A PRIORI COMPONENTS OF SCIENCE: LAVOISIER AND THE LAW OF CONSERVATION OF MASS IN CHEMICAL REACTIONS

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Abstract: In his book “Identité et réalité”, Émile Meyerson argued for a philosophical *a priori* background in the case of all scientific conservation laws. This work discusses one of the specific cases he addressed, the conservation of mass in chemical reactions. It analyzes the attitudes of Antoine Laurent de Lavoisier and Hans Landolt regarding their own experiments concerning this law of conservation. This case study is relevant for the teaching of chemistry (and of science, in general), because it clearly shows the influence of philosophical principles on the development of science, thereby providing a nice example against the inductivist view of science.

Keywords: mass conservation; a priori laws; history of chemistry; philosophy of chemistry; philosophy of science; Lavoisier, Antoine-Laurent; Landolt, Hans; Meyerson, Émile

1. INTRODUCTION

One of the most important principles of science is the law[1] of conservation of mass (or conservation of matter). It is usually

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[1] In this paper, “law” and “principle” will be used as equivalent terms, just for avoiding the too frequent repetition of the word “principle”.

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associated to the name of Antoine-Laurent de Lavoisier (1743-1794) or, sometimes, to that of Mikhail Vasilyevich Lomonosov (1711-1765). The usual attitude of science teachers towards this law (and to other ones) is to regard it as *true*, and to acknowledge that it was *proved*, by a series of *experiments*. Because of its eponym, it is usually thought that a single person called Lavoisier (or, perhaps, Lomonosov) was the one who proved it; and that interpretation is made more convincing by adding a specific date to the historic event.

This is the naïve view conveyed by many textbooks and popular works about the law of conservation of mass. Of course, it is completely mistaken. We know that it is impossible to provide an *experimental proof* that mass (or weight, or matter) is exactly conserved in every imaginable closed system. And the historians of science who have carefully studied the development of this law have shown that Lavoisier has never attempted to prove or to test this law – he simply stated it and used it. Even the weaker assertion that Lavoisier *discovered* the law of conservation of mass by experiments is also false.

The French philosopher of science Émile Meyerson (1859-1933) argued that this and other conservation laws have a philosophical *a priori* foundation that makes them easily acceptable; and that it was only submitted to experimental tests in the late nineteenth and early twentieth centuries. Although Meyerson's account on the principle of mass conservation was written over one century ago, in my opinion his historical description and his epistemological analysis have not been superseded. For that reason, the present article closely follows his interpretation.

This paper addresses the history and the philosophical status of the law of conservation of mass, focusing especially upon the works of Antoine-Laurent de Lavoisier and Hans Heinrich Landolt (1831-1910) – a Swiss chemist who made the most rigorous series of test of that law, around 1900. Given the scientific importance of mass conservation and the flawed exposition of its foundation in teaching, it is claimed that a more careful presentation of this episode might be a significant educational contribution.[2]

2. THE POPULAR VERSION: EXPERIMENTAL DEMONSTRATION

Nowadays, the Internet is a popular source of information consulted by students and teachers; and Wikipedia is one of the sites most frequently accessed by them. Some of its articles have a high quality, but that is not the case of the one concerning the conservation of mass, which contains faulty historical information:

> Historically, mass conservation was demonstrated in chemical reactions independently by Mikhail Lomonosov and later rediscovered by Antoine Lavoisier in the late 18th century. […]
>
> By the 18th century the principle of conservation of mass during chemical reactions was widely used and was an important assumption during experiments, even before a definition was formally established, as can be seen in the works of Joseph Black, Henry Cavendish, and Jean Rey. The first to outline the principle was given by [*sic*] Mikhail Lomonosov in 1756. He demonstrated it by experiments and had discussed the principle before in 1774 [*sic*] in correspondence with Leonhard Euler, though his claim on the subject is sometimes challenged. A more refined series of experiments were [*sic*] later carried out by Antoine Lavoisier who expressed his conclusion in 1773 and popularized the principle of conservation of mass. The demonstrations of the principle led alternatives theories obsolete, like the phlogiston

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[2] This essay was written for presentation at the XI International Conference on History of Science and Science Education (ICHSSE), August 29-31 2018, State University of Paraiba, Brazil. A shorter version, in Portuguese, was published the next year (Martins, 2019a). The English version was circulated during the Conference, but it is now being published for the first time.

> theory that claimed that mass could be gained or lost in combustion and heat processes.[3]

The Wikipedia version emphasizes the idea of demonstration by experiments, either by Lomonosov[4] or by Lavoisier, stating the years when they presented the principle. Both in older and in more recent textbooks we can find similar statements concerning Lavoisier's experimental demonstration (or proof) of the law:

> Using precise balances, Lavoisier was able to demonstrate what other investigators had suspected: The quantity of matter does not change during a chemical reaction. This is the law of conservation of mass […] (Sibring & Schaff, 1980, p. 34)
>
> The first experiments that conclusively demonstrated mass conservation were conducted by Antoine Lavoisier, a French chemist, just before the French Revolution. […] Lavoisier provided the first quantitative proof of the important mass conservation principle. (Marion, 2014, p. 27)

It looks as if most of those authors have managed to reconstruct the past without the inconveniency of consulting the relevant historical sources. If one believes that scientific laws are amenable to experimental proof and if the law of mass conservation is associated to the name of Lavoisier, it seems likely that this scientist was the one who provided the experimental proof of the law.

