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Poincaré's Conception of Mechanical Explanation[*]

"Ce serait oublier quel est le but poursuivi; ce n'est pas le mécanisme, le vrai, le seul but, c'est l'unité." Henri Poincaré (1900)

1. Introduction

Henri Poincaré's views on the foundations of mechanics and the nature of mechanical explanation were influenced by the work of two of the most renowned nineteenth century scientists, James Clerk Maxwell and Heinrich Hertz. In order then to unravel Poincaré's views and own contribution to the subject it is important to see the connection between Maxwell's and Hertz's researches on the one hand and Poincaré's on the other. Consequently, I start this paper with a brief account of Poincaré's encounter with Maxwell's work in electromagnetism. Then, in section 2, I move on to show how Hertz's work on the foundations of mechanics shaped Poincaré own views. In sections 3 and 4, I formulate Poincaré's own conventionalist philosophy of mechanics and show how several methodological considerations, especially the search for unity, mitigated his conventionalism.

Having thus examined Poincaré's views on the foundations of mechanics, in section 5 I turn my attention to his notion of mechanical explanation and his proof that a mechanical explanation of a set of phenomena is possible if (and only if) the principle of conservation of energy is satisfied. I then go on to show how Poincaré secured the possibility of a mechanical explanation of electromagnetic phenomena, and also how, having done so, he ended up with an unlimited number of configurations of matter in motion that could underpin electromagnetic phenomena.

[*] I want to thank Samet Bagce, Jeremy Gray and Andrew Powell for comments and discussions on the material of this paper.

The upshot of this paper will be that Poincaré departed from the traditional conceptions on mechanical explanation and defended a purely structural conception, the strong point of which was that it promoted – as the motto of this paper says – the true and only aim of science, namely unity.

2. Maxwell's theory and Poincaré's *Électricité et optique*

As is well known, Maxwell's investigation into a mature dynamical theory of electromagnetic field rested on the general principles of dynamics (cf. 1873, Vol. 2, chapters 5–9; also Klein, 1972, 69–70). In his monumental *Treatise on Electricity and Magnetism*, Maxwell applied Lagrangian dynamics to a system of linear circuits carrying electric currents. He then formulated the kinetic and potential energies of this system in terms of electric and magnetic magnitudes and proceeded to derive the laws of motion of this system, thereby deriving the equations of the EM-Field (1873, Vol. 2, 233).[1]

Maxwell's theory – especially Maxwell's unification "by a slender tie" of optics and electromagnetism – bewildered Poincaré. But it also offered him a challenge. Poincaré, like many other theorists, was deeply concerned with the messy way in which Maxwell presented his theory in his *Treatise*. In particular, he was very sensitive to the fact that, due to the disorderly way in which the *Treatise* was written, Maxwell's promise to offer a mechanical explanation of electromagnetic phenomena seemed undelivered. Poincaré was convinced that a straight confrontation with Maxwell's *Treatise* was not going to convince the average reader that Maxwell presented and defended a theory of physical optics "founded on the hypothesis of ether" – that is a sort of mechanical theory. Hence, he set himself the formidable dual task to, on the one hand, articulate Maxwell's theory and explain it to the French scientific community, and on the other hand, put an order to Maxwell's thought so that Maxwell's "fundamental idea" and "real thought" are rendered explicit. To these ends, he presented a series of lectures on light and electromagnetic theories – delivered at Sorbonne in 1888 and published as *Électricité et Optique* in 1890 – which primarily aimed to deliver Maxwell's promise: show that electromagnetic phenomena can be subsumed under and represented in a suitable mechanical framework. As he put it, Poincaré aimed to show that "Maxwell does not give a mechanical explanation of electricity and magnetism; he confines himself to showing that such an explanation is possible" (1890/1901, iv).

I shall explain Poincaré's strategy in detail in sections 6 and beyond. But first, there is an important general question to be asked: given that a mechanical explanation of a set of phenomena amounts to showing that the laws that these phenomena obey follow from the laws of mechanics, what is the suitable framework for the formulation of the laws of mechanics? By the time of his Sorbonne lectures, Poincaré thought that this was no other than Lagrange's analytical dynamics. In fact, he never seemed to abandon his view on the suitability of Lagrangian dynamics, since he reiterated and defended it again in his widely read *La Science et L'Hypothése* (1902, 186) However, as I shall show in the next section, it was Hertz's posthumously published book on mechanics which convinced Poincaré that the foundations

[1] For a brief account of Maxwell's derivation of the equations of the field cf. Andrew Bork (1967).

of mechanics itself needed further clarification, and that the Lagrangian framework offered only a first approximation to a well-founded mechanical system.

