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"Natural Laws and Definitions"

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NATURAL LAWS AND DEFINITIONS

An Enquiry into Conventionalism in Relation to the Foundations of Classical Mechanics

The nature of the variously described laws, theories and axioms of classical mechanics cannot be clearly understood without first considering briefly what the role of such statements is in the scientific enterprise. Taking the aims of science to be the explanation and description of the observable world, its method is that of subsuming vast bodies of facts under a few general laws, extracting or imposing order where there was disorder, explaining changing appearances in terms of known relations between fixed entities. But change, to be explained, must first be measured, and on different scales the measure will be different. Or if one constructs different entities to save the appearances, then different relations will be seen to obtain between them.

A scientific explanation then typically consists of statements of fact about defined entities. It is not, however, always possible to decide which of all the statements that can be made about an entity define it and which describe it. Suppose, for instance, that an entity C may be clearly discriminated by the properties P_1, P_2---- P_k. The possession of these properties then defines this entity. But a definition, of itself, conveys no factual information. For C, so defined, to fulfil its role in scientific explanation, one needs to show that it corresponds to an existent and useful entity having some further properties, P_{k+1}, P_{k+2} -- P_n.[1] One may now say of C that it has in fact the properties P_1, P_2 ----

1. That is to say, in science at least, one accepts John Stuart Mill's assertion "that every definition contains an axiom, because by defining we implicitly affirm the existence of the object defined" - cited, Poincaré, "Science and Hypothesis", Dover (1952), p. 44.

P_n, where P_1--- P_k served originally to define C, and P_{k+1} -- P_n to describe it. But it is quite possible that C could be as uniquely defined by some other sub-set of properties P_f ---- P_l, and then all the other properties are descriptive. There may in fact be many different sub-sets of properties that would define C, so that any one property P_j may in some cases serve in the definition of C, and in others be an empirical statement of fact about it. It is important to note that if one regards <u>all</u> the properties P_1 --- P_n as part of the definition of C, one no longer has any statement about the real world, since it is a matter of fact, not logical necessity, that possession of the properties P_1 --- P_k entails the possession of P_{k+1} --- P_n. On the other hand, the entity C has to be defined, and a sufficient number of properties must be set aside to this end.

I should remark at this point that in defining entities by their properties I do not mean to conflict with those, such as P. W. Bridgman[2], who hold that the concepts of science are operationally defined. "If the concept is physical, as of length, the operations are actual physical operations, namely those by which length is measured."[3] Clearly, to determine a property is to carry out a specified set of operations and to observe some consequent result, usually a number denoting, for instance, the position of a pointer on a scale. This number, then, when stated in conjunction with the operations needed to arrive at it, specifies the property, so that the above definitions in terms of properties may equally well be regarded as definitions in terms of the operations by which the properties are measured. I do not have in mind unobservable properties, such as those of Newtonian absolute

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2. Feigl & Brodbeck (Ed.), "Readings in the Philosophy of Science." Appleton-Century-Crofts (1953), pp. 34-46.
3. P. W. Bridgman, "The Logic of Modern Physics," Feigl & Brodbeck, <u>op. cit</u>. p. 36.

time, against which Bridgman rightly directs his attack.

Given then the properties P_1 --- P_n, it remains to decide which of the possible sub-sets should best be taken to define C. Since science, through the application of one blade of Occam's razor , deems it desirable to use as few definitions as possible, those properties will be chosen which can discriminate most sharply and certainly, and which can be most widely applied. For example,[4] the statement "Copper has a certain, high electrical conductivity," was originally a factual assertion about an entity defined in terms of other properties. Now, however, that electrical conductivity can be measured so precisely for such a wide variety of materials, this statement has become part of the definition of copper, displacing other, vaguer, less discriminatory properties such as colour. This change of status has very practical consequences: given a discrepancy in a conductivity measurement of something thought to be copper, previously one would have concluded that the conductivity of copper was not what it had been thought to be, while now one would conclude that the substance was not in fact copper, but some other material. Thus, once a property is chosen as part of the definition of an entity, it becomes in one sense conventional, in that it can no longer be refuted by experiment. But still, such a definition is not devoid of factual content, since the choice of it carries the implication that the order, regularity, constancy in nature embodied in the entity can be more surely characterised by this than by some other definition. If the factual properties of the entity and its relations with other entities could be more simply and generally expressed by using a different definition, this would be done. One instance is the choice of

