Physics and Philosophy Meet: the Strange Case of Poincaré

Howard Stein

The University of Chicago

Poincaré is a pre-eminent figure: as one of the greatest of mathematicians; as a contributor of prime importance to the development of physical theory at a time when physics was undergoing a profound transformation; and as a philosopher--by which I mean to indicate not only a concern with questions traditionally called "philosophical," but still more a certain *character* not improperly expressed by that word whose literal construction is "a lover of wisdom." This character, in specific connection with physics, Poincaré shows in his persistent engagement with questions both at the frontier and the foundations of the science--in his ever-renewed discussion of basic theoretical problems and principles--in his attempts to elucidate the contents of theories, to compare them with one another, and to assess them in relation to empirical evidence and to the prospects for future development. One cannot read these discussions of Poincaré's and doubt the clarity or the honesty of the mind that produced them. Hardly less striking than the tenacity of his attention to the really crucial issues is his undogmatic willingness to return to a question with a fresh point of view, to modify his opinion in the light of new experimental evidence or of some other author's new theoretical considerations. The well known collections of Poincaré's more popular essays on methodological and epistemological subjects, partly extracted from these series of technical disquisitions, give only a pale image of Poincaré as (in the sense I mean) a philosopher. They contain much stimulating matter; but their tone is a little cavalier and a little doctrinaire, and the essential seriousness that counterbalances these traits is not fully displayed.

I think that Poincaré, with all this virtue, made a serious philosophical mistake. In Poincaré's own work, this error seems to me to have kept him from several fundamental discoveries in physics. The hypothesis that Poincaré *would* have made these discoveries if he had not been misled by a philosophical error is not one that lends itself to conclusive assessment; but what I wish to do is to lay out the main circumstances of the case so as to make clear, at least, that the issue of philosophic principle involved, and the questions of fundamental physics under discussion, are of considerable mutual relevance.

Let me begin with a remark about the culminating event, Poincaré's memoir of 1905/6 on the dynamics of the electron.[1] I am by no means the first one to comment on that paper: there is a well known controversy over the question whether or not it de-

[1]Henri Poincaré, "La dynamique de l'électron," *Rendiconti del Circolo matematico di Palermo* **21** (1906), 129-176; reprinted in *Oeuvres de Henri Poincaré,* vol. 9 (Paris: Gauthier-Villars, 1954), pp. 494-550. A brief summary of the main results had previously been given in the *Comptes rendus* of the Académie des Sciences, Paris, **140**, 1504-8 (5 June 1905); Poincaré, *Oeuvres,* vol. 9, pp. 489-93.

serves to be considered as containing a statement of the special theory of relativity--and if not, why not?--i.e., the question, how does Poincaré's theory differ from Einstein's? That such a controversy should be possible at all is certainly a little odd; so *prima facie*, the case is strange. But I have not seen it pointed out just *how* strange; I know of nothing like it in the entire history of physics. There have been many instances of work inadequately appreciated at first, on account of what might be called philosophical preconceptions or prejudices; but here we have to deal with a great work by a great scientist and philosopher of science *whose own author* failed to appreciate its true worth. This is what Poincaré says in the introduction to the paper:

> We cannot content ourselves with formulas simply juxtaposed which agree only by a happy chance; it is necessary that these formulas come as it were to interpenetrate one another. The mind will not be satisfied until it believes itself to grasp the reason of this agreement, to the point of having the illusion that it could have foreseen this.
>
> But the question can be presented from still another point of view, which a comparison will help to explain. Let us imagine an astronomer earlier than Copernicus, reflecting upon the system of Ptolemy; he will notice that one of the two circles, epicycle or deferent, of each of the planets is traversed in the same time. This cannot be by chance, there is therefore among all the planets I know not what mysterious bond.
>
> But Copernicus, by simply changing the coordinate axes that are regarded as fixed, makes this appearance vanish; each planet describes only one circle and the times of revolution become independent ... .
>
> There may here be something analogous; if we admit the postulate of relativity, we shall find in the law of gravitation and in the electromagnetic laws a common number which is the velocity of light; and we shall continue to find it in all the other forces, of whatever origin--which can be explained in two ways only:
>
> Either there is nothing in the world that is not of electromagnetic origin.
>
> Or else this part which is as it were common to all physical phenomena is a mere appearance, something that derives from our methods of measurement. ... In this theory, two equal lengths are, by definition, two lengths that light takes the same time to traverse.
>
> Perhaps it would suffice to renounce this definition, for the theory of Lorentz[2] to be as completely overthrown as Ptolemy's system was by the intervention of Copernicus. If this happens some day, it will not prove that the effort made by Lorentz was useless; for Ptolemy, whatever one thinks of him, was not useless to Copernicus.

---

[2]Poincaré's name for the theory contained in this paper: he presents it as a reformulation of the theory of Lorentz, which he has "been led to modify and complete in certain points of detail".

> Therefore I have not hesitated to publish these few partial results, even though at this very moment the whole theory might seem placed in danger by the discovery of the magnetocathodic rays.

The last remark is rather a nice touch. Poincaré seems almost to *exult* in the fragility of theories in the face of experiment. In fact, the theory Poincaré is concerned with has proved of absolutely fundamental importance to physics, and has not merely survived empirical test but underlies the successful predictions of the most precise measurements that have ever been made. As to the magnetocathodic rays: they have disappeared so completely from the world that students of Poincaré's work have had no idea--rather, have had thoroughly erroneous ideas--of what he was talking about when he used that phrase.[3]

This view of the vulnerability of the entire theory--and Poincaré's suggestion that both the theory of Lorentz, and the hypothesis that all phenomena are electromagnetic in origin, might be rendered otiose by a Copernican shift of point of view--seem hard to reconcile with a claim that has been made repeatedly concerning the root of Poincaré's difference from Einstein: the claim that Poincaré was prevented from accepting, or perhaps even from paying serious attention to, the views of Einstein, by his own commitment to "the program of Lorentz" and/or to an "electromagnetic world-picture," or indeed to *any* special "explanatory program" for physics.[4]

---

[3]The phenomenon was reported first in 1898 by André Broca in the Paris *Comptes rendus*. The subject was taken up again in 1904 by Paul Villard, who concluded that one was dealing here with a new kind of rays--*uncharged* but *magnetic* (it was Villard who gave to the rays the name "magneto-cathodic"). It is clearly to Villard's account that Poincaré refers, as suggesting a fundamentally new process that may put the whole theory in jeopardy. Discussion continued in the pages of the *Comptes rendus* until 1911; I have found no later reference to these rays, which at any rate certainly did not prove to be a fundamental and simple *new* process, but rather a behavior under *complex* conditions of ordinary cathode rays. (For a little more detail, see Howard Stein, "After the Baltimore Lectures," in Kargon and Achinstein, eds., *Kelvin's Baltimore Lectures [etc.],* p. 397, n. 29.)

In translations of Poincaré's paper in C. W. Kilmister, ed., *Special Theory of Relativity* (Pergamon, 1970), and (by H. M. Schwartz) in the *American Journal of Physics* **39** (1971), Poincaré is represented as saying that the whole theory is put in jeopardy by the discovery of cathode rays--an astonishing idea. Arthur I. Miller, in his extensive essay "A Study of Henri Poincaré's 'Sur la Dynamique de l'Électron,'" *Archive for History of the Exact Sciences* **10** (1973), p. 320, n. 290, remarks that the qualifier "magneto" was omitted from Kilmister's translation, and adds: "It is not clear what Poincaré meant by a "magneto-cathode ray." On the other hand, in his book *Albert Einstein's Special Theory of Relativity* (Reading, Mass.: Addison Wesley, 1981), pp. 334-5, Miller says: "In the last paragraph of the Introduction to the 1906 version, Poincaré wrote, concerning Lorentz's theory, that at 'this moment the entire theory may well be threatened' by Kaufmann's new 1905 data." He offers us not a word as to how Poincaré's own phrase "the discovery of the magnetocathodic rays" can bear the construction he puts upon it; he simply *omits* that phrase, and replaces it by his own words--which thus purport, with no justification whatever, to paraphrase Poincaré. (Miller then concludes--*ibid.*, n. 5--on the basis of this gross misreading, that the paragraph in question must be a late addition to the paper, added by Poincaré just before the work went to press.)

