The Norton dome and the nineteenth century foundations of determinism

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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.

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; 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. 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.

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.

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.

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.

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.

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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