Water: The long road from Aristotelian element to H_2O

Hasok Chang[•]

Introduction

In today's science-based civilization, people tend to accept without question the most basic things that science tells us, except on certain controversial subjects such as evolution. This is the case even for people who do not know much science or like it very much. For example, anyone with even the slightest acquaintance with modern science knows and accepts that water is H_2O. Yet it was a very difficult thing for scientists to learn. That is the subject of my recently published book, *Is Water H_2O? Evidence, Realism and Pluralism* (Dordrecht: Springer, 2012). If we take a closer look at the basic items of scientific knowledge that we take so much for granted, almost always we find that they were achieved only with great difficulty, and only after some very challenging questions were resolved, or set aside without being resolved.

These reflections bring us to the most basic question of epistemology: how do we know anything? More specifically, how can we be sure about what scientific theories tell us about unobservable entities? How can such theories be supported by evidence? These questions are at the core of the philosophical debates concerning scientific realism. Many general and abstract answers have been given to the realism question (and to similar questions in the philosophy of science), but I often do not find them convincing because they are not based on sufficient knowledge of how the acquisition and evaluation of scientific knowledge work out in practice. For example, take what perhaps remains the most popular argument for scientific realism, due to Hilary Putnam:

> "The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle. That terms in mature scientific theories typically refer (this formulation is due to Richard Boyd), that the theories accepted in a mature science are typically approximately true, that the same term can refer to the same thing even when it occurs in different theories — these statements are viewed by the scientific realist not as necessary truths but as part of the only scientific explanation of the success of science, and hence as part of any adequate scientific description of science and its relation to its objects."[1]

At least our intuitions need to be trained and sharpened up through exposure to some critically considered scientific work before we can even attempt to evaluate such

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[•] Department of History and Philosophy of Science University of Cambridge, Reino Unido. E-mail: hc372@cam.ac.uk These remarks are based on the lecture delivered at the III Allen G. Debus Lectures at CESIMA, PUC-SP, 29 August 2012.

[1] Hilary Putnam, *Mathematics, Matter and Method (Philosophical Papers, vol. 1)* (Cambridge: Cambridge University Press, 1975), 73.

arguments. And it may well be that after knowing enough about specifics we would no longer be tempted to talk in such sweeping terms about the success and the truth of science.

I wish to make us all aware of the challenge involved in building scientific knowledge, no matter how simple or taken for granted. Without such awareness, we can reach neither a true appreciation of the achievements of science, nor a properly critical attitude regarding the claims of science. In my book I show how contingent the series of decisions were that led people from the traditional assumption that water was an element, to the consensus that it was a compound with the chemical formula H_2O. I have deliberately chosen as the subject of my study one of the most familiar substances in human life and one of the most basic scientific facts about that substance. Through this story of the changing conceptions of water, I also try to advance the debate on some major philosophical issues, including realism and pluralism. And by the end of the story we may well be asking: *What* do we know? What does it *mean* to know something?

In the limited space here, I can only hope to give some highlights of the historical and philosophical arguments I make in the book. Let me begin with a very quick overview of the history. It was only in the 1780s that scientists first proposed the idea that water was a compound substance, not an element as the ancient Greek philosophers had thought. And it was only by the 1860s that a general agreement on the molecular formula of water as H_2O, not HO as proposed by John Dalton, the originator of the chemical atomic theory. Thinking about this situation gives us a striking realization: many of the great and rational thinkers whose political, philosophical and scientific writings we still study with reverence did not have any idea that water was H_2O: Newton, Voltaire, Hume, Franklin, Goethe and Kant, just to mention a few familiar names. What we take as common sense was not at all obvious in a previous age that is not so distant from our own in many ways.

Water in the Chemical Revolution

The first major task I set myself is a re-telling of the Chemical Revolution, through which water first came to be recognized as a compound. This is a very familiar topic in the history of science, but my re-examination will show that there never was sufficiently strong evidence at the time to warrant the triumph of Antoine-Laurent Lavoisier's new chemistry (which regarded water as a compound of hydrogen and oxygen) over the infamous phlogiston theory (which preserved the notion of elementary water).

As advocated and practiced by the likes of Joseph Priestley, phlogiston-based chemistry was actually a highly cogent system of knowledge, grounded in very concrete laboratory operations. Initially the motivation for postulating the phlogiston concept was to explain the heat and light that emerge when something burns. So people imagined that combustible substances possessed fire within them in a latent form, and this hypothetical substance was called “phlogiston”. That may have been only a nice story without much scientific promise, but the situation changed completely when the phlogiston theorists realised the connection between combustion and what they called “calcination”, the process in which a metal was turned into crumbly, earthy stuff, called a calx – which is rust or metallic oxide in modern terms. Calcination, like combustion, came to be seen as a loss of phlogiston. This also implied that metals were rich in phlogiston, and this gave an explanation of why all metals shared common properties: shininess, malleability, electrical conductivity, etc.

