

PHILOSOPHIA SCIENTIÆ

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Philosophia Scientiæ, tome 1, n° 4 (1996), p. 59-67

http://www.numdam.org/item?id=PHSC_1996__1_4_59_0

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**Poincaré: Conservative Methodologist but
Revolutionary Scientist**

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Abstract. In 1906 Poincaré published: «*Sur la dynamique de l'électron*», which undoubtedly made a revolutionary advance in theoretical physics. The present paper seeks to investigate how this work of Poincaré's in physics relates to the methodological views which he expressed in 1902 in his *Science and Hypothesis*. The thesis is that Poincaré's work in science contradicts his methodology. In *Science and Hypothesis*, he wrote: "[...] experiment may serve as a basis for the principles of mechanics, and yet will never invalidate them." But only two years later in his St Louis address, he said that reflection on Kaufmann's experiments on the movement of electrons issuing from radium had convinced him of the need for abandoning Newton's principle in mechanics. Thus in his scientific practice Poincaré did not follow the conventionalism explicitly stated in *Science and Hypothesis*. Instead he followed a methodological approach more like that advocated by Duhem.

In his *Science and Hypothesis* published in 1902, Poincaré expounded a philosophy of science which has come to be known as *conventionalism*. At the time Poincaré was carrying out research in theoretical physics — particularly electrodynamics, and his work in this field led to the publication in 1905 and 1906 of two papers both entitled: *Sur la dynamique de l'électron*. These papers were published almost simultaneously with (though at a distance from!) Einstein's famous 1905 paper: *Zur Elektrodynamik bewegter Körper*, in which Einstein introduced the Special Theory of Relativity. To be more precise [see Giedymin 1982, 192], Poincaré's 1905 paper was published on 5 June. His 1906 paper was submitted in July 1905, but published in 1906. Einstein's 1905 paper was submitted on 30 June, and published on 26 September. This situation has given rise to different interpretations by experts in the history of physics of this period. Giedymin [1982, Ch. 5], and Zahar [1989, Ch. 5] have argued that Einstein and Poincaré discovered the Special Theory of Relativity independently at almost the same time. Miller, on the other hand, argues [1981, and 1984, Chs. 1 and 3] that Poincaré's work, though it had some points in common with Einstein's, was nonetheless lacking in some of the crucial insights and innovations which constitute Special Relativity. This is a fascinating controversy, but not one which I intend to enter. For my purposes it suffices to accept that, in his papers of 1905 and 1906, Poincaré made an important, indeed revolutionary, advance — whether or not that advance really constituted the introduction of the Special Theory of Relativity. As will become clearer later, I consider Poincaré to have taken a revolutionary step, because he reached the conclusion that Newtonian mechanics was inadequate, and needed to be replaced by a new mechanics.

The question I want to consider in this paper is not so much the exact nature of Poincaré's advance in 1905-1906, as the relation of this scientific advance to the methodological views which Poincaré had expressed in 1902. It would seem natural to suppose that

Poincaré made his scientific advance by an application of the methodology which he had expounded in 1902. However I will argue to the contrary that Poincaré made his advance not by following the methodological principles which he had expounded in 1902, but by breaking them. Poincaré, I will argue, was a conservative methodologist, and only became a revolutionary scientist by ignoring his own methodology. More specifically, Poincaré argued on methodological grounds in 1902 that Newtonian mechanics should never be modified in the light of new experimental results. Yet only two years later, the experimental results of Kaufmann led him to the conclusion that Newtonian mechanics must be changed. I will try to demonstrate this by first examining the conventionalist view of Newtonian mechanics, which Poincaré develops in his 1902 *Science and Hypothesis*, and then showing that Poincaré abandons this account in his subsequent scientific work.

Poincaré considers Newton's three laws of motion in turn beginning with the first law, or the principle of inertia. He first argues that this law cannot be established *a priori*. He says:

The Principle of Inertia. A body under the action of no force can only move uniformly in a straight line. Is this a truth imposed on the mind *a priori*? If this be so, how is it that the Greeks ignored it? How could they have believed that motion ceases with the cause of motion? or, again, that every body, if there is nothing to prevent it, will move in a circle, the noblest of all forms of motion? [Poincaré 1902, 91]

Poincaré's statement of the beliefs of the Greeks is perhaps a little misleading. According to Aristotle, only bodies in the heavenly region, and hence composed of the fifth heavenly element (aither), moved naturally in a circle. Sublunar bodies moved naturally in straight lines, either towards or away from the centre of the Earth. So it is not true that Aristotle and his followers believed that every body, if there is nothing to prevent it, will move in a circle. Poincaré's general argument is, nevertheless, convincing. Aristotelian mechanics is quite different from Newtonian mechanics, and yet Aristotelian mechanics was believed to be correct for many centuries. It is hard to see how this historical fact is compatible with Newton's laws of motion being *a priori* truths.

