History and Philosophy of Science in the Service of Scientific Pluralism

Hasok Chang

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Fernando Gil International Prize 2013

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Index

Fernando Gil International Prize

With the corresponding monetary value of 75 thousand euros, the Fernando Gil International Prize for the Philosophy of Science was created by the Portuguese Government through the Fundação para a Ciência e Tecnologia and by Fundação Calouste Gulbenkian. As a way of honoring the memory and work of the renowned philosopher after whom it was named, the Prize is meant to distinguish a work of exceptional quality in the area of Philosophy of Science, authored by investigators of any origin or professional filiation, published in the five years previous to the year of the Prize's edition.

According to the terms of the Prize's regulations, the winner is requested, on the occasion of the award-giving ceremony, to hold an original public lecture, subsequently subject to publication by Fundação Calouste Gulbenkian.

The 2013 Prize was attributed by the international jury to Hasok Chang for the work *Is Water H_2O? Evidence, Realism and Pluralism*. The following lecture was originally given at Fundação Calouste Gulbenkian, the 20th of March, 2014.

History and Philosophy of Science in the Service of Scientific Pluralism

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Abstract

History and philosophy of science can play an active and beneficial role in the development of scientific knowledge, going beyond a mere description of its evolution and a passive judgment on its quality. More specifically, philosophical critique and questioning can often show that currently accepted knowledge lacks inevitability, and historical research tends to reveal that there have been worthwhile alternatives that became unjustly neglected or forgotten. Through such philosophical and historical work we can learn to appreciate the benefits of plurality in scientific knowledge (comparable to the benefits of diversity in society), and actually enhance this plurality by reviving abandoned alternatives and creating new ones. The prize-winning book demonstrates that such potential for plurality exists not only at the cutting edge of scientific research, but also in relation to very basic and taken-for-granted items of knowledge, such as the fact that water is a chemical compound and that its molecular formula is H_2O. Through this line of work philosophy can give itself renewed relevance to wider scholarship, education, and practical life, reversing its recent tendency toward academic specialization and isolation.

1.
Why H_2O?

Is Water H_2O? What a curious and strange subject, one might say, especially for an award-winning philosophy book. And the book is not about the semantic problem of the reference of scientific terms in the mode of Saul Kripke and Hilary Putnam. Rather, it is about the actual history of scientific developments that led to the idea that water is a chemical compound, and that each water molecule is composed of one oxygen atom and two hydrogen atoms. I will begin by presenting a view of one brief moment in this long history, which I hope will convince you that there is something philosophically interesting and significant here. **Figure 1** is an illustration taken from John Dalton, known as the "father of chemical atomism", showing that when he first proposed the atomic theory in chemistry he took the water molecule as HO, not H_2O.[1] And it took the best chemists of Europe 50 years to agree that the correct formula was H_2O. Why did it take so long? On the other hand, how did the 19th-century scientists ever learn such things, with no direct empirical access to atoms?

My choice of subject-matter represents many things about my view of the task of philosophy, about which I will say more later. For now I would just like to stress that I am examining the roots of scientific common sense. I want to show three things: the scientific beliefs that we now take as common sense were not always there; they actually represent great achievements, although we take them for granted; and often these items of scientific common sense also signify the suppression or neglect of alternatives.

In the history and philosophy of science we have become used to the notion of scientific revolutions, as presented by Thomas Kuhn. In a scientific revolution, one paradigm is replaced by another. But the losing paradigm is not

1. John Dalton, *A New System of Chemical Philosophy*, Vol. 1, Part 2 (Manchester/London: R. Bickerstaff, 1810), Plate 5, opposite p. 560.

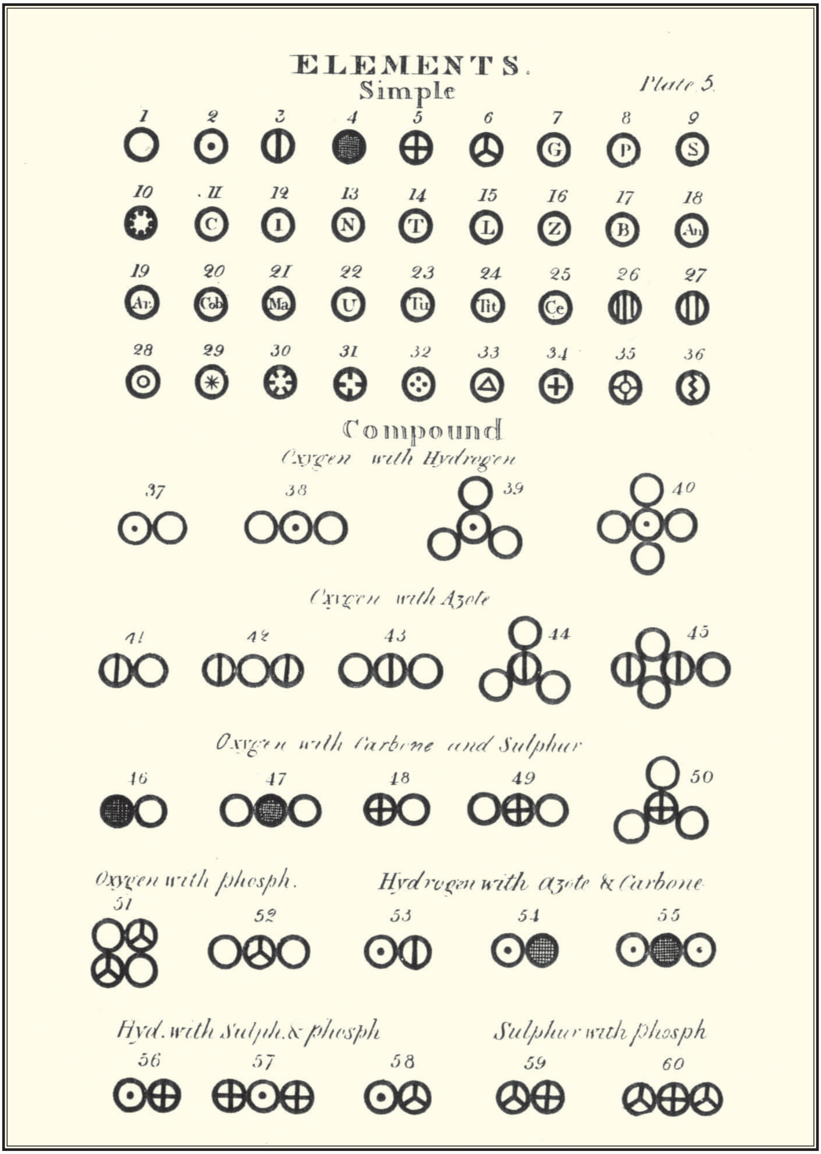

Figure 1. Dalton's view of water as HO, shown in diagram 37, in which the plain circle represents an oxygen atom and the dotted circle a hydrogen atom

simply wrong. Kuhn even maintained that there is typically some knowledge lost when science goes through a revolution, because the victorious new paradigm does not solve all the problems that the old paradigm solved well. This is what later commentators have called the "Kuhn loss" of knowledge, and Kuhn himself saw it as an expected by-product of scientific progress. But do we have to accept Kuhn loss? When a new paradigm arrives, do we have to discard the old one, despite all of its merits and successes? In that question lies the seed of pluralism. Both Kuhn and many practicing scientists share the monist view that "normal" science has to have one and only one paradigm in each field. To counter this tendency, I propose that we engage in a historical–philosophical exercise in the rehabilitation of discarded systems.

