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Conventions and the Order of Nature: Some Historical Perspectives

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Conventions and the Order of Nature: Some Historical Perspectives

HELGE KRAGH*

1. Introduction

According to classical conventionalist philosophy of science, scientific theories are not and never can be be true or false representations of nature. Historically associated with the French early-twentieth century scientists Pierre Duhem and Henri Poincaré, conventionalism holds that scientific laws and theories, far from being inferences based on experiment, are disguised definitions and conventions chosen by the scientists. They are merely the simplest possible and most economical ways of accounting for certain natural phenomena or classes of phenomena [Newton 1997, pp. 11-22].

One does not have to subscribe to either conventionalism or some strong form of constructivism in order to recognize that science crucially involves concepts and frameworks that are not given by nature herself, but are human constructs. As such, they could be different from what they are, for they are essentially chosen by the scientists for pragmatic reasons. It is sometimes said that one should not put labels on natural phenomena, since the labels are concepts

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invented by us and not part of nature herself. True, but such linguistically formulated labels, say in the form of classifications and conventions, are not only unavoidable, they are also epistemically effective and of the greatest importance in real science. They tell us something about nature.

The purpose of this essay is not to contribute to the extensive literature on conventionalism in science [Ben-Menahem 2006], but merely to discuss in an exemplary and not very systematic manner the role that prescriptions of a conventionalist kind may play in the sciences. The examples that I use are taken from the history of the physical sciences, chemistry, physics and astronomy. They are in part discussed with an eye on the educational aspects of science, in particular in relation to the ″nature of science″ (NOS) issue in science teaching [Comas 1998; Flick & Lederman 2006]. To put it briefly, I want to argue that NOS discussions must necessarily include the conventionalist aspects of science and a clear recognition of the difference between conventionalist and non-conventionalist scientific statements.

2. What is a chemical element?

While the formula H_2O for water is a conventionally chosen symbol, it is not completely conventional. It is an empirical fact of nature that when water is decomposed (thermally or electrolytically) the result will always be molecular hydrogen and oxygen (H_2 and O_2) in the weight ratio 1:8 or the volume ratio 2:1. The formula H_2O is a shorthand symbol for this experiental knowledge, which is independent of any convention (except the trivial convention that the words ″water,″ ″hydrogen″ and ″oxygen″ have their usual meanings). By contrast, consider the statement:

> The frequency (*f*) and period (*T*) of an harmonically oscillating system are inversely proportional, that is, $f = 1/T$.

This is obviously a statement devoid of empirical information since it is true semantically, because of the very definitions of the terms. While no experiment can ever disprove the statement, experiments could disprove the formula for water. The information embodied in this formula does not rely in any non-trivial way on the meaning of the terms ″water,″ ″hydrogen″ and ″oxygen.″

So, chemists consider it a fact of nature that water is H_2O.† Likewise, it is fact that the element hydrogen has atomic number $Z = 1$ in the periodic system, and that oxygen has $Z = 8$. However, this has not always been the case and it has factual status only because we have chosen a particular parameter, the atomic number, as the *defining* property of an element. Mendeleev's periodic system of 1869 was, like other systems from the early period, based on the established connection between elements and their atomic weights according to which the measurable atomic weight was the defining property of an element.

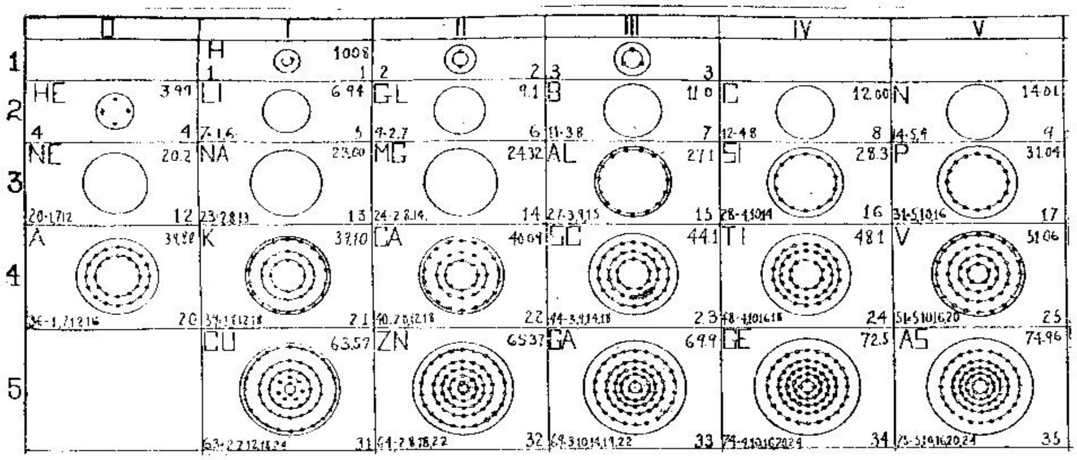
Advancements in the years between 1911 and 1914 – the discovery of isotopy, the Bohr-Rutherford nuclear atom, and Moseley's method of determining the nuclear charge by means of X-ray spectroscopy – led to the proposal of a new definition of what an element ″really″ is. The new proposal, that the nuclear charge (equal to the atomic number) should be the defining property, was controversial and adopted only after many years of discussion: In 1921 the

† Of course, from a philosophical point of view matters are more complicated (they always are). Philosophers have debated the composition of water at great length, and few accept H_2O to be simply a "fact of nature." See, for example, [Van Brakel 2000].

Deutsche Atomgewichtkommission decided to base the periodic system on the atomic number, and two years later the same convention was adopted by the International Commission on Chemical Elements [Paneth 1962; Kragh 2000; Holden 2004].