Unfortunately, when one looks at the past through the glasses of present science and a faulty view on the nature of science,

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[3] Wikipedia's article "Conservation of mass". Available at <https://en.wikipedia.org/wiki/Conservation_of_mass>. Accessed 17 August 2018.

[4] Lomonosov's contribution will not be discussed in this paper. Those interested in the subject may consult papers by Philip Pomper (1962) and Henry Leicester (1975). See also the book by Steven Usitalo (2013) on Lomonosov.

history inevitably becomes opaque. One only sees what he/she wants to see, not what really happened. That is the main origin of pseudohistory.

3. THE SEMI-POPULAR VERSION: EXPERIMENTAL DISCOVERY

Instead of *demonstration*, a few authors refer to an experimental *discovery*: “By carefully measuring the weight of each substance, Lavoisier discovered that matter is neither created nor destroyed during a chemical reaction. It may change from one form to another, but it can always be found, or accounted for” (Haven, 2007, p. 47). “Lavoisier experimentally discovered, around 1785, the law of conservation of mass” (Paty, 1999, p. 614).

It is noteworthy that the association between experiments and the *discovery* of the law of mass conservation by Lavoisier is a version accepted by several philosophers of science. In those cases, it seems that the authors are reconstructing the past from their epistemological beliefs. They know that there can be no experimental *demonstration* or *proof* of a general law from experiments; however, they do accept that, in the context of discovery, experiments may provide the inductive hint for a general law.

> The above experiments do not establish the universality claimed for h_2 [hypothesis 2 = mass conservation] since they concern the transactions of the particular pair mercury-oxygen: after all, the conservation of mass might be an idiosyncrasy of this couple. Lavoisier therefore tested h_2 in several other cases by methodically using the balance. And once he regarded h_2 as sufficiently corroborated – by the standards of his time – he jumped without hesitation to the general conclusion that the conservation of mass holds universally, *i.e.,* for every chemical reaction taking place in a closed system. (Bunge, 1967, pp. 256-257)

Remark that according to Mario Bunge’s retrodiction or philosophical reconstruction of Lavoisier’s research, the French scientist *tested* his hypothesis concerning the conservation of mass, by the *methodical* use of the balance, and *corroborated* it. Well, that is what scientists *should do*, according to Bunge’s understanding of science. However, Lavoisier never actually behaved like that. Let us compare Bunge’s reconstruction to Reijer Hooykaas’ historical account:

> Lavoisier did not find the law of conservation of weight as a result of experiments; on the contrary he started from the metaphysical belief that nothing takes rise on its own account and – though an avowed empiricist [...] – he never performed an experiment to check the truth of this law. (Hooykaas, 1999, p. 222)

Other historians of science agree that Lavoisier accepted the conservation of weight (or mass) as an *a priori* principle and used it, but he never questioned it:

> Doubtless, Lavoisier made ample and good use of this principle […], that is, he applied it as frequently as possible. But what is a principle? The [dictionary] *Petit Robert* says this: “Starting proposition, stated and not derived.” Therefore, a principle is stated *a priori*, before the experiment. Then it is applied to the interpretation of the results and, eventually, one notices in which measure (always approximatively) the principle is followed. So, Lavoisier did not “discover” the law that bears his name after “delicate measurements” […] but he applied it to experiments. When the experiment did not supply the expected results, he did not throw his principle to the nettles, but questioned his experience, and began again, possibly with other instruments. Thus, he did not observe anything other than what he already supposed. (Bensaude-Vincent & Journet, 1993, p. 62)

Those who are not familiar with the details of Lavoisier’s researches may presume that the above description in wrong, since the French scientist is usually described as a very careful and exact experimenter. Other historians of science, however, who have devoted years of study to Lavoisier's work, have presented a devastating view of his scientific approach:

> When we follow Lavoisier's investigative pathway […] – in particular when we reconstruct his experimental ventures at the intimate level recoverable from his laboratory notebooks – we find that he did not have a global method for ensuring that his balance sheets would balance out; that they frequently did not; that he encountered myriad errors, the sources of which he could not always identify with certainty; that he often had to calculate indirectly what he could not measure directly; that he exerted great ingenuity in the management of his data so as to make flawed experiments support his interpretations; and that he devoted much care and effort to the design of experiments so as to obviate such difficulties; but that he often settled for results he knew to be inaccurate, using his faith in the conservation principle to complete or correct the measured quantities. Much of his scientific success, I would claim, is rooted in the resourcefulness with which Lavoisier confronted the many pitfalls that lay along the quantitative investigative pathway he had chosen. (Holmes, 1982, p. 24)

Can we trust Frederic Holmes' analysis of Lavoisier's method? Of course, there is only one way of assessing his view: it is necessary to analyze Lavoisier's original account. What do the primary sources tell us?

4. LAVOISIER'S STATEMENT AND USE OF THE LAW

As several of other researchers of his time, Lavoisier began to *use* the principle of weight conservation without presenting any explicit statement of the same. His first publication that contained the implicit use of the principle (1770) concerned the supposed transformation of water in earth (Lavoisier, 1862, vol.

