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On scientific conduct with respect to the principle of energy conservation

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ABSTRACT

Popper's ideas about scientific theories were normative, and his view in short was that science distinguishes itself from pseudo-science in the way that its theories have to be in principle falsifiable by empirical evidence. However, many of those we consider good scientists hold on to conservation of energy even when it seems to be contradicted by empirical evidence. Is this behaviour really scientific?

What is the epistemological nature of conservation laws? Do they involve any empirical predictions? Should they be granted a special place among the scientific theories? In this paper a proposal will be introduced to grant conservation laws the status of axioms, and some of the related issues will be discussed.

1. INTRODUCTION

Science is generally trusted as a reliable instrument of mankind in the pursuit of truth. The scientific and philosophical society has some general convictions on how science should be conducted; which attitudes are to be considered scientific and which are not; but when giving precise guidelines it is apparent that there is still room for philosophical debate.

One of the issues that are still controversial in the philosophy of science is the principle of the conservation of energy. Many scientists have conducted experiments that on the basis of the existing theoretical framework at first sight seemed to imply that energy was created *ex nihilo*, or that energy disappears. However, this did not lead them to reject the principle of the conservation of energy; they rather felt encouraged to design a new theoretical framework that would account for these accidents leaving the principle of energy conservation intact.

In this paper the question whether this behaviour is scientific or not will be addressed and the proposition that energy conservation is an axiom to modern physicists will be defended. In the following sections respectively the concepts involved will be clarified, the theories of Berkeley, Popper, Duhem-Quine and Poincaré will be discussed outside chronological order, and finally the main arguments will be extracted from the material dealt with.

2. ANALYSIS OF PRINCIPLE OF CONSERVATION OF ENERGY

Before a deeper analysis of the meaning of the principle of conservation of energy can be given, it seems wise to discuss the concept of energy first.

The *Oxford Dictionary of Science* notes the following on energy (2):

> [Energy is a] measure of a system' s ability to do work. Like work itself, it is measured in joules. Energy is conveniently classified into two forms: potential energy is the energy stored in a body or system as a consequence of its position, shape, or state […]; kinetic energy is energy of motion and is usually defined as the work that will be done by the body possessing the energy when it is brought to

> rest. […] The internal energy of a body is the sum of the potential energy and the kinetic energy of its component atoms and molecules.

It is surprising to see that the ability to do work, as distinguished from the work itself, has been since the early days of science a concept that physicists consider quite appropriate in their description of nature. The *Oxford Companion to Philosophy* mentions that before the concept of energy was clearly defined, physicists used a variety of terms, such as 'quantity of motion', or the even more abstract 'vis viva.' (generally translated as 'living force').

In *Energy: Between Physics and Metaphysics*, Mario Bunge observes that first of all, an important characteristic of energy is the fact that it can be transformed from one of its types into another. For instance, potential energy can be transformed into kinetic energy as a ball falls to the ground. Moreover, "[s]uch quantitative conservation is the reason that we regard all the kinds of energy as mutually equivalent" (457). It is worthy to note that in spite of the fact that the types of energy are intrinsically different from one another, the fact that they are considered equivalent commits us to affirming that there is at least a shared quality among them, and for those who dare to go further, that the different kinds of energy are in fact perhaps only different appearances of the very same thing. This observation will be of importance to this discussion later on.

The principle of the conservation of energy has been discussed and considered substantially before it was pinned down in the first law of thermodynamics, generally formulated as follows:

$$dE = dQ + dW$$

In his article *The Nature of Some of Our Physical Concepts-II*, P.W. Bridgman mentions several "paper-and-pencil" properties of this formulation of the conservation of energy, pointing at the *a priori* consequences of its formulation.

Firstly, Bridgman observes that there is no instrumental way to measure the change of internal energy of the system, d*E*. Therefore "the equation as it stands defines d*E*" (25).

Secondly, he mentions, that "writing the law in this form implies in the first place that the universe has been divided into two parts: the 'system', to which the law […] applies, and the rest of the universe, 'external' to the system" (25). This is a very important assumption, because due to the physical nature of d*Q* and d*W* they should in principle both be measurable. Bridgman therefore calls them "fluxes" (29) and they are assumed to be real.

