhem, Poincaré and Zermelo had done. Helm was critical of mechanism, but he thought that the reversibility problem could not be used as an argument against it because the probability of a reversal might be so small that we actually never observe one. The reversibility problem is only an argument against mechanism or the kinetic theory of gases for someone who holds that the second law of thermodynamics is universally valid or that there are fundamentally irreversible processes, even though this conviction goes beyond empirical evidence.

4.13 Planck's problematic position

According to Max Planck, the second law was fundamental to the understanding of all irreversible processes.[34] In his dissertation from 1879, he had proposed that the entropy function could be regarded as the unique function determining the direction of natural processes: a spontaneous process is only possible in the direction in which entropy increases or remains equal, and not in the direction in which entropy decreases (Planck, 1879). With this in mind, Planck worked on extending the scope of the entropy function, allowing it to become, for example, applicable to chemical processes (see for example Planck, 1887b).

[34] According to Planck, a process is irreversible when, given the final state, it is not possible to restore the initial state, by whatever means.

But Planck was also committed to mechanism. Although he also warned against a dogmatic attachment to the mechanistic world view, in general he thought that mechanical explanation was an ideal to strive for in physics, and thought that this method "until so far has indeed everywhere splendidly been confirmed" (Planck, 1887a, p. 51; Kuhn, 1978, pp. 21-22)[35].

By insisting on maintaining both mechanism and irreversibility, Planck placed himself in a problematic position. As early as 1882 he had mentioned a tension between atomism and the second law:

> The second law of thermodynamics, logically developed, is incompatible with the assumption of finite atoms.[1] Hence it is to be expected that in the course of the further development of the theory, there will be a battle between these two hypotheses, which will cost one of them its life. It would be premature to predict the result of this battle with certainty; yet there seem to be at present many kinds of indications that in spite of the great successes of atomic theory up to now, it will finally have to be given up and one will have to decide in favor of the assumption of a continuous matter. (Planck, 1882, p. 475).[36]

The footnote (1) refers to Maxwell's *Theory of Heat* (Maxwell, [1871] 1970), in which Maxwell wrote that the second law "is undoubtedly true as long as we can deal with bodies only in mass and have no power of perceiving or handling the separate molecules of which they are made up", but that it might be violated by the demon. Apparently, it was not the reversibility problem but Maxwell's demon which led Planck to the conclusion that the universal validity of the second law was incompatible with the kinetic theory, and this was probably the reason he thought that it was specifically incompatible with atomism and not necessarily with mechanism. This made things easier for him: to save the second law, he did not have to give up mechanism but only atomism. And in this period he was not very enthusiastic about atomism anyway, since he thought atomic and molecular theories to be somewhat speculative. In fact, his scientific work did involve atoms and molecules, but, according to Heilbron (1986, p. 14), this was only because he found that "he could not stay at the forefront of

[35] "sich bisher in der That überall glänzend bestätigt hatt".

[36] "Der zweite Hauptsatz der mechanischen Wärmetheorie, consequent durchgeführt, ist unverträglich mit der Annahme endlicher Atome. Es ist daher vorauszusehen, dass es im Laufe der weiteren Entwicklung der Theorie zu einem Kampfe zwischen diesen beiden Hypothesen kommen wird, der einer von ihnen das Leben kostet. Das Resultat dieses Kampfes jetzt schon mit Bestimmtheit voraussagen zu wollen, wäre allerdings verfrüht, indeß scheinen mir augenblicklich verschiedenartige Anzeichen darauf hinzudeuten, daß man trotz der großen bisherigen Erfolge der atomistischen Theorie sich schließlich doch noch einmal zu einer Aufgabe derselben und zur Annahme einer continuirlichen Materie wird entschließen müssen."

thermochemistry without recourse to a molecular view of matter", despite his hopes that it would become possible at some point to avoid such molecular hypotheses.

In a letter to his friend Leo Graetz, in 1897, Planck complained that the kinetic theory of gases could not account for irreversibility and was therefore incapable of providing a foundation for the second law of thermodynamics.[37] He was opposed to a statistical interpretation of the second law; this law had to be a universal principle, a true law of nature, therefore its validity could not be merely highly probable.

He was also opposed to Zermelo's view that the second law was in contradiction with mechanism in general.[38] Contrary to Zermelo, Planck still had hopes for reconciliation between mechanics and the second law, and his hopes were directed specifically towards theories of continuous matter instead of molecular theories like the kinetic gas theory.

Between 1897 and 1900, Planck worked on a possible new explanation of irreversibility in the domain of electromagnetism. It was Boltzmann who pointed out to Planck that electromagnetism is based on time-reversible laws, just like the kinetic theory of gases and mechanistic theories in general, and that it is therefore impossible to derive strictly irreversible processes within either of these theories without making time-asymmetric assumptions (Kuhn, 1978, p.77; Needell, 1980). After Boltzmann's criticism, Planck did come up with a new theory about irreversibility in the domain of electrodynamics, based on the hypothesis of "natural radiation". This hypothesis, which Planck proposed in the context of cavity radiation (also called black-body radiation), was in fact analogous to the assumption of molecular disorder that Boltzmann had made in 1896, as Kuhn (1978, p. 80) notes. In both cases, it was assumed that certain quantities were disordered in order to prove the existence of fundamentally irreversible processes; but it was unclear whether this assumption was justified.

We have seen that in the conflict between mechanism and irreversibility, different physicists made different choices depending on the value they attached to mechanism and irreversibility respectively. Planck was in a problematic position because he highly valued both solid mechanical foundations and the absolute validity of the second law of thermodynamics. He tried hard to reconcile these two convictions. Searching for an alternative explanation for irreversible processes, Planck arrived at an explanation similar to one Boltzmann had used earlier in the context of the kinetic theory of gases. But his attempts to account for irreversibility resulted in new and important work: in 1900, Planck introduced his famous quantum constant in the context of cavity radiation.

[37] Max Planck to Leo Graetz, May 23, 1897. The relevant part of the letter is printed in Kuhn (1978), pp. 265-266.
[38] Ibid.

4.14 The reasons for anti-mechanism

On the continent, mechanism and atomism received increasing criticism towards the end of the nineteenth century. There has been some debate in the literature about whether this decrease in popularity of mechanism and atomism was motivated primarily by philosophical concerns, such as empiricism and the wish to avoid speculation about unobservable entities, or by physical concerns, such as the lack of progress made in the kinetic theory of gases and the problems with this theory such as its inability to account for the second law and the specific heats problem.

Clark (1976) has proposed a view based on the methodology of "research programs" described by Imre Lakatos. Clark argues that thermodynamics and the kinetic theory of gases were distinct, rival research programs and that "after some early notable successes", by the 1890's, the kinetic theory of gases was degenerating and no longer progressive. According to Clark, the kinetic theory lost popularity because of scientific difficulties and not because of philosophical preferences. The rival research program of thermodynamics was simply more successful and therefore gained popularity at the expense of the kinetic theory of gases.

Nyhof (1988) has defended the older view that philosophical concerns played the main role in the decline in popularity of the kinetic theory. He argues that the kinetic theory and thermodynamics are "explanans and explanandum respectively" and therefore "cannot be scientific rivals": the kinetic theory is intended to be a mechanical *foundation* for thermodynamics, so when thermodynamics is successful, this success should not harm the kinetic theory (Nyhof, 1988, p. 93). That is, unless one already has doubts about the usefulness of the attempt to give a mechanical foundation for thermodynamics—doubts which generally stem from philosophy of science. The fact that the success of thermodynamics was indeed often used as an argument against the kinetic theory of gases shows that such 'philosophical' doubts were common.

The truth probably lies somewhere in the middle: it is true that there were important physical problems with the kinetic theory of gases which played a role in its decline in popularity, such as the specific heats problem, but at the same time Nyhof is correct in pointing out that the anti-mechanism of, for example, Duhem and Mach was mainly philosophically motivated. An alternative road has been taken by De Regt (1996) who argued that the philosophical views of the participants in the debate influenced their scientific work, so that philosophical and physical factors were in fact intertwined. Furthermore, thermodynamics and the kinetic theory of gases were not as radically separated as this literature may suggest, and figures such as Gibbs, Boltzmann and Clausius made important contributions to both.

What role does the reversibility problem play in this discussion? Clark supports his claim by arguing that one of the reasons the kinetic theory lost popularity was that it could not account for the universal validity of the second law, and he brings this up along with the specific heats problem as the main physical problems connected to the kinetic theory (Clark, 1976, p. 43, 81). As we have seen, it is true that the reversibility problem was raised as a physical objection to the kinetic theory by people such as Duhem, Poincaré and Zermelo. But in fact, whether one regarded the failure of kinetic theory to account for the second law as problematic for the kinetic theory was itself highly dependent on philosophical considerations. The same problem could also be used as an argument against the second law, as Loschmidt had done, or it could be handled through a statistical approach, as Maxwell, Thomson and Boltzmann had done. It was the widespread, philosophically motivated anti-mechanism in the 1890's that determined the interpretation of the reversibility problem as a problem for the kinetic theory of gases. Therefore, when arguing that the decline in popularity of the kinetic theory of gases was motivated primarily by physical concerns, one cannot use the reversibility problem as an example.

Conclusion

The reversibility problem was connected with many different issues in physics and philosophy of science. In the first years after the appearance of the reversibility problem, the main factors determining its interpretation were aversions to materialism (Maxwell, Thomson, Breton) and to the idea of the heat death of the universe (Loschmidt), and the development of the idea of statistical laws of nature (Maxwell, Thomson, Boltzmann). In the last decade of the nineteenth century, especially in Germany and France, the discussion about the reversibility problem became primarily a discussion about the merits of mechanism versus empiricism in physics. Should we believe in the unrestricted validity of our laws of nature, should we base our science upon empirical principles, or should we aim at mechanistic reduction? Can thermodynamics stand on its own or does it need the kinetic theory of gases as a mechanical foundation? The answers to these questions determined the interpretation of the reversibility problem, and it was only when mechanism was losing favour in the 1890's that the reversibility problem came out as an objection against it: basically, the reversibility problem could be used as a physical objection to whatever one was philosophically opposed to.

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Paper 5 Continuity in nature and in mathematics: Boltzmann and Poincaré

The development of rigorous foundations of differential calculus in the course of the nineteenth century led to concerns among physicists about its applicability in physics. Through this development, differential calculus was made independent of empirical and intuitive notions of continuity, and based instead on mathematical conditions of continuity. According to both Boltzmann and Poincaré, the applicability of differential calculus in physics depends on whether these mathematical conditions of continuity can be given a foundation in experience or intuition. However, they reached opposite conclusions: Poincaré argued on the basis of a Kantian philosophy of mathematics that the continuity conditions that are at the basis of the calculus are well-founded in intuition, and that physicists must work with continuous representations of nature, while Boltzmann argued on the basis of an empiricist philosophy of mathematics that the acceptance of these continuity conditions in physics is problematic, and that physicists must ultimately take nature to be discrete.