2, pp. 1-28). Weight considerations provided the main arguments he used, in that paper (Meyerson, 1908, p. 151). He also applied the same principle in his *Opuscules Physiques et Chimiques* published in 1774, where he described his first experiments on calcination (Meyerson, 1908, p. 153).

Let us illustrate Lavoisier's own presentation of the law of mass conservation. He never called much attention to the principle, and its most clear presentation appeared only in 1789, in his *Traité Élémentaire de Chimie* – not at the beginning, but on chapter 13 of the first part, where he discussed wine fermentation:

> It will be seen that, in order to arrive at the solution of these two questions, it was necessary first to be well acquainted with the analysis and the nature of the fermentable body, and the products of fermentation; for nothing is created, neither in the operations of art, nor in those of nature, and it may be advanced in principle that in every operation there is an equal quantity of matter before and after the operation; that the quality and quantity of the principles [elements] are the same, and that there are only changes, modifications.
>
> It is on this principle that the whole art of experimenting in chemistry is founded: one is compelled to assume in all [experiments] a true equality or equation between the principles [elements] of the body which one examines, and those which one extracts from it by analysis. (Lavoisier 1789, vol. 1, pp. 140-141; Lavoisier, 1796, pp. 186-187)[5]

A specific part of the above quotation is regarded as Lavoisier's statement of the principle of conservation of mass: "nothing is created, neither in the operations of art, nor in those of nature, and it may be advanced in principle that in every operation there is an equal quantity of matter before and after the operation" (Holmes, 1982, p. 24). Notice that in this

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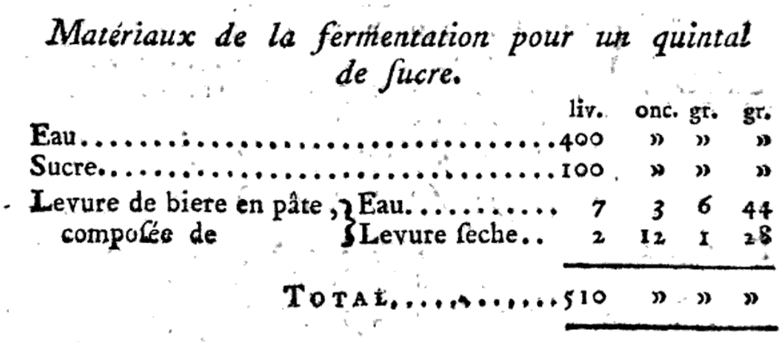
[5] Here I present my own translation of Lavoisier's original French, but I have also provided the reference to the corresponding page of Robert Kerr's English translation (Lavoisier, 1796).

sentence there is no mention of "mass" or "weight"; but "quantity of matter" had already been used as synonymous of mass by Isaac Newton, in the *Philosophiae Naturalis Principia Mathematica*, published one century earlier. We should also remark that Lavoisier's statement was stronger than the law of mass conservation, because he stressed the conservation of the quantity of *each particular element* in the chemical reactions. This is an additional assumption, since one might conceive that there could occur mass conservation even if there were no permanent chemical elements.



Matériaux de la fermentation pour un quintal de ſucre.

		liv.	onc.	gr.	gr.
Eau		400	»	»	»
Sucre		100	»	»	»
Levure de biere en pâte, compoſée de	Eau	7	3	6	44
	Levure ſeche	2	12	1	28
	TOTAL	510	»	»	»

		livre	*once*	*gros*	*grain*
Water		400	0	0	0
Sugar		100	0	0	0
Brewer's yeast paste, containing	Water	7	3	6	44
	Dry yeast	2	12	1	28
		510	0	0	0

Fig. 1. Lavoisier's table of weights for the fermentation of sugar (Lavoisier 1789, vol. 1, p. 143) and the translation of its entries.

In the chapter on fermentation, Lavoisier presented the quantitative details of his experiments. To understand his measurements, it is necessary to know the units of weight he used (Partington, 1961-1970, vol. 3, p. 377). The basic unit was the old French pound (*livre*, or *pois de marc*). We know that it amounted to 489.5058 g. Its subdivisions were: 1/16 of the *livre* was named *once* (30.5941 g); 1/8 of the *once* was named *gros* (3.8242 g); and 1/72 of the *gros* was called *grain* (0.0531 g, or 53.1 mg). So, one *livre* contained 9,216 *grains*.

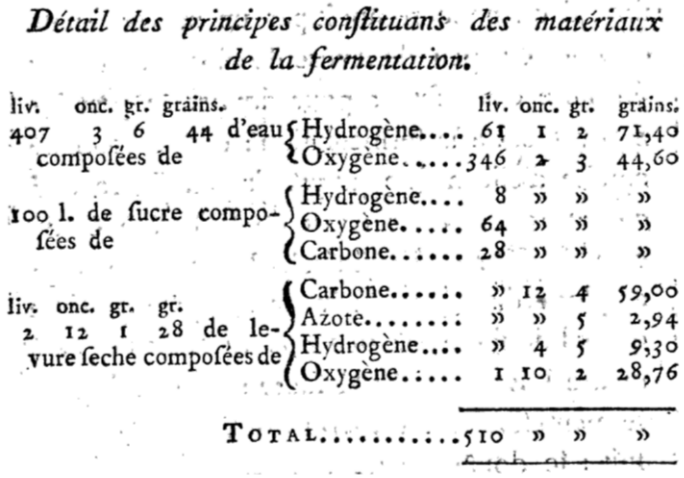
The substances he used for fermentation were water, sugar and yeast; their amounts, according to Lavoisier, were 400 *livres* of water, 100 *livres* of sugar, and 10 *livres* of brewer's yeast paste; and the latter contained 7 *livres*, 3 *onces*, 6 *gros* and 44 *grains* of water and 2 *livres*, 12 *onces*, 1 *gros* and 28 *grains* of dry yeast (Lavoisier 1789, vol. 1, p. 143; see Fig. 1). Notice that his quantitative data suggest a precision of one grain (about 1/20 g) in his weighing.