Furthermore, it does not include any predications about the specific energy of a system at any stage. The law only implies under what conditions the energy changes.

It would lie beyond the scope of this small paper to discuss the properties of energy in more detail, although the critical reader might argue that more nuances could be made on this issue.

3. UNOBSERVABLES IN SCIENTIFIC THEORIES

One of the problems that are to be found explicitly at the roots of the conservation of energy is the fact that energy is not directly measurable. George Berkeley, a famous British empiricist philosopher was one of the first to formulate clear principles that, according to his view, should guide scientists in their scientific practice. In his famous work *De Motu* (On Motion), Berkeley pronounces sharp criticism of the recent work done by Newton. The problems about terms such as *force* and *gravitation* he points at can be equally applied to energy. He writes (317):

> The force of gravitation is not to be separated from momentum; but there is no momentum without velocity, […]; again, velocity cannot be understood without motion, and the same holds therefore of the force of gravitation. Then no force makes itself known except through action, and through action it is measured. […] In brief, those terms *dead force* and *gravitation* by the aid of metaphysical abstraction are supposed to mean something different from moving, moved, motion and rest, but, in point of fact, the supposed difference in meaning amounts to nothing at all.

The analogy with energy is apparent from this example. By its definition it is made likely that by kinetic energy something distinct from motion is understood, but which cannot be understood but through that motion, and potential energy can, in our conceptual framework, not be separated from the force of gravitation, which, as Berkeley correctly observes, we know only through the specific motion that was caused by it. Formulated more directly, we cannot actually *see* energy, force or gravitation. Furthermore, the terms depend on existing concepts and therefore only complicate our conceptual framework. This leads Berkeley to assert that (317):

> *[f]orce, gravity, attraction* and terms of this sort are useful for reasonings and reckonings about motion and bodies in motion, but not for understanding the simple nature of motion itself or for indicating so many distinct qualities.

As an historical side mark, Newton himself also considered the question of the nature of gravitation unanswered and simply not addressed by his theory.

We can, from this example, see that Berkeley does not directly reject unobservable entities in physical research, but he does warn us that we should not consider them as making valid metaphysical or ontological implications. For instance, if a theory assumes that there are such things as 'forces,' then according to Berkeley, that does not mean those forces are really there, but rather that they are useful in our reasoning and making calculations. We are not justified to assume that if a theory, assuming the existence of strange entities, correctly describes reality, that that means that those entities truly exist. Therefore, they cannot help us to *understand* the nature of what is there. It is noteworthy that Berkeley considered forces as acting upon bodies, and therefore they do not *per definition* include the substantial character of the bodies that will make it justified to assume that they truly exist.

4. POPPER'S FALSIFICATIONALISM IN INTERPRETATION

In general unobservables are so closely intertwined with our modern physical theories – in quantum mechanics even more explicitly than in classical mechanics –

that they are hardly considered a problem. However, some doubts on this matter were raised with the philosophical theory of Karl Popper.

Popper concentrated his philosophical efforts in the beginning of the 20th century at the problem of the demarcation within science, i.e. the question what distinguishes real science from pseudo-science. Popper felt that theories such as Freud's *psychoanalysis* were intrinsically different from "our best scientific theories", as he calls them, such as Einstein's theory of special gravitation.

Popper claimed that explaining events through the Freudian theory of *psychoanalysis* was essentially insufficient, because there is always a way to be found for the 'subconscious self' and 'repressed desires or sufferings' to play a role. Furthermore, he argued that it was impossible to prove that the theory was incorrect, because Freudian psychologists always found a way to explain that apparent contradictory evidence nevertheless was in line with the psychoanalysis. Einstein's theory of special relativity however, allowed very precise predictions to be deduced from it. He writes in *Science: Conjectures and Refutations* (344):

> [the conclusions] I now reformulate as follows: [...] Confirmations should count only if they are the result of *risky predictions*. [...] Every 'good' scientific theory is a prohibition: if forbids certain things to happen. The more a theory forbids, the better. [...] A theory which is not refutable by any conceivable event is non-scientific. Irrefutability is not a virtue of a theory (as people often think) but a vice.