The replacement of the atomic weight number A with the atomic number Z was clearly a decision based on a conventional choice. Nature does not tell us what an element is, we have to decide that ourselves. The discussions in the chemical community could have led to a result different from the one that actually occurred. The chemists might conceivably have agreed to keep to the old definition based on the atomic weight, or they might have chosen some third alternative. Although they were not forced to change to the atomic number, there were good reasons of both a rational and a pragmatic kind to do so. Of course, the new definition is the one still accepted today – but this only speaks to its robustness. It does not make it any more ”true” than the older one.

Even with the recognition that the periodic system should be based on an ordinal number different from the atomic weight, there were other possibilities than the standard atomic number associated with the atomic nucleus. In some versions of the periodic system from 1913-1916 one can find oxygen assigned ordinal number 10 rather than 8. A few physicists and chemists believed that there existed two unknown elements between hydrogen and helium, implying that helium and the heavier elements would have ordinal numbers two units greater than the ordinary atomic numbers. With the recognition of the Bohr-Rutherford model of the atom ideas of this kind were abandoned, and today they are regarded as just curious mistakes.



Part of a periodic system and electron configurations of the elements devised by the American physicist Albert Crehore in 1915. Notice the two unnamed elements with ordinal numbers 2 and 3 between hydrogen and helium. Source: A. Crehore, "The gyroscopic theory of atoms and molecules," *Philosophical Magazine* 29 (1915), 310-322.

Atomic weights did not become unimportant with the redefinition of a chemical element in the early 1920s. Ever since John Dalton introduced the concept of atomic weight more than 200 years ago, it has been discussed in which *units* the weight of an element (or an isotopic component) should be given (see [Holden 2004] and [De Bièvre and Peiser 1992] for historical reviews). While the large majority of chemists in the nineteenth century followed Dalton in using the H = 1 scale, at the end of the century Wilhelm Ostwald, Bohuslav Brauner and others proposed as an alternative the O = 16 scale, which after some period of dispute was generally adopted. The later recognition that naturally occurring oxygen is a mixture of three isotopes (^{17}O and ^{18}O in addition to the dominating ^{16}O) led in the 1930s to the uncomfortable situation that chemists used the O = 16 scale while physicists preferred the ^{16}O = 16 convention. The latest major revision came in 1961, when the General Assembly of IUPAC, the International Union of Pure and Applied Chemistry, decided to adopt the unit based on carbon-12 (^{12}C = 12), which is still the official scale among both chemists and physicists.

Changes in units such as those which have occurred in the case of atomic weights are common and in most cases undramatic. They are of a purely conventional character and largely made for practical, administrative and technical reasons. The historical changes from the H = 1 scale to the ^{12}C = 12 scale tell us nothing about nature, but they do tell us something about the nature and development of science.

It is worth pointing out that conventions, whether they belong to science or not, are of a consensual nature and for this reason include a social element. Conventions have practical significance only if they are accepted by a large part of the relevant community, if not necessarily instantly and by the whole community. In the long run it is intolerable or at least highly impractical if a convention is accepted only by a small part of the scientific community, while other parts of the community adopt other conventions. Important conventions have to be agreed upon by the international scientific organizations and to be implemented into textbooks, manuals and research articles.

It can be difficult to achieve the desired consensus in cases where the conventions transcend a single scientific discipline or carry with them political, national or other external significance. This is exemplified by the case of the redefinition of the chemical elements, which faced opposition for both reasons. Many chemists found it intolerable that the concept of an element, the very foundation of the chemical sciences, should be based on a physical parameter that could be measured only by physical methods [Kragh 2000]. Professional rivalry between chemists and physicists was a major reason, and national rivalry a minor reason, why the new definition was officially adopted only a decade after it had been proposed.

3. Empirical and conventional statements

As we have seen, some statements about nature are of a conventional character, essentially decisions made by the scientists and their profesional organizations; other statements have an empirical character, in the sense that they are true or false because nature is how it is. While the latter kind of statements can be tested by means of experiments and observations, the first kind cannot. It is of the uttermost importance, not least in educational contexts, to distinguish between the two kinds of statements and recognize the entirely different epistemological status they have. And it is no less important to recognize that the distinction is not always easy or unproblematic. Here is a somewhat arbitrary collection of examples:

1. One day equals 24 hours?
2. All organic chemical compounds are carbon compounds?
3. In classical mechanics, the kinetic energy is $E_{kin} = ½mv^2$?
4. Spiders are not insects?
5. Carbon-12 has atomic weight 12.000 000?
6. Pluto is a planet?
7. Water consists of the elements hydrogen and oxygen?
8. The speed of light in vacuum is c = 299 792 458 m/s?
9. The (big bang) universe has always existed?
10. Force equals the product of mass and acceleration ($F = ma$)?

All of the statements, which I have here formulated as questions, relate to natural objects or phenomena and therefore, apparently, belong to the domain of science. What matters is not so much whether the statements are true or not, but rather the reasons why they are either true or false (or perhaps can be assigned no truth value). While some of the questions can be answered unequivocally, others are more ambiguous and do not clearly belong to either the conventional or empirical group.

Spiders are not insects (#4), simply because they do not fit our definition of an insect, which, among other characteristics, requires that an adult insect must have six legs (and spiders have eight). The scary creatures look in many ways like insects, but as long as we keep to our definition, they are not part of this class of animals. It makes no sense to study spiders in great scientific detail in order to find out whether they *really* are insects or not, just as little as it makes sense to invest in precise experiments with the aim of discovering whether carbon-12 *really* has atomic weight 12. Should it turn out that high-precision mass determinations result in, say, an atomic weight equal to 12.008 ± 0.001, all we can do is to put the blame for the discrepancy on the experiment. Should we discover a spider with eight legs, then … God knows what.

As already indicated, scientific matters depending on conventions change through history because conventions are negotiable and can be renegotiated. As scientists grow more knowledgeable, they will have a need to change their conventions and definitions, such as illustrated by the case of the chemical elements and the atomic weight units. I shall return to this issue, but first want to offer some brief comments on the questions #3 and #9. According to Newtonian mechanics the kinetic energy of a moving body is indeed ½mv^2, but what is the status of this insight? Did physicists in the past decide that they needed a quantity defined in just this way? Or did they measure the kinetic energy of bodies with varying mass and velocity to discover the relationship?