Now, someone skeptic about this story could challenge the phlogistonists: "if you think that a metal turns into calx by losing its phlogiston, can you turn the calx back to its metallic form by adding phlogiston to it?" And the answer was a very clear "Yes, we can"! Mix a calx with some phlogiston-rich substance, for example charcoal (which is highly combustible, therefore phlogiston-rich); heat the mixture; phlogiston goes from the charcoal to the calx, turning the calx into metal, and the charcoal into ash. In fact this was a process that had been known for many centuries, namely the smelting of metallic ores, which are in large part oxides (that is to say, calxes).

The concept of phlogiston provided unifying explanations and led to many empirical discoveries including the production of oxygen itself, which Priestley performed, on the basis of phlogiston theory, before Lavoisier. And Lavoisier's chemistry had many serious difficulties, some of which were already recognized by his contemporaries. For example, the very name of "oxygen", which Lavoisier coined from Greek roots to mean "acid-generator", embodies a mistaken theory of acidity.

In fact, there is no better case than the composition of water for illustrating the *surprising* cogency of the phlogiston theory. Lavoisier's argument that water was a compound of oxygen and hydrogen made a decisive moment in the Chemical Revolution. But there is notable irony in that situation. No one knew better than Priestley how to make oxygen (which he called "dephlogisticated air"). And hydrogen (called "inflammable air") had been produced and studied in the 1760s by Priestley's compatriot and fellow phlogistonist Henry Cavendish, by dropping pieces of metal into acids. It is very easy to reproduce that simple experiment and see for oneself: get a cup of sulfuric or hydrochloric acid, of fairly low concentration; put a zinc wire into the acid, and watch it dissolve while giving off a stream of hydrogen bubbles. Cavendish also discovered how to explode those two airs together to make water. It was Priestley and Cavendish who taught Lavoisier how to do these things.

Cavendish and Priestley thought for a time that inflammable air was pure phlogiston, driven out from the metal by the action of acid. Their later view was that inflammable air was "phlogisticated water", that is, water containing an excess of phlogiston. It was easy to imagine such a substance being produced when phlogiston was released within a body of an acid, which always contained some water. As for oxygen, or dephlogisticated air, that was "dephlogisticated water". When phlogisticated water and dephlogisticated water combined with each other, the excess and deficit of phlogiston cancelled out, and plain water was produced. To summarize: there were two competing views on the formation of water, both of which were cogent and self-consistent:

(1) Hydrogen + Oxygen → Water
(2) Phlogisticated water + Dephlogisticated water → Water

The second account here may seem strange to us, but Cavendish and Priestley had good reasons to think that water was an essential constituent of gases. For example, everyone knew that vapors were produced from liquids; why not other kinds of gases, too? Lavoisier also readily agreed that gases were produced from water and other liquids, by the addition of heat.

Historically well-informed philosophers have struggled to say what exactly was wrong with this phlogistonist view. It is tempting to say "Priestley was wrong, because phlogiston simply doesn't exist", but how do we know *that*? To most phlogistonists, phlogiston was very real: not only observable, but even directly manipulable. It is no use pointing out that it was impossible to isolate phlogiston in its pure form. If we really required such material isolatability, we would have to renounce a whole range of scientific concepts from quarks to energy. And Lavoisier's own theory included the concept of *caloric*, the matter of heat, which was not isolatable in its pure form, either. Caloric was an absolutely central part of Lavoisier's theory, the subject of the very first chapter of his new textbook of chemistry, and occurring at the top (along with light) of the table of chemical elements published in the same book [**Figure 1**].

Figure 1. Lavoisier's table of simple substances[2]

	Noms nouveaux.	*Noms anciens correſpondans.*
Subſtances ſimples qui appartiennent aux trois règnes & qu'on peut regarder comme les élémens des corps.	Lumière	Lumière.
	Calorique	Chaleur. Principe de la chaleur. Fluide igné. Feu. Matière du feu & de la chaleur.
	Oxygène	Air déphlogiſtiqué. Air empiréal. Air vital. Baſe de l'air vital.
	Azote	Gaz phlogiſtiqué. Mofete. Baſe de la mofete.
	Hydrogène	Gaz inflammable. Baſe du gaz inflammable.
Subſtances ſimples non métalliques oxidables & acidifiables.	Soufre	Soufre.
	Phoſphore	Phoſphore.
	Carbone	Charbon pur.
	Radical muriatique.	Inconnu.
	Radical fluorique	Inconnu.
	Radical boracique,	Inconnu.
Subſtances ſimples métalliques oxidables & acidifiables.	Antimoine	Antimoine.
	Argent	Argent.
	Arſenic	Arſenic.
	Biſmuth	Biſmuth.
	Cobolt	Cobolt.
	Cuivre	Cuivre.
	Etain	Etain.
	Fer	Fer.
	Manganèſe	Manganèſe.
	Mercure	Mercure.
	Molybdène	Molybdène.
	Nickel	Nickel.
	Or	Or.
	Platine	Platine.
	Plomb	Plomb.
	Tungſtène	Tungſtene.
	Zinc	Zinc.
Subſtances ſimples ſalifiables terreuſes.	Chaux	Terre calcaire, chaux.
	Magnéſie	Magnéſie, baſe du ſel d'Epſom.
	Baryte	Barote, terre peſante.
	Alumine	Argile, terre de l'alun, baſe de l'alun.
	Silice	Terre ſiliceuſe, terre vitrifiable.