[4]Such interpretations have been put forward, e.g., by Stanley Goldberg, "Henri Poincaré and Einstein's Theory of Relativity," *American Journal of Physics* **35** (1967), 934-44; Arthur Miller,in the works cited in n. 2 and in "The Physics of Einstein's Relativity Paper of 1905 and the Electromagnetic World

But, the puzzle of Poincaré's attitude having been posed, let us turn to a sketch of the history of his engagement with the problems of electrodynamics in general. He concerned himself with such problems from 1888, when he offered lectures at the Sorbonne on Maxwell's theory (published as Part I of his *Électricité et optique).* At just that time Hertz's great experiments were in progress; so that Poincaré could write in the Introduction to his lectures, when they were published in 1890: "Science has advanced with a rapidity that nothing allowed one to foresee at the moment I began this course. Since that time the theory of Maxwell has received, in the most dazzling way, the experimental confirmation it had lacked." Poincaré not only kept abreast of this work as it developed, he played an active and helpful part in the theoretical discussion of problems raised by the experiments.[5]

I shall make some comments shortly about Part I of the *Electricity and Optics;* but the first work of Poincaré's that bears directly upon the problems addressed in the memoir of 1906 is a series of four papers, published in 1895, under the title "Á propos de la théorie de M. Larmor."[6] These papers, stimulated by Larmor's memoir, "A Dynamical Theory of the Electric and Luminiferous Medium," contain, as Poincaré puts it, "simply a summary of the reflections suggested to me by the reading of that important communication--reflections that have often led me quite far from Mr. Larmor's theory." Indeed, in considering Larmor's theory of the dynamical constitution of the ether, Poincaré is brought to confront the fundamental problem of *the electrodynamics of moving bodies;* and in the middle two of the four papers he passes in review the main theories in the arena, and discusses the problem itself from a rather general point of view. The result of the review is that *none* of the theories proposed[7] satisfies all three of the following desiderata: (1) compatibility with the experiments of Fizeau on the speed of light in moving refractive media; (2) compatibility with the principle of the conservation of electric charge;[8] (3) compatibility with the principle of the equality of action and reaction. The result of the subsequent general discussion is that *no theory of the general type under consideration,* reducing to Maxwell's theory when the bodies are at rest, *can satisfy these three conditions.* More precisely, Poincaré shows that the *only* such theory compatible with the conservation of charge and the principle of action and reaction is the theory of Hertz; and the theory of Hertz is contradicted by the experiments of Fizeau. He concludes that the most satisfactory theory is Lorentz's, which is in accord with the result of Fizeau and with the conservation of charge but *violates the principle of action and reaction.*

There is in this issue about the principle of action and reaction something quite odd in retrospect. Maxwell himself, in the chapter of his *Treatise on Electricity and Magnetism*

Picture of 1905," *American Journal of Physics* **45** (1977), 1040-48; and Tetu Hirosige, "The Ether Problem, the Mechanistic Worldview, and the Origins of the Theory of Relativity," *Historical Studies in the Physical Sciences* **7** (1976), 3-82.

[5]Cf. Heinrich Hertz, *Untersuchungen über die Ausbreitung der elektrischen Kraft* (2nd ed., 1894), pp. 9, 18, 298.

[6]Poincaré, *Oeuvres,* vol. 9 (Paris, 1954), pp. 369ff. (published originally in *L'Éclairage électrique.).*

[7]They are due respectively to Hertz, Helmholtz, Helmholtz and Reif, Lorentz, and J. J. Thomson

[8]Poincaré (ibid., p. 395) says "conservation of electricity and magnetism" (admitting the possibility of a true magnetic charge).

devoted to the electromagnetic theory of light, had deduced from the theory that light exerts a pressure on a body upon which it falls, and that in consequence light rays falling upon just one side of a body might set the latter in motion. In the context of Maxwell's general theory--according to which electromagnetic processes are processes in a material medium, the ether--the force that presses such an illuminated body is exerted by the ether; the reaction-force required by the principle of action and reaction must therefore be exerted by the illuminated body *upon the ether* that is the bearer of electromagnetic processes in general, and of light in particular--upon, that is (in Larmor's phrase), "the electric and luminiferous medium." And it follows that whenever the net forces on the ordinary, non-etherial bodies--on what Poincaré customarily refers to as "ponderable matter"--are not in equilibrium, so that a net change of momentum occurs in the latter, the compensatory change, which maintains conservation of total momentum, must occur *in the ether itself.* This is an immediate conclusion from Maxwell's theorem of 1873. Yet it was evidently Heinrich Hertz, as late as 1890, who first took explicit note of this situation; and commenting on it he regards it as extremely unsettling that in the general case of non-stationary electrodynamic processes, the ether-stresses introduced by Maxwell would set "the interior of the ether itself in motion."[9]

When this issue is considered in specific connection with the theory of Lorentz a peculiar problem does indeed arise. Lorentz postulates the validity of the Maxwell field-equations relative to a certain fixed cosmic system of coordinates. He treats ordinary bodies as constituted fundamentally of charged particles, whose motions, described relative to that same coordinate system, are governed by the forces that act upon them--among these the electric and magnetic forces given by the force law that has become standard in the subject, but which Lorentz himself was the first to introduce as the direct expression of the fundamental action of the electromagnetic field upon ordinary matter. Now, *this* law gives the force on a charged particle without any reference to a body exerting the force: it is divorced from any notion of a "part of the ether" upon which a reaction force may be exerted. Furthermore, Maxwell's field-equations were conceived as holding for a coordinate system relative to which the ether is *locally at rest;* therefore Lorentz's postulate of a fixed global system of coordinates relative to which the field-equations hold signified in effect that in that Lorentz reference-frame the ether is *everywhere* locally at rest--i.e., that the ether is in effect a cosmic immovable body. Thus Lorentz's theory seems to imply that, although bodies do affect the ether by influencing the state represented by the electromagnetic field, they do not affect it by transferring to it any "mechanical motion"--any momentum; and, correspondingly, that there is no mechanical reaction to the Lorentz force. Hence, one may say, arises Poincaré's point about the failure of the principle of action and reaction.

---

[9]Hertz, "Über die Grundgleichungen der Elektrodynamik für ruhende Körper," in the volume cited above, n. 4; see p. 235; and cf. also "Über die Grundgleichungen der Elektrodynamik für bewegte Körper," ibid., p. 284: "It seems moreover not to have been noticed that this system of pressures in general leaves the interior of a homogeneous body, in particular the ether, at rest only when the acting forces have a potential."

However, this problem is *not* the one raised by Poincaré. Here is how he puts the difficulty:[10]

> Let us consider a small conductor A, charged positively and surrounded by ether. Let us suppose that the ether is traversed by an electromagnetic wave which, at a certain moment, reaches A: the electric force due to the perturbation will act upon the charge of A and will produce a ponderomotive force acting on the body A. This ponderomotive force will not be counterbalanced, from the point of view of the principle of action and reaction, by any other force *acting upon ponderable matter.* . . .
>
> One might escape the difficulty by saying that the body A reacts upon the ether; it is no less true that one could, if not realize, at least conceive an experiment in which the principle of reaction *would seem to fail,* since the experimenter can only operate upon ponderable bodies, and cannot reach the ether. This conclusion will seem difficult to admit.

So Poincaré's objection is not that the ether, in Lorentz's theory, cannot receive momentum because it is immobile; rather, his objection is against any appeal *whatever* to the ether in such questions: he expects momentum to be conserved amongst ordinary ("ponderable") bodies.--I have said that I find this odd. Indeed, the question arises, if action and reaction are supposed to occur between ordinary bodies--if all forces are supposed to be exercised *by* ordinary bodies *on* ordinary bodies--then in what sense are optical and electromagnetic processes to be regarded as mediated by the so-called "ether"? What role does this medium have, if not to engage in dynamical interactions? And what is Maxwell, in replacing action at a distance between charges by "contact" action through an intervening medium, supposed in fact to have achieved?

To try to gain some insight into this problem, let us ask how Hertz's theory accommodates the momentum principle to Poincaré's satisfaction. The answer is that Hertz assumes the ether inside a body to move always with the same velocity as the body itself; the mass of the ether is therefore to be counted as part of the mass of the body--and any momentum carried by the ether, in a portion of space filled by ordinary "ponderable" matter, is *ipso facto* momentum of that matter. Here is how Poincaré puts the case (immediately after the passage just quoted):

> The theory of Hertz did not give rise to this difficulty, and was in perfect accord with the principle of reaction. In this theory, and in the simple example we have considered above, there would have been reaction of the body A not merely upon the ether, but upon the air within which this ether is found; and, however rarefied that air may be, there would have been perfect equality between the action suffered by A and the reaction of A upon that air.