2.
The rehabilitation of discarded systems

2.1. Feyerabend on Galileo and the Tower Argument

The case of the "Tower Argument" is very useful in motivating sympathy and understanding for past scientific positions that may at first glance seem preposterous, and for exposing the epistemic insecurity of our own position and our undeserved complacency about it. The Tower Argument, illustrated in **Figure 2**, refers to the very reasonable Aristotelian argument against the Copernican hypothesis that the earth spins around its own axis (as well as orbiting around the sun). Everyone knows that a heavy ball dropped from a tall tower falls down vertically, landing at the foot of the tower. But if the earth is rotating, the tower will have moved on to the east while the ball is falling. So the ball should not fall at the foot of the tower, but off to the side of it, if the earth is moving. Therefore, the earth cannot be moving. Did I exaggerate the size of the effect in my childish picture? No, it is quite the opposite! We are not talking about a small effect. If I drop a ball from my hand while standing, it takes about half a second to hit the ground. A quick calculation shows that the ball must move about 100 meters sideways in that amount of time![2] There are many similar

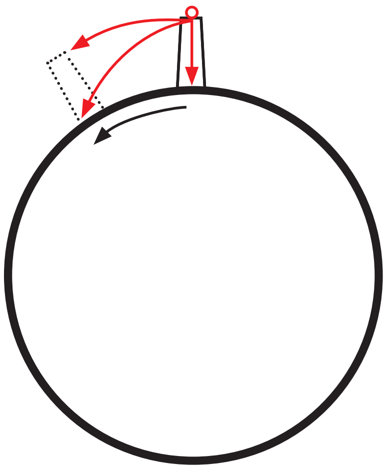

Figure 2. The Tower Argument.

2. A point on the equator must cover the whole circumference of the earth each day. That distance is around 40,000 km per day (nice round number because it was defined that way in the original metric system). There are 86,400 seconds in 1 day (24x60x60). So the speed of movement is 463m per second. This will be less at higher latitudes.

questions. If the earth is spinning so fast, how can birds and clouds keep up, maintaining their positions relative the ground? Why isn't there a great gust of wind going east to west all the time?

Galileo, in his defence of Copernicus, had to create some new physics, in a series of moves that Paul Feyerabend celebrates as illustrative of his own "anarchistic" philosophy of science.[3] To explain why we do not see or feel anything of the incredibly fast movement of the earth, Galileo postulated what has now developed into the principle of "Galilean relativity": shared motion is not perceived. But whether or not we can perceive it, how *could* the horizontal motions of the birds in the air, the air itself, and the ball after I release it, be maintained? In response to this challenge Galileo created the principle of inertia. But it is important to note that what Galileo postulated was not the rectilinear inertia of modern physics originating from René Descartes — "horizontal" for Galileo meant literally "along the surface of the earth", and therefore his inertia was circular, not linear, which is indeed what he thought he needed in order to keep terrestrial objects keeping up with the motion of the surface of the spinning earth.

If we feel that Galileo's reasoning here was irrational or at least inconclusive, then this is an admission of the epistemic insecurity of the great event that we call the Copernican Revolution. It is also an acceptance of the legitimacy of an intellectual sympathy for the defenders of the Aristotelian system at the time.

2.2. The story of phlogiston

In my book, the object of a similar sympathy was the phlogiston theory. I wrote: "I became a pluralist about science because I could not honestly convince myself that the phlogiston theory was simply wrong — or even genuinely inferior to Lavoisier's oxygen-based chemical theory."[4] It was a great formative experience to discover just how mistaken the common

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3. Paul Feyerabend, *Against Method* (London: New Left Books, 1975), pp. 55ff.

4. Hasok Chang, *Is Water H_2O? Evidence, Realism and Pluralism* (Dordrecht: Springer, 2012), p. 253.

opinion about phlogiston was. Let me start by quoting from a popular source: "Antoine-Laurent Lavoisier . . . is considered the father of modern chemistry. . . By ridding the chemical world of the phlogiston theory of combustion using quantitative analysis, Lavoisier was able to push chemistry toward its modern state."[5] Many professional historians had already discredited this myth, but I went further in my rehabilitation of phlogiston.

Before going on, I should explain what phlogiston is. People wondered how so much light and heat could emerge when something burned. So they thought: fire must be present in a latent form within a combustible substance, and the name of "phlogiston" was given to this fire. The phlogiston theorists saw combustion as the separation of phlogiston from the combustible substance, after which the latter assumed a very different form, like wood turning into ashes. Starting from this rather naïve idea, the phlogiston theorists made considerable achievements. For example, they recognized that combustion, calcination and respiration were essentially the same process — namely, "de-phlogistication", or, oxidization in Lavoisier's later terminology. "Calcination" refers to the process by which a metal turns into a "calx", the earthy and crumbly stuff that we call rust, or metal oxide in Lavoisier's terms.

And it was in the chemistry of metals that the phlogiston theory had its most striking successes. Unlike ordinary combustion, calcination was reversible. One could actually take a calx, and reduce it back to its metallic state by adding phlogiston to it. For example, a crucial step in the smelting of metallic ores was the reduction of calx, which was achieved by mixing it up with a phlogiston-rich substance — something very combustible, for instance charcoal — and heating the mixture. The resulting transfer of phlogiston from the charcoal to the calx turned charcoal into ash and calx into metal. This was, of course, a very old technique, which the phlogiston theory beautifully explained. And there were new experiments, too. In

5. "Antoine Lavoisier" http://www.chemistryexplained.com (last accessed on 7 March 2016).

1766 Henry Cavendish reported that when metals dissolved in acids they produced a combustible gas, which he called "inflammable air"; the acid must be attacking the metal and disengaging phlogiston from it, which explains why the resulting gas is combustible. Cavendish even thought for a time that this gas was phlogiston itself.

Joseph Priestley took things even further. He thought he could make a de-phlogisticated version of air by reducing a calx in air in an enclosed space. This is what he was trying to do when he ended up making "dephlogisticated air", which Lavoisier later called oxygen. Priestley devised an even more striking experiment, using Cavendish's inflammable air. If this gas was pure phlogiston, then it should combine with a calx, producing metal and nothing else. This prediction, illustrated in **Figure 3**, seemed a remarkable success. When Priestley put a calx of lead into a space filled with inflammable air, enclosed in

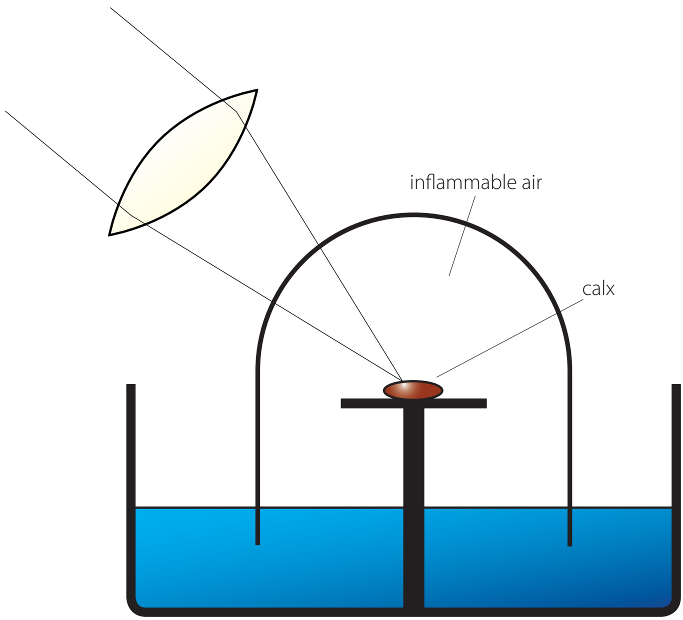

Figure 3. Priestley's reduction of calx in inflammable air

an inverted glass jar over water, and heated it with sunlight focused by a large lens, the calx turned into metal and the volume of inflammable air decreased (the water level went up), all exactly as predicted.