3. Poincaré on "Les idées de Hertz sur la mécanique"

In 1897, Poincaré reviewed Hertz's *The Principles Of Mechanics Presented In a New Form* (1894) for the "Revue Générale des Sciences". Not only was Poincaré deeply impressed by Hertz's "profound reflections on the philosophy of mechanics" but he also took Hertz's criticism as the starting point for developing his own views on mechanics. Concerning the "classical system", which rests on Newton's laws, Poincaré agreed with Hertz that it ought to be abandoned as a foundation for mechanics (cf. 1897, 239). Poincaré argued that a good definition of force is impossible. An attempt to define force as the product of mass times the acceleration of a body would not do. For, even if a definition of acceleration is considered available, the definition of force would require defining mass and this is impossible. To see this, Poincaré invited his readers to note that in order to define mass, one must start off with Newton's law of equality of action and reaction, i. e., $m_A\vec{a}_A = -m_B\vec{a}_B$, where m_A, m_B and $\vec{a}_A, \vec{a}_B$ respectively stand for the masses and the accelerations of two bodies A and B. But A and B cannot be completely isolated from other masses of the universe and, hence we have to take into account all other forces acting upon them. One way to get round this complication is to assume the hypothesis of central forces according to which the force exerted by a body C on B does not affect the action of B on A but gets linearly added to this. If this hypothesis was admitted then we could disentangle the several actions on which the bodies A and B were subjected and apply Newton's third law to them. Yet, Poincaré was very uneasy with this hypothesis. "Do we have the right" he asked, "to admit the hypothesis of central forces?" (1897, 235).

Given that this hypothesis had been severely disputed in current research, Poincaré did not find it appropriate to rest an account of mass on such a dubious basis. Yet this made a definition of mass all the more difficult. For if the hypothesis of central forces is abandoned, then the only meaning that can be given to Newton's third law is this: the movement of the centre of gravity of a system that is not acted upon by external forces will be rectilinear and uniform (cf. ibid). Since, obviously, the position of the centre of gravity of a system depends on the values given to masses, it may seem easy to calculate the masses so that the movement of the centre of gravity of the system under consideration is rectilinear and uniform. However, since the only system that it is not acted upon by external forces is the universe as a whole, the previous procedure for defining mass can only be applied to the universe as a whole. For only *this* system is free from all external forces and therefore its centre of gravity moves in a uniform and rectilinear way. Yet, Poincaré concluded, it is evidently absurd to think that the motion of the centre of gravity of the universe can or will be ever known (cf. 1897, 234–236; also 1902, 120–123). Consequently for Poincaré "it is impossible to form a satisfactory idea of force and mass" (1897, 236).

Moreover, the classical system is incomplete. It deals almost exclusively with movements of the centre of gravity and motions in planes. Yet, these are not the sole laws that regulate natural motion (1897, 237). In particular, Poincaré stressed, it is not surprising that one must regard as incomplete a system of Mechanics "where the principle of conservation of energy is passed over in silence" (ibid.).

Like Hertz, Poincaré was more sympathetic to the "energetic system", which was based on the principle of conservation of energy and Hamilton's principle that regulates the temporal evolution of a system (cf. 1897, 239–240). In fact, as I noted in section 2 and shall show in more detail in section 6, Poincaré chose the energetic system in order to show the possibility of a mechanical explanation of electromagnetic phenomena.

According to Poincaré (1897, 240–241) the basic advantage of the energetic system was that in a number of well-defined cases, the principle of conservation of energy and the subsequent Lagrangian equations of motion can give a full description of the laws of motion of a system. These cases concern systems of conservative forces, that is systems where forces depend only on the relative positions and mutual distances of a certain number of material points, and are independent of their velocities. Then, one can define a potential energy-function U which depends only on the position of material points and is independent of their velocity. In these cases, the principle of conservation of energy takes the following definite form: there is a conserved quantity which is accessible to experience and is the sum of two terms, one being dependent only on the positions of material points (potential energy U), the other being proportional to the square of their velocities (kinetic energy T).

Poincaré suggested that the foregoing procedure gives an unambiguous definition of energy. *Energy*, is then, a constant quantity which can be decomposed into two terms T and U such that T is a homogeneous quadratic function of the velocities and U is a function of the positions only (cf. 1897, 240). If one also takes into account other forms of energy such as thermal, chemical or electric, what Poincaré defined as *energy* in general is nothing but a quantity that remains constant and is the sum of three terms potential energy U, kinetic energy T and internal energy Q such that U is independent of velocities, T is a homogeneous quadratic function of the velocities, and Q is only dependent on the internal state of the system (cf. 1897, 241; also 1902, 142). One cannot therefore fail to notice that what determines a certain quantity as energy is, apart from its constancy, a particular *structural* feature that this quantity possesses, viz., that it can be decomposed into three terms, each of them being defined as above. The distinctive feature of what Poincaré called *energy* is precisely this decomposition. For this only distinguishes energy from other conservative quantities, e. g., any arbitrary function of T+U, or of T+U+Q (cf. ibid).