4. Taken from Ernest Nagel, "The Structure of Science," Harcourt, Brace & World, Inc. (1961) p. 54.

the constant-volume ideal-gas thermometer to define the scale of temperature, in preference to such other devices as mercury-in-glass thermometers, thermocouples, and pyrometers. The ideal-gas scale is chosen because it is in fact independent of the particular gas used, and because it can be shown to be identical with the theoretical thermodynamic scale of temperature. Thus the conclusions of thermodynamics can be applied to real systems if temperatures are measured on the ideal-gas scale. It is true, of course, as Bridgman points out,[5] that auxiliary definitions have to be provided to extend the concept beyond the range to which the original definition applies. From his operational viewpoint, this really means defining a new entity; from the viewpoint presented here, the procedure is merely that of changing to a more convenient sub-set of defining properties, taking care that there is no practical ambiguity in the region of overlap.

These considerations are clear enough in such cases as those of temperature or the chemical elements, where the properties involved relate to more or less directly observable quantities. The basic statements of classical mechanics, however, concern the more highly abstracted entities of length, time, mass and force and are in consequence much harder to label with certainty as conventions on the one hand or empirical statements on the other. "The (Newtonian) axioms of Motion are _theoretical_ statements....; (i.e.) they are not statements about relations between experimentally specified properties, but are postulates implicitly defining a number of fundamental notions that are otherwise left unspecified by the postulates of the theory."[6] It is therefore very tempting to assert that "most, if not all of the general 'principles' of physics are conventions."[7] By applying

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5. P. W. Bridgman, _op. cit._, p. 37 et seq.
6. Ernest Nagel, _op. cit._, p. 160.
7. _Ibid._, p. 260.

the arguments developed above to the basic entities of classical mechanics, we may consider to what extent this view, most notably advanced by Poincaré[8], is valid.

Classical mechanics rests upon the Newtonian axioms of motion, which he stated as follows:

> Law I: Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed thereon.
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> Law II: The alteration of motion is ever proportional to the motive force impressed, and is made in the direction of the right line in which that force is impressed.
>
> Law III: To every action there is always apposed an equal re-action; or, the mutual actions of two bodies upon each other are always equal and directed to contrary parts.[9]

The axioms in turn are based on certain assumptions about the nature of space and time, and these must first be made clear before the axioms themselves can properly be discussed.

One may note at this point that there have been several reformulations of the theory of classical mechanics, notably those of Hamilton and Lagrange. Some of these take other and newer concepts, such as energy, to be more fundamental than the Newtonian notion of force. All such formulations, however, are mathematically equivalent, and it seems therefore proper to conduct the inquiry on the basis of Newton's version as being both the original and most familiar.

In talking of motion and of the measurements of length and position involved, Newton took it for granted "that Euclidean geometry (was) the only theory of spatial relations that (could) provide a theory of mensuration.

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8. Henri Poincaré, "Science and Hypothesis," Dover (1952), Parts II and III.
9. Sir Isaac Newton, "Mathematical Principles of Natural Philosophy," Florian Cajori (Ed.). Berkeley, California (1947) cited, Nagel, op. cit., p. 158.