5.1 Introduction

The development of rigorous foundations of differential calculus during the nineteenth century meant that differential calculus was made independent of empirical and intuitive notions of continuity, and was instead based on mathematically defined continuity conditions. This paper shows that although this development solved earlier problems regarding the consistency of the calculus, it also led to concerns regarding the applicability of differential calculus in physics. Around 1900, both Boltzmann and

Poincaré argued that if the axioms of mathematics have no foundation in experience or intuition, the applicability of mathematics to physical reality becomes problematic. Therefore, the applicability of differential calculus in physics depends for them on the extent to which the continuity conditions that are at the basis of differential calculus can be given a foundation in experience or intuition. However, they reach different conclusions: on the basis of an essentially Kantian philosophy of mathematics, Poincaré argues that these continuity conditions are warranted and that physics should work with continuous representations of nature, so that differential calculus is strictly applicable, while Boltzmann argues on the basis of an empiricist philosophy of mathematics that these continuity conditions are problematic, and that physicists should ultimately take nature to be discrete, although one can use continuous models as approximations.

For both Boltzmann and Poincaré, therefore, the issue of continuity versus discreteness in physics is entangled with philosophy of mathematics. This issue concerns the continuity of space and time as well as the issue of whether matter is atomistic (Wilholt (2002) has shown how Boltzmann's arguments for atomism are based on his philosophy of mathematics). Furthermore, at the time it was commonly thought that all fundamental laws of physics take the form of differential equations; thus, whether nature can be taken to be continuous was of central significance for the status of the fundamental laws of physics. As we will see, for Boltzmann and Poincaré, whether we work with continuous or discrete representations of nature also matters for the issue of determinism. Therefore, for both Boltzmann and Poincaré, philosophy of mathematics is of central relevance for each of these issues in physics.

One can make a distinction between two types of continuity that lie at the basis of differential calculus:

(1) Continuity of possible values: the requirement that variables take a continuous range of values, corresponding with the real numbers.
(2) Continuity of change: the requirement that the relations between these variables can be expressed by functions that are continuous and differentiable.

In this paper, after giving a very brief introduction to the history of differential calculus, I discuss both of these types of continuity in turn. I show that the development of rigorous foundations of analysis led to a formulation of both of these types of continuity that is independent of empirical or intuitive notions, and that this raised the question of whether they can safely be assumed in physics, and on what basis. I then show how for Boltzmann and Poincaré, the issue is decided on the basis of whether these types of continuity have a basis in experience or intuition, and how their different philosophies of mathematics lead them to opposite conclusions.

5.2 A short history of the calculus

Differential calculus is based on a notion of continuity: differential equations describe the way in which change in one variable continuously depends on change in another variable, and it is only when variables change continuously that one can describe this change in terms of a differential equation. During the eighteenth century, the notion of continuity on which differential calculus is based was rarely precisely articulated but essential; continuity was not well-defined within analysis or differential calculus, but rather imported from geometry, experience or intuition. There were various connections between the notion of continuity in mathematics and continuity in physics and in metaphysics (Schubring, 2005; Van Strien, 2014a). However, there was a lack of clear foundations of the calculus: the concept of limit was not well understood and there was a lack of clarity about the nature of infinitesimal quantities, which had to be smaller than any finite quantities.

During the course of the nineteenth century, among others Cauchy, Riemann and Weierstrass developed a rigorous foundation of the calculus: they developed a clear conception of limits, of continuous functions and differentiability, and were able to solve the foundational issues that had been present in eighteenth century calculus. Their aim was to make analysis independent of geometry and of any empirical intuitions. In the new rigorous formulation of the calculus, there was no longer a need for an implicit, intuitive notion of continuity; instead, the calculus was ultimately based on the continuity of the real number line, on the basis of which one could give a mathematical definition of continuity and differentiability of functions. The real number system, in turn, received a rigorous foundation through the work of among others Dedekind and Cantor in the late nineteenth century. After that, the calculus could be regarded as a consistent mathematical framework with a solid foundation.

So one might expect that around 1900, the former problems with the foundations of differential calculus had been fully solved. In (1905b), Poincaré remarks that "It is very difficult, for contemporary mathematicians, to understand the contradictions that our predecessors believed to discover in the principles of infinitesimal calculus" (Poincare, 1905b, p. 293).[1] However, the situation was less clear for the applicability of differential calculus in physics, since, now that differential calculus had been made fully independent of geometry, experience and intuition, it was not clear how it related to physical reality. As Poincaré writes in (1905b):

[1] "Il est très difficile, pour les mathématiciens contemporains, de comprendre les contradictions que nos devanciers croyaient découvrir dans les principes du calcul infinitésimal."

> ...it seems that by being arithmetized, by being idealized so to speak, mathematics has moved away from nature, and the philosopher can always ask whether the methods of differential and integral calculus, now fully justified from a logical point of view, can legitimately be applied to nature (Poincaré, 1905b, p. 293-4).[2]

In (1908, p. 435), Poincaré writes that through the rigorization of the calculus, the former problems have not disappeared, they "have only been moved to the frontier" and appear again when we wish to apply the calculus to nature.

In particular, in its rigorous formulation, differential calculus depends on two continuity conditions that were now fully mathematically defined, without reference to empirical or intuitive notions. The question now arose whether these continuity conditions could be accepted in physics, whether nature can be taken to be continuous in the required ways. As we will see, both Boltzmann and Poincaré argued that the applicability of differential calculus depends on whether these two continuity conditions have a basis in experience or intuition. In section (2), I discuss the first continuity condition, and in section (3) the second.

5.3 The mathematical continuum

5.3.1 The mathematical continuum and the foundations of analysis

The first type of continuity, required for the applicability of differential calculus in physics, is continuity in the range of values that physical quantities such as length, time and mass can take.

In the rigorous formulation of the calculus developed by Cauchy, Riemann and Weierstrass, the calculus is based on the real number system, which means that the range of values that the variables can take has to correspond to the real numbers (the rational numbers do not suffice).[3] In order to describe the change of one variable with respect to another by means of a differential equation, e. g. the change in position with

[2] "...il semble qu'en s'arithmétisant, en s'idéalisant pour ainsi dire, la mathématique s'éloignait de la nature et le philosophe peut toujours se demander si les procédés du calcul différentiel et intégral, aujourd'hui complètement justifiés au point de vue logique, peuvent être légitimement appliqués à la nature."

[3] The reason why the rational numbers do not suffice as a basis for the calculus is that a sequence of rational numbers may converge to an irrational limit, and it is essential for the foundations of the calculus that for any converging sequence, the limit point exists (this is needed among others in order to define continuous functions in terms of limits).

respect to time, one has to work with the assumption that these variables take continuous values, corresponding to (a part of) the real number line; thus, one has to assume that the real numbers correspond to nature.

The assumption that the continuum of the real numbers corresponds to nature was made less trivial by the work on the foundations of the real number system which appeared in the late nineteenth century. There were different ways to construct the real numbers, notably Cantor's construction of the real numbers through Cauchy sequences and Dedekind's construction through Dedekind cuts, and these constructions involve the acceptance of completed infinite sequences or sets (Kline, 1972, p. 982-87). Cantor showed that whereas the rational numbers form a countably infinite set, the real numbers form a set that is uncountably infinite. The continuum of the real numbers thus appeared as an elaborate mathematical construction, and even if one accepts that this construction is consistent and that the real numbers can be well-defined in pure mathematics, one can ask how they relate to physical reality.

Around 1900, the correspondence of the real numbers to nature was not altogether accepted as evident; for example, in (1896), Mach writes about the mathematical continuum of the real numbers:

> There are no objections against the *fiction* or the arbitrary conceptual construction of such a system.
> However, the natural scientist, who is not only doing pure mathematics, has to consider the question whether such a fiction also *corresponds* to something in *nature*? (Mach, 1896, p. 71).[4]

Mach concludes that the assumption that physical quantities take values which correspond to the mathematical continuum of the real numbers is an assumption which we can make as long as it is not in disagreement with experience.[5] For Boltzmann and Poincaré, the issue was a bit more complicated, as we will now see.

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[4] "Gegen die *Fiktion* oder die willkürliche begriffliche Construktion eines solchen Systems ist nichts einzuwenden.
Der Naturforscher, der nicht bloss reine Mathematik treibt, hat sich aber die Frage vorzulegen, ob einer solchen Fiktion auch in der *Natur* etwas *entspricht*?"

[5] Mach argues that the applicability of the mathematical continuum to physical quantities can be accepted as harmless, although fallible assumption about nature. He writes that it is at least thinkable, and compatible with experience, that in reality, there are discrete elements rather than a continuum. "Ueberall, wo wir ein Continuum vorzufinden glauben, heisst das nur, dass wir an den kleinsten wahrnehmbaren Theilen des betreffenden Systems noch analoge Beobachtungen anstellen und ein analoges Verhalten bemerken können, wie an grösseren. Wie weit sich dies fortsetzt, wird nur die Erfahrung entschieden können. So weit die Erfahrung noch keine Einsprache erhoben hat, können wir die in keiner Weise schädliche, sondern nur bequeme Fiktion des Continuums aufrecht halten." (Mach, 1896, p. 77).

5.3.2 Boltzmann on the continuum

In this subsection, I show that according to Boltzmann, the assumption that the values that variables in physics can take correspond to the continuum of the real numbers is unwarranted. This implies that in physics, differential equations should be taken to offer mere approximations. However, Boltzmann did make extensive use of differential calculus in his work in physics, and trusted it to give very good approximations. I first go into Boltzmann's motivation for thinking that we cannot take physical quantities to form a mathematical continuum, before giving a more detailed account of his thoughts on the applicability of differential calculus.

As Wilholt (2002) has shown, Boltzmann's main motivation for his concerns about the applicability of differential calculus in physics was his concern about the notion of infinity. Boltzmann emphasizes at various points in his writings that in physics, infinity means nothing more than a transition to a limit, and that there is no actual infinity in nature (e.g. in Boltzmann, 1877, p. 167; 1904, p. 358). Wilholt (2002) argues that Boltzmann's distrust of infinity is connected to his anti-logicism and empiricism in mathematics. Boltzmann discussed his philosophy of mathematics in a series of lectures on natural philosophy that he gave between 1903 and 1905 (replacing Mach); his own fragmentary notes for these lectures were published in 1990, together with a complete manuscript of some of the lectures that was probably worked out by a student or an assistant. Boltzmann's empiricism in philosophy of mathematics consists in the fact that he argued that the basic concepts and theorems in mathematics must ultimately be grounded in experience. Regarding the foundations of arithmetic, he states that it is hard to say to what degree the concept of number is derived from experience and to what degree it is an a priori conception, but even if it is an a priori conception, this does not mean that it offers absolute certainty, it merely means that it is an innate conception, of which the origin can be explained in terms of Darwinian evolution and which is still susceptible to empirical testing (Fasol-Boltzmann, 1990, p. 160).

The infinite, however, is never given in experience and therefore has no direct empirical meaning. Therefore, set theory can be characterized as an effort to introduce symbols for meaningless notions and to establish rules for their manipulation (Fasol-Boltzmann, 1990, p. 198). Now, Boltzmann notes that in itself, introducing such symbols can be a very fruitful enterprise in mathematics. He remarks that the negative, rational, irrational and imaginary numbers are all introduced as symbols for impossibilities: it is impossible to subtract 5 from 3, and we introduce the number -2 as a symbol for this impossibility, the imaginary numbers come in as symbols for the impossibility to draw the square root of -1, etc.[6] While introducing symbols for meaningless notions is in itself no more than a "play with concepts",[7] the introduction of these different types of numbers has been justified by the fact that they have proven to be very useful in geometry and physics, and in this way they have obtained an empirical relevance (Fasol-Boltzmann, 1990, p. 162, 180). Similarly, set theory shows how one can define the infinite and work with it in a consistent manner. However, it cannot be taken for granted that everything that one can define and work with in a consistent manner in pure mathematics can be applied in physics, and in the case of infinity, Boltzmann does not think its employment in physics can be justified.