However, this definition of energy shows the limitations of the energetic system. When among the several conservative quantities of a system, we cannot single out one which can be decomposed into three separate functions U, T and Q defined as above, we have no clue at all as to which of these conservative quantities we may call energy. For Poincaré this eventuality showed that the energetic system ends up proclaiming a very general, and rather empty, principle of the form "There is something that remains constant" (1897, 242). As it stands, this principle is not informative. The very fact that the world is governed by laws entails that there are quantities that remain constant. Which of them is to be taken as energy?

The energetic system can offer a definite answer only in some particular well-defined cases. But the promise of the energetic system to deliver an unambiguous general definition of energy which would be able to single out one and only one out of the many quantities that remain constant could not be fulfilled.[2]

The energetic system presented a definite advantage over the classical system. In moving from the latter to the former "one has realised progress", yet at the same time, Poincaré noted, "this progress is insufficient" (cf. ibid.).

4. Convention and convenience

Poincaré's thorough investigation into the foundations of mechanics left him still seeking after a sound representation of the mechanical framework. None of the systems he examined could provide an objection-free framework within which the mechanical representation of all natural phenomena could be articulated. But by the time he reached this conclusion he had started being worried about another important and related issue: what is the status of the principles of mechanics? As we shall see in the sequel Poincaré's answer to this question helped him formulate his criterion for choosing among the several mechanical frameworks, and in particular, justify his preference over the energetic system.

Three years after he published his review of Hertz's book, Poincaré addressed the Paris International Congress of Philosophy with the paper "Sur les Principes de la Mécanique" (cf. 1900, 556–557; 1902, 151–154). There, he suggested that the principles of mechanics are neither *a priori* truths nor experimentally determined truths. They are not *a priori* truths since the way the world is has played a significant role in formulating these principles. Take for instance Newton's first law, i. e., the law of inertia. Its truth cannot possibly be demonstrated *a priori*. This law amounts to the claim that if a body is not acted upon by any external forces, its velocity remains unchanged. Yet, one can conceive of worlds in which if a body is not acted upon by any external forces, either its position or its acceleration – and not its velocity – remain unchanged. In such worlds, Newton's first law would not hold. Different laws, expressed in a different mathematical form, would have to be formulated. Hence, the truth of Newton's first law cannot be demonstrated by *a priori* reasoning (cf. 1902, 113–115).

Similarly, the principle of conservation of energy cannot be demonstrated *a priori*. The existence of conservative quantities depends on the contingent fact that the world is governed by laws.

But then are the principle of mechanics experimental facts? Poincaré had already given a definite negative answer to this question in his review of Hertz's book. The systems that appear in the principles of mechanics, such as perfectly isolated systems or systems that pertain to absolute motions, were *not* to be found in nature (1897, 237). Hence one cannot really verify these principles by appealing to experience. No experiential situation can afford us with perfectly isolated systems and the like (cf. 1897, 237; 1900, 557; 1902, 116).

Moreover, no experience can ever falsify a mechanical principle. Poincaré offered two reasons for such a bold position. First, given the fact that the principles of mechanics refer to

2 Besides, the energetic system was beset by the existence of irreversible processes that could not be brought under the scope of the principle of least action (1897, 242; cf. also 1893).

systems that are not accessible to experience, they can never be submitted to a rigorous and decisive test (1902, 116). Second, even if one admitted that a mechanical principle could be submitted to a real test, it could be saved from refutation by some sort of corrective move. Suppose for instance that we found that natural systems did not obey the law of inertia. We could always attribute this deviation to the motion of hidden masses or molecules. Or suppose that we found that the motion of a system could not be brought under the laws of Lagrangian mechanics, as for instance is the case with motion of a ball which rolls without slipping. The relevant laws could be saved by admitting an extra force, that of friction, which amounts to admitting that the motion of the ball involves, in reality, slipping (cf. 1897, 246). Poincaré's conclusion was no other than "The law will be safeguarded" (1902, 117).

If the principles of mechanics are neither experimental truths nor *a priori* ones, what are they? Is there any alternative characterisation left? They are *conventions*, Poincaré affirmed. Conventions then are principles which we hold true but their truth can neither be a matter of *a priori* reasoning nor be established on *a posteriori* grounds. Yet this is not all that there is in Poincaré's theory of conventions. He resisted the view that conventions are merely true by definition. Conventions though they are, the principles of mechanics have a certain experiential input and experimental import. He repeatedly stressed that it is experience that "suggests", or "serves the basis for", or "gives birth to" the principles of mechanics (1897, 237; 1900, 557; 1901, 351). Take for instance the principle of conservation of energy. Poincaré was very clear in that this principle was obtained "in the search of what was common in the enunciation of numerous physical laws; (it) represent(s) the quintessence of innumerable observations" (1902, 177). Its experiential input, then, is the contingent truth that in nature there are conservative quantities.