Were his balances really so exact? We know that his two most accurate instruments were a balance produced by Pierre-Bernard Mégnié that could detect 5 mg for a total weight of 600 g (about one part in 100,000); and another one produced by Nicolas Fortin, which could detect about 25 mg when charged with 10 kg (about two parts in one million). These were the best instruments available at that time (Bensaude-Vincent & Journet, 1993, p. 49). In the fermentation experiments, the total weight was 510 *livres* (without computing the vessels containing the substances), corresponding to about 250 kg. If he could measure those quantities with the accuracy of one *grain* (0.0531 g), then his precision would be about two parts in ten million. However, his best balances could not attain such exactitude.

Lavoisier did not really make his experiments using the very large weight of materials described in his table. Instead of 100 *livres* of sugar, he acknowledged that he used just a few pounds:

> In these results I have carried the precision of calculation to the grain. However, it is not possible yet to carry this kind of experiment to such great exactness. But as I have worked only on a few pounds of sugar – and, in order to make comparisons, I have been obliged to reduce them to the *quintal* [one hundred *livres*] –, I have deemed it my duty to keep the fractions such as the computation had given to me. (Lavoisier 1789, vol. 1, pp. 148-149; Lavoisier, 1796, pp. 194-195)

Therefore, Lavoisier's tables do not provide his actual measurements. They show the proportional values he computed, corresponding to a much larger hypothetical amount of materials.



Détail des principes conſtituans des matériaux de la fermentation.

		liv.	onc.	gr.	grains.
liv. onc. gr. grains. 407 3 6 44 d'eau compoſées de	Hydrogène....	61	1	2	71,40
	Oxygène......	346	2	3	44,60
100 l. de ſucre compoſées de	Hydrogène....	8	»	»	»
	Oxygène.....	64	»	»	»
	Carbone......	28	»	»	»
liv. onc. gr. gr. 2 12 1 28 de levure ſeche compoſées de	Carbone......	»	12	4	59,00
	Azote.......	»	»	5	2,94
	Hydrogène....	»	4	5	9,30
	Oxygène......	1	10	2	28,76
TOTAL...........		510	»	»	»

		livre	*once*	*gros*	*grain*
407 liv. 3 onc. 6 gr. 44 grains water	Hydrogen	61	1	2	71.40
	Oxygen	346	2	3	44.60
100 liv. of sugar, containing	Hydrogen	8	0	0	0
	Oxygen	64	0	0	0
	Carbon	28	0	0	0
2 liv. 12 onc. 1 gr. 28 grains of dry yeast, containing	Carbon	0	12	4	59.00
	Azote	0	0	5	2.94
	Hydrogen	0	4	5	9.30
	Oxygen	1	10	2	28.76
		510	0	0	0

Fig. 2. Lavoisier's table of weights of the several elements contained in the reacting substances used in the fermentation of sugar (Lavoisier 1789, vol. 1, p. 144) and the translation of its entries.

Lavoisier also provided the detailed elementary composition of the materials he used, giving the weights of hydrogen, oxygen, carbon, and nitrogen ("azote") contained in each substance (Fig. 2). Of course, the amount of each element was not *measured*: it was computed taking into account Lavoisier's estimations of the components of each substance, in some previous researches he had made. In the case of sugar, for instance, Lavoisier stated that the proportions of the elements were approximately (*à-peu-près*) the following: Hydrogen 8%, Oxygen 64% and Carbon 28% (Lavoisier 1789, vol. 1, p. 142). Notice that he used those proportions as if they were exact, and that in his table he recorded fractions of hundredths of grain (less than one milligram), which were impossible to measure.

According to Lavoisier, the products of fermentation were these (using the names he used):

	livre	*once*	*gros*	*grain*
Carbonic acid	35	5	4	19
Water	408	15	5	14
Dry alcohol	57	11	1	58
Dry acetous acid[6]	2	7	8	0
Sugar residue	4	1	4	3
Dry yeast	1	6	0	50
	510	0	0	0

According to Lavoisier's data, there was an astonishingly exact match of the total weights of the products and of the reacting substances. Not a single grain was lost or acquired. Anyone who has really made laboratory measurements will be unable to believe that he could obtain this result. It is very likely that in the process of drying the several substances he could not track down the exact amount of water that they lost; and the total