Since Newton's day, however, a large number of alternative pure geometries have been constructed."[10] In the face of these alternatives the issue of conventionalism is immediately raised: are the postulates of geometry, as applied to physical measurement, pure definitions, or have they a definite empirical content? The third alternative, affirmed by Kant, that they are synthetic a priori intuitions is well disposed of by Poincaré. In that case, he says, "they would be imposed upon us with such a force that we could not conceive of the contrary proposition, nor could we build upon it a theoretical edifice. There would be no non-Euclidean geometry."[11] But there is non-Euclidean geometry, and there can in any case be few left today who seriously hold that the contents and structure of the universe must of necessity be limited to what they themselves can conceive. But Poincaré's advocacy of conventionalism in this context suffers from a confusion between "pure" and "applied" geometry. With the former, certainly, "the axioms of geometry are only definitions in disguise. What, then, are we to think of the question: Is Euclidean geometry true? It has no meaning."[12] But then one must ask: are the postulates of pure geometry satisfied by "real" straight lines, for example, defined by the paths of light rays? If not, and this turns out to be the case over astronomical distances, there are two alternatives:

(1) The Euclidean definitions may be retained, when it becomes a matter of fact that light rays and celestial bodies do not travel in straight lines. Then, from Newton's first axiom, it is necessary to postulate some "universal force,"[13] of which Newtonian gravitation is an example,

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10. Ernest Nagel, op. cit., p. 234.
11. Henri Poincaré, op. cit., p. 48.
12. Ibid., p. 50.
13. Ernest Nagel, op. cit., p. 264.

to explain the deviation.

(2) A practical definition of a "straight line" as the path of a light ray may be adopted, when it becomes a matter of fact whether or not the axioms of a given geometry describe the properties of these lines. In this case, it makes perfectly good sense to ask if Euclidean geometry is true. "The question whether the practical geometry of the Universe is Euclidean or not has a clear meaning, and its answer can only be furnished by experience."[14]

Since Newton knew only Euclidean geometry, he did not have the alternative. But now, and in Poincaré's time, the alternative exists and has been variously expressed: - "One and the same physical world can be described in terms of different geometries, if only the formulation of the physical laws is each time adapted to the special geometry used."[15] "Geometry (G) predicates nothing about the relations of real things, but only geometry together with the purport (P) of physical laws can do so. Using symbols, we may say that the sum of (G) + (P) is subject to the control of experience. Thus (G) may be chosen arbitrarily, and also parts of (P)."[14] Poincaré indeed recognised the existence of the alternative: "Our choice among all possible (geometrical) conventions is guided by experimental facts"[12] but was himself in no doubt as to which to take: "Euclidean geometry is, and will remain, the most convenient .... because it is the simplest."[12] But from a scientific point of view, the desirable simplicity is that of the whole structure, that is, to use Einstein's symbolism, of (G) + (P) together, not of (G) alone. And it is now generally agreed that

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14. Albert Einstein, "Geometry and Experience," Feigl & Brodbeck, op. cit., p. 191.
15. Moritz Schlick, "Are Natural Laws Conventions," Feigl & Brodbeck, op. cit., p. 186.

the simplicity of Euclidean geometry is more than offset by the added complexity which its use introduces into the formulation of physical laws. Moreover, given the choice, it is more consonant with scientific tradition to choose defined entities that have physical reality. One may note here the analogy with the example of temperature measurement given earlier, where thermodynamics may be applied to real systems because the practical ideal-gas scale of temperature is identical with the theoretical thermodynamic scale. In that case there is a variety of ways of defining the practical entity AND so that one is chosen which fits the simplest theory. In astronomy, the path of a light ray is the only practical measure of a straight line and so one must adjust the geometrical definitions to match reality if the theorems of geometry are to be applicable to physical measurement. I conclude, therefore, that in the case of his "unshakable commitment"[16] to Euclidean geometry, Poincaré's conventionalism, while logically tenable, is scientifically perverse.