The question is historically complicated because it relates to a period in which the general concept of energy had not yet been established. During the eighteenth century there raged a major controversy in which natural philosophers debated whether or not the Leibnizian concept of *vis viva* (mv^2) was a better representation of the "force" of a body in motion than the Cartesian-based concept of "quantity of motion" (corresponding to mv). For the present purpose we do not have to consider the details of this interesting debate [Smith 2006], but can adopt a modern perspective based on our knowledge of energy. If we want to associate an energy with motion, dimensional reasons imply mv^2 and reasons of simplicity may favour just this quantity. Yet the kinetic energy is not mv^2, but ½mv^2. Where does the innocent factor ½ come from? The factor may be justified by reference to the general principle of conservation of mechanical energy (potential and kinetic), which straightforwardly leads to the expression $E_{\mathrm{kin}} = \frac{1}{2}mv^2$.

And now to the very different #9. According to current standard big bang cosmology the age of the universe is close to 13.4 billion years, the uncertainty being only 0.2 billion years. Assuming that this theory is correct, apparently it follows that the universe has not always existed, an insight which ultimately is based on astronomical observations coupled with advanced physical theory. We would then say that the statement is empirically wrong and not of a conventional or semantic nature.

But not so fast! Semantics is in fact part of the statement, and crucially so. It can be argued that the meaning of the temporal term "always" guarantees that the universe has always existed and that the statement thus is semantically true, irrespective of whether there was a big bang or not. To say that the universe has always existed is not to say that it has existed in an eternity of time, but that is has existed as long as there was time. The negated statement, "the universe has not always existed," invites us to consider the decidedly strange idea of a past time in which there was no universe and therefore nothing at all. According to the generally held conceptions of the terms "time" and "universe," this is an impossible idea. We would consequently have to conclude that the universe, although not eternal, has always existed, but that this conclusion is based solely in conceptual and semantic reasons. It is a conclusion that tells us absolutely nothing about the physical universe and the way it has developed through cosmic history (see also Section 9). It may be added that the mentioned considerations presuppose the traditional meaning of "universe." If there are many universes, for example following one another in a cyclical manner or just one universe preceding the current one, the answer might be different.

4. The concept of force

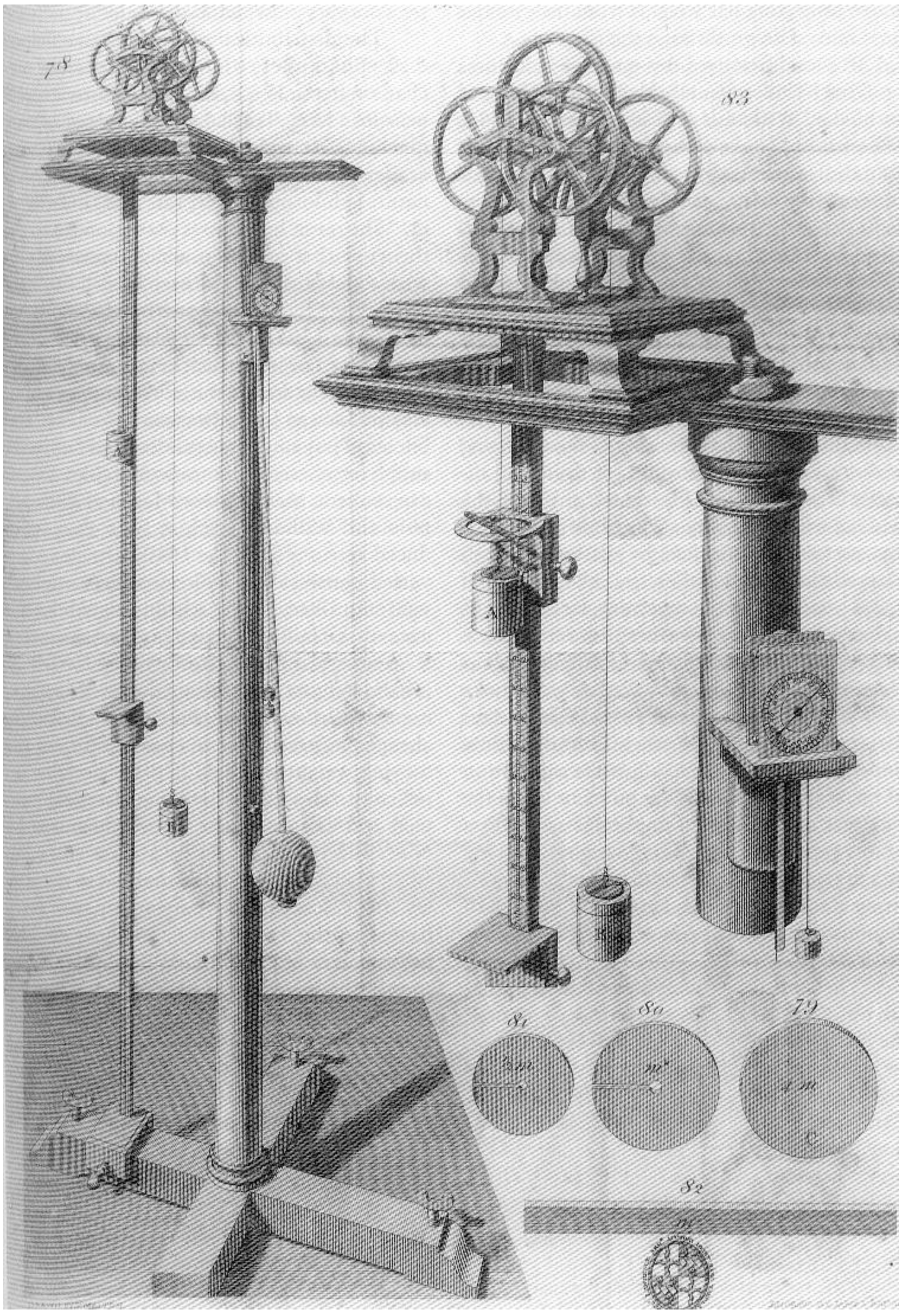
Within the framework of classical mechanics Newton's second law of motion holds true: A force impressed on a body produces an acceleration inversely proportional to the mass of the body. The law is typically written as