The view that the principles of mechanics are not held as experimental generalisations but rather as conventions is the consequence of the fact that experiment alone can neither force these principles upon us nor conclusively invalidate them. In adopting these principles and, subsequently, in interpreting experience in their light, there is always an element of *choice*. That is why they are conventions.

But there again the element of choice involved in adopting a convention should not be taken as suggesting that this choice is arbitrary. A convention is not "the outcome of our caprice; we adopt it because certain experiences have shown us that it will be *convenient*" (1902, 151 – emphasis added).

Convenience then makes conventions non-arbitrary. To the best of my knowledge, Poincaré never presented an articulated view of what exactly is involved in measuring the convenience of a convention. But one can at least distil the following elements:

First, a certain convention is convenient if it can be employed in the study of nature in such a way that it yields approximately correct results. For instance, Newton's first law is convenient because although in nature there are no perfectly isolated systems, there are systems "which are *nearly* isolated". Then, their behaviour can be approximated by employing the law of inertia and approximately correct results can be obtained (1902, 124).

Second, a certain convention is convenient if it can be extended to cover new facts and predict new phenomena. Moreover, insofar as an upheld principle ceases to play this role, it is no longer convenient and therefore it should be abandoned (op. cit., 178).[3]

Third, a certain convention, or rather set of conventions, is convenient if it can form the basis for a unification of apparently dissimilar domains (1902, 186; also Zahar, 1989, 163–164).[4]

So, convenience is Poincaré's yardstick for measuring how useful a (set of) convention(s) is.[5] What I shall show in the next section is that, armed with the foregoing notion of convenience, Poincaré justified his preference to the energetic framework of mechanics and defended Maxwell's view that electromagnetic phenomena can be brought under the same roof as mechanics.

5. Mechanism vs. unity

As we saw in section 3, Poincaré was convinced by Hertz's arguments that no mechanical framework could provide a really flawless foundation for the study of natural phenomena. However, he thought that the energetic framework possessed a definite advantage over the classical system. What was this advantage? In the light of the foregoing discussion of Poincaré's concept of convenience, I want to suggest that the advantage of the energetic framework over the classical was that the former was more convenient than the latter. In particular, I want to suggest that Poincaré based his choice on the view that it is unity and unification rather than mechanism *per se* that counts.

In 1900, Poincaré also addressed the International Congress of Physics in Paris with the paper "Relations entre la Physique Expérimentale et de la Physique Mathématique" (cf. 1900a; this paper was reproduced as chapters nine and ten of his *La Science et L'Hypothése*). There, Poincaré acknowledged what he called most theorists' constant predilection for explanations borrowed from mechanics or dynamics (cf. 1900a, 1170). These attempts had historically taken two particular forms: they were either attempts to trace all phenomena back to the motion of molecules acting-at-a-distance in accordance to laws of central forces; or, they were attempts that suppress central forces and trace all phenomena back to the contiguous actions of molecules that depart from the rectilinear path only by collisions. "In a word" Poincaré said, "they all [physicists] wish to bend nature into a certain form, and un-

[3] Poincaré was quite firm in that no experiments can ever *contradict* a principle of mechanics. For no experiment can conclusively refute such a principle. Yet, he thought, experiments can *condemn* a principle of mechanics, or even a whole mechanical framework, in that persistent failure to account for new facts renders a particular principle or a whole framework no longer convenient (1902, 178; 1905, 146).

[4] Elie Zahar has expressed the view that Poincaré's notion of convenience was much stronger than the one I outlined. He argued that "Convenience, and convenience alone, operates like an index of verisimilitude. A hypothesis proves the more convenient, the nearer it is to the truth (…)" (1989, 161). Zahar ties this understanding of convenience with the view that Poincaré was a structural realist. For a criticism of this view cf. my (1995).

[5] I must say that a fuller account of Poincaré's notion of convenience should include his work on the foundations of geometry. As is well-known, Poincaré preferred the Euclidean framework over the non-Euclidean ones on the grounds that the former is more convenient (1895, 645–646; 1902, 75–76).

less they can do this they cannot be satisfied" (ibid.). But he immediately queried "Is nature flexible enough for this?".