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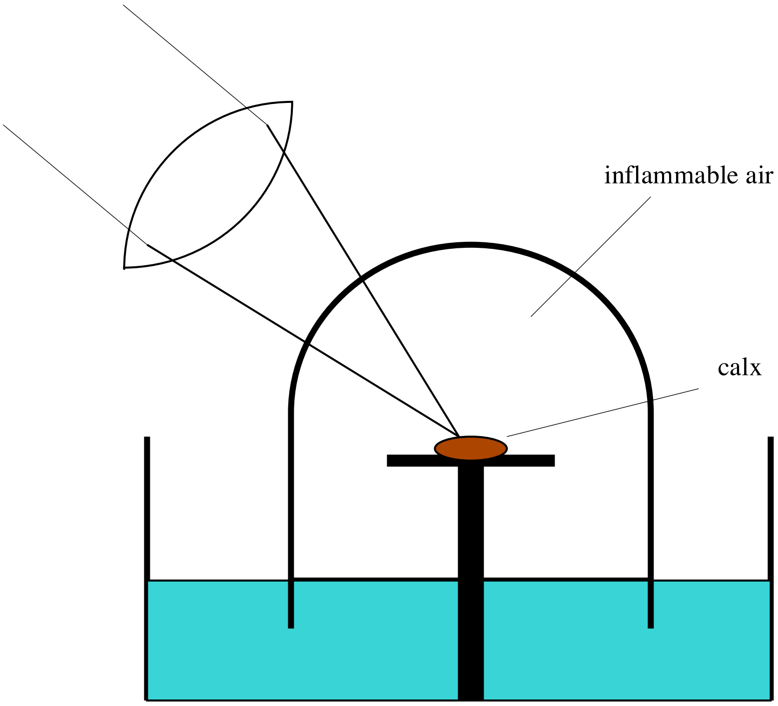
[2] Antoine-Laurent Lavoisier, *Traité élémentaire de chimie* (Paris: Cuchet, 1789), 192.

The situation is similar with the common complaint against phlogiston concerning its lack of weight. Today's physics is quite happy with weightless particles such as photons. Also in the days of phlogiston, other weightless substances were postulated with impunity, such as the electric fluid(s), and not least of all Lavoisier's caloric. A better version of the complaint focuses on the weight gain in calcination: a metal actually gains weight in becoming a calx, which would not happen if it were just losing something, namely phlogiston. The weight gain is explained very nicely by Lavoisier's theory, according to which calx is metal combined with oxygen, hence obviously heavier than the metal by itself. However, this did not quite refute the phlogiston theory, as there were other ways of accounting for the weight gain. It was not necessary to resort to the much-ridiculed idea that phlogiston had "levity" (negative weight).[3]

A much more serious explanation, advanced by Priestley and also by Richard Kirwan, was that in calcination the metal combined with water, while losing phlogiston. When the calx was reduced back to metal, it gave out the water and absorbed phlogiston. This experiment [**Figure 2**], initially devised by Priestley, gives a perfect illustration of the under-determination of theory by evidence in this situation.

Figure 2. The reduction of a calx in inflammable air



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[3] Guyton de Morveau, before he "converted" to the Lavoisierian cause, gave a very reasonable version of this idea, that phlogiston had *relative* levity; since it must be lighter than air, its addition to an object would reduce the object's *apparent* weight. See the passage included in Maurice Crosland, ed., *The Science of Matter* (Harmondsworth: Penguin, 1971), 135, from *Dissertation sur le phlogistique* (1770); see also the exposition in Jean-Pierre Poirier, *Lavoisier: Chemist, Biologist, Economist* (Philadelphia: University of Pennsylvania Press, 1996), 62.

Priestley thought that by heating a calx in inflammable air, which is full of phlogiston, he could reduce the calx back into its metallic form; this experiment was performed with spectacular success. But Lavoisier gave his own interpretation of what happens, saying that inflammable air was hydrogen, which combined with the oxygen from the calx, liberating the metal and producing water; and there was indeed a small amount of water produced, which Priestley had not noticed. But then Priestley re-considered the experiment, and made sense of it as the calx taking phlogiston in and giving out water.

After examining all aspects of the Chemical Revolution, I have found no conclusive reasons of empirical evidence, simplicity, or progressiveness in support of the complete elimination of the phlogiston theory. Rather, there was a genuine methodological incommensurability between the two systems of chemistry. Joseph Priestley was not irrational or unreasonable in his resistance to Lavoisierian chemistry, nor was he alone. So I conclude that phlogiston was killed prematurely.