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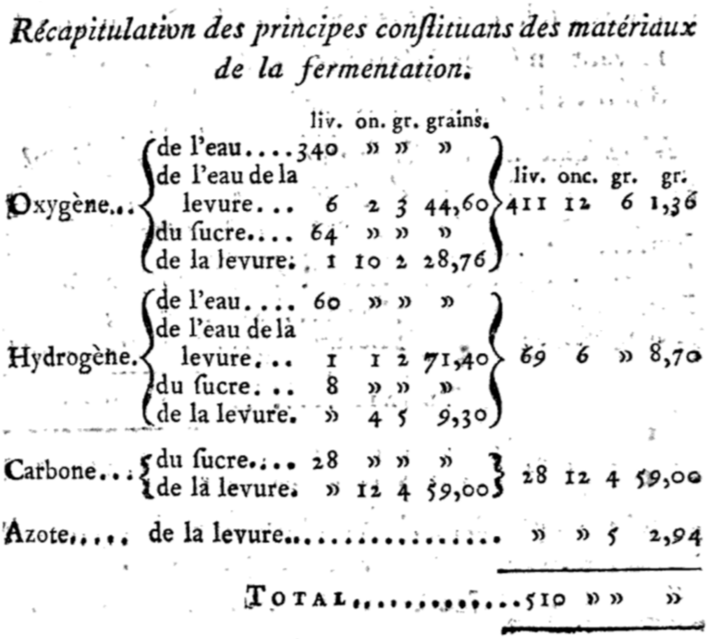
[6] There is a typographical mistake in Lavoisier's book, where the number of *onces* is omitted.

weight of water had to be adjusted, so that the total weight would obey the law of conservation of mass.

> The numerical results of his [Lavoisier's] experiments often are too good to be true: more modern and more precise methods would not yield such marvelous results as he mentions for his experiments on fermentation. (Hooykaas, 1999, p. 222)



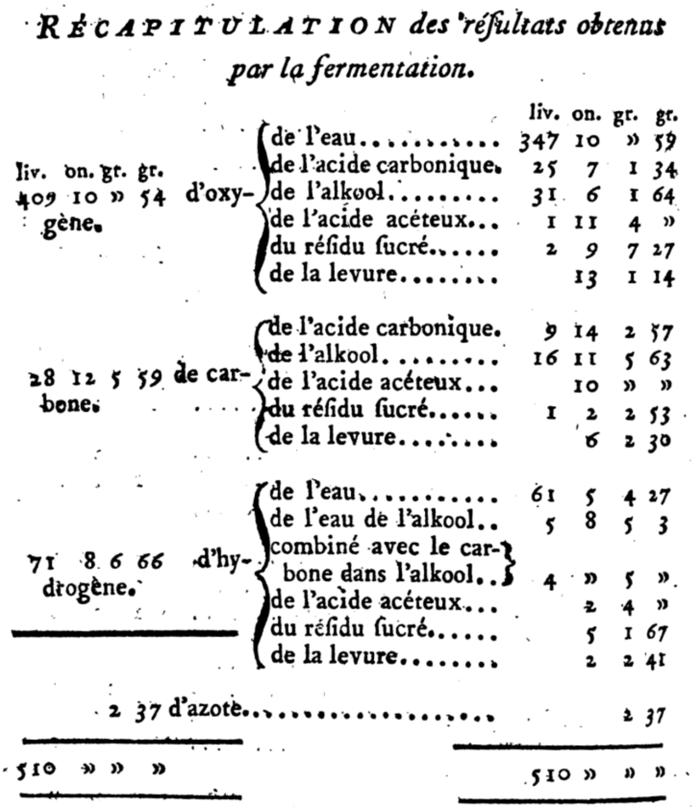
Récapitulation des principes conſtituans des matériaux de la fermentation.

		liv.	on.	gr.	grains.	liv.	onc.	gr.	gr.
Oxygène	de l'eau	340	»	»	»	411	12	6	1,36
	de l'eau de la levure	6	2	3	44,60				
	du ſucre	64	»	»	»				
	de la levure	1	10	2	28,76				
Hydrogène	de l'eau	60	»	»	»	69	6	»	8,70
	de l'eau de la levure	1	1	2	71,40				
	du ſucre	8	»	»	»				
	de la levure	»	4	5	9,30				
Carbone	du ſucre	28	»	»	»	28	12	4	59,00
	de la levure	»	12	4	59,00				
Azote	de la levure					»	»	5	2,94
TOTAL						510	»	»	»

	livre	*once*	*gros*	*grain*
Oxygen	411	12	6	1.36
Hydrogen	69	6	0	8.70
Carbon	28	12	4	59.00
Azote			5	2.94
Total	510	0	0	0

Fig. 3. Lavoisier's table showing the weights of the several elements contained in the substances used in the fermentation experiment (Lavoisier 1789, vol. 1, p. 144).

Lavoisier also provided the amounts of each element of the substances he used, both before and after fermentation. According to his account, the composition of the substances used in the fermentation experiment was that shown in Fig. 3.



RÉCAPITULATION des réſultats obtenus par la fermentation.

liv.	on.	gr.	gr.			liv.	on.	gr.	gr.
409	10	»	54	d'oxygène.	de l'eau	347	10	»	59
					de l'acide carbonique.	25	7	1	34
					de l'alkool	31	6	1	64
					de l'acide acéteux	1	11	4	»
					du réſidu ſucré	2	9	7	27
					de la levure		13	1	14
28	12	5	59	de carbone.	de l'acide carbonique.	9	14	2	57
					de l'alkool	16	11	5	63
					de l'acide acéteux		10	»	»
					du réſidu ſucré	1	2	2	53
					de la levure		6	2	30
71	8	6	66	d'hydrogène.	de l'eau	61	5	4	27
					de l'eau de l'alkool	5	8	5	3
					combiné avec le carbone dans l'alkool	4	»	5	»
					de l'acide acéteux		2	4	»
					du réſidu ſucré		5	1	67
					de la levure		2	2	41
		2	37	d'azote				2	37
510	»	»	»			510	»	»	»

	livre	*once*	*gros*	*grain*
Oxygen	409	10	0	54
Hydrogen	71	8	6	66
Carbon	28	12	5	59
Azote			2	37
Total	510	0	0	0

Fig. 4. Lavoisier's table of the weights of each element contained in the products of fermentation (Lavoisier 1789, vol. 1, p. 148).