Poincaré's work on electromagnetism pushed him to answer this question in the positive. But he also wanted to break away from the historically received understandings of mechanical explanation. He was convinced that mechanical explanations of electromagnetic phenomena are *possible* – they can be, in principle, found – if and insofar as a suitable mechanical framework is chosen and certain general conditions are satisfied. But he thoroughly resisted the idea that an understanding of natural phenomena should involve and be predicated on the condition that these phenomena must be traced back to either of the traditional configurations of hypothetical fluids or molecules. Poincaré's conception of mechanical explanation was tied to his idea that *unity* rather than *mechanism* is what science must aim for (cf. 1900a, 1173). If unity is to be served, then among the alternative possible foundations of mechanics, the energetic framework should be chosen. For the latter was the only framework within which Maxwell's treatment of electromagnetic phenomena could be accommodated. Hence, this framework was more convenient than its competitors. In fact, it could deliver all conditions for convenience, i. e., approximately correct application to known natural systems, coverage of new phenomena, and unification of apparently unrelated domains. As we saw in section 2, Poincaré had already suggested this in his book *Electricité et Optique*, back in 1890. Ten years later, Poincaré was ready to reaffirm, justify and popularise his conviction that the principle of least action, the principle of conservation of energy, and the subsequent Lagrange's equations of motion comprise the "most general laws of mechanics" (1900a, 1173) and to recall his audience's attention to his proof that a mechanical explanation is possible if and only if the foregoing principles are satisfied (1900a, 1171). Let us then see in some detail how this proof goes.

6. The possibility of a mechanical explanation

According to Poincaré the necessary and sufficient condition for a mechanical explanation of a set of phenomena is that there are suitable potential and kinetic energy functions such that they satisfy the principle of conservation of energy. More specifically, suppose that there are two functions U and T such that U depends solely on the generalised co-ordinates q_k of the system under consideration and T depends on the generalised co-ordinates q_k as well as their time-derivatives $\dot{q}_k$; suppose also that U and T can be identified as the potential and the kinetic energies respectively. Then these phenomena are amenable to a *complete* mechanical explanation insofar as the theoretically specifiable Lagrangian equations of motion coincide with the experimental laws directly observed. Poincaré's demonstration is worth presenting in full.

Suppose we want to see whether a dynamical system X is amenable to a mechanical explanation. For this purpose, let us call $q_1, q_2, \ldots, q_n$ a set of measurable parameters of the system X which are directly accessible in experience. Observation can also teach us the laws of variations of these parameters, which are expressible in differential equations with respect to time. A mechanical interpretation of this system may be cast either in terms of the movements of ordinary matter and/or in terms of the motion of a hypothetical fluid. A *complete* mechanical explanation of X will, then, consist in having

(A) the differential equations that are satisfied by the co-ordinates of the hypothetical molecules, being such that they obey the laws of dynamics, and

(B) the relations which define the co-ordinates of the molecules as functions of the given measurable parameters $q_1, q_2, \ldots, q_n$.

Let then the underlying mechanism of the system X under consideration be such that it consists of a large number p of isolated molecules, with masses $m_1, m_2, \ldots, m_p$. Moreover, let the co-ordinates of a molecule m_i be x_i, y_i, z_i. Suppose also that X satisfies the principle of conservation of energy, and therefore, that there is a certain function, say U, of the 3p co-ordinates x_i, y_i, z_i whose gradient is the force impressed on the system. Then, the 3p equations of motion are:

$$m_i \frac{d^2 x_i}{dt^2} = -\frac{dU}{dx_i}, m_i \frac{d^2 y_i}{dt^2} = -\frac{dU}{dy_i}, m_i \frac{d^2 z_i}{dt^2} = -\frac{dU}{dz_i}. \qquad (1)$$

The kinetic energy of the system is equal to

$$T = \frac{1}{2} \sum m_i (\dot{x}_i^2 + \dot{y}_i^2 + \dot{z}_i^2). \qquad (2)$$

According to the principle of conservation of energy, we have T+U = constant. Given the foregoing condition (B), suppose that we can express the 3p co-ordinates x_i, y_i, z_i of the molecules in terms of the n measurable parameters $q_1, q_2, \ldots, q_n$, i. e., $x_i = \varphi_i(q_1, q_2, \ldots, q_n)$, and similarly for y_i and z_i. Then the potential-energy function U can be expressed in terms of the parameters $q_1, q_2, \ldots, q_n$, and the kinetic energy-function T in terms of the parameters $q_1, q_2, \ldots, q_n$ and their first time derivatives $\dot{q}_1, \dot{q}_2, \ldots, \dot{q}_n$, i. e., their velocities. If these functions U and T are known, then the principle of least action is sufficient to determine the equations of motion of the system. We can thus obtain the Lagrangian equations of motion in terms of the measurable quantities $q_1, q_2, \ldots, q_n$ and $\dot{q}_1, \dot{q}_2, \ldots, \dot{q}_n$ as follows:

$$\frac{d}{dt}\left(\frac{\partial T}{\partial \dot{q}_k}\right) - \frac{\partial T}{\partial q_k} + \frac{\partial U}{\partial q_k} = 0. \qquad (3)$$

As Poincaré put it: "If the theory is good, equations [(3)] must be identical to the experimental laws directly observed" (1890/1901, vii).