Now, this is where things get really interesting. Lavoisier, initially very troubled by this experiment, came up with a brilliant re-interpretation: the calx is an oxide of the metal, and the inflammable air must combine with the oxygen and make water, which must have just fallen unnoticed into the pool of water underneath in Priestley's experiment; so he re-named inflammable air "hydrogen", the water-maker. How did he get that idea? Well, because he had heard of another experiment made by Cavendish, in which water was produced when a mixture of dephlogisticated air and inflammable air was exploded using an electric spark. Dephlogisticated air is what Lavoisier re-named as oxygen, and he believed that a calx was a compound of oxygen with a metal. Doing Priestley's experiment over mercury instead of water showed that indeed there was water produced. Was that the end of the story? No, Cavendish immediately came up with his own re-interpretation, according to which inflammable air was not pure phlogiston, but water combined with excess phlogiston. So when it gave up the extra phlogiston to the calx, it just became water.[6]

So one can see that the phlogiston theory was not simply refuted by evidence. It gave intuitive and self-consistent accounts of chemical phenomena, and even *predicted* some important ones. Lavoisier's theory made sense, too, of course, but it also had its own difficulties. For example, think before admiring his apparently modern idea that combustion was combination with oxygen — how did he explain the light and heat coming out in the process? Lavoisier had to have recourse to a hypothetical substance called caloric, the fluid of heat, rejected by modern science just as much as

6. And he re-interpreted dephlogisticated air as "dephlogisticated water" (water with a deficit of phlogiston), which then explained the production of water by the combination of dephlogisticated air and inflammable air.

phlogiston is. **Figure 4** shows Lavoisier's table of chemical elements (1789), listing light and caloric as the first two entries.[7]

All in all, in terms of empirical adequacy, simplicity, fruitfulness, or by any other standard epistemic criteria of judgment, it is not clear that phlogiston chemistry was unequivocally inferior to Lavoisier's oxygen chemistry; the more seriously I examined this history, the less convinced I became. The first chapter of my book gives a detailed account of this episode.

I came to conclude that chemists in the late 18th century should not have abandoned phlogiston. I think science lost something when it killed phlogiston, and I am in fact not entirely alone in that opinion. William Odling, a leading theoretical chemist of Victorian Britain, thought that the phlogiston theorists were making advances towards energy-based chemical dynamics, which were stopped by the triumph of Lavoisier.[8] Even more striking are the musings of the renowned early 20th-century American chemist G. N. Lewis, who did pioneering work on the role of electrons in chemical bonds and also gave us the famous definition of acidity. He said in 1926: "It is only in the last few years that we have realized that every process that we call reduction or oxidation is the gain or loss of an almost imponderable [weightless] substance, which we do not call phlogiston but electrons."[9] The link between phlogiston and electricity was not a retrospective fabrication. At least 23 authors from the 18th century are on record as having highlighted this connection. These included Priestley, and also the apothecary John Elliott, who even proposed in 1780 that phlogiston should be re-named "electron"![10]

What if scientists had kept phlogiston? It is difficult to write counterfactual

7. Antoine-Laurent Lavoisier, *Traité élémentaire de chimie* (Paris: Cuchet, 1789), p. 192.

8. William Odling, "On the Revived Theory of Phlogiston" (Address at the Royal Institution, 28 April 1871), *Proceedings of the Royal Institution of Great Britain* 6 (1870–72), pp. 315–325.

9. Gilbert Newton Lewis, *The Anatomy of Science* (New Haven: Yale University Press, 1926), pp. 167–168.

10. John Elliott, *Philosophical Observations on the Senses of Vision and Hearing; to which are added, a Treatise on Harmonic Sounds, and an Essay on Combustion and Animal Heat* (London: J. Murray, 1780), p. 92.

	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.

Figure 4. Lavoisier's table of simple substances; the left-hand column gives his neologisms, and the right-hand column gives the corresponding old terms.

history of science, but imagine chemists and physicists working through the 19th century with the notion that metals were full of phlogiston, and phlogiston was somehow connected to electricity. Would they not have tried to isolate this substance? They might have used ultraviolet light (which was known by 1802), in which case they would have discovered the photoelectric effect. Or they might have tried to pass phlogiston between metal electrodes, evacuating the space between them in order to help its passage; then they would have made cathode rays. It wouldn't have been left to J. J. Thomson and others 100 years later to discover the electron.

In summary: I believe that the exclusive adoption of the Lavoisierian system actually retarded the progress of chemistry in significant ways. Not only did it deprive people of the understanding of chemical processes that they had reached in terms of phlogiston, but it de-legitimized some important scientific questions, and closed off some fruitful avenues of investigation. It was not Lavoisier's chemistry itself that had these unfortunate effects, but its exclusive and dogmatic adoption by chemists.[11]

11. As Priestley pointed out with bitter irony, it was the same spirit of dogmatism infiltrating the French Revolution that led to Lavoisier's execution at the guillotine in 1794. The dogmatism of counter-revolution in England drove Priestley, who was a notable supporter of the French Revolution, to exile in rural Pennsylvania, where he died a sad and isolated figure.

3.
Pluralism as a method of flourishing under limitations

So my study of phlogiston and the Chemical Revolution made me open to pluralism concerning science. Further historical research made me realize that scientists have in fact often allowed various systems simultaneously. Lavoisier is the exception here, rather than the rule — and so are Newton and Einstein. But the ruling monist ideology in science tends to highlight and glorify those exceptions. As an antidote, I want to give you two examples from my book that exhibit more pluralist modes of science.

3.1. Responses to the "distance problem" in electrolysis

In chapter 2 of the book I discuss the electrolysis of water (see **Figure 5** for a convenient modern representation).[12] This was achieved by William Nicholson and Anthony Carlisle in London very soon after Alessandro Volta's announcement of the battery in 1800, opening up the entire new field of electrochemistry. If anyone was in doubt about the compound nature of water even after Lavoisier's work, this clean decomposition of water into hydrogen and oxygen by the power of electricity should have convinced them — one might think.

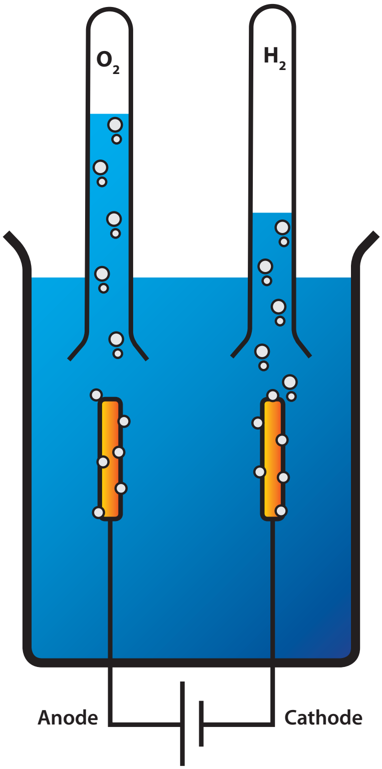

Figure 5. The electrolysis of water

12. https://commons.wikimedia.org/wiki/File%3ASchemas_electrolyse_h2o.jpg

But the story is not so simple. Nicholson and Carlisle themselves already noted a problem in their original publication. If what we are doing is using electricity to break up each particle of water, why does the oxygen emerge from one electrode, and the hydrogen from the other, at macroscopically separated locations?[13] In some later experiments, a distance of as much as 3 feet was achieved.