Popper's philosophy of science is summarised very clearly by Timothy Cleveland, in his article *A Refutation of Pure Conjecture*. According to Cleveland, science in

Popper's sense would start with a theory, which consists in a set of hypotheses, meant to explain a certain phenomena that could not be explained from the existing theoretical framework. From these hypotheses, one can deduce what has to be the case if the hypothesis is true. These facts that can be used to test a theory are called conjectures. For instance:

(hypothesis)	All swans are white.
(conjecture)	The first swan I will see is white.

Now several things should be noted.

(1) Popper observed that the hypothesis (being part of the theory) itself cannot be verified. However, the conjecture can be easily tested.

(2) As for the conjecture, it is in principle both thinkable that it is true, and that it is not true (i.e. seeing a black swan is not a *logical* impossibility)

(3) The fact that the deduced prediction turns out to be correct does not imply that the theory is true. If it turns out to be false, however, we can be sure that the theory is not correct.

(4) The test should be an empirical test; that is to say that it would require us to measure the state of affairs in the material world, instead of merely reflecting on the concepts we use.

Popper argued that in order for a theory to be considered scientific, it should allow conjectures to be deduced from it, in other words, states of affairs in reality should be thinkable in which the theory does not hold.

If a theory withstands a critical test, it is not considered to be correct, but is called corroborated. If a theory withstands many tests, i.e. all its predictions seem to be correct, it can be considered a good theory.

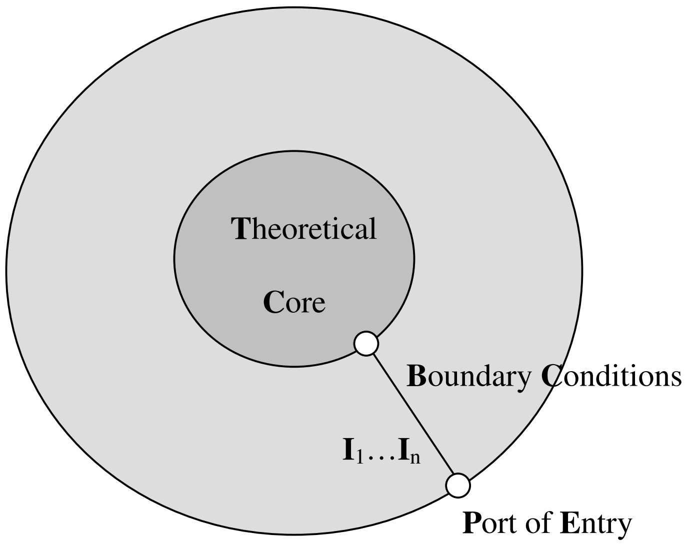
5. THE DUHEM-QUINE PROBLEM

One should be aware of the fact that Popper tried to come up with a criterion that would draw the borderline between that which he considered 'good' science and that which he considered pseudo-science. This is why Duhem and Quine were able to point out important shortcomings in his criterion; they pointed at thinkable instances of scientific conduct that would be considered acceptable in the academic society, but would be rendered pseudo-science by the criterion.

In the time of Galileo it was generally believed that the earth was the centre of the solar system. Imagining that Galileo would offer disbelievers to look through his telescope and 'see for themselves' that they were incorrect would probably not make them change their mind (even though we would probably consider it to be providing evidence refuting the theory that the earth is at the centre of the solar system or at least making it highly improbable). In the spirit of the time, Galileo's telescope was viewed as an instrument of the devil and said to provide a distorted picture of reality. So the question that Popper left unanswered, is: what happens if we simply do not accept a refutation?

In his article *Duhem, Quine and the Multiplicity of Scientific Tests*, Yuri Balashov provides a very clear picture to illustrate the inner working of this problem.