The reason for Boltzmann's rejection of infinity in physics is his belief that the notion of the infinite is inherently paradoxical. He refers to Bolzano, who had argued in his *Paradoxien des Unendlichen* (1851) that despite the paradoxical character of the infinite (which appears e. g. in the fact that for infinite sets, there can be one-to-one correspondence between the set and a part of the set), the infinite can nevertheless be accepted as a concept in mathematics because one can work with it without running into downright contradictions. However, Boltzmann argues that set theory avoids the paradoxes rather than solving them:

> In fact, one cannot say that these contradictions have been completely resolved, but they are, as we say, at least successfully circumvented through set theory. We have thus learned to work with a method, without being in any way compelled to take offence to these contradictions. (Fasol-Boltzmann, 1990, p. 201).[8]

While the contradictions are carefully avoided in set theory, Boltzmann argues that they come to the surface when we make use of actual infinity in physics. Boltzmann discusses only a single example of how things may go wrong when actual infinity is introduced in physics. The example is that of a planet rotating in an elliptic orbit around the sun, where planet and sun are taken to be point masses. One can take the small axis of the ellipse to become smaller and smaller, so that the ellipse becomes more and more eccentric. If one lets the small axis become infinitely small, the ellipse collapses to a straight line, and we find that the planet moves in a straight line towards the sun and then returns. However, if one directly calculates what will happen when the trajectory

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[6] This is reminiscent of the famous remark, attributed to Kronecker, that "God made the integers; everything else is the work of man". Kronecker argued that positive integers were the only numbers that could be accepted in mathematics, and mathematics should be rewritten in terms of only these numbers (Kline, 1972, p. 1197).

[7] "Spiel mit Begriffen".

[8] "Man kann eigentlich nicht sagen, dass diese Widersprüche vollkommen gelöst worden sind, aber sie wurden, wie wir sagen, durch diese Mengenlehre wenigstens mit Erfolg umgangen. Wir lernten da eine Methode zu operieren, ohne dass wir irgendwie genötigt sind, an diesen Widersprüchen Anstoss zu nehmen."

of the planet is a straight line towards the sun, one finds that the planet moves towards the sun without moving back. Thus, this system exhibits a pathological case of indeterminism: there are two different ways to calculate the motion of the planet, with two different outcomes (Fasol-Boltzmann, 1990, p. 199-200). Boltzmann refers to Georg von Vega, who had studied the problem in the late eighteenth century, but according to Wilholt (2008, p. 9-10), the problem goes back to Euler, who however discussed only the first way of calculating the trajectory. Boltzmann argues that the paradox can be avoided by not letting distances become infinitely small; thus, the paradox is caused by the introduction of infinity in physics.

Although Boltzmann only discusses this example, he claims that such paradoxes can be found everywhere if we allow actual infinity to play a role in physics: "If we hold on to the concept of the strictly infinite, we always arrive at such cases in which we cannot make a determination" (Fasol-Boltzmann, 1990, p. 200).[9] Because the continuum of the real numbers involves infinity, Boltzmann argues that it cannot be taken to be inherent in nature. For example, we may arrive at paradoxes if we assume that matter takes continuous values:

> ...if we think of [matter] as truly continuous, we get into set theory; we arrive all the time at cases in which we cannot reach unambiguous conclusions, and the purpose of thought is to always be able to reach unambiguous conclusions; therefore, we must seek to construct our signs in speech, writing and thought in such a way that we can express ourselves unambiguously and understand ourselves unequivocally. (Fasol-Boltzmann, 1990, p. 200)[10]

Thus, in order for physics to be unambiguous and calculations in physics to always have unique outcomes, we cannot take variables to become infinitely large or infinitely small, and we must work with the assumption that there is no actual continuum in nature.

In his published writings, Boltzmann presents his rejection of actual infinity and the mathematical continuum as a defence of atomism. He argues that to form an image of a continuum, e. g. of continuous matter, one always has to start from a finite number of discrete elements, or atoms, and let them become smaller and smaller. The use of differential equations and integrals in physics necessarily involves atomism, in the sense that one has to start with a large number of elements (or "atoms") of small but finite size, and then take the limit in which the size of the elements goes to zero; but as

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[9] "Wenn wir den Begriff des streng Unendlichen festhalten, kommen wir immer zu solchen Fällen, wo wir keine Entscheidung treffen können"

[10] "Wenn wir sie [matter] aber wirklich kontinuierlich denken, kommen wir in die Mengenlehre hinein; wir kommen alle Augenblicke an Stellen, wo wir nicht eindeutig schliessen können, und der Zweck des Denkens ist ja, überall eindeutig schliessen zu können; daher müssen wir unsere Sprach-, Schrift- und Denkzeichen so zu bilden suchen, dass wir uns selbst eindeutig ausdrücken und uns selbst eindeutig verstehen."

one cannot assume actual infinity in nature, the limit situation cannot be taken to be actual. According to Boltzmann, this means that the use of differential calculus in physics depends on the assumption of a large number of discrete elements, and thus, on atomism. As Boltzmann puts it:

> Please forgive me for the somewhat banal expression when I say that he who believes to have gotten rid of atomism through differential equations does not see the forest for the trees. (Boltzmann, 1897a, p. 144).[11]

It has to be noted that the type of atomism for which Boltzmann argues on the basis of his concerns about infinity and differential calculus is more general than the idea that matter is made up of atoms; it is the idea that all physical quantities should ultimately be taken to be discontinuous, including the dimensions of time and space. As Boltzmann expresses it, there are different atomisms that are presupposed by different differential equations; differential coefficients with respect to time require "time-atoms" (Boltzmann, 1897a, p. 146).

Boltzmann devoted a large part of his research to the kinetic theory of gases, which was based on an atomistic conception of gas. This theory was criticized by empiricists such as Mach for being based on hypotheses involving unobservable entities: in the late nineteenth century, it was increasingly popular to avoid atoms and stick to observable entities in physics. Boltzmann vehemently defended atomism against the empiricists, among others by pointing at the fruitfulness of atomistic theories (see e.g. Boltzmann, 1897a). Furthermore, he argued that atomism is no more hypothetical than the use of differential equations in physics: in order to work with differential equations, you have to start from elements of a finite size and let these become smaller and smaller, and the claim that differential equations give an accurate representation of natural processes rests on the assumption that the smaller you take the elements to be, the better the representation will get:

> Whereas in the past, the assumption of a certain size of atoms counted as a raw image going arbitrarily beyond the facts, it now appears as the more natural assumption, and the claim that one can never discover differences between the facts and the limit values, because no such differences have been discovered so far (maybe not even in all cases), adds something new and unproven to the image. (Boltzmann, 1897a, p. 144-45; see also Boltzmann, 1897b, p. 5).[12]

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[11] "Man verzeihe den etwas banalen Ausdruck, wenn ich sage, dass derjenige, welcher die Atomistik durch Differentialgleichungen losgeworden zu sein glaubt, den Wald vor Bäumen nicht sieht."

[12] "Während früher die Annahme einer bestimmten Grösse der Atome als eine rohe, willkürlich über die Tatsachen hinausgehende Vorstellung galt, so erscheint sie jetzt gerade als die natürlichere, und die

But as Wilholt (2002) has emphasized, Boltzmann made a much stronger claim than that atomism is preferable to continuous representations of nature: he in fact argued that because actual infinity cannot be accepted in physics, atomism is indispensable for physics.[13]

The claim that we cannot use actual infinity in our representations of nature implies that differential equations should be taken to describe a limit situation that is not actual, and therefore can merely be taken to offer approximations. Boltzmann did make extensive use of the method of differential calculus in his work in physics, thus it is clear that he thought that the approximations that differential calculus offers are usually very good and that it is a reliable method. However, especially in his earlier work in the kinetic theory of gases it is clear that he thought that discrete representations of nature were in certain cases clearer and safer. For example in (Boltzmann, 1872), after deriving a result through differential calculus, he switches to a discrete method by replacing integrals by sums, which he claims is in this case more clear, but which to a modern reader seems in fact artificial and complicated.

Thus, for Boltzmann, the development of rigorous foundations of the calculus in the nineteenth century did not make the applicability of the calculus unproblematic. Nineteenth century developments in the foundations of differential calculus had based the calculus on the real number system, and in order to give an account of the real numbers in mathematics one needs to accept the notion of infinity (even uncountable infinity); but on the basis of his empiricist philosophy of mathematics, Boltzmann did not think this was justified. Therefore, he argued for a fundamentally discrete conception of nature.

5.3.3 Poincaré on the continuum

In contrast to Boltzmann, Poincaré argues that we can safely work with the assumption that variables in physics take continuous values. He writes about this assumption: "This is the assumption which we implicitly admit when we apply the laws of mathematical

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Behauptung, dass niemals Unterschiede zwischen den Tatsachen und den Limitenwerten entdeckt werden könnten, weil solche bis heute (vielleicht nicht einmal in allen Fällen) noch nicht entdeckt wurden, fügt dem Bilde etwas Neues, Unerwiesenes bei."

[13] Wilholt (2008) has shown that the debate about atomism in the late nineteenth century was to an important degree a conceptual debate: it concerned not merely the degree of ontological commitment that we should make to unobservable entities in our theories, but the very legitimacy of the use of atomistic versus continuous conceptions of matter. He shows that whereas among others Du Bois-Reymond pointed out inconsistencies in the atomic conception of matter, Boltzmann pointed out conceptual problems that can arise in continuous representations of nature.

analysis and in particular those of infinitesimal calculus to nature" (Poincaré, 1905b).[14] Poincaré argues that if we assume that physical quantities take continuous values, and if we furthermore assume that the relations between these quantities can be expressed by differentiable functions (see section 3), we can formulate our laws of physics in terms of differential equations. Moreover, according to Poincaré, because differential equations in physics tend to have unique solutions for given initial conditions, the possibility to formulate our laws of physics in terms of differential equations implies determinism in physics (Poincaré, 1905b).[15]

However, according to Poincaré, in order for the continuum of the real numbers to be applicable to physical reality, it needs to have a foundation in intuition. Like Boltzmann, he thinks that not everything that can be defined in a consistent manner in mathematics is meaningful and applicable to physical reality. But whereas Boltzmann adheres to an empiricist philosophy of mathematics, Poincaré argues that mathematics must have a foundation in (Kantian) intuition, and that without such a foundation, mathematics is just an empty construction that bears no relation to physical reality and is ultimately tautological (Poincaré, 1905a, p. 214-17). Like Kant (and Helmholtz), Poincaré argues for a synthetic a priori status of arithmetic, although he differs from Kant on the nature of the synthetic a priori intuition on which arithmetic is based: whereas for Kant, arithmetic is based on the intuition of time, according to Poincaré it is based on the synthetic a priori intuition that we can keep on counting indefinitely.[16] Thus, the natural numbers have a foundation in synthetic a priori intuition; but this foundation does not suffice for the real numbers.

According to Poincaré, the continuum of the real numbers, and thereby also differential calculus, is ultimately derived from a Kantian intuition of continuity in our experience. We have a Kantian synthetic a priori intuition of continuity in the sense that we necessarily experience e. g. space, time and mass as continuous, and that this

[14] "C'est là le postulat que nous admettons implicitement quand nous appliquons à la nature les lois de l'analyse mathématique et en particulier celles du calcul infinitésimal".