This difficulty, which I call the "distance problem", plagued electrochemistry for many years. No closure on this question came until the idea of free ionic dissociation was established around 1900. That is to say, a whole century of electrochemistry developed without the resolution of this key foundational debate. There was no shortage of competing hypotheses, and theories underlying those hypotheses, but no universal agreement on which one provided the right solution. It is interesting to review some of the leading solutions that were proposed to the distance problem.

Some seized on the distance problem as evidence that the Lavoisierian theory about the composition of water was defective, after all. The most striking example is Johann Wilhelm Ritter, who argued that what happened when electricity was passed through water was *synthesis*, not decomposition: at the positive electrode, positive electricity combines with water and creates oxygen; at the negative electrode, negative electricity combines with water and creates hydrogen. This is why the two gases come out at separate places, at the spots where the two different types of electricity are supplied. So water is seen again as an element, and oxygen and hydrogen as modifications of water by means of another substance, namely phlogiston, or electricity.

All of this came as a very unpleasant surprise to the followers of Lavoisier.

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13. Nicholson's account of their work stated: "it was with no little surprize that we found the hydrogen extricated at the contact with one wire, while the oxigen fixed itself in combination with the other wire at the distance of almost two inches. This new fact still remains to be explained". See William Nicholson, "Account of the new Electrical or Galvanic Apparatus of Sig. Alessandro Volta, and Experiments Performed with the Same", *A Journal of Natural Philosophy, Chemistry and the Arts* 4 (1800), pp. 179–187, on p. 183.

Some other explanation had to be found. The most popular one came from Theodor von Grotthuss, involving an invisible chain of water molecules connecting the two electrodes. In **Figure 6**, a drawing published by Grotthuss himself, each water molecule is electrically polarized, with hydrogen being positive and oxygen being negative.[14] The molecules connect up in a chain by electrostatic attraction, like a set of little bar magnets. When the current starts flowing, decomposition begins. The negative electrode takes the hydrogen particle (electro-positive) from the water molecule right next to it,

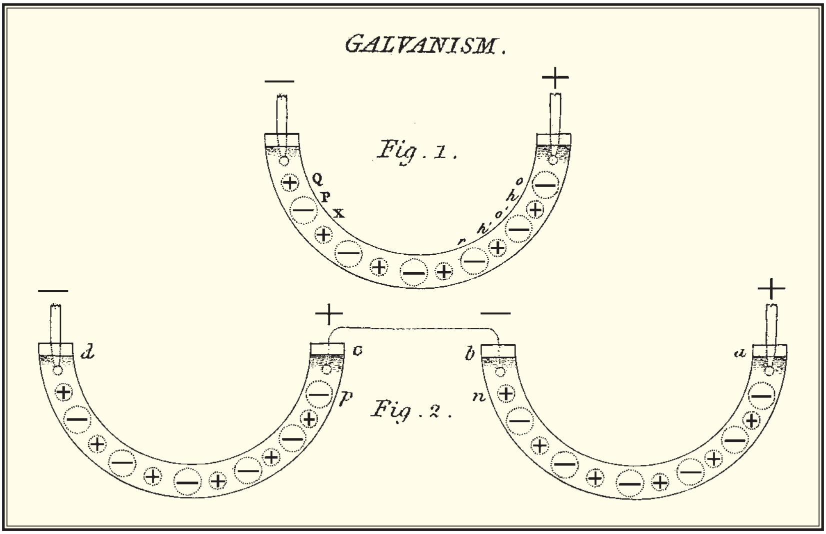

Figure 6. The Grotthuss mechanism proposed as a solution to the distance problem in electrolysis

makes it neutral by giving it negative electricity, and releases it in the form of neutral hydrogen gas. Having lost its partner, the oxygen particle from that water molecule then takes the hydrogen particle from the next molecule,

14. 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), pp. 330–330, Plate IX.

forming a new water molecule. This partner-switch is propagated throughout the chain. And then each of the newly-formed water molecules flips around, due to the electrical repulsion/attraction from the electrodes, so the initial sort of configuration is restored.

A nice story, but not everyone was convinced. Michael Faraday by the 1830s was critical of all earlier theories, and introduced a complex view involving his peculiar notion of force. Rudolf Clausius thought that all possible combinations of the atoms involved must exist, as random chance dictates; so water must contain not only H_2O molecules, but single H and O atoms, and all other combinations – HO, HO_2, H_2O_2, and so on. It seems that everyone had something, and something different, to say about this issue.

Historical accounts of electrochemistry tend to minimize this unruly mid-century period, ignoring everything that came between Humphry Davy and Svante Arrhenius, except perhaps an uncomfortable mention of Faraday in the middle. During this period there was no theoretical consensus on the distance problem, or on the general mechanism of electrolysis, or on the workings of the battery (which is the subject of my next book). Yet there wasn't much anti-realist renunciation of theory, either. The theoretical debate continued robustly, and electrochemists of that time thrived on disagreements, the activity of disputation in fact uniting them into a community — in much the same way as philosophers behave. On the other hand, the experimental practices became relatively stable, standardized and unified. Electrochemistry flourished in this way. The electrochemists were entirely rational not to settle on one theory in that situation.

3.2. Five systems of atomic-molecular chemistry

In chapter 3 of the book I discuss another similar case of pluralism in action. In 19th-century atomic chemistry there were at least five different systems of practice that developed in competition and interaction with each other, which I will characterize very briefly now. By a "system of practice" I mean a coherent set of epistemic activities performed with a view to achieve certain aims. I would like to stress that such systems contain not only different theoretical ideas from each other, but different *activities* geared toward different *aims*.[15]

(1) In the *weight-only system*, chemists focused on deriving atomic weights from the macroscopic combining weights of substances. This system is sometimes characterized as positivist. But as Alan Rocke has argued, it still relied on a notion of chemical atoms (i.e., some minimal units of matter involved in chemical reactions), and what it embodied was not positivism but a skeletal ontology of atoms only possessed of weights. The primary activities of this system were in the realm of analytical chemistry. The main task was ascertaining the composition of each chemical substance in terms of the relative weights of each ingredient, now expressed in terms of atoms. *Explaining* chemical phenomena in any deeper sense was not a priority.

(2) The *electrochemical dualistic system* was firmly based on the electrolysis of various substances using the Voltaic battery. This was a fundamentally different way of *operationalizing* the atom, compared to the atomic-weight determinations. Chemical elements were placed on an electrical spectrum, according to their tendency to appear at the positive or negative electrodes during electrolysis. The explanatory potential in this system was apparent: chemical reactions were understood as consequences of the electrostatic attractions and repulsions between atoms.