[10] "Á propos de la théorie de M. Larmor," §7; Poincaré, *Oeuvres,* vol. 9, pp. 391-2 (emphases added).

But this remains puzzling. In fact, it is not quite a true representation of the situation in Hertz's theory. In the first place, Poincaré's statement that there is "perfect equality" of action upon A and reaction of A upon the air, *however rarefied that air may be,* does not cover the case in regions devoid of air: what, for instance, are we to say of the propagation of light through interstellar space? In the second place, Hertz's hypothesis treats matter as a space-filling continuum; besides the case just mentioned of what may be called the "gross vacuum," it fails to cover the microscopic vacuum that intercedes the particles of matter. Hertz himself regards his hypothesis as a mere makeshift; and on the question of momentum transfer to the ether, he describes the situation as follows:

> In the case of arbitrary admissible electromagnetic excitation, the pressures we have found must set in motion the interior of the ether . . . with velocities that we could calculate if we had some grip upon the mass of the ether. This result seems to possess little intrinsic probability. Yet from the standpoint of the present work, there is no reason to reject the theory on this account; for the result is in contradiction neither with our presuppositions, nor with experience accessible to us. The small mass of air that remains in the best-evacuated spaces is . . . quite sufficient to hold down to an inconsiderable magnitude the streamings that could be aroused by any existing means.[11]

That is a more accurate statement of the case than Poincaré's. Still more instructive is the following supplementary note added by Hertz:

> This circumstance does not necessarily involve an error of the theory, but it does necessarily involve an incompleteness. Furthermore, the difficulty seems to be grounded deep in the roots of our view, for it can be understood without any appeal to the equations. Think of a magnetized steel ball, in free space, rotating about an axis that does not coincide with the direction of magnetization: it continually emits electromagnetic waves, and thus delivers up energy, so that it will gradually come to rest. If we generate the very same system of magnetic waves, by exciting a circulating magnetism in an iron ball at rest through changing electric forces, then one easily sees that, conversely, the iron ball in free space must start to rotate. Such consequences seem of small probability; but we have hardly any right in this domain even to speak of probabilities--so complete is our ignorance concerning possible motions of the ether.[12]

Thus we see the same intellectual check for Hertz as for Poincaré: both of them boggle at the breakdown of the conservation laws of mechanics for ordinary bodies. Hertz's example is interesting in that it exhibits a case involving angular momentum (although

[11]"Grundgleichungen für bewegte Körper," p. 284.
[12]Ibid., pp. 294-5.

one may wonder why he does not cite the simpler case of radiation pressure and linear momentum). But it is a striking contrast with Poincaré that Hertz draws back from his statement of improbability: he is willing to consider that such effects of motion of the ether may after all occur.

Poincaré, at any rate, concludes his review of proposed and possible theories by awarding highest marks to the theory of Lorentz, despite its violation of the principle of reaction for ordinary bodies. But he does not regard the result as satisfactory, and he does not regard it as conclusive. Rather, he suggests that the impossibility of satisfying simultaneously his three desiderata--conservation of charge, Fresnel's "dragging coefficient" (that is, Fizeau's experiment), and principle of reaction--within the framework of the electrodynamics of moving bodies, means that this framework itself must eventually be radically altered. The conclusion of his discussion is worth quoting at some length:

> It is hardly necessary to add that this theory, if it can render us certain services for our purpose, in fixing our ideas somewhat, cannot fully satisfy us, nor be regarded as definitive.
>
> It would appear to me very hard to admit that the principle of reaction is violated, even in appearance, and that it is no longer true if one envisages solely the actions suffered by ponderable matter and if one leaves aside the reaction of this matter upon the ether.
>
> We must therefore, one day or other, modify our ideas in some important point, and break the framework in which we try to fit optical phenomena and electrical phenomena together.
>
> But even restricting ourselves to the optical phenomena proper, what has so far been said to explain the partial entrainment of the waves[13] is not very satisfying.
>
> Experiment has revealed a series of facts which can be summarized by the following formula: it is impossible to render manifest the absolute motion of ponderable matter with respect to the ether; all that one can put in evidence is the motion of ponderable matter with respect to ponderable matter.
>
> The proposed theories account well for this law; but under a double condition:
>
> 1. We must neglect dispersion and several other secondary phenomena of the same kind;
>
> 2. We must neglect the square of the [parameter of] aberration.
>
> Now, this does not suffice; the law seems to be true even without these restrictions, as a recent experiment of Mr. Michelson has proved.
>
> Here too therefore is a lacuna which is perhaps not without some connection with the one that the present article is intended to signalize.
>
> And, indeed, the impossibility of putting in evidence a relative motion of matter with respect to the ether, and the equality which without doubt holds

[13] That is, the Fresnel-Fizeau results.

between action and reaction without taking into account the action of matter upon the ether, are two facts whose connection seems evident.

Perhaps the two lacunae will be filled simultaneously.[14]

The theory of Lorentz of which Poincaré has been speaking is Lorentz's first attempt at an electrodynamics of moving bodies, which satisfied the principle of relativity only to the first order in *v/c* (the "parameter of aberration"). Poincaré's emphasis upon the requirement of relativity ("the impossibility of putting in evidence a relative motion of matter with respect to the ether"), and upon the desirability of satisfying it rigorously and by uniform means for all cases, was of important influence upon later developments. This emphasis, so far as I know, appears for the first time in the paper under discussion. But the point to which I should like now to direct your attention is the affiliation, in this paper, of the principle of relativity with the principle of reaction *for "ponderable" matter.* Between these two principles there appears to be, according to Poincaré, an evident connection. What connection he meant is very clearly revealed by an earlier passage in Poincaré's reflections on Larmor's theory. Larmor supposed the ether to be in streaming motion in any magnetic field, and anticipated an influence upon the velocity of light. At Larmor's suggestion, Oliver Lodge had looked for such an effect, but had found none; and Larmor concluded that the motion of the ether in a magnetic field must be very slow, and its density must therefore be very great. Poincaré comments:

> Thus the motion was so slow that the experiments of Mr. Lodge, although very precise, were yet not precise enough detect it. To say all that I think, I believe that if these experiments had been a hundred or a thousand times more precise, the result would still have been negative.
>
> In support of this opinion I have only opinionative grounds *(raisons de sentiment);* if the result had been positive, one would have been able to measure the density of the ether, and--if the reader will forgive me the vulgarity of this expression---it is repugnant to me to think that the ether is *si arrivé que cela.*[15]

If I understand the colloquial French correctly, the connotation is "such a big success," "that well-established"; and thus, one might say, in the present context, "with authentic credentials as part of what, in physics, *really* counts." In short, *Poincaré did not believe in the ether;* neither momentum exchange with the ether, nor relative motion with respect to the ether, *ought* to be empirically detectable processes, because if they were the ether itself would have to be acknowledged as having "arrived," to a degree that Poincaré finds repugnant.

But how are we to understand this snub: in what sense did Poincaré not "believe in" the ether? Was the ether not, in his opinion, a constituent of that theory of electro-

---

[14]Poincaré, *Oeuvres,* vol. 9, pp. 412-13.

[15]Ibid., pp. 381-2.

magnetic processes which he himself refers to as Maxwell's "eternal title to glory"?[16] If it was not, one would expect to find in his writings on electrodynamics some account, or at least suggestion, of a way to eliminate the notion of the ether from the conceptual framework of that theory. One finds no such account, no such attempt, no such suggestion: not in the lectures on Electricity and Optics; not in the papers on Larmor's and Lorentz's theory; not in the 1906 memoir on the Dynamics of the Electron; not in Poincaré's last review article on the subject, dated 1908 (and reproduced as Book III of the collection *Science and Method).* On the contrary, in each of these works Poincaré explicitly invokes the ether; and if in the articles on Larmor's theory he disparages, as we have just seen, the pretensions of the ether to full empirical effectiveness, the central subject of those very articles is a theory of the dynamical constitution of that medium. I think it a striking circumstance that Poincaré goes so far, in this discussion, as to suggest "breaking the framework in which we try to fit optical and electrical phenomena at once"--which seems to mean an abandonment of the electromagnetic theory of light itself--as the key to the reconciliation of experimental results[17] with the principle of the ineffectuality of the ether;[18] but that he does *not* suggest (explicitly, at any rate) that an electromagnetic theory of light can be formulated without an ether altogether.