$$F = ma = \frac{dp}{dt}$$

where p is the momentum mv. Newton's second law can be understood in widely different ways [Hanson 1965, pp. 99-105; Jammer 1962, pp. 200-240]. Thus, it may be thought of as a formula that summarizes a large body of experience of mechanical phenomena; it may be conceived as a definition of a useful quantity called "force;" or it may be taken to be a technique for measuring force, or acceleration, or mass. And there are other possibilities.

The question of the epistemic status of the second law of motion is far from new, for it has been discussed ever since Newton's own days. Given that the law occupies a central position in introductory textbooks in physics, its status is not only of philosophical interest but also of obvious didactic importance. Textbooks in physics sometimes introduce Newton's second law as an empirical law, one which could be wrong and can be tested experimentally, while other books stress that it is a convention or definition and therefore incapable of disproof. To quote a once widely used university textbook, the statement "is more a definition than a law" [Alonso and Finn 1968, p. 459]. Although Newtonian mechanics has long ago been replaced by quantum mechanics and the theory of relativity, the question of the status of the second law continues to attract scientific attention. Contemporary attempts to test the accuracy of the law is associated with ideas of formulating a so-called modified Newtonian dynamics (MOND) as an alternative explanation of dark matter observations [Hacyan 2009]. If Newton's law is just a definition, it makes no sense to test its accuracy.

The French mathematician, physicist and philosopher Henri Poincaré adopted a conventionalist view of mathematics as well as physics. According to him, the general principles of science were nothing but abbreviated economic



The apparatus designed by the Cambridge mathematician George Atwood in the 1770s was designed to demonstrate and verify Newton's laws of motion under constant forces. While Atwood's machine was often used to *test* Newton's second law, others assumed the truth of the law and considered the machine as merely a heuristic help in understanding it. Illustration from Atwood's description of 1784.

descriptions of observed facts; they were free creations of the human mind and as such they said nothing about those facts. In particular, they did not explain them. For example, in the late nineteenth century the question of curved space, that is, whether physical space might follow a geometry different from the one of Euclid, began to be discussed among a few philosophers and astronomers. Poincaré argued that it was pointless to investigate by means of astronomical observations whether space was really Euclidean or not. What mattered, he said, was only which geometry described space in the simplest and most economical way, and judged by this standard he saw no reason to abandon the traditional flat space.

When it came to the laws of mechanics, Poincaré's attitude was essentially the same. As he pointed out, the Newtonian force can be measured only by the acceleration it impresses on a body with known mass. For this reason he tended to conceive the law as a definition of force and the mass as just a coefficient introduced for reasons of calculations. In his classic work *Science and Hypothesis* he wrote as follows [Poincaré 1952, pp. 104-105]:

> The principles of mechanics appeared to us first as experimental truths, but we have been compelled to use them as definitions. It is *by definition* that force is equal to the product of the mass and the acceleration; this is a principle which is henceforth beyond the reach of any future experiment. … But then it will be said, these unverifiable principles are absolutely devoid of any significance. They cannot be disproved by experiment, but we can learn from them nothing of any use to us.

Yet, Poincaré did not consider Newton's laws to be useless, and he warned against those who "have asked themselves if the savant is not the dupe of his own definitions and if the world he thinks he discovers is not simply created by his own caprice" (p. xviii).

Newton's laws of mechanics, Poincaré pointed out, are idealizations and it does not follow from their conventional status that they are in fact approximately valid in the real world. He summarized his version of conventionalism as follows: "Thus is explained how experiment may serve as a basis for the principles of mechanics, and yet will never invalidate them." As we know today, not only can Newton's laws of mechanics be invalidated experimentally, they have been invalidated. In this respect Poincaré was wrong, although his general conventionalist philosophy of science is not simply disproved for this reason. In any case, the important message is that because conventional concepts and principles are part of the structure of theoretical physics, it does not follow that physics has been reduced to a system of conventions.

5. What is an acid?

It has been known since the era of alchemy that certain subtances share some common properties, such as an acidic taste and reactivity with limestone and many metals. Sulfuric acid and acetic acid both belong to the group of acids, but it is far from obvious what the two liquids have in common except on the phenomenal level. What is the cause of the common properties?

Lavoisier proposed about 1780 that oxygen must be present in all acids and responsible for their characteristic properties (it is no coincidence that the word *oxy-gen*, derived from Greek, means "the acidic principle" or "the root of acidity").

Lavoisier's favoured definition was short-lived and had to be abandoned when it turned out that, for example, HCl, HCN and H_2S do not contain oxygen. Sure, one could have kept to the definition, but only at a price, including that hydrochloric acid would be denied status as an acid. The chemists found this to be too high a price, and in 1838 Justus von Liebig in Germany suggested a new definition: an acid is a substance that contains hydrogen in such a form that it can be substituted with a metal (e.g., $2HCl + Zn \rightarrow ZnCl_2 + H_2$). Later in the century, after the acceptance of Svante Arrhenius' electrolytic theory of dissociation, an acid was understood as a substance that in aqueous solutions can split off its hydrogen in the form of H^+ ions. In agreement with this definition, in 1909 the Danish chemist S. P. L. Sørensen introduced the pH measure for the acidity of a solution, defined as $pH = - \log [H^+]$, where $[H^+]$ – or $[H_3O^+]$ in a later notation – denotes the molar concentration of the ions [Szabadvary 1964].