Having denied that the principle of inertia is an *a priori* truth, Poincaré goes on immediately to deny that it is an experimental fact:

Is, then, the principle of inertia, which is not an *a priori* truth, an experimental fact? Have there ever been experiments on bodies acted on by no forces? and, if so, how did we know that no forces were acting? The usual instance is that of a ball rolling for a very long time on a marble table; but why do we say it is under the action of no

force? Is it because it is too remote from all other bodies to experience any sensible action? It is not further from the earth than if it were thrown freely into the air; and we all know that in that case it would be subject to the attraction of the earth. [Poincaré 1902, 91-92]

Moreover, Poincaré argues that if the principle of inertia were an experimental law, it might, in future, be modified in the light of observation and experiment and replaced by a more accurate law. But Poincaré thinks that the revision of the laws of Newtonian mechanics is not a serious possibility. As he says:

An experimental law is always subject to revision; we may always expect to see it replaced by some other and more exact law. But no one seriously thinks that the law of which we speak will ever be abandoned or amended. Why? Precisely because it will never be submitted to a decisive test. [Poincaré 1902, 95-96]

Suppose, for example, we observe what seems to be a deviation from the principle of inertia. Such an apparent deviation, Poincaré argues, need never force us to abandon the principle of inertia, because we can always get round the difficulty by postulating that the deviation is due to invisible molecules:

If, then, the acceleration of bodies we cannot see depends on something else than the positions or velocities of other visible bodies or of invisible molecules, the existence of which we have been led previously to admit, there is nothing to prevent us from supposing that this something else is the position or velocity of other molecules of which we have not so far suspected the existence. The law will be safeguarded. [*ibid.*, 96]

Poincaré next applies essentially the same analysis to Newton's other two laws of motion - the law of acceleration, i.e. that force is mass times acceleration, and the law of equality of action and reaction. He concludes that all three laws are really nothing but definitions. As he says:

The principles of dynamics appeared to us first as experimental truths, but we have been compelled to use them as definitions. It is *by definition* that force is equal to the product of the mass and the acceleration; this is a principle which is henceforth beyond the reach of any future experiment. Thus it is by definition that action and reaction are equal and opposite. [*ibid.*, 104]

Poincaré does not deny that experiments and observations were important in building up the laws of classical mechanics, but he thinks that, because these laws have been turned into definitions, they will never be altered in the light of future observations and experiments. As he puts it:

[...] experiment may serve as a basis for the principles of mechanics, and yet will never invalidate them. [*ibid.*, 105]

Poincaré sums up his overall view regarding the principles of Newtonian mechanics by making his famous claim that these principles are conventions. As he says:

If these postulates possess a generality and a certainty which was absent in the experimental truths from which they were derived, it is because they reduce in final analysis to a simple convention that we have a right to make, because we are certain beforehand that no experiment will come to contradict it. This convention, however, is not absolutely arbitrary; it is not the child of our caprice. We admit it because certain experiments have shown us that it will be convenient, and thus is explained how experiment has been able to build up the principles of mechanics, and why, nevertheless, it cannot overthrow them. [*ibid.*, 136]¹

It can hardly be denied that the conventionalist view of Newtonian mechanics which Poincaré here advocates is very conservative in character. The claim is that the principles of Newtonian mechanics are conventions which will never be altered in the light of any future experimental findings. If Poincaré had remained faithful to this position, he would never have made the great contribution to physics which is contained in his papers of 1905 and 1906. However, he did not remain faithful to his conventionalist philosophy of 1902. Indeed he changed his mind on the question only two years later in his address to the International Congress of Arts and Science at St Louis, Missouri, which was delivered on 24 September 1904. I will analyse this address in a moment, and we shall see that Poincaré's change of opinion actually develops from some doubts and qualifications which are expressed in the later chapters of *Science and Hypothesis*. To these I now turn.

After his discussion of Newtonian mechanics in *Science and Hypothesis*, Poincaré goes on in the later chapters to consider some of the theories of contemporary physics, and, in particular, Lorentz's theory of electrodynamics for which he has a great admiration. There is only one problem. Lorentz's theory contradicts Newton's principle of the equality of action and reaction, which Poincaré has just claimed to be an irrefutable convention. As Poincaré says:

[...] if the theory of Lorentz [...] were true, Newton's principle would not apply to matter *alone* [...]. [Poincaré 1902, 171]

¹ I have here altered the standard English translation slightly to give a more accurate rendering of the French.

Poincaré does not abandon his conventionalist account at this point, but rather argues that Newton's principle continues to hold for matter *and ether*. As he says:

Assume that Newton's principle of the equality of action and re-action is not true if applied to matter *alone*, and that this can be proved. The geometrical sum of all the forces applied to all the molecules would no longer be zero. If we did not wish to change the whole of the science of mechanics, we should have to introduce the ether, in order that the action which matter apparently undergoes should be counterbalanced by the re-action of matter on something. [*ibid.*, 170]

This would seem to be a satisfactory resolution of the difficulty, and yet, a few pages later, Poincaré, after summarising the view, says that it is very likely not correct.