I believe that the explanation of this puzzle lies in what I have referred to earlier as Poincaré's philosophical mistake.

Poincaré distinguishes three broad classes of "hypotheses" in physics: On the one hand, there are the "real generalizations," or empirically testable hypotheses. These are most crucial to the business of science: "when once confirmed by experiment [they] become fruitful truths";[19] and "whether verified or condemned, they will always be fruitful"[20]--since in either case they will have served the dual purpose of motivating the design of experiments and interpreting the significance of their results.[21] At the opposite extreme is the class of those hypotheses that Poincaré characterizes as "hypotheses only in appearance," since they are more properly "disguised definitions or conventions";[22] these are the source of mathematical rigor in science, for they establish the framework of mathematical structure within which we seek to place phenomena;[23] they are chosen for convenience--experience guides, but does not dictate, our choice.[24] In this class are found the principles of "mathematical magnitude" or the theory of the real-number continuum and its multi-dimensional generalizations; of geometry; and also of mechan-

---

[16]Ibid., p. 373.

[17]Conservation of charge; Fizeau's experiment.

[18]Conservation of momentum for "ponderable" matter.

[19]*Science and Hypothesis,* Introduction, in Henri Poincaré, *The Foundations of Science: Science and Hypothesis, The Value of Science, Science and Method,* trans. George Bruce Halsted (Lancaster, Pa.: The Science Press, 1946), pp. 28, 134, 136.

[20]Ibid., p. 136.

[21]Ibid., p. 134.

[22]Ibid., p. 28.

[23]Ibid.

[24]Ibid.

ics.[25] Between the two extremes--which correspond to the traditional philosophical types of the empirical and (although with a difference) the *a priori* (more specifically, both in their specific content and in their role as "the source of rigor in science," the Kantian *synthetic* a priori)[26]--there stands, in Poincaré's analysis, the class of what he calls "neutral" *(indifferent)* hypotheses: (1) We assume, he says, for purposes of calculation, *either* that matter is continuous, *or* that it is atomic; in either case, we get the same results (although with more difficulty on the atomic hypothesis): the choice is indifferent. (2) In optical theory, two vectors play a role, of which one is interpreted as a streaming velocity, the other as an intensity of vortex motion. But which vector is taken to be which is a matter of indifference: all experimental consequences are the same.[27]

Discussion of Poincaré's views about hypotheses has tended to focus upon the issue of his so-called "conventionalism"; that is, upon his doctrine about hypotheses that cannot be refuted because they are definitions in disguise. As Poincaré sees--and participates in--the practice of science, however, the sort of hypothesis that is most often being put in active play is neither this basic mathematical sort (these are constantly *used,* but never *in question);* nor the truly central kind, the empirical hypotheses (for these, to be fruitful, must--at any given time, and prior to verification--not be too numerous)[28]. It is rather just the class of seemingly innocuous "neutral" hypotheses that is the characteristic stock-in-trade of the mathematical physicist--and even (to make such a distinction) of the theoretical physicist. Consider the two examples Poincaré has given of "neutral" hypotheses. One was the hypothesis of continuous or discontinuous structure of matter. Thus the whole working-out of the kinetic-molecular theory is an instance of the development of a neutral hypothesis--and it is exactly what Poincaré has in mind when he remarks, a bit cuttingly, that the continuous and atomic hypotheses lead to the same results, the only difference being that on the atomic hypothesis it is a little harder to reach them. As for the second example, that from optical theory, it is a pair of alternative hypotheses about the dynamical constitution of the ether; and in fact one finds that Poincaré begins his reflections on the theory of Larmor by discussing just this pair of

[25]Ibid., p. 29, and chs. ii-vi; cf. also pp. 123-5.

[26]To avoid misunderstanding: the "disguised conventions" of Poincaré are not, in his characterization, synthetic *a priori;* they are (as "disguised definitions") rather, in effect, analytic. But the subjects governed by them are exactly those regarded by Kant as governed by synthetic *a priori* principles of a mathematical nature (except for the arithmetic of the whole numbers, where Poincaré claims that a *bona fide* synthetic *a priori* principle does rule: namely, the principle of proof by mathematical induction).

[27]Ibid., p. 135.--For completeness, it should be added that there is still another class of hypotheses mentioned by Poincaré at this point. In the Introduction, he spoke of three such classes, the disguised conventions being one; here, however, where he is discussing specifically hypotheses *in physics* (in distinction from mathematics), he no longer mentions the conventions--these have already been considered at length; but he does speak of *another* "third" kind of hypothesis, characterized as "those which are perfectly natural and from which one can scarcely escape." These are, like the conventions, assumptions of a mathematical sort: assumptions of continuity and differentiability, or of symmetry; and Poincaré says of them: "[They] form, as it were, the common basis of all the theories of mathematical physics. They are the last that ought to be abandoned."

[28]Ibid., pp. 134-5, 136.

hypotheses, which correspond to the theories of the optical ether of Fresnel on the one hand, Neumann (and MacCullagh) on the other, both in a special interpretation due to Kelvin.[29]

Now there is something obviously wrong with Poincaré's brief characterization of "neutral" hypotheses. We know, of course, that the atomic hypothesis has consequences of far more fundamental import than Poincaré envisages. This might be a simple, culturally conditioned, mistake about that theory: Poincaré is evidently assuming a somewhat mediating (and deflationary) stance in the atomist-energeticist controversy--defending the legitimacy of the atomic hypothesis, but relegating it to the status of an artifice. But examples apart, there is something *conceptually* wrong with the account. What the case of the atomic hypothesis shows is that whether a choice between alternatives leads to testably different consequences may depend upon the stage of development of theory and experimental technique. On the other hand, in a case in which it can be established, rigorously and once for all, that two alternatives have *precisely the same* empirical consequences, it seems as pointless to distinguish them as different hypotheses as it would be so to distinguish two routines for calculating the same function: two *such* hypotheses, I myself would suggest, ought to be considered simply *equivalent.*

Poincaré might agree with this. I think he would say that he calls hypotheses "neutral," not *per se,* but in relation to a context: that "indifferent" means "equivalent *modulo* the context." And so he might agree that atomic *vs.* continuous is a "neutral" pair only in certain limited applications--outside of which the hypotheses become empirical ones. He might agree. His recorded statements are ambiguous. He tells us that the kinetic theory of gases has given rise to objections that "we could hardly answer if we pretended to see in it the absolute truth,"[30] and not just a kind of "metaphor . . . useful to give a certain satisfaction to the mind."[31] But he also says--at the same place!--that this theory has been useful "in revealing to us a relation true and but for it profoundly hidden, that of the gaseous pressure and the osmotic pressure." Later, referring to the statistical account of irreversible processes, he says: "This conception, which attaches itself to the kinetic theory of gases, has cost great efforts and has not, on the whole, been fruitful; but it may become so."[32] If it may be fruitful, then it is presumably not indifferent; and Poincaré refers at once to "M. Gouy's original ideas on the Brownian motion," according to which, in this process, "one would think one saw Maxwell's demon at work."[33] Yet again, however, still later, in his discussion of the philosophy of LeRoy, having mentioned with approval LeRoy's distinction between "the fact in the rough" and "the scientific fact," he says: "The examples given by M. LeRoy have greatly astonished me. The first is taken from the notion of atom. The atom

---

[29]"Á propos de la théorie de M. Larmor," Part I, §1; Poincaré, *Oeuvres,* vol. 9, pp. 369-373.

[30]*Science and Hypothesis,* p. 141.

[31]Ibid., p. 142.--This attitude of Poincaré's accounts nicely for his lack of concern with the difficulties for Hertz's electrodynamics of moving bodies that arise from atomism.

[32]Ibid., p. 152.

[33]I have slightly modified the translation of this last quotation, to put it into closer accord with the French.

chosen as an example of fact! I avow that this choice has so disconcerted me that I prefer to say nothing about it."[34]--Note that it is the status of *"scientific* fact," already distinguished from "crude fact," that Poincaré is here so emphatically barring to atoms; and this at a time when, for instance, the charge and the mass of the electron were already known to reasonable approximation--and when Poincaré himself was reflecting seriously upon the variation of the mass of electrons with their speed.