According to Lavoisier's data, the amounts of each element obtained in the products of the fermentation experiment were different from the initial weights, although the total weight was the same (Fig. 4).

Lavoisier did not attempt to explain the considerable differences between the initial and final weights of the elements, which amounted to about 2 *livres* in the cases of Oxygen and Hydrogen. Transforming into kilograms and rounding off his figures, the initial mass of Oxygen was 201.577 kg and after fermentation it was only 200.516 kg; that of Hydrogen was initially 33.980 kg and it increased to 35.026 kg. Following a strictly empiricist point of view, the experiment *refuted* the conservation of mass of each element, in fermentation.

We must understand that the amount of each element was not *measured*, it was computed from the analysis of each product according to previous experiments. The lack of agreement shows that Lavoisier's analysis of each substance had significant errors, of the order of 3 to 5%.

At the end of the same chapter of his book, Lavoisier stated:

> I shall finish what I have to say about vinous fermentation, by observing that it may provide a means of analyzing sugar and every vegetable fermentable matter. Indeed, as I have already pointed out at the beginning of this article, I may consider the substances submitted to fermentation, and the result obtained after fermentation, as an algebraic equation; and supposing successively each element of this equation as unknown, I can obtain from it a value, and thus rectify experience by calculation and calculation by experience. I have often taken advantage of this method to correct the first results of my experiments, and to guide me concerning the precautions that should be taken for repeating them […] (Lavoisier 1789, vol. 1, p. 151; Lavoisier, 1796, p. 197)

So, Lavoisier recognized that he used the law of mass conservation to *correct* the results of his experiments. He adjusted the quantities so that there was an exact match between the sum of the masses of the reacting substances and that of their products. The data he published was not what he measured, it was what the experiment *should have shown*, according to the principle of conservation of mass.

> In Lavoisier's publications the numerical values of the quantities of substances always agree 100% with the yields of reactions found for their component elements. This is too good to be true and he admitted this himself when he said that he "corrected experience by calculation". (Hooykaas, 1999, p. 223)

Whenever a disagreement occurred, this was never regarded by him as a *refutation* of the law of mass conservation, but the anomaly could conduct him to take additional precautions in further trials, because the lack of concordance was interpreted by him as some mistake that occurred *in the experiments*.

5. LAVOISIER AND THE CONSERVATION OF CALORIC

It is remarkable that Lavoisier's thought was frequently guided by conservation hypotheses, in several fields – not only in chemistry and physics:

> There is therefore, at least for most of the territorial productions of the kingdom of France, an equation, an equality between what is produced and consumed; thus, to know what is produced, it is enough to know what is consumed […] (Lavoisier, 1791, p. 9)

It is well known that Lavoisier died in the guillotine, during the French revolution, because of his involvement in tax collecting – not because of his scientific works. In 1770, at the age of 26, Lavoisier had acquired a share in the French "Ferme générale", a company which collected taxes for the king and distributed a bulky part of the received money between its members (Aykroyd, 1935, pp. 12-17). His position as a *fermier* *général* and other jobs he obtained because of that involvement, such as the supervision of tobacco and gunpowder production (Scheler, 1973), were the main sources of his personal income. He kept detailed accounts of finances, registering the amounts of incoming and outgoing money and the resulting balance. Of course, money does not usually disappear, nor is it created from nothing – it is conserved in financial transactions. Lavoisier's financial book-keeping was similar to his computations concerning the conservation of matter:

> His [Lavoisier's] training in keeping accounts and preparing balance-sheets as a *Fermier Général* influenced his scientific work, and there is a close resemblance in form between his official memoranda (printed in the *Oeuvres*, especially in Vol. VI) and his scientific memoirs. (Partington, 1961-1970, vol. 3, p. 376)

> [...] nothing is created, either in the operations of art, or in those of nature, and one can state as a principle that in every operation there is an equal quantity of material before and after the operation. It is recognized that this statement was the operating principle on which Lavoisier based his "balance sheet" method of experimentation [...] (Holmes, 1982, p. 24)

> […] Lavoisier's scales were more than a precision instrument. They materialized an intellectual strategy of balancing inputs and outputs that Lavoisier used daily in his book-keeping activity as a tax collector and also in his reflections on rational economics — both domestic, and national. (Bensaude-Vincent & Simon, 2008, p. 86)

It is not widely known that, besides the principle of conservation of weight, Lavoisier also accepted another conservation law which was later rejected: that of quantity of heat (or caloric).