Given some geometrical system, which for classical mechanics will certainly be Euclidean, questions of the space and time to which Newton's axioms apply still arise. Newton considered motion as taking place in absolute space, which he carefully distinguished from relative space, the latter being merely "some movable dimension or measure of the absolute spaces; which our senses determine by its position to bodies; ... But because the parts of space cannot be ... distinguished from one another by our senses, ... instead of absolute places and motions, we use relative ones: ... but in philosophical disquisitions, we ought to abstract from our senses, and consider things themselves, distinct from what are only

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16. Ernest Nagel, op. cit., p. 266.

sensible measures of them. For it may be that there is no body really at rest, to which the places and motions of others may be referred."[17] Unfortunately, it has been shown to be impossible by any mechanical experiment to ascertain whether a body really is at rest with respect to absolute space. Newton himself recognised this in the case of coordinate systems having uniform velocity relative to absolute space, for in such a case the differential equations of motion are invariant. He did, however, think it possible, by such experiments as that of the rotating bucket, to demonstrate absolute rotation. In this experiment, a bucket part-filled with water is rotated until the water comes to rest relative to the bucket. The surface is then no longer plane, but paraboloidal. This deformation, Newton asserted, since it is not the result of the water's motion relative to the bucket, can only be the result of its motion, or more specifically, its rotation, in absolute space. However, as Ernst Mach has pointed out,[17] such an experiment merely demonstrates rotation relative to some system of bodies other than the bucket. "Try to fix Newton's bucket and rotate the heaven of fixed stars and then prove the absence of centrifugal forces."[18] Mach argued that "inertial properties are contingent upon the actual distribution of bodies in the Universe."[19] Absolute space, abstracted from the spatial relations between bodies, is quite unobservable and so scientifically useless, if not meaningless. "It is sufficient to take a coordinate system defined by the fixed stars as the frame of reference for the rotation."[19] Then the equations of motion remain the same for any frame of reference

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17. Cited Ernest Nagel, op. cit., p. 207.
18. Ernst Mach, "Newton's Views ofTime, Space and Motion," Feigl & Brodbeck, op. cit., p. 168 et seq.
19. Ernst Mach, cited by Ernest Nagel, op. cit., pp. 209-210.

moving with uniform velocity relative to the fixed stars, and, short of relativity, this is as far as one can go. Such frames of reference are called "inertial" or "Galilean". Again, all this can be regarded as pure convention. It is quite possible, indeed very common, to use a non-inertial coordinate system for convenience in analysing motions in, for instance, fluid mechanics. When this is done, the resulting changes in the equations of motion are explained in terms of "forces" invented precisely for that purpose (centrifugal and Coriolis forces). The earth's surface itself is a non-inertial frame, though in most measurements the effects of the earth's rotation are not significant. But it is still true that the basic reference frame of classical mechanics is taken from the class of "inertial" frames. Why? The reason is that if a non-inertial frame were selected, then in any other frame the equations of motion would be modified and "the specific force-functions that would have to be supplied in order to analyse motions in terms of the Newtonian axioms would be different in nearly every special problem, and would have to be invented ad hoc for each case"[20] and since it is the object of science to find the most general relations, such a choice would be highly undesirable, if indeed it did not render the practical application of Newton's axioms impossible. Thus, again, the choice of a reference frame, while formally conventional, is practically determined by the physical structure of the world.

Now, armed with Euclidean geometry and an inertial reference frame, one may attack the axioms themselves. The first axiom asserts that a body under the action of no force can only move uniformly in a straight line (with rest as a special case). This axiom, like Euclidean geometry, has

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20. Ernest Nagel, op. cit., p. 211.

been held to be an a priori truth, and, as in that case, the refutation is well put by Poincaré: "If this is so, how is it that the Greeks ignored it? How could they have believed that motion ceases with the cause of motion? or, again, that every body, if there is nothing to prevent it, will move in a circle, the noblest of all forms of motion."[21] The choice then lies, as before, between experimental law and definition. To the former, Poincaré at once objects, "Have there ever been experiments on bodies acted on by no forces? and, if so, how did we know that no forces were acting?"[21] Because the body moved uniformly in a straight line? But then the law certainly is a definition, either of the absence-of-force, or of uniform motion. Since one has Euclidean geometry and an inertial reference frame, the latter reduces to a definition of equal time-intervals. But even to give this meaning to the axiom, an independent definition either of absence-of-force or of equal time-intervals is needed. If both can be independently defined, then the axiom may indeed be regarded as an empirical law. It may be relevant to note here that notions of force and time were used, and measurements made of them, long before the first axiom was formulated.