[15] This is not entirely true: in 1876, Lipschitz had shown that a differential equation of the form $\frac{d^2r}{dt^2} = F(r)$ only has a unique solution if F(r) satisfies a condition that is now known as Lipschitz continuity (see Van Strien, 2014b).

[16] According to Poincaré, arithmetic is based on "the affirmation of the power of the mind which knows itself capable of conceiving the indefinite repetition of the same act when once this act is possible" (Poincaré, 1902, p. 39); "We have the faculty of conceiving that a unit can be added to a collection of units; thanks to experience, we have occasion to exercise this faculty and we become conscious of it; but from this moment we feel that our power has no limit and that we can count indefinitely, though we have never had to count more than a finite number of objects" (Poincaré, 1902, p. 47; and see Folina, 1992, p. 94). In (1905a), however, he leaves open the question of whether the intuition involved is inner or sensible intuition, as an issue that should be left to psychologists and metaphysicians (Poincaré, 1905a, p. 221). For Helmholtz' account of the foundations of arithmetic and its influence, see Helmholtz (1887), Darrigol (2003).

continuity is essential for coherent experience.[17] The continuum of the real numbers is a construction made on the basis of our experience of continuity, it is an invention that we have made in order to interpret our experience mathematically.

However, Poincaré emphasizes that the nature of the continuum that we experience directly is essentially different from that of the mathematical continuum: the former, which Poincaré calls the physical continuum, is characterized by a "kind of fusion of neighbouring elements" (Poincaré, 1905b).[18] This can be explained by the case of sensations of weight: Poincaré writes that it has been found that when lifting weights, we cannot, on the basis of our muscular sensations, distinguish a weight of 10 gram from one of 11 gram, nor one of 11 gram from one of 12 gram, but we can distinguish 10 gram from 12 gram; in other words, our sensations of weight are non-transitive (Poincaré, 1902, p. 46).[19] If the sensations of lifting these weights were to be represented by numbers, say A, B, and C, we would arrive at a contradiction, namely we would have A=B, B=C, and A<C; thus, our sensations do not directly correspond with numerical values. They can only be made to correspond to numerical values through introducing the assumption that weight is divisible into smaller elements than we can perceive, so that between any two values that we can measure, a third one can be found. If we suppose that it is always possible to make more precise measurements, without limit, we are led to assume that weight is infinitely divisible (Poincaré, 1902, p. 47). In this way, our attempt to mathematize our experience of a physical continuum leads us to invent what Poincaré calls the continuum of the first order, namely the rational numbers (we would call this a dense set rather than a continuum).

Folina (1992, p. 120) interprets this argument as stating that we necessarily have to conceive of weight as being infinitely divisible, because this is the only way to avoid contradictions of the A=B, B=C, and A<C type. This is however a problematic claim: it would mean that it is contradictory to assume that matter consists of atoms which are

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[17] "The claim being made by Poincaré is that identity over change (in spatio-temporal location and appearance) is essential for conceptualising experience in anything like the way in which we do, and the epistemological foundation for identity over change is spatio-temporal 'connectedness' or *continuity*." (Folina, 1992, p. 137).

[18] "sorte de fusion des éléments voisins".

[19] The example comes from Fechner, it is related to the (Weber-) Fechner law of perception (Poincaré refers to Fechner in (1917 [1913], p, 68)). Poincaré argues that measurement in general shows us a physical continuum rather than a mathematical continuum. It is of course possible to make more precise measurements of weight, for example by using a scale which can distinguish between 10 and 11 gram, but no matter how precise we make our measurements, in the end we always have to appeal to our senses to read off the measurement apparatus, "which will bring along the characteristics of the physical continuum and its essential imprecision" (Poincaré, 1905b, p. 295).

In (1917 [1913], p. 71), Poincaré refines his view by saying that the physical continuum is not directly derived from the senses, in the sense that it can only be constructed when certain sensations are isolated, through abstraction, e.g. by only paying attention to weight.

smallest weight elements, but also through the assumption that weight is divisible into small elements without being infinitely divisible, we can attribute numbers to sensations of weight without arriving at contradictions. However, it seems that Poincaré would regard the possibility that there are smallest elements of weight as being contrary to intuition, on the basis of the fact that we cannot possibly have an experience of such smallest elements which cannot be broken down any further. He makes this argument in the case of length: it is not possible in principle to visually perceive smallest length elements, since it is a necessary feature of perceptions of length that every length element we can observe can be divided further, and therefore we cannot conceive of length otherwise than as being infinitely divisible.[20]

To arrive at the continuum of the real numbers, infinite divisibility does not suffice; the latter only gives us the rational numbers. Poincaré writes that the irrational numbers come in through the requirements of geometry and geometrical intuition: we have the intuition that lines which cross always meet in a point, and that if we cut the number line into two sections, there is a point at which the division is made; thus, if we cannot find a rational point for which this holds, we introduce extra points corresponding to the line crossings or cuts (Poincaré, 1902, p. 48-49). In this way we arrive at the irrational numbers, which together with the rational numbers form the continuum of the second order (which is a continuum in the proper sense of the term). Again, it is possible to maintain that the irrational numbers do not correspond to anything in physical reality, and that e. g. length only takes rational values, but this is according to Poincaré not in accordance with intuition.

Thus, according to Poincaré, there is an intuitive basis for the continuum of the real numbers in the sense that it is a construction made to model the continuity that is given in (outer and geometric) intuition. He writes that "It is the external world which has imposed the continuum upon us, which we doubtless have invented, but which it has forced us to invent" (Poincaré, 1905a, p. 285). Folina argues on this basis that for Poincaré, the applicability of the real numbers to physical reality (as well as the continuum of the real numbers itself) has a synthetic a priori status (Folina, 1992, p. 120-141). This would be quite a strong claim: Poincaré writes on synthetic a priori propositions that they are "imposed upon us with such force that we could not conceive the contrary proposition, nor build upon it a theoretic edifice" (Poincaré, 1902, p. 64). Thus, if the applicability of the real numbers to physical reality has synthetic a priori

[20] "We might conceive the stopping of this operation [of further division] if we could imagine some instrument sufficiently powerful to decompose the physical continuum into discrete elements, as the telescope resolves the milky way into stars. But this we can not imagine; in fact, it is with the eye we observe the image magnified by the microscope, and consequently this image must always retain the characteristics of visual sensation and consequently those of the physical continuum" (Poincaré, 1902, p. 47).

status, it would mean not only that we necessarily conceive of e. g. space, time and mass as forming a mathematical continuum, but also that the continuum of the real numbers is necessary for a coherent interpretation of experience, and that we cannot build theories on the basis of a discontinuous conception of reality. It would mean that Boltzmann's type of atomism, in which time and space and all other physical quantities are quantized, is fundamentally unintelligible and that no physical theory can be based on it.

However, in (1905b), Poincaré acknowledges that it is possible to maintain that nature is ultimately discontinuous. He writes that for physicists, there is no need to question the possibility to work with a continuous conception of nature and to apply differential calculus in physics, but this possibility may be denied on metaphysical grounds: "the given world is a physical continuum, and scholars assume that the actual world is a mathematical continuum, but some metaphysicians have preferred to admit that the world is discontinuous".[21] He refers to two authors who have argued for a discontinuous conception of reality: a philosopher, François Evellin, and a mathematician, Joseph Bertrand.[22] Poincaré seems to argue that a discontinuous conceptions of reality is not so much impossible but will make physics much more complicated; in particular, on this basis one cannot work with differential equations in physics, and therefore the laws of physics will have to take a different form.[23] Strictly speaking, it is possible to reconcile the assumption that physical quantities take discrete values with experience, but at the cost of a disparity between the ontological level and our Kantian intuition, and at the cost of higher mathematical complication. Moreover, he mentions a third alternative besides assuming that nature corresponds to a mathematical continuum or that it is fundamentally discontinuous: it is also possible to maintain that at the fundamental level there is a 'physical continuum' of the same type that is given directly in sensation, which differs from the mathematical continuum in being non-transitive. However, to work out a theory on this basis will be even more complicated: "it would not be as easy as with the system of Mr. Evellin, and it would

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[21] "le monde donné est un continu physique, et les savants supposent que le monde réel est un continu mathématique, mais quelques métaphysiciens ont préféré admettre que le monde est discontinu".

[22] See Evellin (1894), Bertrand (1878); on Bertrand's views, see Van Strien (2014b).

[23] In (1902), Poincaré treats continuity of matter as a matter of convenience: "In most questions the analyst assumes at the beginning of his calculations either that matter is continuous or, on the contrary, that it is formed of atoms. He might have made the opposite assumption without changing his results. He would only have had more trouble to obtain them; that is all." (Poincaré, 1902, p. 135). His point here is that atomism is a 'neutral hypothesis' in the sense that it can be neither empirically confirmed nor falsified; by implication, the same holds for the assumption that matter is continuous (which is, according to Poincaré, the more convenient option). On Poincaré's atomism, see Ivanova (2013).

doubtless be difficult to give this idea a mathematical form and make it compatible with absolute determinism" (Poincaré, 1905b).[24]

Thus, for Poincaré, the applicability of differential calculus in physics depends on the fact that the mathematical continuum has a basis in Kantian intuition: we cannot conceive of e. g. space, time and mass as being discontinuous. This does not mean that it is strictly speaking impossible to work with the assumption that nature is fundamentally discontinuous, but it does mean that a discontinuous conception of nature goes against our intuition of continuity. Moreover, a discontinuous conception of nature will make it much more complicated to work out theories in physics, since it means that one cannot make use of differential equations.

5.4 Continuity of change

5.4.1 Continuity of change and the foundations of analysis

A second type of continuity that is needed for the applicability of differential calculus in physics is, roughly speaking, the idea that change in nature takes place in a continuous manner. Specifically, it is the assumption that the relations between variables in physics can typically be expressed by functions that are (a) continuous and (b) differentiable. These properties can be defined as follows:

- A function f(x) is *continuous* if at each point, an infinitely small change in x correspond to an infinitely small change in f(x). The graph of such a function is an unbroken curve.

[24] "cela ne serait pas aussi simple qu'avec le système de M. Evellin, et il serait difficile sans doute de donner à cette idée une forme mathématique et de la rendre compatible avec le déterminisme absolu". For Poincaré, the possibility to formulate laws of nature in terms of differential equations implies determinism; he seems to take for granted that these equations will always have unique solutions for given initial conditions and will thus be deterministic (Poincaré, 1917 [1913], p. 8). Therefore, to give up continuity in physics threatens the possibility to have a deterministic physics. However, the connection between determinism and differential equations was in fact not so straightforward: while Poincaré refers to Bertrand's (1878) paper in which Bertrand argues for a discontinuous conception of reality, he does not remark on the fact that the motivation for Bertrand's argument was to save determinism. Bertrand's paper was a reaction to an argument by Boussinesq, who had shown that there can be mechanical systems for which the (differential) equations of motion fail to have a unique solution for given initial conditions and thus allow for indeterminism. Bertrand argues that the indeterminism that Boussinesq describes is an artefact of the use of differential equations, and that it can be avoided through the assumption that physical reality is fundamentally discrete and that differential equations offer mere approximations (Van Strien, 2014b).

- A function f(x) is *differentiable* if it has a derivative at each point, which means that the graph has a non-vertical tangent line at each point. The graph of such a function is a curve without sharp bends.