Poincaré's explicit statements, then, are in some points hard to reconcile with one another and with the actual scientific situation of the time when he made them. But I think that it is possible to form a reliable estimate of his fundamental view by comparing his statements with his practice. As I have remarked, the basic mathematical presuppositions of physics were seen by Poincaré as defining a framework within which *it is the task of the theoretical physicist to fit all phenomena.* This "fitting" activity, which Poincaré often refers to as an enterprise of "classification," is therefore bound by two sets of rules: it must respect the results of experiment--the "real generalizations," the "facts"; and it must conform to the standards of the definitive framework of mathematical physics (which framework includes the principles of mechanics). The theoretical physicist is therefore in a position somewhat like that of an artist commissioned to represent some definite subject within the restraints of a well-defined classical form: he operates under a combination of freedom and constraint not unlike that of, say, Aeschylus composing the *Oresteia. What Poincaré calls the "neutrality" of a hypothesis consists in this freedom:* for any given limited set of "facts," there will always be, according to Poincaré, an infinite number of ways of accommodating them in the fundamental framework; the choice among such ways is "neutral."--One sees here the source, or at least one source, of confusion: for if the theoretical physicists's choice among hypotheses is neutral in so far as many hypotheses would serve, it is at the same time *not* neutral in so far as many others would *not* serve. This sort of amendment or clarification is what I have conjectured, a little earlier, that Poincaré might well have accepted as compatible with what he really meant by "neutrality."

This interpretation seems to me abundantly confirmed in Poincaré's work; perhaps nowhere more clearly than in Part I of the *Électricité et optique.* The avowed intent of this work is to render accessible the theory of Maxwell, which had long been regarded as exceedingly obscure. The obscurity consisted essentially in this, that Maxwell claims to give a *dynamical* theory of electromagnetic processes as *processes in a material medium*--the ether; but does not give a clear, detailed, and coherent account of the dynamical structure of that medium. The attitude implied by this, although Maxwell expresses it with great clarity, seems to have been too radical to be easily assimilated by physicists of the time. In the period 1885-1890, three important expositions of the principles of Maxwell's theory appeared: those of Heaviside, of Hertz, and of Poincaré. The accounts of Heaviside and of Hertz are similar in viewpoint--a viewpoint conveniently summarized in Hertz's famous statement, "To the question 'What is the Maxwell theory?' I should not know a shorter and more definite answer than this: The Maxwell the-

[34]*The Value of Science,* in *The Foundations of Science,* p. 325.

ory is the system of the Maxwell equations."[35] And indeed this viewpoint--that the essential content of Maxwell's theory consists in what it says about the electric and magnetic fields, in relation to one another, to charges, and to the distribution of energy--can also reasonably claim to be the viewpoint of Maxwell himself. One of its consequences for exposition is to encourage the elimination of detailed stipulations about the ether--just the elimination that puzzled Maxwell's readers.

Poincaré's exposition adopts a very different course. Remarking justly that the most puzzling issue in understanding Maxwell's theory, in so far as a picture of the ether is concerned, is to form some conception or model of what constitutes a static electric charge,[36] Poincaré proceeds as follows: First he reviews the mathematical theory of electrostatic phenomena: the theory of Laplace's and Poisson's equations, generalized for the case of variable dielectric coefficient. To this *complete* theory of the relations for electrostatic phenomena he devotes thirteen pages.[37] Then he expounds *two distinct "models"* of the processes in dielectric media, which reduce completely to the mathematical theory already developed if one restricts one's attention to the electrical relationships, and which have in common that they each represent changing dielectric displacement as a current of fluid (i.e., they embody concretely and literally the "displacement current" of Maxwell); but which are otherwise qualitatively altogether unlike. This exposition--conceptually rather complicated, and mathematically rather hard--takes up *sixty-six pages;*[38] at the end of which Poincaré explains that if he had presented only one theory of the displacement current, his readers might have been led to believe that Maxwell regarded dielectric displacement as "the veritable displacement of a veritable matter"--whereas by presenting two incompatible theories, he has made it clear that each is to be viewed merely as a convenient fiction.[39]

I must confess that my own impression, in reading these (really quite fascinating) chapters of Poincaré was that a mild psychological aberration is here revealed. The virtuosity of the performance is far more striking than its pedagogical or scientific value; it seems to me that Poincaré allowed himself to be somewhat carried away by his enjoyment of his own mathematical facility. However that may be, we do have a clear illustration of the use of "indifferent" hypotheses; and one that fits the interpretation I have offered.

But there is deeper instruction to be gained from the *Électricité et optique.* The central thesis of that work--the key it offers to the understanding of Maxwell--is this: *"Maxwell does not give a mechanical explanation of electricity and magnetism; he confines himself to showing that this explanation is possible."*[40] What does Poincaré mean by such a demonstration of possibility? He tells us plainly: a mechanical explanation of a certain process

[35]*Ausbreitung der elektrischen Kraft,* p. 23.

[36]*Électricité et optique,* Part I, pp. xvi-xviii.

[37]Ibid., ch. i.

[38]Ibid., ch. ii-iii.

[39]Ibid., p. 79.

[40]Ibid., p. vii; emphasis in original. Cf. also *Science and Hypothesis,* p. 176; this chapter (xii) of the latter book consists of extracts, slightly modified, from the introductions to Poincaré's *Théorie mathématique de la lumière* and *Électricité et optique* (Part I).

is possible if and only if each system supporting that process can be fully characterized as what is called a *Lagrangian dynamical system:* that is (to use an elegant later mathematical terminology), if its "configurations" can be represented as the points of a differentiable manifold, and if a kinetic-energy function can be defined as a positive definite quadratic form on the tangent-vector space of that manifold, and a potential energy as a function on the manifold, in such a way that the laws of the process are given by the differential equations of Lagrange. *If* this can be done, Poincaré claims, then one can always, in infinitely many different ways, interpret the manifold of configurations with its kinetic-energy function as that of a *constrained system of particles* whose motions are governed by the constraints and by the forces that result from the potential energy. For this claim Poincaré offers a proof in the Introduction to the *Électricité et optique.*[41]

The situation here is a bit complex. First: Poincaré's proof is quite inadequate. What he does is in effect to count the number of conditions implied by the problem he poses and the number of variables available for its solution, and argue that the number of variables can always be made large enough to guarantee the existence of a solution--and this in infinitely many ways. But that kind of counting argument will not do. The problem is in fact a very deep one. Next: the theorem stated by Poincaré is indeed true: it is in effect the same as a theorem, very famous in differential geometry, proved in 1954 by John Nash.[42] Finally, however: the theorem fails to accomplish Poincaré's purpose (or what ought to have been his purpose). For the system of particles that will satisfy Poincaré's demand need have none but an *abstract* relation to the physical process for which a "mechanical explanation" is sought. For instance, it is always possible to fulfil the conditions of Poincaré with a system of particles *all of which are constrained to move in a single straight line*--like a string of beads. The motions will then bear a *formal* relation to the physical process, in the sense in which a kind of super-abacus may, by its computations, "represent" the evolution of a physical system; but the actual location and physical form of those motions will be entirely different from the process they "represent" or simulate. Furthermore, in introducing an arbitrary potential function to define the forces that act in the system, Poincaré is wildly far from Maxwell's aim of eliminating action-at-a-distance from electromagnetism. In short, what we have is a deep and remarkable piece of mathematics (although treated with uncharacteristic *shallowness* by Poincaré), but on the physical side, an all-too-formal, hopelessly inadequate conception of the very nature and purpose of mechanical explanation--or, more generally, of physical theory.

Indeed, when we juxtapose what, according to his formulation of his theorem, Poincaré means by "mechanical explanation," with his characterization of the achievement of Maxwell, something verging upon the grotesque is seen. Poincaré says: "Despite the efforts of Maxwell, we do not yet possess a complete mechanical explanation of these phenomena [of electromagnetism]; none the less the works of this physicist have a capital importance: they demonstrate the possibility of such an explanation." In

---

[41]Ibid., pp. ix-xiv.