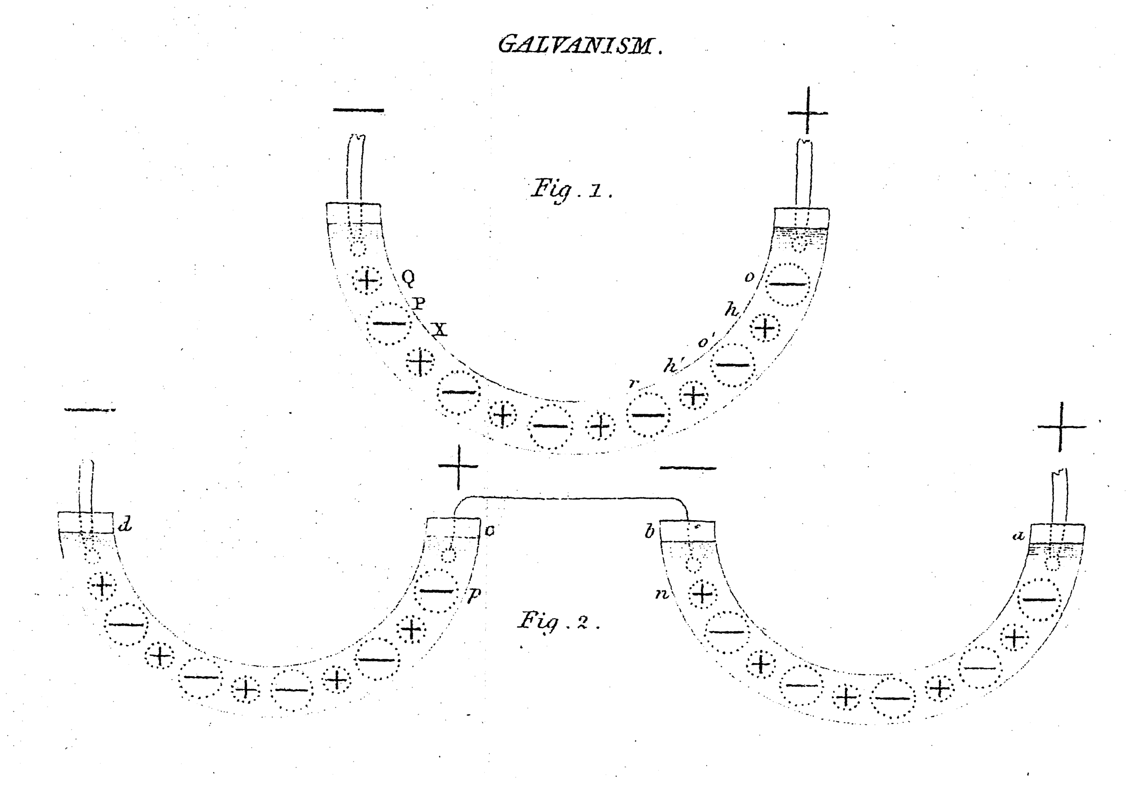
Electrolysis and the “distance problem”

Now, if Lavoisier was right about how water was composed, it should also have been possible to decompose it into hydrogen and oxygen. He in fact thought he had done this by passing steam over hot iron, but in that case the oxygen from the water combined with the iron to form a calx while hydrogen was liberated as a gas. As I just mentioned, Priestley could easily interpret this as the iron absorbing water and giving out phlogiston. Therefore it is understandable that there was a considerable level of excitement following the invention of Alessandro Volta's "pile" (battery) in the year 1800, which allowed the electrolysis of water with hydrogen and oxygen gases as the only products. What more could one ask, as proof of the compound nature of water?

But there was a problem, which was already recognized in the very first paper on the subject, by William Nicholson in London: if electrolysis broke down each molecule of water into hydrogen and oxygen, how could it be that the two gases emerged in different places, at positive and negative electrodes separated by a macroscopic distance from each other? If this problem was not solved, electrolysis actually threatened to become a piece of evidence *against* Lavoisier's theory. Indeed, Johann Wilhelm Ritter in Germany advanced an anti-Lavoisierian interpretation, according to which electrolysis was *synthesis*: at one electrode, the combination of water with negative electricity forms hydrogen; at the other electrode, positive electricity and water make oxygen; water is an element, and hydrogen and oxygen are compounds. If one identified negative electricity with phlogiston, for which there were some good reasons, then Ritter’s view lined up exactly with the phlogistonist view of Cavendish as shown here.

Ritter's theory of electrolysis was not so much refuted as repelled and excluded by the mainstream of chemistry, by this time heavily Lavoisierian. There was never a convincing solution to the problem of the separate production of hydrogen and oxygen until Svante Arrhenius’ theory of free ionic dissociation almost a century later, which argued that there are pre-existing H^+ and OH^- ions in water, which get picked up at the negative and positive electrodes. Meanwhile chemists and physicists pursued various other hypothetical mechanisms, such as an invisible transfer of oxygen or hydrogen through the water over to the other side, or a chain of partner-swapping water molecules linking the two electrodes proposed by Theodor Grotthuss, shown in **Figure 3**.