Consider first time measurement. Equal time intervals may be demonstrated by any periodic device, such as a water-clock or pendulum. It is then observed, using such a time-scale, that many other processes exhibit regularity. If now some other of these processes are used as clocks, a wider range and more precise nature of regularity may be observed, and so, in accordance with the general principles outlined earlier, these will be preferred as time-defining processes. This procedure of "successive definition"[22] is continued until some practical limit of precision is

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21. Henri Poincaré, <u>op</u>. <u>cit</u>., pp. 91-92.
22. Ernest Nagel, <u>op</u>. <u>cit</u>., p. 181.

reached. Until recently, the rotating earth was the standard clock; this has now been replaced by the periodic oscillation of certain specified crystals. Again the close analogy will be noted with the development of the ideal-gas temperature scale.

As for the definition of absence-of-force, this is hardly possible until force itself has been defined, and here one must turn to the second and third axioms.

The second axiom can be cast into modern form to read: "The rate of change of momentum of a body is proportional to, and along the line of action of, the applied force" or $F = \frac{d(mv)}{dt}$. Several points need to be made about this axiom. In the first place, while force is plainly a directed quantity, it by no means follows that it can be represented by a vector. Indeed, from the fact that a line of action is specified, it is clear that six coordinates are required to determine a force -- three for the components of the force, and three to fix its point of application. Secondly, it further follows from the specifying of a line of action that the axioms of motion apply only to "point" bodies, since for such distributed forces as pressure, there is no unique line of action. The notion of "point" bodies or masses indicates a limiting procedure. If real bodies may be considered as made up of large numbers of point masses interacting in certain ways, then with the addition of the third axiom, the theory may be extended to cover the mechanics of real solids and fluids. (This concept of point masses must not, incidentally, be taken to imply a molecular model. In fluid mechanics, for instance, it is explicitly assumed that the fluid elements, to which Newton's axioms are applied in deriving the equations of fluid motion, contain enough molecules to approximate to elements in a continuum having the mean macroscopic properties of the fluid. When this assumption breaks down, as in low-pressure gaseous (Knudsen) diffusion, the equations of fluid mechanics do not apply, and kinetic theory, where the point masses are molecules, must be resorted to.) This extension in application of the axioms naturally requires further assumptions about the nature of the interactions between point masses. At one extreme the assumption that the masses can move freely and react only on collision leads to the kinetic theory of gases; at the other, the assumption that their relative spatial positions are precisely fixed leads to the mechanics of rigid bodies. In between, various relations between forces and relative positions, such as Hooke's law of elasticity, Newton's laws of viscosity and gravity, laws of surface tension, electrostatics, magnetism, electromagnetism, enable models to be constructed which approximate the behaviour of real solids and fluids. In short, it is quite clear that force is not meant to be directly observed or measured at all. It is a convenient entity, to be eliminated in any particular case between the Newtonian axioms and the relevant experimental law. Then as further research indicates discrepancies in any field, it is the experimental law which is modified, rather than the axioms, because modifying the axioms would necessitate changing every other experimental law based on them. Nevertheless, as with Euclidean geometry, the axioms have survived only so far as the laws to which they give rise are in fact manageably simple. Again using Einstein's symbols, it is (G) + (P) which is subject to the control of experience and in which one requires the greatest simplicity and scope, where now (G) represents the Newtonian axioms, and (P) the purport of the physical laws derived in terms of the entities of mass and force defined by the axioms. The analogy is close enough for one to conclude, as in the case of geometry, that even regarded as definitions, the axioms are by no means arbitrary and devoid of empirical content.