Differentiability implies continuity, and may be regarded as a strong type of continuity. Both are needed in order to work with differential equations, although it is not necessary problematic if there is a limited number of points at which a function is non-differentiable. In this section, I show that also in the case of continuity and differentiability of functions, developments in pure mathematics led to questions about the status of these assumptions in physics.

Throughout the eighteenth and the main part of the nineteenth century, it was generally believed that any continuous function had to be differentiable (except possibly at a limited number of points) (Kline, 1972, p. 955). With the rigorous formulation of the calculus in the nineteenth century came precise mathematical definitions of continuity and differentiability of functions; it could then be considered whether the actual relations between physical quantities satisfied these conditions. Moreover, the development of rigorous foundations of analysis led to increased study of atypical or 'pathological' functions (Kline, 1972, p. 972-73; Lützen, 2003, p. 187-88). In 1872, Weierstrass disproved the earlier conviction that continuity implies differentiability by giving an example of a function that was everywhere continuous but nowhere differentiable. This function is an early example of what we now call a fractal: it is strongly irregular and no matter how much you zoom in, it will retain its irregular character and will not start to resemble a straight line at any scale (Chabert, 1994, p. 369). The discovery of continuous but nowhere differentiable functions implied that differentiability of functions could no longer be taken for granted on the basis of continuity.

5.4.2 Boltzmann on continuity of change

When Weierstrass presented his continuous but nowhere differentiable functions, they were regarded as mere mathematical curiosities. But in (1898), Boltzmann noted that the function he had developed to express the entropy of a thermodynamic system, his H-curve, had the property of being nowhere differentiable. He added that he was afraid that for this reason, his function would be mocked by mathematicians (Boltzmann, 1898, p. 328). But he argues that there is no reason to reject the use of continuous but nowhere differentiable functions in physics: in fact, he argues, it is thinkable that motions at the microlevel are so irregular as to correspond to Weierstrass functions.

It is thus at least *thinkable*, according to Boltzmann, that the relations between physical quantities are best represented by functions which are non-differentiable. Boltzmann was not alone in making this argument. In 1909, Perrin (famous for his work on Brownian motion) argues that trajectories of particles in Brownian motion are so irregular that it is impossible to find a tangent to them, and then suggests that they may correspond to Weierstrass functions:

> ...we can also not fix a tangent at any point of the trajectory, not even in the crudest way. This is one of those cases where one cannot help thinking of those continuous functions which do not admit of a derivative, which could wrongly be regarded as simple mathematical curiosities, since nature can suggest them just as well as differentiable functions. (Perrin, 1909, p. 30-31).[25]

Strictly speaking, the fact that Boltzmann denies that physical quantities (including space and time) can be taken to have continuous values implies that he cannot take the relations between these quantities to correspond to functions that are continuous or differentiable. Nevertheless, Boltzmann argues that it is not problematic for physicists to work with continuous and differentiable functions. This has to do with the fact that according to Boltzmann, physicists necessarily have to work with 'pictures' of nature which should not be taken to be true representations, but are rather idealized models (on Boltzmann's picture theory of science, see De Regt, 1999). In (1899b), Boltzmann writes that "No equation represents phenomena with absolute exactitude; they all idealise the phenomena; they all emphasize the common features of the phenomena and neglect the divergent; they all, therefore, transcend experience". For the construction of theories in physics, the question is thus not whether the actual relations between quantities in physics correspond to functions which are continuous and differentiable, but whether, on the basis of the assumptions of continuity and differentiability of functions, we can build models that are in good enough agreement with experience.

Boltzmann argues that there is enough ground in experience to conclude that we can always work with continuous functions in physics: he writes that it is a "sufficiently proven fact of experience"[26] that the position of a material body changes continuously in time, so that it forms a continuous function (Boltzmann, 1899a, p. 282). In (1897b, p. 9) he refers to this statement as the "law of continuity" and adds that it is a necessary

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[25] "Bien entendu, on ne peut non plus fixer de tangente en aucun point de la trajectoire, même de la façon la plus grossière. C'est un des cas où l'on ne peut s'empêcher de penser à ces fonctions continues qui n'admettent pas de dérivée, qu'on regarderait à tort comme de simples curiosités mathématiques, puisque la nature peut les suggérer aussi bien que les fonctions à dérivées."
Cassirer later remarked that this proposal by Perrin "shatters one of the essential bases on which the edifice of classical analysis as well as that of classical physics rests". He adds: "It is now shown that 'macrostates' do not permit immediate inference to 'microstates.' Leibniz, at times, formulated his continuity principle in such a manner as to demand exactly this analogy." (Cassirer, 1956 [1936], p. 164-65).

[26] "hinlänglich sicher gestellte Erfahrungstatsache"

condition for the re-identification of a body over time. The assumption of differentiability of functions, however, is less certain, as it is thinkable that certain functions in physics are best represented by Weierstrass functions:

> ...we also know examples of very rapid oscillations and cannot prove exactly whether in certain cases there are not motions, such as for example thermal motions of molecules, which can be better represented by one of the Weierstrass functions than by a differentiable one. (Boltzmann, 1899a, p. 283).[27]

However, he argues that although it cannot be proven that we can always work with differentiable functions in physics, there is as yet no ground in experience for thinking that this is not the case (with the probable exception of his H-function):

> We can shape our picture the way we want and simply include the differentiating work therein from the outset, justifying it on the basis that afterwards, the picture is consistent with experience. (Boltzmann, 1899a, p. 283; see also Boltzmann, 1897b, p. 13, 26-27).[28]

The construction of theories in physics thus involves assumptions such as the assumption that we can work with differentiable functions, which cannot fully be proven empirically. In (1899a), Boltzmann uses this as an argument against empiricists such as Mach, who hold that physics should contain no hypothetical elements: Boltzmann argues that this would entail that you cannot take functions in physics to be differentiable (Boltzmann, 1899a). But Boltzmann thought that the assumption of differentiability is not problematic as long as you accept that physical theories are in any case constructions involving assumptions and hypotheses that are not fully proven empirically. You can just stipulate differentiability, and there is no reason to expect that you couldn't arrive at good-enough models of reality in this way.

5.4.3 Poincaré on continuity of change

The question whether Weierstrass functions may have physical relevance also comes up in the work of Poincaré. His attitude towards these functions was somewhat ambivalent. In (1898), Poincaré praised Weierstrass for making clear that the idea that continuous functions always have a derivative is based on intuitions that have no place in pure

[27] "...wir kennen auch Beispiele sehr rascher Oszillationen und können nicht exakt beweisen, ob nicht in gewissen Fällen Bewegungen vorhanden sind, wie z. B. die Wärmebewegungen der Moleküle, welche durch eine der Weierstrassschen Funktion ähnliche besser als durch eine differenzierbare dargestellt werden."

[28] "Wir können ja dann unser Bild formen, wie wir wollen und einfach die Differenzierarbeit von vornherein in dasselbe aufnehmen, es damit rechtfertigend, dass das Bild hinterher mit der Erfahrung stimmt."

mathematics. But then again, the aim of his paper was to praise the work of Weierstrass (it was probably written on the occasion of Weierstrass' death in 1897). Also in (1908, p. 432-34), Poincaré acknowledges that Weierstrass' example of continuous but nowhere differentiable functions shows that we cannot trust our intuitions to give certainty in the case of differentiability of functions.

However, in general, Poincaré was a strong defender of the role of intuition in mathematics, and critical about the tendency to diminish this role and to reduce mathematics to logic. Weierstrass functions are deeply counterintuitive, and that they had come to play an important role in treatments of analysis was according to Poincaré a sign of the unhealthy separation of mathematics from empirical reality:

> Logic sometimes makes monsters. Since half a century we have seen arise a crowd of bizarre functions which seem to try to resemble as little as possible the honest functions which serve some purpose. No longer continuity, or perhaps continuity, but no derivatives, etc. (...) Heretofore when a new function was invented, it was for some practical end; to-day they are invented expressly to put at fault the reasonings of our fathers, and one never will get from them anything more than that. (Poincaré, 1908, p. 435).[29]

The worst aspect, for Poincaré, was that it was argued that these non-differentiable functions are the most general type of functions, and that the continuous and differentiable functions, which are the only ones which we can comprehend and which are of any use in physics, are reduced to a small subclass of possible functions (Poincaré, 1899; Poincaré, 1908, p. 435).

Poincaré was thus convinced that Weierstrass functions are counter-intuitive and have no role to play in physics. According to him, we can safely assume that functions in physics are continuous and differentiable. The assumption of continuity is according to Poincaré a form of the principle of sufficient reason: he writes about the principle of sufficient reason that it is "very vague and elastic" and can take many forms, but "The form under which we have met it most often is the belief in continuity, a belief which it would be difficult to justify by apodeictic reasoning, but without which all science would be impossible" (Poincaré, 1902, p. 173). For example, the problem of fitting a graph to data points only makes sense if it is required that the graph has a simple form, and such a graph will be continuous and differentiable, with small higher order derivatives:

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[29] The French original: "on vit surgir toute une foule de fonctions bizarres qui semblaient s'efforcer de ressembler aussi peu que possible aux honnètes fonctions qui servent à quelque chose. Plus de continuité, ou bien de la continuité, mais pas de dérivées, etc., etc. (...) Autrefois, quand on inventait une fonction nouvelle, c'était en vue de quelque but pratique; aujourd'hui, on les invente tout exprès pour mettre en défaut les raisonnements de nos pères, et on n'en tirera jamais que cela." (Poincaré, 1899)

> ...I consider a priori a law represented by a continuous function (or by a function whose derivatives of high order are small), as more probable than a law not satisfying these conditions. Without this belief, the problem of which we speak [of fitting a graph through data points] would have no meaning; interpolation would be impossible; no law could be deduced from a finite number of observations; science would not exist. (Poincaré, 1902, p. 170).

Thus, according to Poincaré, the possibility of science depends on assumptions of continuity of functions. This is connected to the fact that science cannot do without assumptions of simplicity.[30] Moreover, he claims that the assumptions of continuity and differentiability of functions are assumptions which we can freely make, without having to fear falsification:

> ...any function always differs as little as you choose from a discontinuous function, and at the same time it differs as little as you choose from a continuous function. The physicist may, therefore, at will suppose that the function studied is continuous, or that it is discontinuous; that it has or has not a derivative ; and may do so without fear of ever being contradicted, either by present experience or by any future experiment. (Poincaré, 1905a, p. 288).[31]

In (1905b), he makes the same argument, and adds: "Thus, the physicist can always apply the rules of differential calculus without fearing a contradiction with experience" (Poincaré, 1905b, p. 296-7).[32] Thus, because any function can be approximated as closely as one wishes by a continuous and differentiable functions, physicists can *choose* to work with functions that are continuous and differentiable.

Whether Poincaré was right in claiming that every function can be approximated as closely as one wishes by a continuous and differentiable function, is a different matter. This claim can partly be supported by a theorem by Weierstrass, which states that any continuous function can be approximated as closely as one wishes by a differentiable

[30] Poincaré writes on simplicity assumptions: "No doubt, if our means of investigation should become more and more penetrating, we should discover the simple under the complex, then the complex under the simple, then again the simple under the complex, and so on, without our being able to foresee what will be the last term.
We must stop somewhere, and that science may be possible, we must stop when we have found simplicity." (Poincaré, 1902, p. 133).