Figure 1: A String Model of Testing



According to Balashov, virtually every scientific theory includes a 'theoretical core' that is utterly inaccessible to empirical testing, i.e., there is no empirical test thinkable that would establish whether what the hypothesis included in the scientific theory asserts to be true is the case. This theoretical core is therefore unfalsifiable. However, only with the help of some auxiliary theories ($I_1..I_n$) a falsifiable conjecture can be deduced from the hypotheses in the theoretical core. The falsifiable conjecture is called the Port of Entry, and with a string of auxiliary theories it is linked with the boundary condition (an element of the theoretical core from which, with the help of auxiliary theories, a prediction can be deduced)

In the example of Galileo's contemporaries, the theoretical core would consist in their ideas about the solar system. A boundary condition would be a certain configuration of the solar system. However, this configuration cannot be directly observed. Therefore, auxiliary theories are required, that would include the theory of optics, which would predict that what we see through the telescope is a reliable reflection of reality.

The core of the Quine-Duhem-problem consists in the fact that, as Balashov mentions, 'the holistic nature of scientific tests makes the rejection of a particular hypothesis, as a result of an adverse experience, essentially inconclusive" (608). The contemporaries of Galileo were not compelled to admit that they were wrong, simply because they did not accept the auxiliary theory of optics, which one has to believe in, in order to accept sightings through the telescope as falsifying evidence.

6. POINCARÉ ON CONVENTIONAL TERMS IN SCIENCE

Exactly this observation can be equally well applied to the conservation of energy, it being an example *par excellence* of a principle that resides in the centre of a theoretical core and is therefore inaccessible for empirical testing. As a result of this, it has been argued that scientists hold on to the principle of energy conservation even though empirical evidence seems to contradict it.

In his article *Energy as the Basic Concept for a Unified Interpretation of Physical Phenomena*, Siluan F. Baldin notes, that 'the individual branches of science became isolated from each other to a considerable degree" (204). Each of these branches subsequently invented its own kind of energy, such as electric, mechanical, gravitational, magnetic and kinetic energy that each correspond to a specific discipline of physics.

The question whether this conduct is really scientific was raised surprisingly long before Duhem and Quine would introduce their problem in the society of philosophy of science. In *La science et l'hypothèse*, a work that was published as early as 1902 (an interesting historical side note is that this was exactly the period in which Emmy Noether did her famous discoveries on the connection between conservation laws and symmetry – this paper will not deal with those, however), the French mathematician, physicist and philosopher of science Henri Poincaré started out with the principle of energy conservation and subsequently argued that all those terms we have not properly defined, should be excluded from the principle. He writes (127-128) (and the reader will have to forgive me preferring the original text over the English translation):

> Il ne nous reste plus qu' un énoncé pour le principe de la conservation de l' énergie ; *il y a quelque chose qui demeure constant*. Sous cette forme, il se trouve à son tour hors des atteintes de l' expérience et se réduit à une sorte de tautologie. Il est clair que si le monde est gouverné par des lois, il y aura des quantités qui demeureront constantes. […] [L]e principe de la conservation de l' énergie, fondé sur l' expérience, ne pourrait plus être infirmé par elle.

In other words, Poincaré felt that the only thing the principle of conservation of energy is in fact saying, is that *there is something that remains constant*.

In conclusion, Poincaré observes (135-136):

> Les principes de la mécanique se présentent donc à nous sous deux aspects différents. [1] D'une part, ce sont des vérités fondées sur l'expérience et vérifiées d'une façon très approchée en ce qui concerne des systèmes presque isolés. [2] D'autre part, ce sont des postulats applicables à l'ensemble de l'univers et regardés comme rigoureusement vrais.
>
> Si ces postulats possèdent une généralité et une certitude qui faisaient défaut aux vérités expérimentales d'où ils sont tirés, c'est qu'ils se réduisent en dernière analyse à une simple convention que nous avons le droit de faire, parce que nous sommes certains d'avance qu'aucune expérience ne viendra la contredire.

Especially interesting in his account is that he was conscious of the fact that there has in fact never been a 'real' test of the principle of energy conservation, but rather only in approximations and almost isolated systems.