[42]John Nash, "C^1 Isometric Imbeddings," *Annals of Mathematics* **60** (1954), 383-95; "The Imbedding Theorem for Riemannian Manifolds," *Annals of Mathematics* **63** (1956), 20-63.

fairness I should add that this statement by no means represents Poincaré's full and final assessment of Maxwell's significance. But remember that both to Maxwell himself, and to us, the really great achievement of his theory was its indication that there is no electromagnetic action at a distance; that such action is propagated, with its energy (and one should add: momentum and angular momentum) with the speed of light; and that, in particular, light itself *is* such an action. And remember that a puzzling thing about Poincaré's discussion of the principle of reaction was what looked like a failure to take quite seriously the implications of this propagation--a failure, as I earlier put it, to "believe in the ether": that is, to take seriously *the functions* of the ether in *actual physical processes* according to the theory of Maxwell. All of this, I suggest, is part of one central philosophical mistake.

One way to describe this mistake--which I think is related to a wider philosophical tradition--is to say that Poincaré construed the function of theoretical work as essentially administrative--an activity of information-processing. The job of the theorist *par excellence*--Lorentz, for instance, or Maxwell: the creator of theories--is to find the simplest and most systematic arrangement for storing scientific information--within the limits defined by those assumptions so entrenched as to function as *a priori* principles, i.e., as "definitions in disguise." The job of the mathematical physicist working out special consequences is that of information retrieval, and transformation of the retrieved information into usable form. The information itself comes from experiment.

Why is this altogether sane and reasonable account a mistake? Poincaré himself emphasized the chief point, in his own discussion of hypotheses in physics: experimental results are, intrinsically, mere historical facts, from which alone nothing of interest follows:

> [A] collection of facts is no more a science than a heap of stones is a house.
>
> And above all the scientist must foresee. Carlyle has somewhere said something like this: "Nothing but facts are of importance. John Lackland passed by here. Here is something that is admirable. Here is a reality for which I would give all the theories in the world." Carlyle was a fellow countryman of Bacon; but Bacon would not have said that. That is the language of the historian. The physicist would say rather: "John Lackland passed by here; that makes no difference to me, for he never will pass this way again."[43]

But, having recognized the crucial role of "hypotheses" in the process of interpreting and using the data of experience, Poincaré treats the relationships thus: we not only make observations, we also gauge their reliability, and *generalize* and even *correct* the results (as in tracing a curve through a plot of data); thus we obtain the "real generalizations," *which alone are fruitful.* Beyond this, we *organize* the results obtained in a way Poincaré describes as "hierarchical";[44] this organization not only facilitates information retrieval, it also reveals the gaps in our knowledge and so guides future research. The

[43] *Science and Hypothesis,* pp. 127-8.

[44] See, e.g., *Science and Method,* in *The Foundations of Science,* p. 363.

special characteristic of Poincaré's view that I am trying to convey is most clearly expressed by him as follows:

> Let us compare science to a library that ought to grow continually. The librarian has at his disposal for his purchases only insufficient funds. He ought to make an effort not to waste them.
>
> It is experimental physics that is entrusted with the purchases. It alone can enrich the library.
>
> As for mathematical physics, its task will be to make out the catalogue. If the catalogue is well made, the library will not be any richer, but the reader will be helped to use its riches.
>
> And even by showing the librarian the gaps in his collections, it will enable him to make a judicious use of his funds; which is all the more important because these funds are entirely inadequate.

This lucid metaphor expresses a false theory of inductive reasoning in science; or rather of that process of reasoning which is an intricate combination of the components Peirce called "abduction"--the invention of hypotheses--and "induction"--their testing. Poincaré's metaphor would have been a truer one if he had said that theoretical physics writes, not the catalogue, but the *books*--which experimental physics has to decide whether to buy, and whether to keep.

This mistake is a characteristic one in the empiricist tradition. It involves what seems to me a very odd paralogism. One recognizes--and I cannot say too strongly how right I think this is--that experience is our only touchstone of true information. One observes, with Hume, that the data of experience do not logically entail knowledge of "objective existence" or of the future. In application to the results of science, one draws sophisticated conclusions about the *merely* hypothetical status of the objects and agencies of scientific theory--e.g., atoms or the ether. But one neglects thereby the earlier insight--the Berkeleyan insight, which precedes Hume--that in a fundamental empiricist epistemological analysis all the objects and agencies of common sense and ordinary life have that same status. In effect, this is to distinguish, whether tacitly or explicitly, between the "reality"--or just persuasiveness--of what the ordinary processes of common sense make of the empirical data, and the reality or persuasiveness of the conclusions reached by the sophisticated processes of science.

In Hume the distinction is explicit. Hume attributes to inductive reasoning no "rational" force, only a kind of biological effect; and he remarks that long and intricate trains of argument do not have this effect of compelling conviction.[45] But setting aside analytical critique of this position, it is mistaken by the standards of common sense itself: common sense, viewing the actual facts of the development of science, can hardly fail to be impressed by, e.g., Newton's prediction that a force of attraction exists between any two bodies--a prediction made on the basis of a train of reasoning quite long and intricate enough to be ineffectual by Hume's account (although Hume took

[45] *An Enquiry Concerning Human Understanding,* sect. 7, Part I, next to last paragraph.

Newton's theory of attraction as a paradigm of what he himself hoped to do for the science of man). Again, common sense is impressed by Maxwell's theoretical prediction of electric waves; and Poincaré calls the confirmation of this prediction "dazzling"; and yet he says that the theorist only "makes out the catalogue"!

Five years after the papers stimulated by Larmor's theory, Poincaré addressed himself in a novel way to the problem of momentum-balance in the theory of Lorentz.[46] This work was composed for a volume honoring Lorentz on the occasion of the twenty-fifth anniversary of his doctorate. The important results of this new discussion were these:

(1) If we attribute to each point of the electromagnetic field, besides the density and flow of ordinary matter, an additional mass-density equal to the electromagnetic energy-density divided by the square of the velocity of light, and a corresponding *flow* of this mass proportional to the standard electromagnetic energy-flow, then momentum will after all be conserved in the theory of Lorentz.

(2) On the same condition, angular momentum too will be conserved.

(3) On these assumptions alone, total mass will fluctuate--since total electromagnetic energy fluctuates as the field receives energy from or communicates it to "ponderable matter"; and therefore also the principle of the conservation of velocity of the center of gravity of an isolated system will fail. To secure this conservation, we must assume further that the mass associated with electromagnetic energy remains when that energy is converted to another form.

In 1903, Max Abraham postulated the momentum of the electromagnetic field, referring to Poincaré as the first to set forth the idea.[47] But there is an important difference between Abraham's view and Poincaré's. Abraham unquestionably puts to good use the postulate he bases upon the suggestion of Poincaré--of which he says (and justifies the assertion): "The existence of an electromagnetic momentum is of fundamental importance for the dynamics of the electron." But this is not at all the opinion of Poincaré. He introduces the electromagnetic momentum as that of "a fictitious fluid." For conservation of mass, and of velocity of the center of gravity, this fluid must be conserved; and Poincaré calls the fluid "energy." This, to one who knows something of where science went, sounds quite momentous. But here is what Poincaré says about it:

> We must agree that this non-electric energy remains at the point at which its transformation [from electric energy] occurred, and is not subsequently car-

[46]"La théorie de Lorentz et le principe de reaction," *Archives néerlandaises des Sciences exactes et naturelles,* 2nd series, **5** (1900); Poincaré, *Oeuvres,* vol. 9, pp. 464-488.

[47]Max Abraham, "Prinzipien der Dynamik des Elektrons," *Annalen der Physik,* 4th series, **10** (1903), 105-179; see p. 110. (Abraham also cites J. J. Thomson, who proposed a notion of electromagnetic momentum in 1893 on the basis of his rather idiosyncratic--and somewhat vague--theory of "moving Faraday tubes of force.")