(3) In what I call the *physical volume–weight system*, first constructed by Amedeo Avogadro, chemists took not only weights but also volumes as measurable properties of atoms and molecules. The focus was on finding out the atomic–molecular constitution of various substances, not so much on the

15. The notion of a system of practice, similar to paradigms but more precise and more flexible, is a historiographical and philosophical innovation given in this book. It is developed further in Hasok Chang, "Epistemic Activities and Systems of Practice: Units of Analysis in Philosophy of Science After the Practice Turn", in Léna Soler, Sjoerd Zwart, Michael Lynch and Vincent Israel-Jost, eds., *Science After the Practice Turn in the Philosophy, History and Social Studies of Science* (London and Abingdon: Routledge, 2014), pp. 67–79.

explanation of bonding. A key assumption was 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, and also the H_2 and O_2 formulas for hydrogen and oxygen gas, for instance. **Figure 7a** illustrates Avogadro's initial view of the synthesis of water from hydrogen and oxygen. The equal volume—equal number hypothesis seems to yield the H_2O formula, as water is formed by two volumes of hydrogen gas reacting with one volume of oxygen gas. However, the reasoning here would dictate that there should be one volume of water vapor formed in this reaction, while experiment reveals that there are two volumes of vapor produced. Avogadro resolved this problem by a further hypothesis, as illustrated in **Figure 7b**, postulating that hydrogen and oxygen gases were constituted of double-atom molecules, and that the product H_4O_2 would then split in half, yielding two particles of H_2O. But there were various objections to Avogadro's ideas; it seemed arbitrary and physically groundless to assume that two like atoms (and no more) could clump together. This

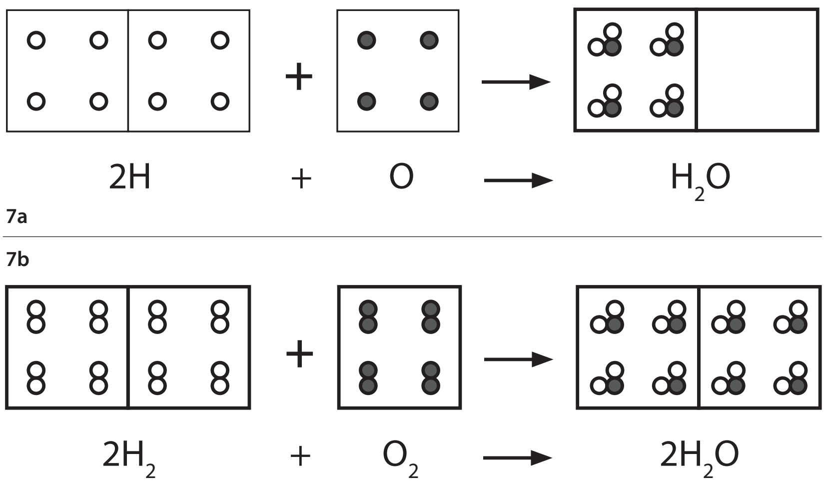

Figure 7a and 7b. Avogadro's reasoning about the composition of water.

was particularly offensive to the practitioners of the electrochemical dualistic system, who could only imagine electrostatic repulsion between two like atoms, both with the same sign of electric charge.

(4) The disillusionment with the physical volume–weight system was probably largely responsible for the rise of 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, anchoring their taxonomy to actually performable operations of substitution. Jean-Baptiste Dumas, for example, came up with the "type theory", which laid down the research program of classifying organic molecules into "types" defined by the structural templates of certain simple inorganic substances, such as water and ammonia. Figure 8 shows Alexander Williamson's grouping of ethyl alcohol and ether as compounds belonging to the water type, formed by the successive substitution of the hydrogen atoms in water by ethyl radicals.

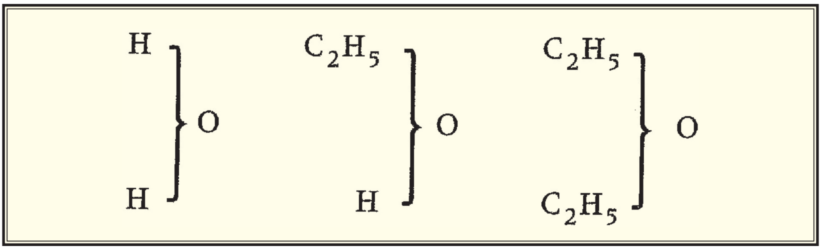

Figure 8. Williamson's representation of water, ethyl alcohol and ether

(5) Many type-theorists 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 practiced what I call the *geometric–structural system*. Often inspired by the crystallographic tradition, these chemists (and physicists) attempted to get directly at the real three-dimensional geometry of molecular structures.

But what good was it to maintain all these different systems? In short, they were good at different things, and there was no single system that could excel in all aspects. Different systems stimulated and supported research in different directions, and served different epistemic aims and values. Even those who aim at an eventual unification of different theories should be able to see that it can be beneficial to have different system develop simultaneously, until a unifying system would later pull them together. By the 1860s, the concept of valency facilitated the synthesis of the last three systems on my list, enabling a consensus on atomic weights and molecular formulas, including H_2O with atomic weights as 1 and 16, coming away from Dalton's HO formula with atomic weights 1 and 8.

But this was not a simple happy-ending of unification. The synthesis was only possible through the renunciation of certain aims. Organic structural chemistry entirely neglected the need to explain how and why chemical bonds were made and broken. Many electrochemists retained the idea of electrostatic attractions and repulsions between atoms, and advanced their science by the discovery of ions. They joined in with those who were concerned about thermodynamics and the kinetic theory of matter, to create the new field of physical chemistry.

So the general picture of chemistry after the grand synthesis of late 19th-century structural theory is not one of a perfect unified science living happily ever after, but another pluralistic configuration of imperfect systems competing and interacting with each other. It is only hubris to think that we are now beyond such primitive beginnings. Thinking about cutting-edge topics such as quantum gravity, dark matter and dark energy, genome–environment interaction, and the nature of consciousness, it is easy to see that the more we know, the more complex things can get. The shining moments of unification are just important stopping points, not the final destinations.

4.
Benefits of pluralism

I hope that these highly condensed historical accounts are sufficient to convey some concrete sense of the pluralism I have in mind. Now I would like to give a more general discussion and defence of pluralism, again very briefly. By "pluralism" about knowledge I mean a commitment to maintain multiple systems of practice in each field of inquiry. Pluralism about science is an unusual and controversial stance, so it requires a careful defence, which I attempt to provide in chapter 5 of the book.

One important lesson that I learned from observing Peter Lipton, my predecessor at Cambridge, is that every significant philosophical point can be expressed in a joke. So here is one for pluralism. It's a brief dialogue at school:

> *Teacher:* Your composition on "My Dog" is exactly the same as your brother's. Did you copy his?
> *Student:* No, sir. It's the *same dog*.

I wish one day we could have crowds of people laughing at the suggestion that there must be only one right scientific theory in each field because it's the same reality we're talking about. That is my dream, a counter to the age-old dream of the single unified theory of the universe.

4.1. Benefits of toleration

The main argument in favor of pluralism is that different benefits will spring from different systems of practice, and from their interactions with each other.

What I call "benefits of toleration" come from simply allowing various systems to co-exist, with sufficient respect for each other, so that each system can pursue its own potential. There is an immediate point: given the unpredictability of scientific development, it is rational to keep multiple lines of inquiry open. Larry Laudan's pessimistic meta-induction from the history of science is a good reminder of the ultimate insecurity of our theoretical positions.[16] Kyle Stanford's "problem of unconceived alternatives" has the same disturbing effect on any alleged security about the theory-choices that scientists make.[17] Rational agents faced with unpredictability ought to hedge their bets, even if what they are doing is searching for the one ultimate truth. To put it in Bayesian terms, all theories with non-negligible prior probabilities should be monitored for signs of life (that is, increases in posterior probabilities) as further evidence comes in. It is most irrational to insist that only the theory with the highest probability *at the moment* should be preserved and all others killed off.