A more general definition of acids was proposed in 1923, independently by Johannes N. Brønsted in Denmark and Thomas M. Lowry in England according to whom an acid is a proton donor and a base a proton acceptor [Kauffman 1988]. This definition has certain advantages, in particular that it does not presuppose water as a solvent. On the other hand, if the definition is taken literally is becomes unwieldy because it then implies that nearly all hydrogen compounds must be classified as acids. It is not helpful to understand water, ammonia and methane (H_2O, NH_3, and CH_4, respectively) as acids, yet they are so according to the Brønsted-Lowry definition. They can be ascribed experimentally determined acid strengths (K_a), although these are very small. Correspondingly, their pK_a values, defined as $pK_a = - \log [K_a]$, are very large.

As will be understood, the understanding or definition of the nature of acids rests basically on pragmatic reasons. Quantities such as K_a and pK_a are useful, but they do not reflect essential features in the molecular world. The same is the case with the pH concept, which so obviously is nothing but a convential and practical measure of the acidity of an acid. The equation pH = – log $[H^+]$ is of great practical use in experiments, but it is itself beyond the reach of experiment because pH cannot be measured independently of $[H^+]$. All the same, pH is more than *just* a convention and it is certainly not an an arbitrary convention. It is an eminently useful measure of acidity which makes it possible to recognize chemical relationships that otherwise would remain concealed. In general, conventions are associated with epistemic potentials and in this respect one convention may be recognized to be epistemically stronger than an alternative convention, although it is neither more nor less true.

In the early years of the twentieth century it was not common to distinguish sharply between weak and strong electrolytes. In accordance with Arrhenius' theory it was generally believed that strong electrolytes such as NaCl, HNO_3 and HCl were only partially dissociated in aqueous solutions, only with a higher degree of dissociation than H_2S and other weak electrolytes [Wolfenden 1972; Laidler 1993, pp. 209-216]. Let there be *N* particles of a binary electrolyte AB in a solution. According to Arrhenius' basic assumption, the dissociated ionic components would enter an equilibrium state together with the undissociated molecules, $AB \leftrightarrow A^+ + B^-$. Denoting the degree of dissociation α, the result will be a total number of particles

$$N(1 - \alpha) + 2N\alpha = N(1 + \alpha), \quad \text{where} \quad 0 \leq \alpha \leq 1$$

For example, based on measurements of freezing-point depressions and electrical conductivity Arrhenius concluded in 1887 that a strongly diluted solution of one mole of HCl would give rise to 1.93 moles of ions (H^+ and Cl^-) rather than 2 moles. Likewise, in his early pH experiments from about 1910 Sørensen found in a 0.100 M HCl solution the value pH = 1.04. The results of Arrhenius and Sørensen were seen as reasonable at the time, when it was generally accepted that $\alpha < 1$ for all electrolytes.

Today any beginning student of chemistry will recognize that Sørensen's value cannot be true, for the strong electrolyte HCl is – by its definition as a strong electrolyte – completely dissociated in a dilute acqueous solution. This implies that we know in advance that in a 0.1 M solution, $[H^+] = 0.1$ and consequently pH = 1. We do not have to measure pH in order to know the result. For a chemist about 1915 the question was empirical, one that could only be answered by experiment. By the mid-1920s it was recognized that strong electrolytes are in fact completely dissociated, contrary to Arrhenius' view, which resulted in a corresponding *definition* of strong electrolytes. Notice that the changed definition of what constitutes a strong acid was not purely conventional but rather necessitated by measurements that proved strong electrolytes to be completely ionized. Arrhenius' understanding of strong electrolytes was contradicted by experiment and no conventional strategy could save it.

6. The rise and fall of Pluto

In 1930 the American astronomer Clyde Tombaugh detected an insignificant celestial body which he identified as a trans-Neptunian planet and named Pluto.

Very little was known of the new planet, except that it was small and very far away. It was eventually understood that although Pluto revolves periodically around the Sun, and in this respect qualifies as a planet, it is a most unusual one. Not only is its mass curiously small, even less than the mass of the Moon (the ratio $M_{moon} : M_{pluto}$ is about 5), it also moves in a highly eccentric orbit with a considerable inclination relative to the orbits of the other planets. Its orbital inclination is 17.15° and its eccentricity is 0.25, both values much larger than for the other planets. How small can a planet be and still be a respectable planet? In the 1990s astronomers found a number of cometary objects in the so-called Kuiper belt which were about as heavy as Pluto and moved in similarly eccentric and inclined orbits. Suddenly Pluto's status as a planet became problematic: if Pluto counted as a planet, should not the new objects count as planets too? The astronomers were not happy about the prospect of adding a hundred or more planets to the nine already accepted.

Nature does not worry whether the spider is an insect or not, whether methane is an acid or not, or whether Pluto is a planet or not. Such worries she graciously leaves to the scientists, in the latter case to the astronomers. The issue came up for a final discussion at the 26th General Assembly of the International Astronomical Union (IAU) during its meeting in Prague in 2006, where it was decided by a majority vote to declassify Pluto from a genuine planet to a so-called dwarf planet [IAU; Weintraub 2007]. Among the reasons for the decision was that Pluto during its journey in space crosses the orbit of Neptune, something a respectable planet is not supposed to do. Thus, the vote restored the number of planets to the one before Tombaugh's discovery. It is worth contemplating what it was Tombaugh discovered back in 1930. He discovered a celestial object, but would one say today that he discovered a ninth planet? If so, was his discovery "de-discovered" by the IAU General Assembly in 2006?