Figure 3. The Grotthuss chain[4]



The development and interplay of these various theories make a very long story, which I try to tell in the second chapter of my book. For now, here is just an instructive anecdote. Arrhenius ionic theory was proposed in his doctoral dissertation, which received a very unenthusiastic response initially. Arrhenius recalls:

> "I came to my professor, [Per Cleve], whom I admired very much, and I said, 'I have a new theory of electrical conductivity as a cause of chemical reactions.' He said, 'This is very interesting,' and then he said 'Good-bye.' He explained to me later that he knew very well that there are so many different theories formed, and that they are almost all certain to be wrong, for after a short time they disappeared; and therefore by using the statistical manner of forming his ideas, he concluded that my theory would not exist long."[5]

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[4] Christian Johann Dietrich (Theodor) Grotthuss, "Memoir upon the Decomposition of Water, and of the Bodies which it Holds in Solution, by means of Galvanic Electricity", *Philosophical Magazine*, 25 (1806): 330–39, Plate IX.

[5] Svante Arrhenius, quoted by Harry B. Gray and Gilbert P. Haight, Jr., *Basic Principles of Chemistry* (New York: W. A. Benjamin Inc, 1967), 90.

HO or H_2O? The atomic chemistry of water

For those accepting that water was made up of hydrogen and oxygen, the advent of the chemical atomic theory raised a further question: how many atoms of each element combine to make water? John Dalton, from his original 1808 publication onward, candidly acknowledged that he had no way of answering such questions with certainty. There is a fundamental circularity between atomic weights and molecular formulas. In the case of water, what was known from experiments was that hydrogen and oxygen always combined in a 1:8 ratio by bulk weight (in approximate modern numbers). From that we can deduce that the ratio of atomic weights is 1:16, if we know that water is H_2O; or we can deduce that the molecular formula of water is H_2O, if we know that the atomic weights are 1:8. But we need to know one in order to know the other — and to begin with, we know neither. Dalton applied his "rules of greatest simplicity" to break the circularity: since water was the only chemical compound of hydrogen and oxygen that he knew, he assumed that it was the simplest possible atomic combination: HO.

Amedeo Avogadro almost immediately proposed an alternative account that held that water was H_2O. Interestingly, Avogadro's ideas were rejected by Dalton himself and most other chemists as *ad hoc*, speculative and implausible, and not generally adopted until half a century later. Avogadro began with the observation from Joseph-Louis Gay-Lussac that gases combined chemically with each other in very simple ratios of volume; in particular, the combination of hydrogen and oxygen to make water was always in a 2:1 ratio of volume. Doesn't the H_2O formula just jump out from that observation? It does, if one assumes that equal volumes of all gases contain equal numbers of particles. But there was a problem for Avogadro: the combination of two volumes of hydrogen and one volume of oxygen make two volumes of water vapor, while the reasoning about the number of particles dictated that there should only be one volume of water. A normal person might have taken this as a refutation of his hypothesis.

Not Avogadro. He thought that the water molecule must divide in half, doubling the number of molecules and therefore the volume. But this involved cutting the oxygen atom in half, which would have been considered absurd. So Avogadro decided that the number of particles should be doubled from the start, meaning that oxygen and hydrogen gases were made up of H_2 and O_2 molecules.

To almost everyone else all this seemed arbitrary and physically groundless. Why would two atoms of the same element stick together, and if two of them can, why not three or more?[6] Especially to those who paid attention to electrochemistry and followed the widespread idea that atoms that combined with each other had opposite electrical charges (refer to **Figure 3** again), it seemed highly unlikely that two atoms of hydrogen both charged positive, or two atoms of oxygen both charged negative, could remain stuck without anything else to hold them together.

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[6] Jean-Baptiste Dumas's study of vapour densities also seemed to reveal contradictions, such as the molecule of phosphorus and sulphur being P_4 and S_6, not binary like hydrogen, oxygen and nitrogen.

So there was no simple and straight line of development that led from Dalton's HO formula for water to the modern H_2O formula. As summarized in **Table 1**, there were various competing systems of atomic weights and molecular formulas in use through much of the 19th century.

Table 1. Competing systems of atomic weights and molecular formulas in use in the 19th century

	Atomic weights				Molecular formulas		
	H (hydrogen)	O (oxygen)	C (carbon)	Ag (silver)	Water	Silver oxide	Hydrochloric acid
Gmelin (also Liebig, Thomson, Wollaston)	1	8	6	108	HO	AgO	HCl
Dumas (1828)	1	16*	6	216	H_2O	AgO	HCl
Berzelius (1826)	1	16	12	216	H_2O	AgO	H_2Cl_2
Gerhardt (4-volume formulas**)	1	16	12	108	H_4O_2	Ag_2O	H_2Cl_2
Laurent (2-volume formulas)	1	16	12	108	H_4O_2	Ag_2O	HCl

The consensus on H_2O was achieved only as a result of a complex interaction between five different systems of atomic–molecular chemistry. I can only give a very brief characterization of these systems today, but I hope their distinctiveness from each other will be clear enough. I would like to stress that these different systems not only contained differing theoretical ideas, but also different experimental and theoretical activities geared toward different aims. So these systems of practice are much more like Thomas Kuhn's paradigms than mere theories, although they co-existed and flourished during a normal state of science, contrary to the expectation of monopoly by one dominant paradigm in Kuhnian normal science.

Let me now give a brief description of the five systems of atomic chemistry.