There is also a consideration of division of labor. For example, if we want empirical adequacy for actual applications, we should feel quite free to maintain multiple theories if they are good in different domains, even if they contradict each other in some deep sense. And sometimes it is good to have one aim satisfied in multiple ways. For example, having multiple accounts of the same phenomena enriches our understanding, even if they are empirically equivalent to each other. So give me the Heisenberg, Schrödinger, Dirac, Feynman, *and* Bohm formulations of quantum mechanics; each provides a different kind of insight, and an additional joy of understanding.[18] Add to all this the fact that people have different aims and each of us may have multiple aims, and the case for pluralism becomes even stronger; there is no convincing reason to think that science has only one overriding aim or value.

16. Larry Laudan, "A Confutation of Convergent Realism", *Philosophy of Science* 48 (1981), pp. 19–49.

17. P. Kyle Stanford, *Exceeding Our Grasp: Science, History and the Problem of Unconceived Alternatives* (New York: Oxford University Press, 2006).

18. Recall Pierre Duhem's infamous declaration that the "ample and weak" mind of the English physicist could only understand something by making a mechanical model of it; the "strong and narrow" mind of the French physicist derived all the necessary understanding from formal mathematical systems, with no need for childish models. But if the English are the way they are, I think they should be entitled to understand the universe in a way that is satisfying to them. See Pierre Duhem, *The Aim and Structure of Physical Theory* (New York: Atheneum, [1906] 1962), pp. 64ff.

And once we grant that there are multiple human needs that science is called upon to satisfy, it is easy to recognize that we will most likely not be able to come up with *the* perfect system that satisfies all needs.

4.2. Benefits of interaction

So much for the benefits of toleration. A whole other set of benefits arise from having multiple systems not only co-exist but interact with each other. In situations where none of the available systems by itself can achieve a certain aim well enough, we may attempt to do better by an *ad hoc* integration of different systems. Sandra Mitchell observes that complex biological systems, such as communities of social insects, can only be understood by integrating different models in this way.[19] My favorite example is the global positioning system: GPS uses satellites kept in place by Newtonian physics, and atomic clocks ruled by quantum mechanics and corrected by special and general relativity; with all that it maps the surface of the round earth on a geostatic grid, and gives advice to people on the ground from a flat-earth point of view.

Even when different systems are not being pulled together to achieve a specific aim, one system of practice can benefit from the co-optation of empirical results, theoretical ideas, mathematical techniques, instruments or materials borrowed from another system, even an apparently opposing one. For example, Lavoisier would not have arrived at his new chemistry without co-opting phlogistonist results, such as Priestley's production of oxygen and Cavendish's work on the composition of water.[20]

And even in the absence of integration or co-optation, different systems can interact through competition. Philosophers of science typically mistake competition as the mere score-keeping of successes and failures. But in fact

19. Sandra D. Mitchell, *Biological Complexity and Integrative Pluralism* (Cambridge: Cambridge University Press, 2003).

20. Why would elements of knowledge need to be co-opted from another system, rather than developed within one's own system? This is because each system develops under certain constraints, which may prevent the production of elements that would actually help its own progress.

competition sharpens our view of each system, making it more difficult to be complacent about our assumptions or to leave weak arguments unexposed. And success in a neighboring system may induce us to pay attention to aims that we might otherwise neglect. Conversely, it is also useful to keep even failing systems around, as a reminder of valuable unachieved aims.

5.
HPS in the service of pluralism in and about science

I would now like to return to the question about the nature and function of the field that we call philosophy of science, a subject that we are honoring by devoting the legacy of Fernando Gil to it. My own preference, which may not have been Professor Gil's own, is always to take it in a form that is integrated with history of science.

History and philosophy of science (HPS), as a distinct academic discipline, *is* and *should be* an expression of pluralism concerning science. Such pluralism is not currently orthodox, either within science or in philosophy of science. Modern science has developed largely in a monist spirit — looking for the one scientific truth about the one reality that we all inhabit, assuming that there is one right answer to each scientific question, and one best method for arriving at that right answer.[21] History and philosophy of science can only become really significant if it rejects such monism. Let's ask: why should any talented scholars want to devote their lives to HPS — to what may seem like a futile study of the long-abandoned past of science, or an idle questioning of the nature and foundations of scientific knowledge? Because such investigations are not truly futile or idle, no matter what mainstream scientists may tell us. Because it is possible, and beneficial, to consider alternative ways of knowing. Because it is important that we preserve our capacity to make independent judgments about the claims and the merits of science, rather than allowing ourselves to become blinded by the brilliant light of the astonishing successes of some parts and aspects of modern science. HPS practiced in this pluralist spirit opens our scientific minds, and that is good for science, and good for all of us who rely on science in our lives.

21. Adding this monism to the typical optimism of scientific realists produces the common notion that today's science is so successful that it has at least got the basic outlines of the right answers about many aspects of nature, so much so that scientific people feel entitled to declare as absurd or irrational anything that conflicts with the current scientific wisdom.

These reflections on the role of HPS raise a significant question about the general place of philosophy in modern society. Nowadays we have a common impression that philosophers just sit around and think about useless things, while scientists make real investigations and deliver real results. Even professional philosophers feel the pressure of the success of science and often respond with a subservient naturalism, which would reduce epistemology to cognitive psychology or neurophysiology, ethics to evolutionary biology, and metaphysics to physics and cosmology.[22] I wish to resist this self-denigrating naturalism in philosophy, fashionable as it is these days. The relation between philosophy and science needs to be seen in a new and different light.

Let's ask again: what is the use of philosophy? Let me propose a paradox: *philosophy is useful precisely because it is useless*. Now I must spell out what I really mean by that. We tend to call something a "philosophical" question if it is something that we do not normally need to deal with in the course of routine action. When we say "the philosophy of *X*," we mean a field of study which deals with questions that are relevant to another field *X* but normally not addressed in *X* itself. There are various reasons why relevant questions may be excluded. The questions may be too general; they may threaten some basic beliefs within one's system; asking them may be pointless because every specialist knows and agrees on the correct answers; the answers may not make any significant practical difference; and so on. And in the end, questioning has to be selective because it is simply impossible to ask the infinity of all possible questions. But philosophy can function as the embodiment of the ideal of openness, or at least a reluctance to place restrictions on the range of valid questions. *Professional* philosophy exists so that seemingly useless questions, and our capacity to ask such questions, are preserved for society. These questions may become relevant one day, and they may be important

22. It is an absurd conceit that we philosophers can "think" better than anyone, so we can step in and draw some wise conclusions from the scientific material, which scientists themselves are missing because they are sloppy or limited in their thinking.

for some people today, too. Philosophy of science exists so that scientific knowledge can be preserved and developed in a broad sense that goes beyond the currently dominant paradigms. In that sense it is an inherently pluralist enterprise.[23]

To serve this pluralist function properly, the philosopher of science needs to delve seriously into the details of the science under examination. It makes little scientific impact to talk about the underdetermination of theory by evidence, for example, if we do not consider actual alternatives to actual theories. But shouldn't philosophy concern itself with general principles of knowledge, rather than getting mixed up in specific details of science? I couldn't disagree more. General and abstract principles certainly fall into the domain of philosophy, but only where science neglects them. It is useful to think back to the time when all scholarly endeavor was considered "philosophy"; after every successful specialism carved itself out, what we now call philosophy is the leftovers, the inconvenient and awkward questions. It is an unhelpful conceit to think that we philosophers only deal with the lofty universal questions. There are also concrete questions that we ought to worry about, if no one else does.