[31] And in (1905b): "Il y aura donc toujours moyen de représenter les observations, quelles qu'elles soient, par des fonctions qui s'écarteront moins que ne le comporte l'incertitude des mesures et qui jouiront de la continuité, de la propriété d'avoir une dérivée, de toutes les propriétés des fonctions analytiques. Une fonction quelconque étant donnée, on peut toujours trouver une fonction analytique qui en diffère aussi peu que l'on veut."

[32] "Ainsi le physicien peut toujours appliquer les règles du calcul infinitésimal sans craindre un démenti de l'expérience"

function. However, this theorem does use continuity as a premise, and it is generally speaking not the case that any function can be approximated as closely as one wishes by a continuous function.[33] Moreover, the claim that one can always work with continuous functions was soon undermined by the development of quantum mechanics, which showed that one cannot always work with continuous descriptions of nature. Poincaré acknowledged this: in 1911, he wrote a paper about Planck's treatment of black body radiation, which was a step in the direction of the development of quantum mechanics, and concluded after a careful study that there is no way in which the phenomena that Planck described could be represented in a continuous manner by means of differential equations (Poincaré, 1911). However, Poincaré was not completely prepared to give up continuity, and not much later he left the issue open of whether it would ever be possible to account for quantum mechanics without giving up continuity:

> Will discontinuity reign over the physical universe, and is its triumph final? Or will we recognize that this discontinuity is only apparent and conceals a series of continuous processes. The first who saw a collision thought to observe a discontinuous phenomenon, and we now know that he just saw the effect of very rapid but continuous changes in velocity. To seek to give an opinion on these questions at this moment would be a waste of ink. (Poincaré, 1917 [1913], p. 192).[34]

In any case, at least until he became aware of the new research in black body radiation and quantum mechanics, Poincaré was convinced that physicists can always work with functions that are continuous and differentiable. Poincaré's claim that any function can be approximated as closely as one wishes by a continuous and differentiable function implied that continuity, discontinuity, differentiability and non-differentiability of functions can all be made compatible with experience. It is therefore always possible for

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[33] And Poincaré's claim did not go completely uncontested. In (1905b), Poincaré argues that any function can be approximated as closely as one wishes by an analytic function, where analyticity is a stronger property than differentiability. But this claim was criticized by Hadamard in (1923): "I have often maintained, against different geometers, the importance of this distinction. Some of them indeed argued that you may always consider any functions as analytic, as in the contrary case, they could be approximated with any required precision by analytic ones. But, in my opinion, this objection would not apply, the question not being whether such an approximation would alter the data very little, but whether it would alter the solution very little." (Hadamard, 1923, p. 33). See also Wilson (2006, p. 308-9) on the problems with Poincaré's claim that functions in physics can always be taken to be analytic. Wilson emphasizes that analytc functions have a specific character that one cannot expect all functions in physics to have; in particular, if you know how the function behaves within a certain interval, you can derive how it behaves elsewhere.

[34] "La discontinuité va-t-elle régner sur l'univers physique et son triomphe est-il définitif? Ou bien reconnaîtra-t-on que cette discontinuité n'est qu'apparente et dissimule une série de processus continus. Le premier qui a vu un choc a cru observer un phénomène discontinu, et nous savons aujourd'hui qu'il n'a vu que l'effet de changements de vitesse très rapides, mais continus. Chercher dès aujourd'hui à donner un avis sur ces questions, ce serait perdre son encre."

physicists to exclude non-intuitive functions such as Weierstrass functions and to use functions that are continuous and differentiable; in fact, the possibility of science depends on such assumptions.

Conclusion

For Boltzmann and Poincaré, continuity and discreteness in physics are primarily issues in philosophy of mathematics and epistemology. Boltzmann argues that we cannot make use of actual infinity in our representations of nature, because this leads to paradoxes: we therefore have to conceive of physical reality in a discrete manner. Wilholt (2002) has pointed out that this leaves open the issue of whether nature in fact *is* discrete: it is merely a statement about the form our theories must take so as to avoid paradoxes in our thought. Poincaré, in contrast, argues that we have to conceive of physical reality in a continuous manner, ultimately because continuity is an a priori form of our sensations; but he too leaves open the question whether there is actual continuity in nature. He is uninterested in all questions concerning the ultimate nature of reality: his Kantian convictions entail that the ultimate nature of reality is inaccessible anyway. Therefore, for Boltzmann and Poincaré, continuity and discreteness in physics are not metaphysical issues. Neither do they treat the issue of continuity versus discreteness in physics purely as an empirical issue, although one can argue that Boltzmann treats the second type of continuity, that of continuity of change, as an empirical issue to some degree. But concerning the first type of continuity, it is not just an issue of finding out whether continuous or discrete models work best: rather, both Boltzmann and Poincaré argue for discrete resp. continuous representations of nature on the basis of epistemology and philosophy of mathematics.

The reason why, for Boltzmann and Poincaré, the issue of continuity in physics is so much bound up with philosophy of mathematics is that they both objected against the nineteenth century development through which mathematics (in particular analysis) was made independent of physical reality and of empirical and intuitive notions - they were concerned that through this development, the applicability of mathematics in physics was endangered. Because they were convinced that the foundations of analysis need to have a basis in experience or (Kantian) intuition, they needed to consider the question how the mathematical conditions of continuity that are at the basis of analysis relate to experience or intuition.

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Paper 6 Determinism around 1900

This short paper contains a discussion of the implications of the developments described in paper 4 and paper 5 for the history of determinism in physics. In particular, I give a brief discussion of determinism in Mach, Poincaré and Boltzmann, in light of their views on the irreducibility of thermodynamics to mechanics, and on the applicability of differential calculus in physics.

Towards the end of the nineteenth century, the status of mechanics changed in two (related) ways. First, as Pulte (2009) has argued, whereas in the eighteenth and early nineteenth century, mechanics was often thought to have absolute certainty and considered to be built up deductively from axioms which hold necessarily, this was no longer the case by the end of the nineteenth century. At that point, mechanics was instead based on hypotheses or conventions and thus no longer had a status of absolute certainty. Second, during the late nineteenth century mechanics lost its status of fundamental theory of physical reality. As we have seen in paper 4, physicists such as Mach, Poincaré and Duhem argued against the attempt to reduce all of physics to mechanics: they argued that physics should be based on observable phenomena and empirical principles rather than on hypotheses about unobservable entities such as atoms which interact in a mechanical manner. Specifically, they argued for the irreducibility of thermodynamics to mechanics, among others using the argument that mechanics is incompatible with the irreversibility that is characteristic of processes in thermodynamics.

This anti-reductionist view on physics is correlated to an abandonment of the idea that there is a small set of laws through which all processes in physics can be fully described and which have the property of being deterministic. Without a fundamental theory to which everything reduces, there will typically be a larger set of laws which may be domain-specific, such as the laws of thermodynamics and those of electrodynamics. One can still expect that for every process in physics, one can find laws which uniquely determine the process, but this has to be shown case by case: under this view, there is no guarantee that there is a small set of fundamental equations of which one can prove that they always have a unique solution for given initial conditions and which fully describe each process. Furthermore, the introduction of laws of nature which only allow processes to take place in one temporal direction, such as the second law of thermodynamics, could potentially undermine the time symmetrical aspect of Laplacian determinism, according to which the past and the future are equally determined by the state of the universe at the present instant.

Indeed, in Poincaré and Mach we find a conception of determinism in physics which is not tied to the laws of mechanics, and which is not a provable feature of physics. Mach characterises determinism as a methodological presupposition of science: scientists aim at finding functional dependencies between occurrences, and in order to do so they need to work with the assumption that in equal circumstances, the same always happens. In (1872), he refers to this principle as the law of causality, and writes that the principle of sufficient reason is a form of it.[1] For Mach, this is not a necessary a priori principle in the sense that it holds with absolute certainty, but it is nevertheless an assumption which is needed for doing science. In (1905), he writes:

> The correctness of the position of "determinism" or "indeterminism" cannot be proven. It can only be decided if science is completed or demonstrably impossible. These are suppositions that we bring to the contemplation of things, depending on whether one attaches a greater subjective value to the successes or to the failures that research has attained so far. But during research, every thinker is necessarily a theoretical determinist... (Mach, 1905, p. 291-92).[2]

Similarly, for Poincaré, determinism is in the first place a working hypothesis which is needed for doing science. The aim of science is to find laws of nature, which according to Poincaré are essentially classifications of antecedents and consequents. As such, the laws of nature are necessarily approximate and incomplete: we can never give a full account of the antecedents which give rise to a certain consequent, and we can never

[1] "Als a priori einleuchtend [plausible], lässt sich bei wissenschaftlichen Untersuchungen blos das Causalgesetz betrachten oder der Satz vom zureichenden Grunde, der lediglich ein andere Form des Causalgesetz ist. Dass unter gleichen Umständen stets Gleiches erfolgt oder dass die Wirkung durch die Ursache vollkommen bestimmt sei, bezweifelt kein Naturforscher. Es kann dahingestellt bleiben, ob das Causalgesetz auf einer mächtigen Induction ruht, oder in der psychischen Organisation seinen Grund hat, weil ja auch im psychischen Leben gleiche Umstände gleiche Folgen nach sich ziehen." (Mach, 1872, p. 50).

[2] "Die Richtigkeit der Position des "Determinismus" oder "Indeterminismus" lässt sich nicht beweisen. Nur eine vollendete oder nachweisbar unmögliche Wissenschaft könnte hier entscheiden. Es handelt sich hier eben um Voraussetzungen, die man an die Betrachtung der Dinge heranbringt, je nachdem man den bisherigen Erfolgen oder Misserfolgen der Forschung ein grösseres Subjektives Gewicht beimisst. Während der Forschung aber ist jeder Denker notwendig theoretisch Determinist."

make perfect predictions (Poincaré, 1905a, p. 340ff). However, in order to do science, one needs to work with the assumption that it is possible to make such classifications, to find ever more accurate laws of nature which will enable one to make better predictions; and according to Poincaré, exactly this assumption constitutes determinism (Poincaré, 1905a, p. 347). Determinism is thus a working hypothesis which can never be disproven and which can be confirmed to the extent that science is successful:

> Science is deterministic; it is deterministic *a priori*; it postulates determinism, because without it, science could not exist. It is also deterministic *a posteriori*; if it started out by postulating determinism, as a necessary condition for its existence, it then demonstrates determinism precisely by existence, and each of its conquests is a victory of determinism. (...) science, rightly or wrongly, is deterministic; wherever it enters, it brings determinism. (Poincaré, 1917 [1913], p. 244).[3]

In (1905a), he writes that, since we choose to work with the assumption of determinism in science, it can be maintained that "we are determinists voluntarily" (Poincaré, 1905a, p. 347).

Another development which led to changes in the conception of determinism in late nineteenth century physics was the development of rigorous foundations of mathematics, and the implications of this development for how mathematics was thought to relate to physical reality (paper 5). Especially relevant are the concerns about the applicability of differential calculus in physics which arose in this period, since mathematically, determinism in classical physics is usually formulated in terms of differential equations. Whether differential equations directly correspond to nature is of central relevance for the status of determinism in physics: we have seen in paper 2 that Bertrand argued in 1878 that the non-uniqueness of solutions to differential equations in the case of Norton dome-type indeterminism does not imply ontological indeterminism, because it is an artefact of the use of differential equations, and differential equations offer according to him mere approximations of what is going on in reality.