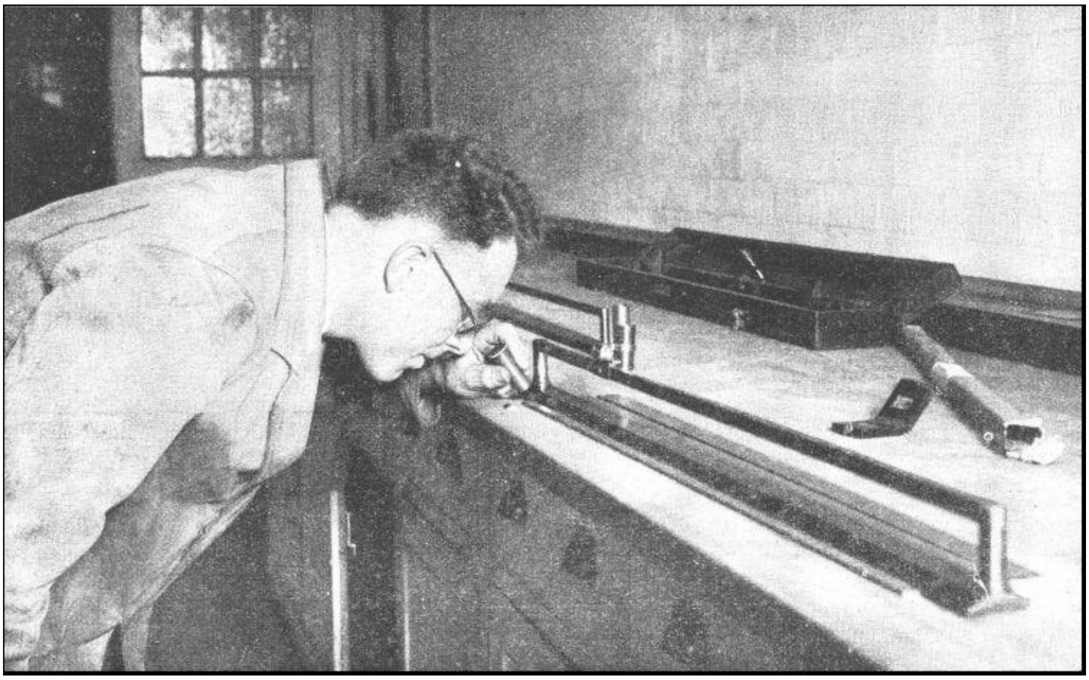
Incidentally, there are other cases in the history of astronomy when the number of planets has changed as a result of theoretical reinterpretation. The most famous one is related to the Copernican Revolution, when the number of planets were reduced from seven to six – solely the result of placing the Sun rather than the Earth in the centre of the universe. The smaller number made Rheticus, Copernicus' one and only pupil, rejoice. For, as he wrote in his work *Narratio Prima* of 1541, "What is more agreeable to God's handiwork than this first and most perfect world should be summed up in the first and most perfect number?" (Rosen 1959, p. 147).

At a first blush it may appear odd, even miraculous, that the astronomers have such power that they can turn a planet into a non-planet. Of course, the astronomers gathered in Prague in 2006 held no power over the universe at all and they could not cause even the slightest change in Pluto's course over the heaven. Happily unaware of the sinister decision made in Prague, Pluto just moved along as it had done for countless ages. As it was the case with the chemists' redefinition of the elements, so was the new definition of planets made for pragmatic and consistency reasons. Should astronomers one day decide to restore Pluto as a planet, they are free to do so.

7. The speed of light

The radius of the Earth is approximately 6366 km. It could presumably have had a very different radius, and yet the figure is in no way accidental, for in a certain sense we have chosen it. The circumference of the Earth $2\pi R$ is a quantity measured geodesically, but given our historically derived convention of the unit length it cannot be much different from what corresponds to R = 6366 km. This results from the metric convention proposed by the French Academy of Sciences in 1791 and adopted by the National Assembly in Paris the same year. According to this convention one length unit (a metre) should be one ten-millionth of the length of the Earth's meridian along a quadrant through Paris [Alder 2004]. In other words, a full meridian was *defined* to have the length 40 000 km, from which follows (assuming a spherical Earth) a radius of R = 40 000/2π km = 6366 km.

The new universal length unit was later transformed into a prototype metre bar made by platinum and since 1889 by a platinum-iridium alloy. This kind of material standard was used until 1960, when the Conférence Générale des Poids et



Measuring a length unit. In this case it is not a metre bar, but the British equivalent, the Imperial Standard Yard established by the Parliament in 1855.

Mésures (CGPM) redefined the metre to be 1 650 763.73 wavelengths of an orange emission line in the spectrum of the krypton isotope Kr-86. Suffice to say that there were good technical reasons for this apparently awkward and definitely unpedagogical definition. Still later, in 1983, the CGPM decided to replace the spectroscopic definition with the current one, which is directly based on the velocity of light in vacuum:

> The metre is the length of the path travelled by light in vacuum during a time interval of 1/299 792 458 of a second.

The number 299 792 458 is the speed of light in vacuum *c* given in the unit m/sec. The various redefinitions of the metre, from 1791 to 1983, are chosen for technical reasons (precision, reliability, reproducibility, etc.) and of course with due consideration to a continuity between the definitions. The old metres differ from the present one, but for most purposes the difference is insignificant [Layer 2008].

It is no accident that the speed of light enters directly in the definition of the metre, for *c* is recognized to be an absolutely fundamental constant of nature. This has not always been the case. When Ole Rømer determined the finite speed of light in 1676 it was merely one physical parameter among others, in principle not different from the speed of sound. It soon turned out that light propagates with the same speed throughout the universe, such as became clear with James Bradley's discovery of the aberration of the fixed stars in 1728. More than a century later Maxwell realized that *c* could be expressed by vacuum constants appearing in his theory of electromagnetism. Finally, with Einstein's special theory of relativity from 1905 the velocity of light became elevated to a truly fundamental constant of nature, a quantity that did not even depend on the motion of the light source relative to the detector.