(1) In the *weight-only system*, chemists focused on deriving atomic weights from the macroscopic combining weights of substances. This system has often been characterised as positivist, but what it really shows is not positivism but ontological austerity, postulating atoms possessing only weight. It was a purified version of Dalton's thinking, which had included considerations of the shapes and sizes of atoms and their mechanical interactions with each other. Instead, the practitioners of the weight-only system focused on the activities of analytical chemistry rather than *explanations* of chemical phenomena.

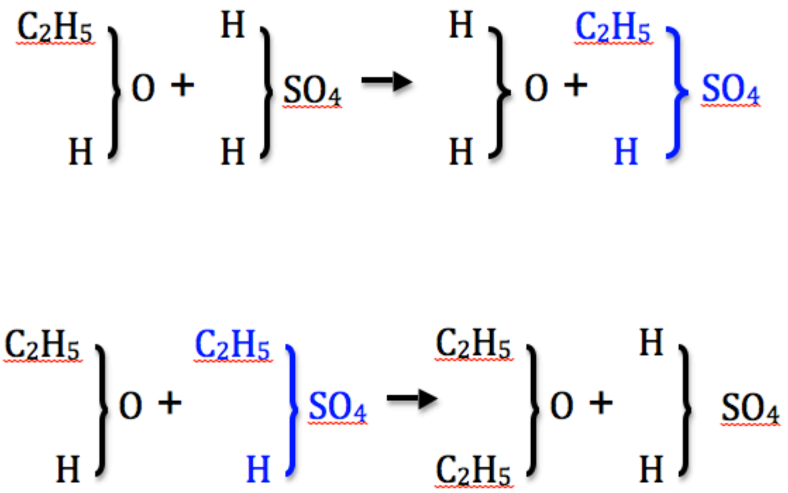
(2) A focus on explanations was found most strongly in the *electrochemical dualistic system*. This system was firmly based on the electrolysis of various substances using the Voltaic battery. Elements were placed on a spectrum of electronegativity–electropositivity, depending on their tendency to appear at the positive and negative electrodes when their compounds were electrolyzed. The explanatory potential in this system was apparent, understanding chemical reactions as consequences of the electrostatic attractions and repulsions of atoms.

(3) In the physical volume–weight system, first constructed by Avogadro, chemists took both weights and volumes as measurable properties of atoms. The focus here was not so much on the explanation of bonding as on finding out the real properties of the atoms

and molecules of various substances; in that sense, this was a realist system of practice. Many chemists assumed that equal volumes of all kinds of gases contained an equal number of molecules; Avogadro's program was to defend this assumption, whatever the consequence. This is how he gave us the H_2O formula for water, for instance. But there were various objections to Avogadro's ideas, as I have indicated.

(4) The disillusionment with the physical volume–weight system was probably largely responsible for the rise of what I call the substitution–type system. Instead of speculating about the real properties of atoms and molecules, an influential group of organic chemists began to focus on classification as their main aim and activity. Jean-Baptiste Dumas led the way here with his "type theory", a research program of classifying organic molecules into "types" defined by the structural templates of certain simple inorganic substances, such as water and ammonia. The idea of types was picked up by many other chemists. For instance, Alexander Williamson argued that (ethyl) alcohol and ether belonged to the water type. What was powerful about this kind of reasoning was its operational basis; each branch shown in the type formula could be replaced by another substance, and that would yield a different yet related substance. For example, **Figure 5** shows Williamson's model of the process of "continuous etherification", in which alcohol turns itself into ether with a loss of water, with the help of sulphuric acid.

Figure 5. Alexander Williamson's model of "continuous etherification" (the production of ether from ethyl alcohol with the help of sulfuric acid) by two hydrogen–ethyl substitutions



(5) Although it was widely suspected that there was something real in the type-structures that were modifiable as predicted by substitution, many type-theorists in the early days denied that their structural formulas were meant to represent the actual geometry of molecular structures. Those who did concern themselves with getting at the structures practised what I call the geometric–structural system. Often inspired by the crystallographic

tradition, which went much farther back in time than Dalton's work, these chemists attempted to get directly at the geometry of molecular structures.

Each of these systems had their own distinct aims, and their own list of successes and failures, too. It was only as a result of some complicated developments and interactions of these systems that a consensus slowly emerged on atomic weights and molecular formulas, including H_2O. It was not simply a matter of reviving and publicizing Avogadro's hypothesis in a clearer and more convincing form. Many clues had to be fitted together, and some of the decisive clues arose from very subtle developments in organic chemistry in the 1840s and the 1850s.

When the consensus did come, it was not taken by everyone in a realist manner; many leading chemists still doubted the existence of physical atoms, and had reservations about taking the models of structural chemistry literally. There is a very interesting statement illustrating this point, from August Kekulé, made *after* his successful theory of the ring structure of the benzene molecule (famously associated with his story of seeing in a dream snakes biting each other's tails): "The question whether atoms exist or not has but little significance from a chemical point of view; its discussion belongs rather to metaphysics."[7]

And the synthesis of systems leading to the consensus on molecular structures also left some important questions unanswered, which were eventually taken up by the newly emerging field of physical chemistry. Perhaps the most important of these questions concerned the mechanism of chemical reactions: what exactly made atoms combine and separate from each other. Later, in the 20th century, it was recognized that the explanation of chemical reactions required quantum mechanics, and that was a development reached in a completely different line of work from structural chemistry.