Through the development of rigorous foundations of analysis in the nineteenth century, it became clear that the applicability of differential calculus to physics depends on the acceptance of mathematical conditions of continuity. We have seen in paper 5

[3] "La science est déterministe; elle l'est a priori; elle postule le déterminisme, parce que sans lui elle ne pourrait être. Elle l'est aussi a posteriori; si elle a commencé par le postuler, comme une condition indispensable de son existence, elle le démontre ensuite précisément en existant, et chacune de ses conquêtes est une victoire du déterminisme. (...) la science, à tort ou à raison, est déterministe; partout où elle pénètre, elle fait entrer le déterminisme."

how Poincaré argued on the basis of a Kantian philosophy of mathematics for the acceptance of these continuity conditions. Like determinism, continuity is a principle which can neither be strictly proven nor falsified but which is needed for doing physics. The two are intimately connected; in (1905b), Poincaré argues that the acceptance of continuity conditions makes it possible to formulate laws of physics in terms of differential equations which have a unique solution for given initial conditions, and therefore, the acceptance of continuity is at the basis of a deterministic physics.

Boltzmann, on the other hand, because of concerns about the notion of infinity, argued that we must ultimately take nature to be discontinuous; while with proper care, differential calculus can be used as a calculational tool in physics, differential equations cannot be taken to correspond directly to nature. Israel (1992) has argued that Boltzmann did not have a conception of determinism in terms of differential equations, and that this can be explained through his concerns about the applicability of differential calculus in physics. He points at Boltzmann's work in hereditary mechanics: in the 1870s, Boltzmann found that for elastic phenomena, the state of the system at an instant does not suffice to determine future states; rather, the future states of the system can be determined on the basis of its present state plus its past history (note the similarity with the physiological determinism of Cournot and Boussinesq, described in paper 3). To describe such systems, Boltzmann used integro-differential equations. This approach was further developed in the early twentieth century by Picard and Volterra and came to be known as hereditary mechanics (Israel, 1992). In 1910, Painlevé criticized the program, because it went against the Laplacian idea that the state of a system at an instant must be fully determined by the state at the previous instant, and the idea that processes can be fully described through differential equations. Painlevé even stated that "The conception according to which, in order to predict the future of a material system, one has to know its entire past, *is the very negation of science*" (quoted in Israel, 1992, p. 269).[4] Israel notes that Boltzmann was not bothered by the implications of hereditary mechanics for determinism, and argues that this can be explained by the fact that Boltzmann did not have a conception of determinism in terms of differential equations in the first place, and that this relates to his concerns about the applicability of differential calculus in physics.

Thus, despite the fact that Boltzmann, in contrast to Mach and Poincaré, argued for a mechanistic conception of physics, he was not committed to the idea that there are laws of mechanics in the form of differential equations which always have a unique solution for given initial conditions. Rather, like Poincaré and Mach, Boltzmann argues that

[4] "La conception d'après laquelle, pour prédire l'avenir d'un système matériel, il faudrait connaître tout son passé, est la négation même de la science".

determinism is a necessary precondition for science: he argues that science is made possible through a "principle of unique determination of natural processes",[5] which in mechanics can be formulated as stating "that the same movement always occurs, when the immediate environment is in the same state" (Boltzmann, 1899, p. 277).[6] Boltzmann's determinism is thus a local determinism: we cannot know the state of the universe at an instant, so we are limited to considering a smaller part of it.[7] Moreover, there is no a priori certainty that this determinism holds (Boltzmann argues against the claim that we can know anything with a priori certainty; see Fasol-Boltzmann, 1990, p. 160). However, it is a condition for the possibility of science: if processes are not determined by local circumstances, there is no way of investigating them (Boltzmann, 1899, p. 276-77).

Thus, Mach, Poincaré and Boltzmann all have determinism as a working hypothesis or precondition for science, as a regulative principle, which can be verified to the extent that science is successful, and on which the possibility of science depends. Romizi (2013, p. 93-144) has argued that around the nineteenth century, starting with Kant, there was a process of "de-ontologisation" of scientific determinism, as it was increasingly admitted that "the world can at most appear as 'deterministic' in a certain respect, or relative to certain characteristics, or thanks to certain forms of ordering of the knowing subject" (Romizi, 2013, p. 143).[8] Indeed, the type of determinism for which Poincaré, Mach and Boltzmann argue is not necessarily an ontological determinism: it's a precondition for physical theories rather than a claim about the world. Moreover, determinism in this form does not have to hold absolutely or on a universal scale. We have seen that Boltzmann's determinism is a local determinism which lacks absolute certainty. Mach also doesn't think that determinism holds necessarily; moreover, he rejects Laplace's formulation of determinism on a universal scale, because it depends on a notion of absolute time which is according to him not warranted.[9] Poincaré argues in

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[5] "Prinzip der eindeutigen Bestimmtheit der Naturvorgänge"

[6] "dass immer dieselbe Bewegung erfolgt, wenn die unmittelbare Umgebung sich in demselben Zustande befindet".

[7] If you take the locality to be spatial and not temporal, this is compatible with hereditary mechanics.

[8] "die Welt kann höchstens in einer gewissen Hinsicht, oder relativ zu gewissen Merkmalen, oder dank gewisser Ordnungsformen des erkennenden Subjekts als 'deterministisch' erscheinen".

[9] According to Mach, time is relational, and therefore, to describe how something changes in time, one has to describe how it changes relative to a part of the world functioning as a clock (e.g. describing the motion of the heavenly bodies in time can be regarded as equivalent to describing the motion of the heavenly bodies relative to the rotation of the earth). But for the universe as a whole, there is no clock relative to which we can describe its change, hence for the universe as a whole there is no time. Therefore, Laplace's statement that the state of the universe at an instant is the cause of the state of the universe at the next instant is according to Mach mistaken (Mach, 1872, p. 36-37; Romizi, 2013, p. 133).

(1917 [1913], p. 244-45) that determinism can be confirmed to the extent that science is successful, but he remains agnostic about whether it holds absolutely; but in (1917 [1913], p. 8) and in (1908, p. 395), he does argue for universal determinism in Laplacian terms, and writes that a perfect intelligence with perfect knowledge can make perfect predictions and that for such an intelligence, there is no pure chance. All of them argue that physicists must look for deterministic laws, but neither of them unambiguously argues that there are laws of mechanics in the form of differential equations through which every process in physics is uniquely determined.

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Conclusion

One conclusion that can be drawn from the papers in this thesis is that it is not the case that around the nineteenth century, determinism was commonly accepted as a provable fact about physics. To prove that there is determinism on the basis of the laws of mechanics, at least two things are needed:

- One needs to be able to prove that the laws of mechanics always have a unique solution for given initial conditions. This requires existence and uniqueness conditions for the solutions to differential equations, which were only developed in the course of the nineteenth century (and even then they left open the possibility of Norton dome-type indeterminism).
- One needs to work with the assumption that the (differential) equations of mechanics form a complete and rigorous description of reality - this was increasingly questioned towards the end of the nineteenth century.

Nevertheless, most (but not all) physicists in this period did accept determinism. Determinism was often taken to be an a priori truth or a methodological presupposition rather than a provable fact about physics.

Whereas in the current literature on determinism in physics, determinism is usually defined in terms of uniqueness of solutions to the equations describing physical systems, in the nineteenth century these were not regarded as equivalent. It is possible that the mathematical equations do not tell all there is to say about a physical system, or that they offer mere approximations, and that for example one has indeterminism in the equations and determinism in physical reality. One can thus make a distinction between ontological determinism and mathematical determinism:

(A) [ontological determinism] Given the present state of a system, there is only one possible future evolution.

(B) [mathematical determinism] The state of the system can be described by mathematical (differential) equations, which always have a unique solution for given initial conditions.

We have seen in paper 1 that while Laplace endorses (A), there is no evidence that he does so on the basis of (B), for he could not, strictly speaking, prove that (B) is the case. The argument that Laplace gives for determinism is not based on uniqueness of solutions to equations; rather, it goes back to eighteenth century Leibnizian metaphysics, and is based on the law of continuity and the principle of sufficient reason. However, Laplace did expect both (A) and (B) to be the case: his metaphysical argument for determinism also lends support to the idea that one can find laws of nature, of a mathematical form, through which processes in physics are uniquely determined. For eighteenth century authors such as Johann Bernoulli and Condorcet, the law of continuity directly supports (A); but at the same time, the law of continuity is at the basis of the use of differential equations in physics, and of the expectation that these equations have unique solutions for given initial conditions. There was thus a correspondence between mathematics and metaphysics.

The treatment of the Norton dome-type indeterminism (Lipschitz indeterminism) in the nineteenth century shows that if confronted with a falsification of (B), some physicists and mathematicians have still maintained (A). Poisson and Duhamel argue that if the equations of motion for a system fail to have a unique solution for given initial conditions, one needs to bring in other considerations to determine what will happen, for it is clear to them that there can only be one possible future evolution of the system. Bertrand also argues that there must be determinism in reality, and concludes that if the equations of motion fail to have a unique solution for given initial conditions, this indicates a mismatch between the mathematical equations and physical reality. For these authors, determinism is an a priori truth, that can be maintained even if the theory fails to reflect it; thus, for them, (A) is fundamental.

Boussinesq, on the other hand, uses Norton dome-type indeterminism as an argument for genuine indeterminism and free will: he argues that in processes in which free will is involved, neither (A) nor (B) holds. However, things are different for physiological processes in which no free will is involved: he argues that such processes are deterministic in a sense that is irreducible to physical determinism. Specifically, in physiology, the future evolution of a system is not determined on the basis of the state of the system at an instant, but it is determined on the basis of the entire past of the system (Cournot made a similar argument). Thus, Boussinesq argues that in physiology, as long as no free will is involved, (B) fails while a variation on (A) holds (see paper 2 and 3).

In paper 3, we have seen that around the mid-nineteenth century, there was a debate about the possibility of intervention of vital and mental causes in physical systems, and the question whether all processes in nature are fully determined by the laws of physics. While Helmholtz and Du Bois-Reymond argued on the basis of the law of conservation of energy that there was no way in which non-physical causes, such as vital and mental causes, could intervene in a physical system, among others Maxwell, Stewart, Cournot and Boussinesq maintained that the possibility of intervention of such non-physical causes cannot be excluded. The fact that mechanics allows for unstable systems on which the slightest disturbance can have a large effect means that, even if mechanical systems, if left to themselves, are deterministic, there may be unobservably small interventions by a vital or mental agency which are not determined physically and which change the course of the system. For this reason, one cannot infer that there is determinism at the ontological level on the basis of determinism in the equations of physics.

In the late nineteenth century, mechanics lost its status as fundamental theory to which all of physics is ultimately reducible (paper 4). This development can explain a weakening in determinism in physics that took place in this period: for Mach, Poincaré and Boltzmann, determinism is a precondition for science which can only be proven to the extent that science is successful. They thus have (B) as an ideal, rather than as an established truth (paper 6).

Furthermore, this research project has shown that the relation between mathematics and physical reality is crucial for the status of determinism in physics, and that there were fundamental changes in this relation around the nineteenth century. If there is even the slightest mismatch between the mathematical equations of physics and physical reality, the equations may function very well for calculations and predictions in the large majority of cases, but they may allow for singularities and cases of indeterminism which do not correspond with physical reality. In the late nineteenth and early twentieth century, there were several physicists and mathematicians who took the possibility of such a small mismatch seriously (among others Bertrand, Boussinesq and Boltzmann). In particular, I have argued that the development of rigorous foundations of analysis led to concerns about the applicability of differential calculus in physics (paper 5).