Remarkably, the numerical value of c is no longer determined experimentally, but rigidly fixed by definition:

$$c \equiv 299\,792.458 \text{ km/sec}$$

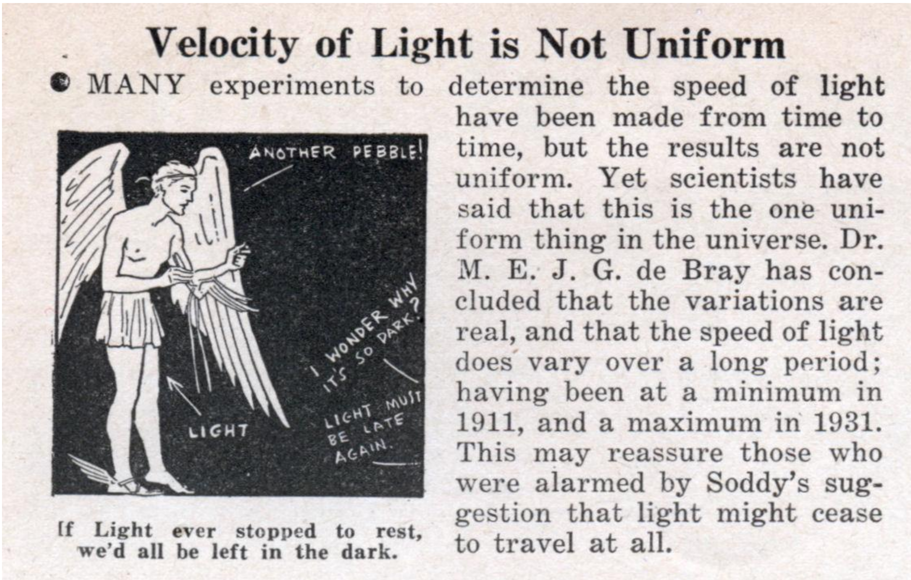
No more and no less. The conventional nature of the quantity is underlined by its lack of uncertainty. While previous values of c were established experimentally, and therefore given with an experimental uncertainty, this is no longer the case. Here are some earlier determinations of the constant:

1882	$c = 299\,860$	$\pm$ 30	km/sec
1932	$c = 299\,774$	$\pm$ 10	km/sec
1958	$c = 299\,792.5$	$\pm$ 0.4	km/sec
1967	$c = 299\,792.56$	$\pm$ 0.11	km/sec

As expected, the experimental uncertainty has diminished over time, but of course it remains finite until the 1983 redefinition. The current "uncertainty" $\Delta c = 0$ is not an expression of a superbly precise experiment, but of a convention.

The reason why physicists can decide the numerical value of c is that the quantity is a dimensional constant. Had it been a pure number, or varying in space and time, they would not have had this freedom. In principle it is no longer of interest, indeed not even meaningful, to measure the speed of light; it is no more meaningful than it was to measure the length of the standard metre bar in

1900. Notice how some physical quantities can change their status from empirical to conventional, or from conventional to empirical. While the length of the standard in Paris was fixed conventionally until 1960, after that year it became an empirical quantity which could meaningfully be measured.



Velocity of Light is Not Uniform

● MANY experiments to determine the speed of light have been made from time to time, but the results are not uniform. Yet scientists have said that this is the one uniform thing in the universe. Dr. M. E. J. G. de Bray has concluded that the variations are real, and that the speed of light does vary over a long period; having been at a minimum in 1911, and a maximum in 1931. This may reassure those who were alarmed by Soddy's suggestion that light might cease to travel at all.

If Light ever stopped to rest, we'd all be left in the dark.

Given that the value of the speed of light is fixed by definition, it would seem that it cannot possibly vary in time. This is not so much because it is a constant of nature, but because it is defined to have a definite value. Nonetheless, there are modern cosmological theories that operate with a varying speed of light (VSL) in the cosmic past [Kragh 2006]. These cosmologies of the VSL type are controversial, for other reasons because of the problematic basic assumption $c = c(t)$. Some critics have argued that since c is just a conventional conversion factor between length and time, it is illegitimate to postulate that it varies in time [Ellis & Uzan 2005]. As one critic has phrased the objection [Duff 2004, p. 3]:

> It is operationally meaningless and confusing to talk about time variation of arbitrary unit-dependent constants whose only role is to act as conversion factors. For example, aside from saying that *c* is finite, the statement $c = 3 \times 10^8$ m/s has no more content than saying how we convert from one human construct (the meter) to another (the second). Asking whether *c* has varied over cosmic history … is like asking whether the number of liters to the gallon has varied.

In spite of this and other objections, work on VSL cosmologies continues.

8. Constants of nature

The speed of light is a fundamental constant of nature in the sense that it cannot be reduced to other quantities, such as can less fundamental constants. For example, the acceleration of gravity at the surface of the Earth, $g \cong 9.8$ m/s^2, can be expressed by Newton's constant *G* and values for the mass *M* and radius *R* of the Earth: $g = GM/R^2$. There is only a handful or two of the truly fundamental constants [Uzan & Lehoucq 2005; Kragh 2011, pp. 168-175]. Apart from *c*, the most important of the constants are

Mass of the electron	$m = (9.109\ 382\ 15 \pm 0.000\ 000\ 45) \times 10^{-31}$ kg
Mass of the proton	$M = (1.672\ 621\ 777 \pm 0.000\ 000\ 074) \times 10^{-31}$ kg
Elementary charge	$e = (1.602\ 176\ 487 \pm 0.000\ 000\ 040) \times 10^{-19}$ C
Gravitational constant	$G = (6.674\ 28 \pm 0.000\ 67) \times 10^{-11}$ m^3/kg/s^2
Planck's constant	$h = (6.626\ 068\ 96 \pm 0.000\ 000\ 33) \times 10^{-34}$ J s

The numerical values and their standard uncertainties are those given by the National Institute of Standards and technology [NIST]. The constants are empirically determined and consequently their values include experimental uncertainties. In this respect, *c* stands out as a remarkable exception. The difference may be illustrated by a thought experiment in which the values of the constants are suddenly reduced by a factor of, say, one million. We can by fiat change $c = 3 \times 10^8$ m/s to $c = 300$ m/s without the change having any observable effects at all. We simply would not notice the change. If, on the other hand, the elementary charge suddenly changed from $e = 1.6 \times 10^{-19}$ C to $e = 1.6 \times 10^{-25}$ C (and the other constants remained the same), it would have most drastic consequences, including the immediate destruction of all life in the universe.