In the seventeenth and eighteenth centuries, some authors regarded heat as produced by microscopic (invisible) motion, and other supposed that it was a substance. Francis Bacon, René

Descartes, Robert Boyle and Daniel Bernoulli had endorsed the view of heat-motion; Pierre Gassendi, Leonhard Euler, Herman Boerhaave, Georges-Louis Leclerc (comte de Buffon), Joseph Black, Jean-André De Luc and Johan Carl Wilcke supported the heat-substance concept (Barnett, 1946; Boyer, 1943; Morris, 1972, p. 28). Calorimetric concepts and experiments, developed in the second half of the eighteenth century, were connected to the assumption of heat as a substance, and after the works of Wilcke, De Luc and Black,

> […] the triumph of the first of these theories seemed complete. From then on, heat is treated as a real substance that passes from one body to another, without any change of its quantity (which one had learned to measure) and that, if it ceases to be manifested to our sensation and if the thermometer does not detect it, none the less continues to exist in a particular state. (Meyerson, 1908, p. 173)

Boerhaave attempted to measure the weight of heat and he was unable to find any definite change of weight when a body was heated, concluding as a result that heat was an imponderable substance (Boyer, 1943, p. 448) – an inference that was later accepted by Lavoisier.

> The notion of imponderable substances was well established during the eighteenth century. Electricity was generally understood to consist of one or two imponderable fluids. Magnetism could be similarly explained. So could light and heat. Lavoisier extended the explanatory model by making two imponderable fluids, light and caloric, simple substances for the chemist. (Hooykaas, 1999, p. 74)

Lavoisier came to accept the idea of a heat-substance, which he called by several names, such as “igneous fluid”, “matter of heat” and finally “caloric” from 1787 onwards (Morris, 1972, p.

2)[7]. In his chemistry, caloric was one of the elementary substances (Lavoisier, 1789, vol. 1, p. 192; Siegfried, 1982, p. 33), and the first chapter of his *Traité Élémentaire de Chimie* is devoted to its presentation. According to Lavoisier, caloric is not heat, it is its cause (Lavoisier, 1789, vol. 1, pp. 4-5).

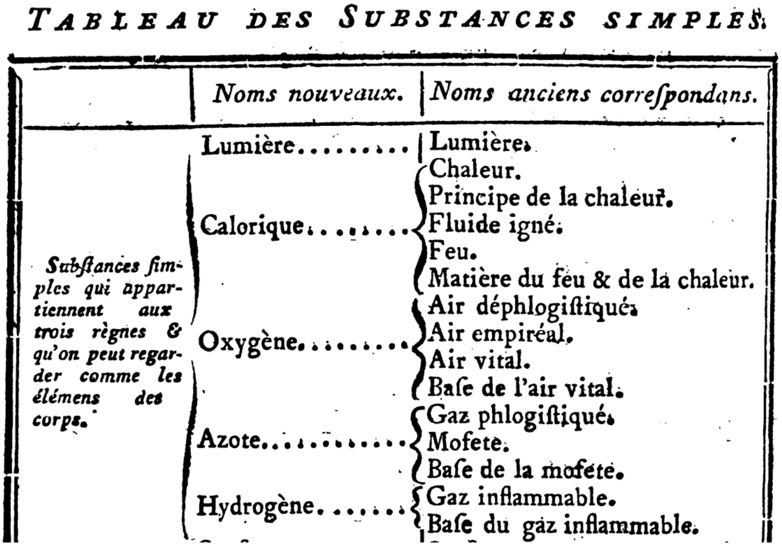


TABLEAU DES SUBSTANCES SIMPLES.

	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.

Fig. 5. The beginning of Lavoisier's table of simple substances (elements), which included light (*lumière*) and caloric (*calorique*), together with oxygen, nitrogen and hydrogen (Lavoisier, 1789, vol. 1, p. 192).

According to Lavoisier, gases are produced by the chemical combination of a material substance with caloric, and oxygen gas is a specific instance of such a combination (Meyerson, 1908, p. 151; Guerlac, 1976, p. 220)[8]. For that reason, although

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[7] The name "caloric" has been applied for the first time in the book *Méthode de nomenclature chimique*, jointly authored by Guyton de Morveau, Lavoisier, Bertholet and Fourcroy (Guyton de Morveau *et al.*, 1787, p. 78). There, caloric was described as a chemical element, an idea that was later retained by Lavoisier.

[8] Lavoisier first published this idea in his 1777 paper, "De la combinaison de la matière du feu avec les fluides évaporables: et de

oxygen is a simple substance (element), it is possible to decompose *oxygen gas*. This is a topic dealt with by Lavoisier in the fifth chapter of his *Traité*, "The decomposition of oxygen gas by sulfur, phosphorus, and coal, and the formation of acids in general", where we find descriptions such as this:

> This experiment clearly proves that at a certain temperature, oxygen has more affinity with phosphorus than with caloric; that, consequently, phosphorus decomposes the oxygen gas, that it seizes its base, and that the caloric, which becomes free, escapes and dissipates by distributing itself among the surrounding bodies. (Lavoisier, 1789, vol. 1, p. 60)

Amounts of ponderable matter can be assessed with a balance. Caloric, however, is an imponderable substance, it cannot be weighed – but its quantity can be ascertained by calorimetric experiments. Following the pioneering ideas and experimental researchers of Joseph Black and other previous authors, Lavoisier and Pierre-Simon de Laplace studied phenomena of heat transference between bodies by means of a very sophisticated ice calorimeter. They also investigated the heat generated by chemical reactions and by living bodies (Guerlac, 1976). In their joint paper published in 1783, describing the instrument and its use (Lavoisier, 1862, vol. 2, pp. 283-333), Lavoisier and Laplace mentioned the two opposing theories of heat.