Before the second axiom can be used to define force, one must resort to the third axiom to define mass, and the process may be outlined in the following way:

Let two bodies A, B, mutually interact (directly or by means of mechanical, electrical, hydraulic or other devices). Then, from the third axiom

$$F_{AB} = - F_{BA} \quad --- (1)$$

where F_{AB} denotes the force exerted on A by B, F_{BA} the force exerted on B by A, and the minus sign indicates the opposite direction. Then, by substituting from the second axiom, equation (1) becomes

$$\frac{d(M_A V_A)}{dt} = - \frac{d(M_B V_B)}{dt} \quad --- (2)$$

It is now assumed that the mass M of a body is a constant property of it, in particular, that it is independent of the velocity V. (2) may now be rewritten

$$M_A \frac{dV_A}{dt} = - M_B \frac{dV_B}{dt} \quad --- (3)$$

and by writing the acceleration $\frac{dV}{dt} = a$, (3) becomes

$$\frac{M_A}{M_B} = - \frac{a_B}{a_A} \quad --- (4).$$

Then, under these circumstances, it is an empirical fact, that under all variations of temperature, pressure and other conditions, and for velocities much less than that of light, the accelerations a_A, a_B are opposite in direction and constant in ratio irrespective of the velocities of the bodies. Thus the original assumption of the constant mass of A and B is validated by experimental results. The scalar nature of mass is also shown by these results, since it is the ratio of two directed quantities with the same direction and line of action. The next step is to select a unit of mass, say M_B, when we have

$$M_A = - \text{[illegible]} \frac{a_B}{a_A} \quad \text{- - -} \quad (5)$$

and thus the mass of any body A may in principle be determined. (In practice, of course, use is made of applications of the axioms to rigid-body mechanics, and Newton's law of gravitation, to determine masses indirectly by comparing weights, but this is only a matter of convenience.)

Similar experiments with a third body C made with A and B first separately, and then together, establish that mass is an additive, or extensive property. Experiments with the three masses not co-linear establish the parallelogram addition of induced accelerations and hence, by substitution in the second axiom, that force may indeed be represented vectorially. All these are empirical facts, and to the extent that the second and third axioms are regarded as embodying the notions of mass as a constant scalar extensive property of a body, and of force as a vector quantity, they are far from being pure definitions and imply a great deal of factual information.

One may now return to the first axiom in the light of the foregoing development. It may be rewritten to state that, in the absence of interactions with other bodies, a body will not change its velocity in magnitude or direction. This implies that, in the general case, not the velocity, but the acceleration of a body is governed by the effects of other bodies. Thus the differential equations of motion of the body will be at least second-order, and the associated boundary or initial conditions may therefore involve both the position and velocity of the bodies concerned. In Poincaré's words, "The acceleration of a body depends only on its position and on that of neighbouring bodies, and on their velocities."[23] He goes on

23. Henri Poincaré, op. cit., p. 92.

to ask: "Has this generalised law of inertia been verified by experiment, and can it be so verified? ... Newton ... certainly regarded this truth as experimentally acquired and demonstrated. .... It was so proved by the laws of Kepler. According to those laws, in fact, the path of a planet is entirely determined by its initial position and initial velocity; this, indeed, is what our generalised law of inertia requires."[24] Dismissing as incredible the hypothesis that we have merely observed a special case of a higher order system, he says "we may admit that so far as astronomy is concerned our law has been verified by experiment."[25] But he goes on to assert that no future experiment will ever invalidate it. We could never apply the decisive test, he says, of having all the bodies in the universe return "with their initial velocities to their initial positions after a certain time. We ought then to find that they would resume their original paths. But this test is impossible;"[25] and in other fields than astronomy, specifically physics, "if physical phenomena are due to motion, it (sic) is to the motion of molecules which we cannot see."[25] If necessary, then, discrepancies can be explained away in terms of "the position or velocity of other molecules of which we have not so far suspected the existence. The law will be safeguarded."[25] This further reasoning seems to me entirely unconvincing. If we did live in a third or higher-order world, the equations of motion, investigated in different ways, would presumably require different auxiliaries to reduce them to second-order. Molecules have been examined through a wide variety of properties -- mechanical, thermal, electrical, magnetic, chemical -- and in most cases their interactions have been described with perfect consistency by second-order equations of

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24. Henri Poincaré, op. cit., p. 94.
25. Ibid., pp. 95-96.

motion.