Thus, in the period from the eighteenth until the early twentieth century, determinism appeared in various forms, but rarely as a provable fact about physics. Rather than being an established feature of physical theory, determinism could play a role as a regulative principle in physics. Moreover, determinism was connected with principles of causality and continuity and with the principle of sufficient reason, principles which in this period played central roles in physics and mathematics.

Around the nineteenth century, it was commonly accepted that there can be no uncaused events - this was crucial in the nineteenth century treatment of Norton dome-type indeterminism, for example, and it was even accepted by an indeterminist such as Boussinesq, who argued that in cases in which the equations of physics leave the future of a physical system undecided, one needs to assume an intervention of the will or a vital principle to determine what will happen. For Poincaré, Boltzmann and Mach, it was a methodological presupposition of science that in equal circumstances, the same always happens.

Furthermore, we have seen how both in the eighteenth and in the early twentieth century, assumptions of continuity in nature, or arguments for why nature can be taken to be continuous in the relevant sense, provided a foundation for the applicability of differential calculus in physics, and for the possibility to formulate laws of nature in terms of differential equations. The determinism of Laplace and his predecessors was based on an interpretation of the principle of sufficient reason and the law of continuity: these principles were taken to imply that the state of the universe continuously determines subsequent states, through infinitesimal steps, and that there can be no discontinuous changes between states (paper 1). The same reasoning was also at the basis of differential calculus and its application in physics. Through the development of rigorous foundations of differential calculus, in the nineteenth century, it became clear that the applicability of differential calculus in physics depends on the acceptance of mathematical conditions of continuity. For Boltzmann and Poincaré, the acceptability of these mathematical conditions of continuity depended on whether they could be given a basis in intuition or in experience - Poincaré argued that physicists necessarily have to work with a continuous conception of nature, and that it is on this basis that differential calculus can be applied in physics (paper 5). However, for Poincaré, the principles of continuity that are involved in the applicability of differential calculus in physics are epistemological rather than metaphysical.

Whatever the philosophical significance of the introduction of quantum mechanics was, it did not lie in the fact that quantum mechanics undermined a claim that was before then considered to be conclusively proven, namely that fundamental physics is deterministic. Rather, quantum mechanics went against commonly accepted principles in physics, such as continuity between states and there being no uncaused events.

Summary

It is commonly thought that before the introduction of quantum mechanics, determinism was a straightforward consequence of the laws of mechanics. However, around the nineteenth century, many physicists, for various reasons, did not regard determinism as a provable feature of physics. This is not to say that physicists in this period were not committed to determinism; there were some physicists who argued for fundamental indeterminism, but most were committed to determinism in some sense. However, for them, determinism was often not a provable feature of physical theory, but rather an a priori principle or a methodological presupposition.

Determinism was strongly connected with principles of causality and continuity and the principle of sufficient reason; this thesis examines the relevance of these principles in the history of physics. Moreover, the history of determinism in this period shows that there were essential changes in the relation between mathematics and physics: whereas in the eighteenth century, there were metaphysical arguments which lent support to differential calculus, by the early twentieth century the development of rigorous foundations of differential calculus led to concerns about its applicability in physics.

The thesis consists of six papers. In the first paper, "On the origins and foundations of Laplacian determinism", I argue that Laplace, who is usually pointed out as the first major proponent of scientific determinism, did not derive his statement of determinism directly from the laws of mechanics; rather, his determinism has a background in eighteenth century Leibnizian metaphysics, and is ultimately based on the law of continuity and the principle of sufficient reason. These principles also provided a basis for the idea that one can find laws of nature in the form of differential equations which uniquely determine natural processes.

In "The Norton dome and the nineteenth century foundations of determinism", I argue that an example of indeterminism in classical physics which has attracted attention in philosophy of physics in recent years, namely the Norton come, was already discussed during the nineteenth century. However, the significance which this type of indeterminism had back then is very different from the significance which the Norton dome currently has in philosophy of physics. This is explained by the fact that

determinism was conceived of in an essentially different way: in particular, the nineteenth century authors who wrote about this type of indeterminism regarded determinism as an a priori principle rather than as a property of the equations of physics.

In "Vital instability: life and free will in physics and physiology, 1860-1880", I show how Maxwell, Cournot, Stewart and Boussinesq used the possibility of unstable or indeterministic mechanical systems to argue that the will or a vital principle can intervene in organic processes without violating the laws of physics, so that a strictly dualist account of life and the mind is possible. Moreover, I show that their ideas can be understood as a reaction to the law of conservation of energy and to the way it was used in physiology to exclude vital and mental causes.

In "The nineteenth century conflict between mechanism and irreversibility", I show that in the late nineteenth century, there was a widespread conflict between the aim of reducing physical processes to mechanics and the recognition that certain processes are irreversible. Whereas the so-called reversibility objection is known as an objection that was made to the kinetic theory of gases, it in fact appeared in a wide range of arguments, and was susceptible to very different interpretations. It was only when the project of reducing all of physics to mechanics lost favor, in the late nineteenth century, that the reversibility objection came to be used as an argument against mechanism and against the kinetic theory of gases.

In "Continuity in nature and in mathematics: Boltzmann and Poincaré", I show that the development of rigorous foundations of differential calculus in the nineteenth century led to concerns about its applicability in physics: through this development, differential calculus was made independent of empirical and intuitive notions of continuity and was instead based on mathematical continuity conditions, and for Boltzmann and Poincaré, the applicability of differential calculus in physics depended on whether these continuity conditions could be given a foundation in intuition or experience.

In the final paper, "Determinism around 1900", I briefly discuss the implications of the developments described in the previous two papers for the history of determinism in physics, through a discussion of determinism in Mach, Poincaré and Boltzmann. I show that neither of them regards determinism as a property of the laws of mechanics; rather, for them, determinism is a precondition for science, which can be verified to the extent that science is successful.

Samenvatting

Men gaat er doorgaans van uit dat voor de introductie van de quantummechanica, determinisme een rechtstreekse consequentie was van de wetten van de mechanica. Echter, rond de negentiende eeuw beschouwden veel natuurkundigen, om uiteenlopende redenen, determinisme niet als een bewijsbare eigenschap van de natuurkunde. Dit wil niet zeggen dat natuurkundigen in deze periode geen determinist waren; er waren een aantal natuurkundigen die fundamenteel indeterminisme verdedigden, maar de meesten accepteerden determinisme in de een of andere vorm. Echter, zij beschouwden determinisme vaak niet als een bewijsbare eigenschap van de natuurkunde, maar eerder als een a priori principe of als een methodologische vooraanname.

Determinisme was sterk verbonden met principes van causaliteit en continuïteit en het principe van voldoende grond; dit proefschrift laat de relevantie zien van deze principes in de geschiedenis van de natuurkunde. Verder laat de geschiedenis van het determinisme in deze periode zien dat er essentiële veranderingen plaatsvonden in de relatie tussen wiskunde en natuurkunde: terwijl er in de achttiende eeuw metafysische argumenten waren die de differentiaalrekening ondersteunden, leidde tegen het begin van de twintigste eeuw de ontwikkeling van rigoureuze funderingen van de differentiaalrekening tot bezorgdheid over haar toepasbaarheid in de natuurkunde.

Dit proefschrift bestaat uit zes artikelen. In het eerste artikel, getiteld "Over de oorsprong en fundering van Laplace's determinisme", betoog ik dat Laplace, die in het algemeen beschouwd wordt als de eerste belangrijke verdediger van het wetenschappelijk determinisme, zijn formulering van dit determinisme niet direct kon afleiden uit de wetten van de mechanica. Zijn determinisme heeft een oorsprong in achttiende eeuwse Leibniziaanse metafysica, en is uiteindelijk gebaseerd op de wet van continuïteit en het principe van voldoende grond. Deze principes vormden ook een basis voor het idee dat het mogelijk is om natuurwetten te vinden in de vorm van differentiaalvergelijkingen waardoor natuurlijke processen uniek gedetermineerd zijn.

In "De Norton dome en de negentiende-eeuwse grondslagen van het determinisme" laat ik zien dat een voorbeeld van indeterminisme in de klassieke natuurkunde dat in recente jaren aandacht heeft gekregen in de filosofie van de natuurkunde, namelijk de Norton dome, al tijdens de negentiende eeuw besproken werd. Echter, de betekenis die dit type van indeterminisme in die tijd had is sterk verschillend van de betekenis die de Norton dome tegenwoordig heeft in de filosofie van de natuurkunde. Dit kan worden verklaard door het feit dat in de negentiende eeuw, determinisme op een essentieel verschillende manier beschouwd werd: de auteurs die dit geval van indeterminisme bespraken beschouwden determinisme eerder als een a priori waarheid dan als een eigenschap van de vergelijkingen van de natuurkunde.

In "Vitale instabiliteit: leven en vrije wil in fysica en fysiologie, 1860-1880" laat ik zien hoe Maxwell, Cournot, Stewart en Boussinesq de mogelijkheid van instabiele of indeterministische mechanische systemen gebruikten om te beargumenteren dat in organische processen, de wil of een vitaal principe kan interveniëren zonder de natuurwetten te breken, zodat een strikt dualistische opvatting van leven en de geest mogelijk is. Verder laat ik zien dat hun ideeën begrepen kunnen worden als een reactie op de wet van energiebehoud en op de manier waarop deze wet binnen de fysiologie werd gebruikt om vitale en mentale oorzaken uit te sluiten.

In "Het negentiende-eeuwse conflict tussen mechanisme en irreversibiliteit" laat ik zien dat er tegen het eind van de negentiende eeuw een wijdverspreid conflict was tussen de doelstelling om fysische processen tot mechanica te reduceren en de realisatie dat bepaalde processen onomkeerbaar zijn. Terwijl het zogenaamde omkeerbaarheids-bezwaar bekend is geworden als een bezwaar dat gemaakt werd tegen de kinetische gastheorie, verscheen dit zogenaamde bezwaar in feite in vele uiteenlopende argumenten en het was vatbaar voor zeer verschillende interpretaties. Pas toen tegen het eind van de negentiende eeuw het project om de volledige natuurkunde tot mechanica te reduceren aan populariteit verloor werd het omkeerbaarheids-bezwaar gebruikt als een argument tegen mechanicisme en de kinetische gastheorie.

In "Continuïteit in de natuur en in de wiskunde: Boltzmann en Poincaré" laat ik zien dat de ontwikkeling van rigoureuze funderingen van de differentiaalrekening in de negentiende eeuw tot bezorgdheid leidde over de toepasbaarheid van differentiaalrekening in de natuurkunde: door deze ontwikkeling werd differentiaalrekening onafhankelijk gemaakt van empirische en intuïtieve noties van continuïteit en in plaats daarvan gebaseerd op wiskundige condities van continuïteit, en voor Boltzmann en Poincaré was de toepasbaarheid van differentiaalrekening in de natuurkunde afhankelijk van of er voor deze condities een fundering gevonden kon worden in intuïtie of ervaring.

In het laatste artikel, "Determinisme rond 1900", geef ik een korte bespreking van de implicaties van de ontwikkelingen beschreven in de vorige twee artikelen voor de geschiedenis van het determinisme in de natuurkunde, door middel van een bespreking van determinisme in Mach, Poincaré en Boltzmann. Ik laat zien dat geen van hen determinisme beschouwt als een eigenschap van de wetten van de mechanica; voor hen is determinisme eerder een voorwaarde voor de wetenschap, die geverifieerd kan worden voor zover wetenschap succesvol is.

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