The existence of functions U and T, specified as above, is a *necessary* condition for a complete mechanical explanation of X: if no functions U and T can be constructed such that U+T is constant and the subsequent Lagrangian equations of motion are identical with experimental laws, then no mechanical explanation of X is possible.

But Poincaré showed that the foregoing condition is also *sufficient*. Suppose that we can express the dynamical behaviour of X in terms of two functions T and U – being the kinetic and the potential energies of X respectively – such that $U=U(q_1, q_2, \ldots, q_n)$ and $T=T(q_1, q_2, \ldots, q_n, \dot{q}_1, \dot{q}_2, \ldots, \dot{q}_n)$, or for brevity, $U=U(q_k)$ and $T= T(q_k, \dot{q}_k)$. Suppose also that the Lagrangian equations of motions formed with the aid of these functions conform with the known experimental laws. Then, it is guaranteed that a complete mechanical explanation of X is possible. All we need for this is the supposition of a great number of molecules $m_1, m_2, \ldots, m_p$ and 3p functions of $q_1, q_2, \ldots, q_n$:

$$\phi_i(q_1,q_2,\ldots,q_n),\ \psi_i(q_1,q_2,\ldots,q_n),\ \theta_i(q_1,q_2,\ldots,q_n), \qquad (4)$$

where (i=1,2,3,…,p), or for short, $\phi_i(q_k)$, $\psi_i(q_k)$, $\theta_i(q_k)$. We can then consider these functions as the masses and the co-ordinates $x_i = \phi_i$, $y_i = \psi_i$, $z_i = \theta_i$ of the p molecules of the system. These functions should satisfy the following condition:

$$T(q_k,\dot{q}_k) = \frac{1}{2}\sum m_i(\dot{x}_i^2 + \dot{y}_i^2 + \dot{z}_i^2) = \frac{1}{2}\sum m_i(\dot{\phi}_i^2 + \dot{\psi}_i^2 + \dot{\theta}_i^2) \qquad (5)$$

where $\dot{\phi}_i = \dot{q}_1 \frac{\partial \phi_i}{\partial q_1} + \ldots + \dot{q}_n \frac{\partial \phi_i}{\partial q_n}$, etc.

Therefore, according to Poincaré, the possibility of a mechanical explanation of a set of phenomena is there if and insofar as these phenomena satisfy the principle of conservation of energy and there are suitable experimental quantities that can be identified as the kinetic and the potential energy of the system (cf. 1890/1901, v–viii; 1902, 219–223).

As I mentioned in section 2, Maxwell's mature formulation of electromagnetism was based on the claim that the behaviour of electromagnetic phenomena can be studied in the light of the principle of conservation of energy and the subsequent Lagrangian equations of motion. In view of his foregoing demonstration, Poincaré's was convinced that electromagnetic phenomena admit of a complete mechanical explanation. This is, according to Poincaré, Maxwell's fundamental idea. As Poincaré put it:

> "In order to demonstrate the possibility of a mechanical explanation of electricity, we do not have to preoccupy ourselves with finding this explanation itself; it is sufficient to know the expressions of the two functions T and U which are the two parts of energy, to form with these two functions the equations of Lagrange and, afterwards, to compare these equations with the experimental laws" (1890/1901, viii).

Maxwell did precisely this and therefore "was then certain of a mechanical explanation" of electricity" (Poincaré, 1902, 224).

In his *Electricité et Optique*, Poincaré did not content himself with just citing Maxwell's appeal to Lagrangian dynamics and the principle of conservation of energy. Rather, he worked through and made precise the whole derivation of the laws of induction and electrodynamics (1890/1901, 135–148).

However, as Howard Stein (1982, 311–312) has correctly pointed out, both Maxwell and Poincaré achieved a rather limited result. They applied Lagrangian dynamics only to the case in which the sole electric currents are closed currents in well-defined linear circuits. In particular, they applied the Lagrangian method in cases where displacement currents can be neglected. In fact, it was Lorentz who first noticed that "The equations that determine the motions of electricity in three dimensional bodies do not result, in Maxwell's book, from a direct application of the laws of mechanics; they rest upon the results previously obtained for linear circuits" (1892, 168).

