Continuity in nature and in mathematics: Boltzmann and Poincaré

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Synthese, DOI: 10.1007/s11229-015-0701-9

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 strictly mathematical conditions of continuity. However, for Boltzmann and Poincaré, the applicability of mathematics in physics depended on whether there is a basis in physics, intuition or experience for the fundamental axioms of mathematics – and this meant that to determine the status of differential equations in physics, they had to consider whether there was a justification for these mathematical continuity conditions in physics. For this reason, their ideas about continuity and discreteness in nature were entangled with epistemology and philosophy of mathematics. They reached opposite conclusions: Poincaré argued that physicists must work with a continuous representation of nature, and thus with differential equations, while Boltzmann argued that physicists must ultimately take nature to be discrete.

Introduction

Through the development of rigorous foundations of differential calculus during the nineteenth century, differential calculus was based on continuity conditions that were strictly mathematically defined, and was made independent of any empirical or intuitive notions of continuity. 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 are not justified from a physical point of view, 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 justified in physics. However, they reach different conclusions: Poincaré argues that these continuity conditions are warranted and that physicists should work with continuous representations of nature, so that differential calculus is directly applicable, while Boltzmann argues 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 argued that 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. Moreover, 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 to 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 their different philosophies of mathematics led Boltzmann and Poincaré to opposite conclusions regarding this issue.

1. 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 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 was 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). 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 non-zero but smaller than any finite quantity.

In 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 of 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 intuition. 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] The situation was less clear for the applicability of differential calculus in physics: 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):

> ...it seems that by being arithmetized, by being idealized so to speak, mathematics has moved away from nature, and the philosopher can always ask himself 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]

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

[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."

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, Boltzmann and Poincaré had fundamentally different thoughts on this issue. In section (2), I discuss the first continuity condition, and in section (3) the second.

2. The mathematical continuum

2.1 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 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 involved 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

[3] The reason why the rational numbers do not suffice as a basis for differential 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).

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 a harmless but fallible assumption about nature, 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.

2.2 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

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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 writes that it is at least thinkable, and compatible with experience, that in reality, there are discrete elements rather than a continuum: "Wherever we believe to find a continuum, this only means that we can make observations of the smallest observable parts of the system in question that are analogous to observations of larger parts, and observe an analogous behaviour. How far this continues can only be decided through experience. As long as experience has raised no objections, we can maintain the in no way harmful but merely convenient fiction of the continuum." (Mach, 1896, p. 77).

infinity. Boltzmann emphasizes at various points in his writings that in physics, infinity means nothing more than a transition to a limit (e.g. in Boltzmann, 1877, p. 167; 1904, p. 358) and that actual infinity has no place in physics. 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", 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). [7] Similarly, set theory shows how one can define the

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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] De Courtenay (2002, 2010) argues that Boltzmann embraced the arithmetization of analysis, and that according to Boltzmann, mathematics is an autonomous domain, of which the applicability to nature is not intrinsic, but dependent on practical context. This autonomy of mathematics, however, is hard to reconcile with the central role that Boltzmann gives to experience in his philosophy of mathematics in his *Lectures on natural philosophy* (Fasol-Boltzmann, 1990). According to the views that Boltzmann here presents, the basic concepts and theorems in mathematics must ultimately be grounded in experience and the justification for the introduction of mathematical entities depends on whether they can be made empirically relevant, which undermines the autonomous development of mathematics.

infinite and work with it in a consistent manner. However, in the case of infinity, Boltzmann does not think that any justification can be derived from its use in physics.

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 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 physical reasoning. 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 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 overdetermination: 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 places at which we

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[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."

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 is continuous:

> ...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 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:

> One can forgive me for the somewhat banal expression when I say that one 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 time and space. As Boltzmann expresses it, there

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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."

[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."

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 authors such as Mach, who favoured a phenomenalist approach in physics, in which hypotheses involving unavoidable entities such as atoms were avoided. In the late nineteenth century, such phenomenalist and anti-atomistic approaches gained popularity, being supported among others by Poincaré, Duhem, and Ostwald (Van Strien, 2013); however, the search for atomistic theories always remained a strong research field. Boltzmann vehemently defended atomism against the trend of phenomenalism in physics, among others by pointing at the fruitfulness of atomistic theories (see e.g. Boltzmann, 1897a). Furthermore, he argued that atomism was no more hypothetical than the use of differential equations in physics: in order to work with differential equations, you have to start with 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]

But as Wilholt (2002) has emphasized, Boltzmann's claim is much stronger than that atomism is preferable to continuous representations of nature: he in fact argues 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

[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 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.