Taking a pluralist viewpoint allows us to recognize that any specialist system of science only deals with a restricted range of things in a restricted range of ways. No system of knowledge in a field of science should be given the right to suppress, exclude, or de-legitimize all others. We need as many of the best systems as we can afford to maintain, in order to give ourselves maximal exposure to reality. Gaining knowledge in such a maximal way also contributes to the full actualization of human potentials. After many centuries of struggle, we have learned to accept and benefit from a spirit of pluralism in the realms of politics, culture, language, cuisine, art, and ecology. I believe

23. Is philosophy as I conceive it a normative enterprise in relation to the practices that it considers? More specifically, is philosophy of science normative in relation to science? These are difficult questions to answer unequivocally, and I think the subtlety of the issue can be captured as follows: philosophy of science can be *critical* without being *prescriptive* in relation to specialist science.

it is time to admit it in science, too. Pluralism will not only enrich scientific knowledge itself, but also contribute to the maturing of the role of science in society, by helping science move beyond the arrogance of youth and enter into an open-minded and constructive engagement with other spheres of life. Pluralist History and Philosophy of Science could trigger a decisive transformation in the nature of science. Without interfering with the ongoing work of today's scientific specialists, we can cultivate a complementary kind of science that recovers lost knowledge from the past, extends that recovered knowledge, and raises critical awareness about all knowledge. HPS seriously undersells itself if it does not claim its *knowledge-generating function* in relation to science. When we dig up strange past systems of knowledge and make sense of them, or when we recover forgotten experimental results, we are enriching the stock of human scientific knowledge. Even mere critical awareness enhances the quality of knowledge, because we do not truly know anything unless we know how we know it. And if critique should actually lead to alternatives, even better. Pluralism gives us this broad view on what constitutes scientific knowledge, and shows how history and philosophy of science can contribute to it. A full integration of pluralist HPS of science into science education and public intellectual life would be a momentous step, enabling the educated public to participate once again in the cultivation of our knowledge of nature.

6.
Two metaphorical images

In closing, I would like to present two images that capture my view of science and its pluralistic development.

One important attitude underlying pluralism is humility concerning our own abilities and achievements. Priestley had a particularly instructive notion of humility, which was dynamic: "every discovery brings to our view many things of which we had no intimation before". He had a wonderful image for this, shown in **Figure 9** here: "The greater is the circle of light, the greater is the boundary of the darkness by which it is confined." As knowledge grows, so does ignorance. Priestley continued:

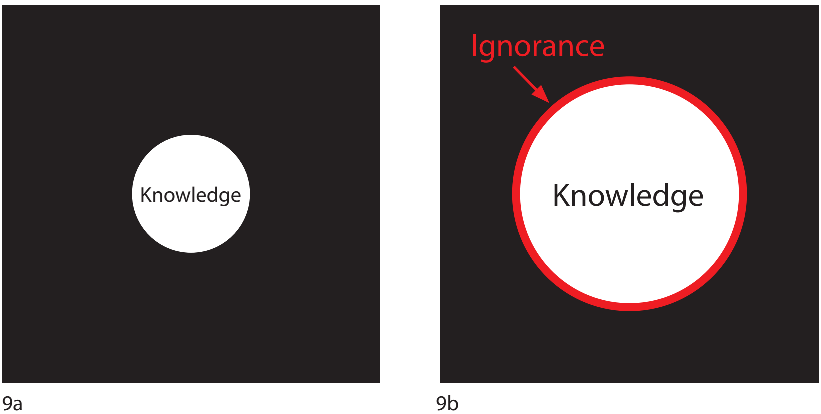

Figure 9a and 9b. Priestley's metaphor concerning the growth of knowledge and ignorance

"But notwithstanding this, the more light we get, the more thankful we ought to be. For by this means we have the greater range for satisfactory contemplation. In time the bounds of light will be still farther extended; and from the infinity of the divine nature and the divine works, we may promise ourselves an endless progress in our investigation of them: a prospect truly sublime and glorious."[24] For Priestley this was a theological vision, but it can be simply about the abundance of nature.

The next image, shown in **Figure 10**, is a representation of the shape of knowledge cultivated in a pluralist way. This is a silver tree, which I learned to grow from a paper published in 1806 by a long-forgotten author, Charles Sylvester, who gave the following simple instructions: "If a thin coat of a solution of nitrate of silver be laid upon a piece of glass, and in the centre of this be laid a bit of zinc wire, in a little time a beautiful tree of silver will appear as if growing from the wire."[25] I used a copper wire instead of Sylvester's zinc, and a clear plastic envelope instead of a glass pane. When the copper is inserted into the solution, it gets immediately covered in a deposit of silver; such replacement reactions had been well-known to chemists and alchemists for centuries. But in Sylvester's experiment the silver deposit keeps growing. It is very interesting to ask why silver would grow on silver, and in these sharp branch formations. This is a piece of scientific knowledge that I recovered from 200-year-old science, which comes as a delightful little surprise even to many professional chemists today. The image is also a fitting metaphor for the beautiful and abundant shape of knowledge that may be cultivated in a pluralist science.

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24. Joseph Priestley, *Experiments and Observations on Different Kinds of Air, and Other Branches of Natural Philosophy, Connected with the Subject*, 2nd ed. (Birmingham: Thomas Pearson, 1790), vol. 1, pp. xviii-xix.

25. Charles Sylvester, "Observations and Experiments on Galvanism, the Precipitation of Metals by each other, and the Production of Muriatic Acid", *A Journal of Natural Philosophy, Chemistry, and the Arts* 14 (1806), pp. 94–98, on p. 96. Similar metallic trees are often produced these days, but usually they are made with the help of external batteries. Sylvester's experiment drives itself; it is also far simpler than the procedures that alchemists had worked out for producing similar effects. In my modern rendition of the experiment shown here, a 1-molar solution of silver nitrate ($AgNO_3$) works very well. It is shown here against a dark green background, a waterproof cardboard piece.

Figure 10. A silver tree

Hasok Chang biography

Hasok Chang is Hans Rausing Professor of History and Philosophy of Science at the University of Cambridge. Previously he taught for 15 years at University College London, after receiving his PhD in Philosophy at Stanford University following an undergraduate degree at the California Institute of Technology. He is the author of *Is Water H_2O? Evidence, Realism and Pluralism* (Springer, 2012), winner of the 2013 Fernando Gil International Prize, and *Inventing Temperature: Measurement and Scientific Progress* (Oxford University Press, 2004), joint winner of the 2006 Lakatos Award. He is also co-editor (with Catherine Jackson) of *An Element of Controversy: The Life of Chlorine in Science, Medicine, Technology and War* (British Society for the History of Science, 2007), a collection of original work by undergraduate students at University College London. He is a co-founder of the Society for Philosophy of Science in Practice (SPSP), and the Committee for Integrated History and Philosophy of Science. He has recently been the President of the British Society for the History of Science.

Fernando Gil's Life

The Philosophy as a whole

The philosophical works of Fernando Gil embrace a diversity of fields, ranging from epistemology to esthetics, from moral philosophy to politics. It is however notable that whatever field he worked in, he always did it from the perspective of philosophy perceived as a whole, i.e., from the conviction that each specific knowledge refers to all the others and somehow presumes them.