The most satisfactory theory is that of Lorentz; it is unquestionably the theory that best explains the known facts, the one that throws into relief the greatest number of known relations, the one in which we find most traces of definitive construction. That it still possesses a serious fault I have shown above. It is in contradiction with Newton's law that action and re-action are equal and opposite — or rather, this principle according to Lorentz cannot be applicable to matter alone; if it be true, it must take into account the action of the ether on matter, and the re-action of the matter on the ether. Now, in the new order, it is very likely that things do not happen this way. [Poincaré 1902, 175]

This last sentence perhaps contains the seed of Poincaré's revolutionary advance in physics. Let us see how this seed develops in his St Louis address of 1904.

In his address of 1904, Poincaré takes up again the question of reconciling Newton's principle of the equality of action and reaction with Lorentz's theory of electrodynamics. He concludes the section discussing this topic with the words:

[...] I have long thought that these consequences of theory, contrary to Newton's principle, would end some day by being abandoned, and yet the recent experiments on the movements of the electrons issuing from radium seem rather to confirm them. [Poincaré 1904, 102]

These experiments, as Poincaré specifies on the next page, were those carried out by Kaufmann in Göttingen in 1901-1903. An excellent account of them is given in [Miller 1981, 47-67]. The point was that the electrons emitted by radium had very high velocities of the order of 90% of the velocity of light. For such electrons, Kaufmann established that mass was not constant but increased with velocity. However, as Poincaré points out, this seems to contradict

Lavoisier's principle of the conservation of mass, which is assumed in Newtonian mechanics.

Some calculations performed by Max Abraham, a colleague of Kaufmann's at Göttingen, appeared to offer a way out of the difficulty. Abraham considered the portion of the electron's inertia arising from the electromagnetic field — its so-called 'electromagnetic mass', which could be distinguished from its 'mechanical mass'. Abraham's calculations supported the remarkable result that an electron's mass is entirely electromagnetic i.e. that its mechanical mass is zero. This suggests the speculation that all mass might be electromagnetic, but Poincaré casts doubt on such a view on the grounds that some 'positive electrons' (i.e. positive ions) are very much heavier than electrons, and so perhaps have mechanical as well as electromagnetic mass. Having distinguished between electromagnetic and mechanical mass, is it possible that Lavoisier's principle continues to apply to mechanical mass? Poincaré states this possibility only to reject it immediately. Thus he says:

There is still a resource; the ultimate elements of bodies are electrons, some charged negatively, the others charged positively. The negative electrons have no mass, this is understood; but the positive electrons, from the little we know of them, seem much greater. Perhaps they have, besides their electrodynamic mass, a true mechanical mass. The real mass of a body would, then, be the sum of the mechanical masses of its positive electrons, the negative electrons not counting; mass so defined might still be constant. [Poincaré 1904, 103]

but immediately adds: "Alas! this resource also evades us." [*ibid.*]. To show why this approach will not work, Poincaré refers back to an earlier discussion of the principle of relativity, and of Lorentz. This shows, he claims that it is necessary that "*the masses of all the particles be influenced by a translation to the same degree as the electromagnetic masses of the electrons.*" [*ibid.*, 104, Poincaré's italics]. Poincaré is thus led to the following striking and dramatic conclusion:

So the mechanical masses must vary in accordance with the same laws as the electrodynamic masses; they can not, therefore, be constant.

Need I point out that the fall of Lavoisier's principle involves that of Newton's? This latter signifies that the center of gravity of an isolated system moves in a straight line; but if there is no longer a constant mass, there is no longer a center of gravity, we no longer know even what this is. This is why I said above that the experiments on the cathode rays appeared to justify the doubts of Lorentz concerning Newton's principle.

From all these results, if they were confirmed, would arise an entirely new mechanics, which would be, above all, characterized by this fact, that no velocity could surpass that of light [...]. [*ibid.*]

Poincaré now started work on developing the new mechanics to which he refers here, and this led to his papers of 1905 and 1906.

There seems to me little doubt that Poincaré's scientific practice in the years 1902-1906 ran counter to the philosophical views which he had expressed in 1902. The way in which he developed physics did, however, accord very well with the methodological principles of his contemporary: Pierre Duhem. According to Duhem:

An experiment in physics can never condemn an isolated hypothesis but only a whole theoretical group. [Duhem 1904-1905, 183, Duhem's Italics]

Moreover, if the group in question is contradicted by the results of experiment and observation, it is possible to change any of the hypotheses of the group. No hypothesis is sacrosanct. Indeed Duhem explicitly criticizes Poincaré for maintaining that scientists will never be led to abandon the principles of Newtonian mechanics. (For references and a discussion, see my [1993, Ch. 5, especially pp. 102-3]. According to Duhem, a scientist who possesses what he calls good sense (*le bon sens*) will be able to guess successfully which of the various hypothesis involved should be altered in the face of a contradiction with experiment and observation. This was exactly what Poincaré did in the face of Kaufmann's experimental results. Thus Poincaré was not, after all, a conventionalist, but a Duhemian scientist possessing good sense (*le bon sens*) in a high degree.

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