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 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 Boltzmann did not think this was justified. Therefore, he argued for a fundamentally discrete conception of nature.

2.3 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; and "This is the assumption which we implicitly admit when we apply the laws of mathematical 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, there needs to be some foundation for the applicability of the continuum of the real numbers to physical reality. 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

[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 only has a unique solution if F(r) satisfies a condition that is now known as Lipschitz continuity (see Van Strien, 2014).

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; see also Folina, 1992). 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 our experience of continuity. The continuum of the real numbers is an invention we make in order to interpret our experience of continuity 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).[17] 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 comparison of sensations of weight is non-transitive (Poincaré, 1902, p. 46).[18] 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 to

[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).

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

[18] 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.

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 smallest weight elements. This is surely not the case: 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 thinks we have to conceive of weight being infinitely divisible because we cannot possibly have an experience of smallest weight 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.[19]

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).

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 we experience as well as 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

[19] "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).

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 correspondence of the values that physical quantities can take with the real numbers has synthetic a priori status, it would mean not only that our experience of e.g. space, time and mass leads us to the 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 inconceivable 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 "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".[20] He refers to two authors who have argued for a discontinuous description of reality: a philosopher, François Evellin, and a mathematician, Joseph Bertrand.[21] Poincaré seems to argue that 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 intuition of continuity, and at the cost of higher mathematical complication. A discontinuous description of reality will make physics much more complicated, in particular because it would mean that one cannot work with differential equations in physics, and therefore the laws of physics would have to take a different form.[22]

Moreover, he mentions a third alternative besides the assumption 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 type that is given directly in sensation, which differs from the mathematical

[20] "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".

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

[22] 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).

continuum in being non-transitive. However, to work out a theory on this basis “would not be as easy as with the system of Mr. Evellin, and it would probably be difficult to give this idea a mathematical form and make it compatible with absolute determinism" (Poincaré, 1905b).[23]

Thus, for Poincaré, the mathematical continuum of the real numbers has a basis in the fact that we cannot conceive of e.g. space, time and mass as being only finitely divisible, and we cannot conceive of two lines crossing without meeting in a point. A discontinuous description of nature, although strictly speaking possible, thus goes against the way in which we conceive of physical quantities; moreover, it will make it much more complicated to work out theories in physics, since it means that one cannot make use of differential equations.

3. Continuity of change

3.1 Continuity of change and the foundations of analysis

A second type of continuity assumption that is needed for the applicability of differential calculus in physics is, roughly speaking, the assumption 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.

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[23] "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, 2014).

- 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.

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 relations between physical quantities always satisfy 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.

3.2 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 argued that there was no reason to reject the use of continuous but nowhere differentiable functions in physics: in fact, he argued, 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 that are non-differentiable. Boltzmann was not alone in making this argument. In 1909, Perrin (famous for his work on Brownian motion) argued that trajectories of particles in Brownian motion are so irregular that it is impossible to find a tangent to them, and then suggested 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).[24]

Moreover, the fact that Boltzmann denies that physical quantities (including space and time) can be taken to have continuous values implies that strictly speaking, 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 that should not be taken as true representations, but rather as 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. De Courtenay (2002) argues that for Boltzmann, a mathematical framework like that of differential calculus can itself be understood as a model: it may be in agreement with experience to a sufficient degree, but cannot be expected to directly represent nature (rather, it offers an 'arithmetical analogy' of the phenomena).

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"[25] 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)

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[24] "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).

[25] "hinlänglich sicher gestellte Erfahrungstatsache"

he refers to this statement as the "law of continuity" and adds that it is a necessary 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 some cases there are not motions, such as for example thermal motions of molecules, which can be better represented by a function similar to the Weierstrass function than by a differentiable one. (Boltzmann, 1899a, p. 283).[26]

However, Boltzmann 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).[27]

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 phenomenalists (in particular Mach) who had argued against the use of hypotheses about unobservable entities such as atoms in physics: Boltzmann argues that to rule out all hypothetical elements in physics would entail that you cannot take functions in physics to be differentiable, while Mach in fact used differential equations freely in his work in physics (Boltzmann, 1899a). It has to be noted here that, contrary to what Boltzmann suggests, Mach did not actually go as far as to rule out all hypothetical elements in physics, as can be seen by his treatment of the issue of continuity which is discussed in section 2.1.