> ried along by the matter in which one ordinarily localizes it. There is nothing in this convention that should shock us, since it only concerns a mathematical fiction. If one adopts this convention, the motion of the center of gravity of the system is again rectilinear and uniform.
>
> To extend the proposition to the case where there is not only destruction, but creation of [electromagnetic] energy, it suffices to assume at each point a store of non-electric energy, at whose expense the electromagnetic energy is formed. One will maintain the foregoing convention; that is, instead of localizing the non-electric energy as one ordinarily does, one will regard it as immobile. On this condition, the center of gravity will again move in a straight line.[48]

Pace Poincaré, these suppositions *are*--from a physical point of view--"shocking." The assumption that the non-electric energy remains "immobile" is jarringly discordant with the principle of relativity; and the picture that this assumption gives of energy-conservation is monstrous: where we see energy exchanged between bodies and electromagnetic field, we are asked to imagine an infinite bank of "fictitious" energy distributed everywhere, and to suppose that electromagnetic energy is exchanged only with the energy in this bank (which, while in the bank, remains "immobile"--in absolute space!). Of course, electromagnetic energy can be drawn from the bank only where and when some ordinary body *also* loses energy (and correspondingly for the reverse exchange). There are, indeed, *two* energy conservation laws: one of "ordinary" energy, including both that of bodies and that of the electromagnetic field; the other of the "fictitious fluid" energy, which itself coincides *in part* with the field energy, and whose other part bears no relation to anything else physical. This, however, is all acceptable--because the fictitious energy is merely fictitious, and its inertial mass is fictitious, and the whole set of reflections is a mathematical *tour de force!*--in short, a system of neutral hypotheses served up, in Lorentz's honor, to show how far a skilled mathematical artist could succeed in fitting Lorentz's theory into the framework of conventions of physics.

Poincaré's efforts to achieve such fitting continued to be quite strenuous and--although I myself find it hard to grasp how this was possible, in the light of what seems almost frivolous in the material I have just described--not only strenuous, but deeply serious. At the International Congress of Physics, in the same year as the Lorentz Festschrift, Poincaré reported on the state of the science, and emphasized strongly the desirability of having a theory that should once for all and rigorously reconcile electrodynamics with the principle of relativity; for Lorentz's theory, as of the year 1900, did so only--in the jargon of the subject--"to the second order in the aberration parameter *v/c.*" Poincaré's words are worth quoting:

> Experiments have been made which should have disclosed the terms of the first order; the results have been negative; could that be by chance? No one

[48] Poincaré, *Oeuvres,* vol. 9, p. 470.

> has assumed that; a general explanation has been sought, and Lorentz has found it; he has shown that the terms of the first order must destroy each other, but not those of the second. Then more precise experiments were made; they also were negative; neither could this be the effect of chance; an explanation was necessary; it was found; they always are found; of hypotheses there is never lack.
>
> But this is not enough; who does not feel that this is still to leave to chance too great a role? Would not that also be a chance, this singular coincidence which brought it about that a certain circumstance should come just in the nick of time to destroy the terms of the first order, and that another circumstance, wholly different, but just as opportune, should take upon itself to destroy those of the second order? No, it is necessary to find an explanation the same for the one as for the other, and then everything leads us to think that this explanation will hold good equally well for the terms of higher order, and that the mutual destruction of these terms will be rigorous and absolute.[49]

That has *not* the tone of the library cataloguer; perhaps, rather, to revert to my earlier comparison, of the dedicated poet; but in fact, after all, it has the tone of a philosopher and scientist.

The remainder of the story is, briefly, this: Lorentz, stimulated by Poincaré's insistence on the desirability of such a "rigorous and absolute" satisfaction of the principle of relativity, produced in 1904 a theory that *almost* achieved this. That is the "theory of Lorentz" which Poincaré conceived himself as "modifying and completing in certain points of detail" in his definitive work of 1905/6. In this work, Poincaré did indeed succeed completely. But we have already seen his own estimate of the value of that work: he thought it just another bit of complicated tinkering! And in fact, in a review article with the same title as the 1906 memoir, published in 1908, and reproduced as "Book III. The New Mechanics" in *Science and Method,* the formulation of 1906 is simply not mentioned; nor is electromagnetic momentum or the inertia of energy (fictitious or real): Poincaré, snubbing his own contribution, just says that the facts compel us "to adopt the theory of Lorentz, and consequently *to renounce the principle of reaction.*"[50]

The themes of physics that we have been discussing came to genuine convergence when Einstein, after proposing his theory of the electrodynamics of moving bodies in 1905, turned his own attention to the "mechanical" questions involved in the transfer of energy and momentum. Einstein showed, in 1906, that what is required for the conservation of velocity of the center of gravity of an isolated system containing bodies interacting with the electromagnetic field is *the inertia of energy*--the equivalence of mass and energy that has come to loom so large in the "real world," in *every* sense of that phrase; but the inertia, not merely of *electromagnetic* energy, but of *all* energy. This means that

---

[49]*Rapports du Congrès de Physique de 1900,* vol. 1, pp. 22-3; reproduced in *Science and Hypothesis,* chs. ix and x; for the passage concerned, see *The Foundations of Science,* pp. 147-8.

[50]Poincaré, *Oeuvres,* vol. 9, p. 570; *The Foundations of Science,* p. 505.

the "fictitious fluid" energy of Poincaré's paper of 1900 should *not* be conceived to remain "immobile" when energy is absorbed by a body, but should rather be conceived to be carried along by the body--in short, the "fictitious" energy is to be *identified with* the energy already known in physics; so the word "fictitious" is struck, all energy has inertia, and the features Poincaré had to excuse by calling the theory "a mathematical fiction" fall entirely away. This gain is *essentially* connected with the conceptual structure of Einstein-Minkowski space-time. It is because the basic kinematic framework of Einstein's theory of 1905 is different from that of Poincaré's theory of 1900 that this harmony is found.

But why did Poincaré not find this in his own work of 1905/6? And why did he pay no attention to the circumstance after Einstein did discover it--why did he continue to maintain that Lorentz's theory is incompatible with the principle of reaction? Observe that this incompatibility means *a failure to fit into the framework of classical mechanics;* this is the implication of the title used by Poincaré for Book III of *Science and Method:* "The New Mechanics." And remember that mechanics was part of what Poincaré had declared to be the *conventional* framework of physics.--Ah, yes; but not quite so deeply established a part as was geometry (a distinction that had already been made by Poincaré in *Science and Hypothesis;*[51] not indeed with respect to corrigibility--for Poincaré had there said, in rebuttal of Hertz, that any fears that experience could induce us to change mechanics are groundless[52]--but in that mechanics has as its *subject matter* the same *objects* that are treated of by physics[53]). So, retreating a step from the claim that the conventions are to be defended at all cost to a *hierarchical* view of what must be sacrificed *first,* Poincaré *preferred* the prospect of "break[ing] the framework in which we try to fit optical phenomena and electrical phenomena together"; was *pressed* to the point of "breaking the framework" of mechanics; but simply refused to consider his own, or Einstein's, work of 1905 as a legitimate *modification of the framework of space and time,* in the interest of a coherent *and fruitful* electrodynamics and mechanics.

I suggest, therefore, that two issues from Poincaré's philosophy of science here combine: his "conventionalism" on the one hand, which dictates his priorities; his doctrine, on the other hand, that one *ought not to expect* hypotheses of the "indifferent" kind to *be* fruitful.

Finally, there is a deep irony to be remarked on concerning Poincaré's views of what is "real" *vs.* "fictitious," and what is *"arrivé,"* in the light both of subsequent developments in physics and of some more recent philosophical claims. As to the latter, it has tended to be a cliché among so-called "scientific realists" that the development of science after 1900 established that atoms--so questionable to the more radical empiricists of the later nineteenth century (and, as we have seen, partly also to Poincaré)--have been shown to be real; whereas the ether has turned out not to be. Now, regarding the ether as an unlikely candidate for his scientific club, Poincaré thought that motion rela-

---

[51]*The Foundations of Science,* pp. 123-5.

[52]*Ibid.,* p. 102.

[53]In contrast, geometry and kinematics have as their subject matter *space* and *time*--and, one may say, " moving points"-- not empirically given objects at all, but mathematical idealizations.

tive to it should have no meaning--and therefore should be rigorously undetectable; and that momentum should not be exchangeable between bodies and the ether, but exists only where “ponderable” mass is in motion. Einstein has taught us that “motion relative to the ether” is indeed strictly meaningless; but also that *there can be no* direct momentum exchange between distant bodies, and that momentum conservation therefore can *only* hold if there is “real” propagation of momentum through the space between such bodies: in *this* sense, the ether--at least its momentum--is *arrivé.* At the same time, with momentum there always goes energy; and with energy, inertia (as Poincaré himself just missed discovering); and inertial mass implies gravitational mass. Therefore the momentum conveyed to a mirror when it reflects light is after all carried by what is in the literal sense a “ponderable”--weighable--mass: the mass of the light itself; which is no more fictitious, from any fundamental physical or philosophical point of view, than the mass of the sun or the moon, or of any body I can pick up in my hands.