What about scientific realism?

One general question plagues me through all the historical discussion: did the scientists have sufficient evidence to justify the verdicts that they reached? Regarding each of the three episodes, I arrive at the judgment that there was no system that deserved a monopolistic dominance, and that not having one dominant system in each situation was not, or would not have been, harmful to the progress of science. There can be, and have been, successful systems of science which do not affirm the truth of the statement that water is H_2O. This admission of contingency makes scientific realists worry: if the course of the development of scientific knowledge is contingent, can we still preserve the notion of scientific truth and its pursuit?

In response to this worry, I advance a novel doctrine called "active scientific realism", which takes realism as a commitment to maximize our contact with reality in order to learn as much as we can. This is in contrast to standard realism, which is a claim that accepted scientific theories possess truth, at least approximately or partially. I think a

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[7] Quoted in Mary Jo Nye, *Molecular Reality* (London/New York: Macdonald/American Elsevier, 1972), 4.

proper, full-blooded "ism" should be an *ideology* in a broad sense of the term — a commitment to think, act, and live in a certain manner (as in the case of communism, capitalism, or even positivism). So I take realism as a commitment to engage with what is real, with "external reality", not with our own constructions or with what we wish reality would be like. In the context of inquiry (scientific or otherwise), this ought to mean a commitment to maximize our *learning* from reality. We can debate whether this learning can ever bring us truth, certainty or objectivity in an ultimate sense, but that is a separate matter.[8]

If realism should be a commitment to learn from reality, what *is* reality? What do we mean by external reality, and what is involved in learning about it? Instead of entering into serious metaphysics, I want to give you a pragmatist and *operational* definition of reality. I propose to think of external reality as whatever it is that is not subject to one's own will. As pragmatists have pointed out, nature's *resistance* to our ill-conceived plans is one of the most important sources of our very notion of reality. William James stated, right after his (in)famous definition of truth: "Experience, as we know, has ways of *boiling over*, and making us correct our present formulas."[9] James added: "The only objective criterion of reality is coerciveness, in the long run, over thought."[10]

But it is not only when our expectations are frustrated that we make contact with reality. Something that we cannot control may turn out the way we expect, and that is precisely what happens when we make successful predictions. Knowledge is a state of our being in which we are able to engage in successful epistemic activities, which can only happen if there is insufficient resistance from reality.[11] As Wittgenstein once said, "It is always by the favour of Nature that one knows something."[12]

So scientific realism as I mean it is an active doctrine, recommending that we should seek out contact with reality and in such ways as to maximize our learning, rather than a hubris of armchair philosophy about how we can obtain or have obtained the objective truth about the universe. Realism in this sense may sound just like empiricism, and that is how it should be — it doesn't make much sense that empiricism and realism have been set against each other in debates on scientific realism.[13] But there is one important difference between empiricism and active realism. Empiricism can be taken as a rather passive or defensive doctrine, emphasizing that the only source of knowledge we can have is experience and that

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[8] My view of realism is fully compatible with Roberto Torretti's pragmatic view of realism, and certainly against what he calls the "nostalgic crypto-theology" of the standard realists. Roberto Torretti, "'Scientific Realism' and Scientific Practice", in *The Reality of the Unobservable*, ed. Evandro Agazzi and Massimo Pauri (Dordrecht: Kluwer, 2000), 113–22, on 115.

[9] William James, *Pragmatism and The Meaning of Truth* (Cambridge, Mass.: Harvard University Press, 1978), 106, emphasis original; quoted by Hilary Putnam, *Pragmatism: An Open Question* (Oxford: Blackwell, 1995), 8.

[10] "Spencer's Definition of Mind as Correspondence", in *James's Essays in Philosophy* (Cambridge, Mass.: Harvard University Press, 1978), 21; quoted by Putnam, *Pragmatism*, 11.

[11] These statements are intended as implicit definitions of terms such as "knowledge", "reality" and "resistance".

[12] Ludwig Wittgenstein, *On Certainty* (New York: Harper & Row, 1969), 66e, §505.

[13] Typical scientists are both empiricists and realists, and that is not (only) because they are philosophically unsophisticated.

we should avoid treating other things as legitimate sources of knowledge; this does not involve any recommendation about what kind of experience and how much of it we should try to have.