Whereas the constants of nature are not themselves conventional, their numerical values obviously are. These values are expressed in a system of units which is conventionally chosen and reflects the social and cultural circumstances at the time of the adoption of the system. An advanced civilization on some distant planet would presumably recognize the significance of the same constants of nature, but it would not provide them with the same numerical values since it would adopt a system of units suited to the particular circumstances of this civilization. However, things are different when it come to the class of *dimensionless constants*, that is, combinations of constants of nature which are pure numbers – with no dimensions and no units. Important examples of this class are

$$\frac{M}{m} \cong 1836, \qquad \frac{2\pi e^2}{hc} \cong 0.0073, \qquad \frac{2\pi GM}{hc} \cong 5\times10^{-39}$$

The second of the quantities is the so-called fine-structure constant, close to 1/137, and the third quantity is known as the gravitational coupling constant.

The values of the constants of this type will be the same whatever system of units is used, hence the distant civilization will arrive at the same numbers as we do. In a word, they are deantropomorhic. Scientists can agree on many conventions, decide how things are, but whatever they agree upon they cannot change the ratio *M/m* of the masses of a proton and an electron. Nor can they conventionally decide that 24 litres of a gas at normal circumstances (pressure, temperature) shall contain, say, 8.31×10^{15} molecules. The gas contains 6.02×10^{23} molecules (Avogadro's number), a figure which is chosen by nature and which we cannot change however much we might like to.

9. Conventional time

The time parameter used in dynamical laws and many other scientific contexts illustrates in various ways the conventional aspects of a fundamental physical concept. Scientists use essentially the same concept of time that is used in daily life, but there are cases in which the ordinary time measure t has been found to be less appropriate. For this reason scientists have sometimes suggested to make use of a new measure of time, for example to introduce a measure θ that relates logarithmically to the ordinary one:

$$\theta = k \log t + \theta_0$$

If such a time measure is chosen, it will lead to a new description of natural phenomena. This game, to substitute t–time with some other θ–time, has for long been played in the cosmological arena, first in attempts to explain away the expanding universe and yet retain the observed galactic redshifts and Hubble's law. According to the conventionalist viewpoint of the British cosmologist E. Arthur Milne, who introduced the idea in the 1930s, it is not meaningful to ask if the universe is really expanding or not. The two time-scales merely result in two different physical pictures of the same reality, and which one to use is a matter of convenience. Following Milne, in a little known paper of 1940 Karl Popper argued that the discussion concerning the static-versus-expanding universe had no real empirical substance as it was a discussion about conventions. Popper illustrated his point with an analogy [Popper 1940]:

> The [two] theories are logically equivalent, and therefore do not describe different facts, but the same facts in alternative languages. To ask whether "in reality" the universe expands, or *c* decreases, or the frequencies speed up, is not more legitimate than, when prices of goods fall throughout the economic system, to ask whether "in reality" the value of the money has increased or the value of the goods has decreased.

It should be pointed out that Popper was not a conventionalist. On the contrary, he recognized the principal danger of conventions, namely that they make it too easy to evade conflicts between theory and experiment. According to the conventionalist view, a distinction between falsifiable and non-falsifiable theories will be ambiguous or even impossible, thereby directly challenging the heart of Popper's philosophy. In his main work, *The Logic of Scientific Discovery*, he said: "The only way to avoid conventionalism is by taking a *decision*: the decision not to apply its methods. We decide that, in the case of a threat to our system, we will not save it by any kind of *conventionalist stratagem*" [Popper 1959, p. 82].

θ–time stretches infinitely far back in time as t approaches zero. Rather than saying that the universe was created at $t = 0$, on θ–time one would say that the universe has existed in an eternity of time and therefore has no singular beginning. Several physicists have considered θ–time more fundamental in a cosmological context and used the transformation to question the reality of the big bang or to understand it in a different way [Lévy-Leblond 1990]. Using θ–time one can argue that "the universe is meaningfully infinitely old because infinitely many things have happened since the beginning" [Misner 1969, p. 1331].

Units for physical quantities are conventional and accordingly they have changed through history. This is also the case for the unit of time, one second, which is presently defined as 9 192 631 770 periods of a certain spectral line in cesium-133. How do we know that this radiation is emitted uniformly, that is, with contant frequency? The same question can be asked about earlier forms of standard clocks, indeed of any clock. How do we know that a pendulum clock keeps track of time? Or the Sun, during its course over the sky? In practice we often compare a clock with a better one, ultimately the standard clock used to define the time unit. However good clocks we use, we can never avoid the question of the uniformity of time, since there is no way to compare clocks with "time itself." We are forced to choose a standard clock and *postulate* that it runs smoothly. This is a basic convention in all measurements of time.

Conventions are usually carefully justified and far from arbitrary choices, yet by their very nature they could have been chosen differently. Strictly speaking, the only requirement is that they are internally consistent. As we have already seen, widely different conventions are possible, but the price for such possibilities is often forbiddingly high. Thus, our convential choice of a standard clock – or, historically, a whole series of standard clocks – is justified by our knowledge that by using such a time measure the world becomes rationally comprehensible. Had we adopted a very different standard clock, this would not had been the case. We could in principle introduce a time measure $\psi = \psi(t)$, where ψ varies in some irregular way with t; by convention ψ–time now runs uniformly and therefore t–time runs highly irregularly. With such a measure of time a pendulum would not move periodically, nor would atoms emit light of definite frequencies. And the moons of Jupiter would move in haphazard ways that did not reveal Kepler's third law. Physical science would be impossible and we would understand practically nothing of the world around us.

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