Science, esthetic, ethic

His works on scientific knowledge, for instance, in *Mimesis e Negação* (IN/CM, Lisbon, 1984) and particularly in *Provas* (IN/CM, Lisbon, 1986), already contain the appeal to an approach that favors the role of the subject in knowledge like we find in his later works such as *Tratado da Evidência* (IN/CM, Lisbon, 1996 [*Traité de l'évidence*, Millon, Grenoble, 1993]) or *A convicção* (Campo das Letras, Oporto, 2003; [*La Conviction*, Flammarion, Paris, 2000]). These in turn refer, in a clear and elaborated way, to the esthetic experience, subject of *Viagens do Olhar*. *Retrospecção, Visão e Profecia no Renascimento Português*, in collaboration with Helder Macedo (Campo das Letras, Oporto, 1998) and of *A Quatro Mãos. Schumann, Eichendorff e outras notas*, in collaboration with Mário Vieira de Carvalho (IN/CM, Lisbon, 2005). The questions regarding proof and conviction are still present both in moral philosophy (*O Hospital e a Lei Moral*, Atlântico, nº. 7, 2005, pp. 29-31) and in political philosophy (*Impasses, seguido de Coisas Vistas, Coisas Ouvidas,* in collaboration with Paulo Tunhas and Danièle Cohn, (Europa-América, Mem Martins, 2003).

In his first books we find, both from the perspective of a philosophical anthropology of phenomenological origin (*Aproximação Antropológica*, Guimarães Editores, Lisbon, 1961) and of philosophical logics (*La Logique du Nom*, L'Herne, Paris, 1972), themes and intuitions that his later works explored with greater richness and depth. The three books in which Fernando Gil gathers articles not integrated in the previously mentioned works (*Modos da Evidência*, IN/CM, Lisbon, 1998; *Mediações*, IN/CM, Lisbon, 2001; *Acentos*, IN/CM, Lisbon, 2005) show the inner articulation of the various objects of thought and the search for links between them.

Teaching and the transmission of knowledge

It is therefore wrong to understand Fernando Gil as an encyclopedist, even though, and besides his various collaborations in encyclopedias, the reading of some of his texts (cf. *Cruzamentos da Enciclopédia*, Prelo, special number, 1986) or even his personal path as a university student – first in Johannesburg, then in Lisbon, in law, and then, already in Paris and before the doctorate in logics, an unfinished thesis about Céline under the supervision of Lucien Goldmann – might give this idea. The philosophy of Fernando Gil is a unifying philosophy — insofar as such is possible in any philosophy —, that is to say, a philosophy that, without concealing discontinuities, intends to search for the connections between the various domains of human thinking. Leibniz and Cassirer are predecessors that easily cross one's mind.
His teaching obviously reflected this general tendency of his thinking. In Portugal, first in the University of Lisbon, as from 1976, and later, as from 1979, in the Faculty of Social and Human Sciences of the New University of Lisbon where he was, from 1988 on, Full Professor of Philosophy of Knowledge. And in France, where he was, since 1989, "Director of Studies" in the École des Hautes Études en Sciences Sociales, Paris. Furthermore, he was a Visiting Professor at several universities, namely, in the last years of his life, at the Johns Hopkins University, Baltimore.

Contributions for the development of phylosophic and scientific knowledge in Portugal

Besides teaching, Fernando Gil was involved in the work for different encyclopedias. He was consultant of the Encyclopedia Britannica, of the Encyclopedia Universalis and the Encyclopedia Einaudi (of which he co-ordinated a Portuguese edition, published by Imprensa Nacional, 1984). He also made all his knowledge available for projects promoting the development of philosophical and scientific knowledge in Portugal. In this sense, he was special adviser to the President of the Republic, Mário Soares, during both his mandates, as well as to the Minister of Science and Technology, Mariano Gago. He also was a permanent collaborator of the Foundation for Science and Technology and, later, of the Calouste Gulbenkian Foundation. The effects of these collaborations were decisive for the development of philosophical research in Portugal, which owes him very much.

Some of the works he directed testify to his extraordinary dedication, namely *Controvérsias científicas e filosóficas* (Fragmentos, Lisbon, 1990), *O balanço do século* (IN/CM, Lisbon, 1990), *A ciência como cultura* (IN/CM, Lisbon, 1992) and *A ciência tal qual se faz* (Lisbon, Sá da Costa, 1999). And also *Fichte: crença, imaginação, temporalidade*, organized with Virginia López-Domínguez and Luísa Couto-Soares (Campo das Letras, Oporto, 2002), *O processo da crença*, coordinated in collaboration with Pierre Livet and João Pina Cabral (Gradiva, Lisbon, 2004) and *Terrorismo e relações internacionais* (Gradiva and Calouste Gulbenkian Foundation, Lisbon, 2006). And, maybe even more important in thias context than the previously mentioned works, the creation of the magazine *Análise* in 1984, for whose coordination he was responsible until edition number 20 (1998), following more ephemeral projects like *Estudos filosóficos* and *Filosofia e epistemologia*.

Work impact

The importance of Fernando Gil for philosophy – that led, among other editions, to the publication of books such as those organized by António Braz Teixeira *A razão apaixonada*, (IN/CM, Lisbon, 2008) and Maria Filomena Molder *Paisagens dos confins. Fernando Gil* (Vendaval, Lisbon, 2009) – is not limited to the just mentioned.
The impact of his philosophical activity manifests itself in the influence of his works, especially in Portugal, Brazil, France and Italy, and on the philosophical constructions that owe him their source of inspiration.

Distinctions

Finally, some prizes and honours he was awarded should be mentioned. Grande Oficial da Ordem do Infante D. Henrique (1992), Prémio Pessoa (1993), Chevalier de l'Ordre des Palmes Académiques (1995), Doctor Honoris Causa of the University of Aveiro (1998). Two of his books were also chosen for awards: *Mimésis e Negação* ("Prémio de Ensaio" of the Portuguese PEN Club, 1985), *Viagens do Olhar. Retrospecção, Visão e Profecia no Renascimento Português*, in collaboration with Helder Macedo (Prize Jacinto Prado Coelho, 1998, "Prémio de Ensaio" of the Portuguese PEN Club, 1988).

Works by Fernando Gil

· *Aproximação Antropológica*, Guimarães Editores, Lisboa, 1961
· *La Logique du Nom*, L'Herne, Paris, 1972
· *Mimésis e Negação*, IN/CM, Lisboa, 1984
· *Provas*, IN/CM, Lisboa, 1986
·*Traité de l'évidence*, Millon, Grenoble, 1993
· *Modos da Evidência*, IN/CM, Lisboa, 1998
· *Viagens do Olhar. Retrospecção, Visão e Profecia no Renascimento Português*, em colaboração com Helder Macedo, Campo das Letras, Porto, 1998
· *La Conviction*, Flammarion, Paris, 2000
· *Mediações*, IN/CM, Lisboa, 2001
· *Impasses, seguido de Coisas Vistas, Coisas Ouvidas*, em colaboração com Paulo Tunhas e Danièle Cohn, Europa-América, Mem Martins, 2003
· *A Quatro Mãos. Schumann, Eichendorff e outras notas*, em colaboração com Mário Vieira de Carvalho, IN/CM, Lisboa, 2005
· *Acentos*, IN/CM, Lisboa, 2005