The core idea of active realism is certainly not my invention, though the terminology is. A most important influence on my thinking has been the operationalism of Percy W. Bridgman, the Nobel Prize–winning American experimental physicist who wrote philosophical books during his summer vacations. In my interpretation of Bridgman, his insistence on giving a well-defined method of measurement to every physical concept shows an active-realist commitment intended to turn every theoretical statement into a site of contact with reality. Karl Popper's injunction for scientists to seek higher falsifiability and more severe tests can also be seen as a demand for more contact with reality. Imre Lakatos' demand for progressiveness in scientific research programs can also be readily interpreted in the active realist vein. Theoretical progress for Lakatos meant the making of novel predictions, which are new contact-points with reality, and empirical progress meant some of those predictions being successful, establishing knowledge. A connection that will more readily recognizable to those who have been following the recent scientific realism debate is with Ian Hacking's "experimental realism", with the famous slogan encouraging active contact with reality: "Don't just peer; interfere!"[14]

Nearly all sides in the scientific realism debate should be able to subscribe to active realism. But there is a more controversial side to it, too. Active realism recommends that we should pursue *all* systems of knowledge that can provide us informative contact with reality; if there are mutually incommensurable paradigms, we should retain all of them at once. But will that not interfere with the pursuit of the one truth about nature? I think what we need to do is abandon that sort of notion of truth, which is meaningless in practice. When we consider what "truth" means in practice, the concept splinters into several different ones, including one that is internal to a given system and nearly synonymous with "success". Realism should be about promoting realistic ways of learning from reality, not a vain attempt to prove that we are in possession of the unique truth about nature. That is also to say that active realism demands pluralism, which is what I would like to address briefly before closing.

Pluralism in science

Contingency implies choice: past scientists could reasonably have made choices that would have led to systems of science that are different from what we have today. Rather than try to avoid this implication, I embrace it and develop it into a full-blown doctrine of pluralism in science, in the last chapter of the book. Any serious scientific question is bound to admit more than one rationally justified way of answering it. Scientists may have a tendency to agree on one theory (or system) at a given time, but at least philosophers should

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[14] Ian Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983), 189.

be aware that there might be worthwhile alternatives that scientists reject without good enough reasons. The judgment that a system of knowledge was rejected without sufficient epistemic warrant is a weighty one to make. First of all, it involves a claim that it would have been better to let it survive. Secondly, judgment comes with a demand for action: so, if I think that phlogiston chemistry was killed off prematurely, what am I going to *do* about it? If there is lost potential there, it should be recovered and developed. What I advocate is pluralism in practice — not the armchair pluralism of declaring "Let a hundred flowers bloom", but an active pluralism of actually cultivating the 99 neglected flowers.

But why is it better to be pluralistic? Why keep multiple systems of knowledge alive? The immediate reason for this is that we are not likely to arrive at the one perfect theory or viewpoint that will satisfy all our needs. Call it pessimism, but I do not think it is unwarranted pessimism. I would rather think of it as reasonable humility about human capabilities. If we are not likely to find the one perfect system, it makes sense to keep multiple ones, which will each have different strengths. Different benefits, practical and intellectual, will spring from different systems of knowledge. It is also important to note that the co-existence of multiple systems can facilitate productive interactions between them — through integration, co-optation and competition. These benefits of interaction are just as important as the more widely recognized benefits of toleration; both are essential planks in my program of pluralism.

It is important to distinguish pluralism from relativism. Relativism involves an idle permissiveness and renunciation of judgment. Pluralism does not renounce judgment, yet maintains that it is better to foster a multitude of worthwhile systems, rather than only one. Pluralism as I conceive it actively engages in the work of proliferation; it is about knowledge-building, not just knowledge-evaluation.

My questioning of the exclusive truth of the statement "Water is H_2O" will raise eyebrows and disturb common assumptions, and that is fully intended. Independently of the details of my various arguments, it will be beneficial for people to realise that it is not crazy to subject the most fundamental beliefs of modern science to critical scrutiny, and to contemplate the possibility of scientific systems that deny them or do without them. In any case, modern science no longer subscribes to the notion that water is simply H_2O.[15]

The familiar chemical and physical properties of water depend essentially on the presence of various ions, and on the continual connections and re-connections between neighboring molecules, which belie the single-molecule formula of H_2O. If we had a simple heap of H_2O molecules, it would not be recognizable as water. Of course, the "H_2O" view still contains an important truth about the constitution of water, and continues to have heuristic utility. But it would be wrong to take it as an eternal, unqualified and exclusive truth; rather, it was merely one important resting-point in the continuing and multi-faceted

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[15] For scientific details see D. Eisenberg, & W. Kauzmann, *The Structure and Properties of Water* (Oxford: Oxford University Press, 2005); Robin Hendry gives a succinct summary in "Chemistry," in *The Routledge Companion to the Philosophy of Science*, ed. S. Psillos, & M. Curd (London: Routledge, 2008), 520-30.

progressive saga of science. There is no benefit to be gained from a dogmatic adherence to a simple-minded idea that science itself has already modified and transcended.

Let me conclude with an answer to the question posed in the title of my book and this lecture: Is water H_2O? Yes, of course, but it is many other things as well, just like an electron is surely a particle but also a wave. The question "what is water?" admits many cogent answers, each of them part of a coherent system of scientific practice. None of those systems should be given the right to suppress, exclude, or delegitimize the others, because we need all of those systems to give ourselves maximal exposure to reality. The knowledge gained by such maximal exposure to reality also contributes to the full actualization of the human potential. After many centuries of struggle, we have learned to accept and benefit from such a spirit of pluralism in the realms of politics, culture, language, cuisine, art, and ecology. I believe it is time to admit it in science, too.