But Boltzmann argues 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.

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[26] "...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."

[27] "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."

3.3 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 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 acknowledged that Weierstrass' example of a continuous but nowhere differentiable function 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).[28]

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

[28] 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)

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:

> ...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 and differentiability of functions. In this sense, continuity is an a priori principle required for the possibility of science. According to Friedman's (1999) interpretation of Poincare, a priori principles which are required for the possibility of science all fit in a hierarchical structure. Logic is required for the possibility of arithmetic, arithmetic is required for the possibility of geometry, geometry is required for the possibility of mechanics, etc., and at these different levels, we find different types of a priori principles, e.g. arithmetic is based on synthetic a priori principles and geometry is based on conventions. In Friedman's interpretation, levels higher than mechanics are purely empirical. However, the principle of continuity is not easily classified in this way. It is of the same type as assumptions of simplicity, which according to Poincaré are essential for science.[29] These assumptions are required for the possibility of science, but do not belong to a specific level in the hierarchy of the sciences.

Moreover, Poincaré 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

[29] 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).

do so without fear of ever being contradicted, either by present experience or by any future experiment. (Poincaré, 1905a, p. 288).[30]

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).[31] 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 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.[32]

Moreover, the claim that one can always work with continuous functions was soon undermined by the development of quantum theory, which showed that continuous descriptions of nature are not always possible. 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 theory) and concluded after a careful study that there was 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

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[30] 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."

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

[32] For a function to be approximable as closely as one wishes by a continuous function, this function must be the (pointwise) limit of a sequence of continuous functions. Functions for which this holds are called Baire class I functions. Not all discontinuous functions are Baire class I functions which means that not all functions can be approximated as closely as one wishes by a continuous function.

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 finite interval, you can derive how it behaves elsewhere.

later he left the issue open of whether it would ever be possible to account for quantum theory 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).[33]

In any case, at least until he became aware of the new research in black body radiation and quantum theory, 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 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

The development of rigorous foundations of differential calculus made clear that in order to formulate the laws of physics in terms of differential equations, one needs to work with the assumption that the values that physical quantities can take correspond to the mathematical continuum of the real numbers, and that relations between these quantities can be expressed by functions that are both continuous and differentiable. That these assumptions were non-trivial can be seen by the fact that both Boltzmann and Poincaré were concerned about their foundations. In particular, both of them were concerned that through the development through which mathematics, and in particular differential calculus, was made independent of physical reality and of empirical and intuitive notions, the applicability of mathematics in physics was endangered. They

[33] "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."

therefore considered whether the continuity conditions that are needed for the applicability of differential calculus could be justified in physics.

Boltzmann and Poincaré were not so much concerned with the question whether nature in fact *is* continuous or discrete. Nor is the issue merely to find out whether continuous or discrete descriptions work best in a given situation. Rather, they based their arguments for continuity or discreteness in nature on epistemological concerns. Boltzmann argues that the need to avoid paradoxes in physics leads to a rejection of infinity and the mathematical continuum in physics; therefore, laws which are formulated in terms of differential equations cannot be taken to directly correspond to nature, but we can conceive of theories involving differential equations as idealized models. Poincaré argues that the way we conceive of physical quantities leads us to the mathematical continuum, and that the possibility of science depends on assumptions such as the assumption that functions in physics are continuous and differentiable. Thus, they favour either continuous or discrete descriptions of nature on the basis of epistemological considerations; and the applicability of differential calculus in physics depends on these.

Acknowledgments

I would like to thank two anonymous referees for their helpful comments. Furthermore, I would like to thank Eric Schliesser and Boris Demarest for commenting on earlier drafts of this paper. I am also indebted to audiences at the workshop on Scientific Metaphysics in Ghent (17–18 February, 657 2014), the "Mathematizing science: limits and perspectives II" conference in Norwich (1–3 June, 2014), and the HOPOS conference in Ghent (3–5 July, 2014) for helpful feedback. The research for this paper was largely done at Ghent University, with funding from the Research Foundation Flanders (FWO). I would also like to thank the Max Planck Institute for the History of Science, where I finished the paper.

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