In brief, Poincaré argues that what energy is can only be defined in particular cases, but it is impossible to give a general definition of it, which seems quite familiar, bearing in mind what was established in the second section of this paper. The FOLDOC Dictionary of Philosophy observes that Poincaré argued, that "scientific theories are conventional claims best supported by appeal to their simplicity and utility rather than to their truth" (1). Moreover, he thinks we are justified in postulating those conventions, because, as I quoted, we are certain in advance that no experimental outcome can contradict it. In the especial case of energy, Bridgman noted that Poincaré's case was that "energy is most appropriately described as a convention, made to suit our convenience, but with no further significance" (26). The reader will notice this is in line with what Berkeley argued for.

7. ENERGY AS AN AXIOM

With these observations it is appropriate to round of the theoretical discussion of thinkers on the subject.

What we are arriving at is a confirmation of exactly the objections one could point at from the perspective of Popper; the principle of the conservation of energy is not falsifiable. The question is whether the fact that scientists still hold onto the principle of conservation of energy, is essentially to be viewed as dogmatic, or rather that it does not imply any problems for scientific conduct in general.

The proposition that will be briefly defended here is that the role of the principle of energy conservation overall is best understood as an axiom, and is therefore not directly unscientific. Three main arguments for this proposition will be presented, referring back to the material presented earlier in this essay.

First of all, as Bridgman observed, the first law of thermodynamics can be seen as a definition, defining dE on the basis of dW and dQ. The problem about a definition is that it is of course not falsifiable, because it always holds *by definition*. Therefore it does not involve any conjectures in Popper's sense.

Secondly, the concept of energy is far more powerful if it is in fact conserved. If for instance, energy could be created in many instances, then (1) it would not have been as powerful a tool to physics in the first place and (2) it might more easily be defined in terms of a property of matter (to do work). As for the first consequence, if energy can be randomly created and randomly disappear, then it is of no use in scientific description of reality, because the aim of science generally is to predict events, which is not possible if energy would behave randomly. As for the latter, if we were to say that matter possesses the intrinsic quality to do work (to have energy) than introducing the term energy would only lead to a more complex system of definitions. It would then, for instance, be easier to include in our concept of matter the idea that it can do work without requiring external energy flows to come to its aid.

Thirdly, as Poincaré observed later in his work *La science et l'hypothèse*, the propositions of Euclid's geometry can also be considered conventions (136):

> Comparons avec la géométrie. Les propositions fondamentales de la géométrie, comme par exemple le postulatum d' Euclide, ne sont non plus que des conventions, et il est tout aussi déraisonnable de chercher si elles sont vraies ou fausses que de demander si le système métrique est vrai ou faux.

He writes that Euclid's axioms are principles that are generally agreed upon and, within his mathematical framework, are not called into doubt (Poincaré asserts in the citation the even stronger claim that it is generally unreasonable to call them into doubt). The fact that Euclid's definitions are conventions, points at the similarities with the principles of energy conservation.

A more detailed discussion of this topic would of course be desirable, but would go beyond the scope of this paper.

8. CONCLUSION

In conclusion, firstly the principle of energy conservation cannot be directly empirically tested. In order to subject it to a critical test auxiliary theories are needed, which can be, and in the history of science have been, considered refuted in stead of the principle of energy conservation. The issue points exactly at the crucial problem about Karl Popper's philosophy of science, as Quine and Duhem formulated it – or in fact one could argue that Poincaré implicitly said the same as early as in the beginning of the 20^{th} century.

Secondly, it can be established that the principle of energy conservation is viewed as an axiom in many scientific circles.

The principle of the conservation of energy has proven useful in the past. Many thermodynamical achievements would not have been possible if physicists did not feel justified in positing the first law of thermodynamics, even though Poincaré argued that there has been no real test on the basis of which one could attain more than just intuitive confirmation for it.

In analogy with Euclid's axiom's, the fact that the principle of energy conservation can be considered an axiom of modern science shows that it is descriptive of the way we approach the subject matter; which, in the case of physics, is nature.

Perhaps in the end, as later philosophers would argue for, science needs to have some assumptions (such as the assumption that induction is justified) that cannot be easily justified in order to perform its functions.

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