Put another way, in all these fields Newtonian forces (i.e. as defined from the second and third axioms) are observed to be functions solely of the positions and velocities of the bodies concerned and not of any higher derivatives. If this is not to be admitted as confirming evidence, why should astronomical data be? If one can have auxiliary molecules, why not auxiliary (but invisible) celestial bodies? This, of course, was precisely what was done last century to explain irregularities in the planetary motions. In the case of the orbit of Uranus, the postulated planet was observed, in that of Mercury it was not, and it is just this empirical observation, among others, which is held to justify the replacement of classical by relativistic mechanics. Similarly, observed discrepancies in the molecular and sub-molecular fields have led to the development of quantum and wave mechanics. In short, the first axiom, as a "generalised law of inertia", is perfectly open to refutation in the physical sciences and is no more an empty convention than is, say, the Law of Conservation of Energy.

To sum up the discussion of the Newtonian axioms of motion, I suggest that they may most reasonably be regarded as a mixture of definition and statements of empirical fact. They presuppose Euclidean geometry, inertial space and an accepted standard of time. I have tried to show that these presuppositions are not purely arbitrary but contain implicit assumptions about the structure of the physical world and so may have to be abandoned as experiment shows these assumptions to be invalid. Then the third axiom may be regarded as defining mass, it being an empirical fact that mass is a positive, scalar, extensive, constant property of a body for velocities small compared with that of light. The second axiom defines force, it being an empirical fact that the defining accelerations, and hence the forces, may be characterised by vectors. Finally, the first axiom may be generalised into a principle of inertia which has been supported by a wide range of experimental evidence. There is, of course, a variety of ways of assigning the definitional functions and factual implications of classical mechanics among the three axioms. "There is no one official formulation of the theory, and in different contents different modes of articulating it may be assumed .... Such shifts in modes of approach are not necessarily signs of confusion. They may illustrate only the flexibility with which definitions and empirical statements can sometimes be interchanged within a highly systematised body of knowledge."[26]

The flaws of Poincaré's conventionalism seem to lie in two suppositions: (1) That if a statement can be construed as a definition, it must be devoid of empirical content. In the natural sciences, however, definitions, to use Mill's phrase, conceal axioms, and carry definite factual implications. (2) That if each of a set of statements relating to an entity or entities can be separately regarded as a definition, the whole set together can be so regarded. That this is fallacious, I have tried to show in the early part of the paper.

"Thus is explained," concludes Poincaré, "how experiment may serve as a basis for the principles of (classical) mechanics, and yet will never invalidate them."[27] Whether he would have maintained this position in the face of later developments in quantum mechanics on the one hand, and relativity

26. Ernest Nagel, <u>op</u>. <u>cit</u>., p. 182.
27. Henri Poincaré, <u>op</u>. <u>cit</u>., p. 105.

on the other,is an interesting but perhaps unprofitable question.

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(2) Ernest Nagel, "The Structure of Science", Harcourt, Brace and World, Inc. New York 1961

(3) Feigl & Brodbeck (Editors), "Readings in the Philosophy of Science", Appleton-Century-Crofts, Inc. New York 1953:

a) P. W. Bridgman, "The Logic of Physics", pp. 34-46.

b) Albert Einstein, "Geometry and Experience", pp. 189-194.

c) Moritz Schlick, "Are Natural Laws Conventions", pp. 181-188.

d) Ernst Mach, "Newton's Views of Space, Time and Motion", pp. 165-170.