In the first chapter of his pathbreaking (1892), Lorentz attempted to and succeeded in remedying Maxwell's (and Poincaré's) shortcoming. He considered a system of homogeneous and isotropic bodies, conductors or dielectrics, in the ether and applied the Lagrangian

method to this system. He thereby showed how Maxwell's equations can be derived.[6] Hence, even if Maxwell and Poincaré rested their case on a rather limited result, Lorentz showed the availability of a Lagrangian formulation of electromagnetic phenomena. In consequence, Poincaré's promise to subsume electromagnetism under a suitable mechanical framework had been fulfilled.

7. One too many mechanical explanations

Yet, Poincaré's demonstration of the possibility of a complete mechanical explanation has the following important corollary: if there is one mechanical explanation of a set of phenomena, i. e., if there is a possible configuration of matter in motion that can underpin a set of phenomena, then there is an infinity of them. This follows easily from the sufficiency part of Poincaré's demonstration since one can choose *any* number of molecules one pleases, and one has the same liberty in specifying their masses and their velocities (cf. 1890/1901, viii). Besides, the foregoing corollary was supported by the following theorem proved by the French mathematician Gabriel Königs:

> "One can always imagine an articulated system such that a point of this system describes a curve or an algebraic surface whatever; or more generally, one can imagine an articulated system such that in virtue of its connections, the co-ordinates of the several points of this system satisfy whatever given algebraic equations." (Poincaré, 1897, 249).

The thrust of Königs theorem is this. For any given configuration of a material system, the motions of a set of masses (or material molecules) is given by a system of linear differential equations of the co-ordinates of the masses. These differential equations of motion are normally attributed to the existence of forces between the masses (or the molecules). Königs's theorem says that the differential equations of motion would be satisfied even if one replaced all forces in favour of a suitably chosen system of fixed connections between these masses (or molecules). Hence, the differential equations of motion of a system may be traced back to – and be satisfied by – a number of different mechanical configurations (cf. also Poincaré, 1900a, 1171).

Poincaré thought that these formal results concerning the multiplicity of mechanical configurations that could underpin a set of phenomena described by a set of differential equations were only natural. They were only the mathematical counterpart of the well-known historical fact that in attempting to form potential mechanical explanations of natural phenomena, scientists had chosen several theoretical hypotheses, e. g., forces acting-at-a-distance, retarded potentials, continuous or molecular media, hypothetical fluids etc. Poincaré was sensitive to the view that even though some of these attempts had been discredited in favour of others, more than one potential mechanical model of electromagnetic phenomena was still available (cf. 1900a, 1166–1167).

So, the search after a *complete* mechanical explanation of electromagnetic phenomena was heavily underdetermined by possible configurations of matter in motion. For different underlying mechanisms could all be taken to give rise to the laws of electromagnetic phenomena.

6 For an analysis of Lorentz's derivation cf. Stein, 1982, 324–329.

How then can one choose between these possible mechanical configurations? How can one find the correct complete mechanical explanation of electromagnetic phenomena? For Poincaré this was a misguided question. As he said "The day will perhaps come when physicists will no longer concern themselves with questions which are inaccessible to positive methods and will leave them to the metaphysicians" (1902, 225).

Empirical facts alone could not dictate any choice between different mechanical configurations that satisfy the same differential equations of motion. But then, Poincaré thought, nothing else can. He advised his fellow scientists to content themselves with the possibility of a mechanical explanation of all conservative phenomena and abandon hope of finding an actual mechanical configuration that underlies a particular set of phenomena. He stressed: "We ought therefore to set limits to our ambition. Let us not seek to formulate a mechanical explanation; let us be content to show that we can always find one if we wish. In this we have succeeded" (1900a, 1173).

But then, he thought, this attitude would serve most the true and only aim of science, namely unity. This attitude left out the more controversial aspects of his contemporary scientific theories, such as atomism, in order to incorporate what Poincaré thought to be their more fundamental aspects, namely their mathematical form together with their empirical content. As he said "(…)The end we seek (…) is not the mechanism. The true and only aim is unity" (ibid.).

8. A concluding appraisal

We saw that for Poincaré, offering a mechanical explanation of a set of phenomena was tantamount to bringing the observable laws of these phenomena under the principles of mechanics. But his encounter with Hertz's work on the foundations of mechanics convinced Poincaré that there was a real issue to be dealt with: what principles should be taken as the principles of mechanics? No extant mechanical framework was totally satisfactory. But Poincaré suggested that there is space for grounded choice among the different possible mechanical frameworks that could serve as the basis for a mechanical explanation of natural phenomena. This choice was guided by the degree of convenience that the different frameworks possess. And the degree of convenience of a mechanical framework was heavily dependent on how unifying this framework was. In fact Poincaré's choice of the energetic framework was guided by the fact that this framework could bring mechanics and electromagnetism under the same roof, as exemplifications of the same underlying structure.