One point should be emphasized here. I have been led, in this discussion, to place a certain emphasis upon what might be called the “metaphysical” side of Poincaré’s teaching: his denial of “reality,” or “factual” status, to the constructions of the theorist. But this should not be understood as a stand in favor of “realism” as opposed to “instrumentalism” in the interpretation of scientific theories. The issue concerns, rather, an inadequacy in what may be called Poincaré’s conception of the dialectic of scientific concepts and theories, and of their relation to experiment. And that inadequacy I have earlier associated with what I called a very odd paralogism that recurs in the empiricist tradition: in effect, a double standard of epistemological critique, which occurs with some frequency among empiricist and instrumentalist philosophers of science. Let me quote myself, from an article devoted entirely to the issue of realism and anti-realism. I said there (with an allusion to an earlier (unpublished) version of this very paper):

> Poincaré was certainly an instrumentalist; and in my discussion I took him to task for unwillingness to regard certain things as “real.” . . . [L]et me single out just one of the physical points involved. The contested reality was that of the ether; and Poincaré, because he regarded the ether as a fiction rather than a reality, was unwilling to take very seriously (although he was willing to play with) the idea that charged particles exchange momentum with the ether. This, however, is a very odd position for an instrumentalist to take . . . ; for there is no warrant at all in the instrumentalist view for *grading* the entities of a theory in *degrees* of reality or fictitiousness--regarding particles as *more real* than the ether. This is a double standard that recurs in the empiricist tradition, but it is not itself a genuine part of empiricism or instrumentalism--it is, on the contrary, a bit of unregenerate realism, doing the work of the Devil among the empiricists and instrumentalists.[54]

---

[54]Howard Stein, “Yes, but . . . --Some Skeptical Remarks on Realism and Anti-Realism,” *Dialectica* **43** (1989), pp. 47-65; for the passage quoted, see p. 56.

In conclusion, let me return to my remarks at the beginning: The matters of physics that have been under discussion here were of the highest importance at the time; and very few investigators made deeper or more clarifying contributions to them than Poincaré. If those contributions were in some degree impaired by philosophical mistakes, then precisely because those mistakes *mattered* in work of such fundamental importance, I think they offer an extremely valuable subject of reflection for the philosophy of science.[55]

---

[55]I have been asked, in the course of discussion of this paper (after an oral presentation), to state more exactly what I see as Poincaré's "mistake"; and also what *my* stand would be on the question whether Poincaré or Einstein was truly the author of the special theory of relativity. On the latter point, in particular, it was remarked--assuming Einstein were to be given the preference--that it would seem odd to call someone the *author* of a theory on the grounds that he was the first one to "take it seriously." Let me attempt to clarify my view by some further comment here.

The basic mistake that I ascribe to Poincaré is that of seeing the significance of theoretical work as residing *essentially and exclusively* in its function in *organizing* knowledge (putative as well as real): that is, organizing the "real generalizations"--which count as *presently claimed knowledge,* although it is always possible that they may later fail experimental test. He recognizes that in *forming* those (lower-level) "hypotheses" that he calls "real generalizations" one goes beyond the actual data (and, as I have remarked in the text, even "corrects"--i.e., formally, *contradicts*--some of the data); *this* he takes to be a legitimate predictive activity. But he does not consider the "hypotheses" he calls "neutral," or those of very high level that are "definitions in disguise," to have any analogous legitimate function: cf. the discussion in the text of his figure of science as a library, and the theorist as drawing up the catalogue.

A consequence, in Poincaré's specific theoretical work, of this general philosophic doctrine--which doctrine I think one can here see him obeying with some consistency--is that he regards the theories *he* is concerned to develop as "convenient--or *more-or-less* convenient--*fictions."* Indeed, I am inclined to venture the psychological hypothesis that Poincaré, whose confidence in his own *mathematical* powers was very great indeed, had some diffidence about trespassing on the domain of *physical prediction;* and that this theory of the fictitiousness, or "indifference," of his own physical theories was for him *liberating:* that it made it possible for him to speculate quite boldly, because of--as *I* should maintain--the *fiction* (!) that those speculations were in principle sterile exercises.

And this is the crucial difference, as I see it, between Poincaré's relation to the special theory of relativity and Einstein's. *Both* of them *discovered* this theory--and did so independently. So far as its mathematical structure is concerned, Poincaré's grasp of the theory was in some important respects superior to Einstein's. But Einstein "took the theory seriously" *in the sense that he looked to it for* NEW INFORMATION *about the physical world*--that is, in Poincaré's language, he regarded it as "fertile": as a source of new "real generalizations"--of empirically testable consequences. And in doing so, Einstein attributed physical significance to the basic notions of the theory itself in a way that Poincaré did not. Furthermore, this point of view led Einstein to *seek*--and successfully--*consequences* of the theory that (a) transform conventional notions (I here mean the word "conventional" in its ordinary sense; but I mean also to suggest the transformation of what in his own special sense Poincaré calls "conventions") and (b) lead to novel predictions. Of the latter, Whittaker's notorious discussion credits Einstein with the relativistic formula for the transverse Doppler effect; but everyone at the present day will agree that the discovery of the inertia of energy was far worthier of citation. And I have explained in the text above how close Poincaré himself really was to this discovery--which yet he failed to make. That Poincaré did not regard such new consequences as a desideratum for a theory, indeed that in a certain sense he viewed it as unreasonable and illegitimate for a theory to be expected to have such consequences, is something I have tried to show in citations of his own words. In his actual practice, he does not suggest or look for such new consequences. And, indeed, in the memoir of 1906, when he comes to discuss how the Newtonian law of gravitational attraction might be modified so as to fit in the framework of the theory he has developed--that is, when he proposes possible Lorentz-invariant forms of a law of gravitation--he concludes as follows:

[T]he first question that presents itself is that of knowing whether [these forms of the law] are compatible with the astronomical observations; the divergence with the law of Newton is of the order of [the square of the magnitude of the velocities of the gravitating bodies--on a scale on which the velocity of light is unity], that is to say 10,000 times smaller than if it were of the order of the velocities themselves, that is to say [than] if the propagation were made with the velocity of light *ceteris non mutatis.* [The point here is that the divergence will be 10,000 times smaller than that which had been calculated by Laplace on the hypothesis that gravitation is propagated with the velocity of light: Laplace's result had seemed to exclude such propagation, as incompatible with the observations.] It is therefore allowable to hope that the divergence will not be too great. But only a deeper discussion will be able to settle the matter. (--"La dynamique de l'électron," in Poincaré, *Oeuvres,* vol. 9, p. 550.)

This is all perfectly reasonable; but the one thing one misses is something that is conspicuous in Einstein's parallel reflection upon his general theory of relativity, roughly ten years later. Einstein too, of course, is concerned to show that his new theory agrees with Newton's to a good enough approximation; but for *him* it is crucial to try to find *detectable* deviations from the predictions of Newton. And so he is led to the famous three initial empirical tests of general relativity; and first of all to the extra motion of the perihelion of a planet, of detectable magnitude in the case of Mercury.

Perhaps this attempt at clarification has grown too long, and too circumstantial, to be clear. Let me cite in evidence one more item, however: a piece of testimony; but the testimony of Einstein, concerning a face-to-face discussion he had with Poincaré at the first Solvay Conference, in Brussells, in 1911. The statement occurs in a letter from Einstein to H. Zangger, dated November 15, 1911; I quote it from the admirable book of Abraham Pais, *'Subtle is the Lord...' The Science and the Life of Albert Einstein* (Oxford University Press, 1982), p. 170; but my translation differs somewhat from that of Pais. Einstein writes: "Poincaré was (towards the relativity-theory) simply opposed in general *[einfach allgemein ablehnend],* [and] showed for all his acuteness little understanding of the situation."

If you are still inclined to ask, How is this possible--on the part of a man who was one of the independent *authors* of that theory?--if this seems bizarre, paradoxical--then I refer you to my introductory remarks, in which I have called the case strange, and *uniquely* so! But how it *was* possible is just what I have tried to shed light on in this paper.