Unification under the same structure was, ultimately, what science was about. "(W)hat is science?", Poincaré once asked. And he replied:

> "(I)t is before all a classification, a manner of bringing together facts which appearances separate, though they are bound together by some natural and hidden kinship. Science, in other words, is a system of relations. … (I)t is in the relations alone that objectivity must be sought; it would be vain to seek it in beings considered as isolated from one another." (1905, 181).

We can see then that promotion of unity was the ultimate *justification* for Poincaré's conception of mechanical explanation. Poincaré broke away from the tradition which conceived of mechanical explanations as designing possible configurations of matter in motion which

could account for the observable behaviour of a set of phenomena. He was confident that given that the electromagnetic phenomena satisfy the principle of conservation of energy, they are amenable to at least one such explanation, i. e., there is some configuration of matter in motion that can underpin electromagnetic phenomena. The latter could not dictate which among the many possible mechanical configurations was correct. But they did dictate that they were amenable to some mechanical explanation.

I think though that one can hardly resist the following conclusion. What Poincaré thought to be susceptibility to a mechanical explanation was nothing but the result of a purely structural feature of a certain set of phenomena and had little to do with what these phenomena are: any set of phenomena whatever admits a mechanical explanation if and insofar as it satisfies the principle of conservation of energy. But as we saw in section 3, energy was defined in purely structural terms as the conservative quantity that can be decomposed into three functions U, T and Q such that U is dependent on the positions only, T is a homogeneous quadratic function of the velocities, and Q is dependent only on the internal state of the system. So, any set of phenomena whatever can be mechanically explicable, if and insofar as there are some physical magnitudes that can be identified with the foregoing structurally specified quantities U, T and Q. In effect then, for Poincaré, a set of phenomena is mechanically explicable if and only if their laws can be shown to possess the structure of the energetic system.

Besides, one may ask: in what sense is the principle of conservation of energy a distinctively mechanical principle? One may think that since energy itself is definable in terms of spatio-temporal variables and masses, it is a distinctively mechanical quantity. However, there are a few things to be said here. First, as we saw in detail in section 3, the energetic system was unable to offer a general definition of energy. Moreover, in the well-defined cases were a definition of energy can be given, apart from the potential energy and the kinetic energy of the system – which are clearly definable in terms of spatio-temporal variables and masses – one has also to include the internal energy of the system – which depends solely on the internal state of the system. Is the internal state of a system definable in mechanical terms? In other words, is the chemical, thermal or electrical state of a system definable in terms of masses and spatio-temporal variables? This seems a far from trivial question, which, I think, admits only a case-by-case, and not a general, answer.

But a general point worth making is that the energetic system took the concept of energy as a *primitive concept* along with space, time and mass. In this system, energy is not defined exclusively in terms of spatio-temporal variables and masses. However, as Mach (1912, 601) observed, energy was nonetheless taken as a *mechanical* concept since scientists thought that some sort of mechanical action is always the basis of any transformation of energy, the later being observed as a quantity of heat, a change in the electric potential and the like.[7]

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7 Mach (1912, 604 & 606) however resisted the view that the principle of conservation of energy is a mechanical principle. He thought that it "is a condition of logical and sound scientific thought generally" in that positing conservation principles is required for a neat and economical representation of natural phenomena (cf. ibid.).

As Stein has rightly observed, Poincaré's "notion of mechanical explanation is far too wide from the point of view of physical theory" (1982, 311). It does not really exclude any phenomena from being mechanical.[8]

After the turn of this century, scientists started to abandon attempts at mechanical explanations of natural phenomena. For instance, they accepted the electromagnetic field as an independent and irreducible entity along other mechanical entities (cf. Klein, 1972, 78–79). In view of these facts, it is important to keep in mind that Poincaré's conception of mechanical explanation really cut the Gordian Knot. He showed that, on the one hand, complete mechanical explanations can be had, but, on the other hand, "we must not attach to them an importance that they do not deserve" (1901/1921).

Poincaré's conception of mechanical explanation brought into focus and rested upon what some of the most worthwhile theories of his era shared in common. It was proposed precisely in order to bring out the *invariant elements* of his contemporary scientific theories and promote their objectivity. There is where its methodological value lies. To paraphrase a claim that Poincaré made for Maxwell, Poincaré's conception of mechanical explanation "throws into relief the essential – i. e., what is common to all theories; everything that suits only a particular theory is passed over almost in silence" (1902, 224).

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[8] In another paper, Stein noted that although Poincaré's theorem is mathematically deep, it is "quite irrelevant to the physical situation as we now see it, since although the classical electromagnetic field is still seen to be a dynamical system in the sense of Lagrange (...) any question of a strictly Newtonian substructure has long been proved entirely nugatory" (1982a, 569).

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