> *The quantity of free heat remains the same in the simple mixture of bodies*. This is evident if heat is a fluid which tends to equilibrium; and if it is simply the living force which results

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la formation des fluides élastiques aeriformes" (Lavoisier, 1862, vol. 2, pp. 212-224). There is a full and commented English translation (Best, 2015-2016) of another of Lavoisier's early works on the subject, "Réflexions sur le phlogistique, pour servir de suite à la théorie de la combustion et de la calcination" (Lavoisier, 1862, vol. 2, pp. 623-655).

> from the internal motion of matter, the principle in question is a consequence of that of the conservation of living forces. The conservation of free heat, in the simple mixture of bodies, is therefore independent of any hypothesis as to the nature of the heat; it has been generally accepted by physicists, and we will adopt it in the following researches. (Lavoisier, 1862, vol. 2, p. 287)[9]

Laplace did prefer the heat-motion hypothesis and Lavoisier always adopted the heat-substance concept (Guerlac, 1976, pp. 244-245), but they reached a compromise and stated that the experimental investigations they presented were independent of the particular interpretation adopted (Morris, 1972, pp. 30-31). In all those researches, they were guided by the idea that heat is conserved: it may be hidden in bodies, but it cannot be created or destroyed.

Independently of the particular justification, the law of conservation of heat was accepted by all researchers who did calorimetric experiments. Johan Carl Wilcke, Jean-André De Luc, and Joseph Black accepted the indestructibility of the matter of heat, before Lavoisier (Meyerson, 1908, p. 191). The conservation of heat was *suggested*, *accepted* and *applied* as a reliable law, just as in the case of the law of conservation of matter. We know that the law of conservation of heat was flawed. We accept that *energy* is conserved; heat is one of the many forms of energy and it can be created or destroyed, when there are energy transformations. Heat itself is not conserved, except in some very special phenomena – those that only involve heat conduction between inert bodies.

6. WAS LAVOISIER'S LAW AN APRIORISTIC TRUTH?

It is clear that in the fermentation experiment Lavoisier was *assuming* and *using* the law of conservation of mass, but he was

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[9] "Free heat" was understood by Lavoisier as the part of caloric that was not chemically combined with material substances.

not *testing* it. Indeed, historians of science have never found a single experiment in his published works or unpublished notes that could be interpreted as a test of that law.

> Lavoisier did not find the law of conservation of weight as a result of experiments; on the contrary he started from the metaphysical belief that nothing takes rise on its own account and – though an avowed empiricist [...] he never performed an experiment to check the truth of this law. (Hooykaas, 1999, p. 222)

He frequently *applied* the law, long before he explicitly stated it. According to Holmes, "His first notable experiments on the transmutation of water in 1768-70 relied on that method, and it pervaded all of his experimental investigations through the next two decades" (Holmes, 1982, p. 24).

> Lavoisier's applications of the law of the conservation of mass were inspired by his faith in an incontrovertible truth. Thus, in his early publications he did not weigh all the substances taking part in a reaction, but commonly used the law to weigh them indirectly. (Hooykaas, 1999, p. 222)

Consulting several of Lavoisier's works, it is possible to notice that he regarded the conservation of mass as an obvious statement and similar to the mathematical equality between two terms and their sum. In his memoir on the decomposition of water (1781)[10], after describing the burning of hydrogen and oxygen, Lavoisier affirmed:

> […] we could not ascertain the exact quantity of the two airs with which we had thus made the combustion; but, as it

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[10] The full work is reproduced in the second volume of Lavoisier's *Oeuvres* (Lavoisier, 1862, pp. 334-359): "Mémoire dans lequel on a pour objet de prouver que l'eau n'est point une substance simple, un élément proprement dit, mais qu'elle est susceptible de décomposition et de recomposition" (1781).

> is no less true in physics than in geometry that the whole is equal to its parts, from the fact that we had obtained only pure water in this experiment, without any other residue, we thought that we were justified in concluding that the weight of this water was equal to that of the two airs which had served to form it. (Lavoisier, 1862, pp. 338-339)

At that time, mathematics was regarded as *a priori* knowledge; therefore, Lavoisier's statement "it is no less true in physics than in geometry that the whole is equal to its parts" seems to imply that he also regarded the law of mass conservation as undeniable and *a priori*. Indeed, at one place he explicitly uses the phrase "*a priori*" to describe that idea:

> This experiment gave results similar to those of the previous one. Its result was also that when phosphorus burned, it absorbed a little more than one and a half its weight of oxygen, and I have obtained also the certainty that the weight of the new substance that was produced was equal to the sum of the weights of the burned phosphor and of the oxygen that it had absorbed – and that was easy to predict *a priori*. (Lavoisier 1789, vol. 1, p. 63)

In another work, where Lavoisier described the combustion of alcohol, oil and other substances (1784)[11], he presented the law of mass conservation as "evident":

> The combustion of olive oil does not contain as much uncertainty as that of the spirit of wine, because olive oil is not capable to volatilize easily; one can know with rigorous accuracy the quantity burned, by difference of the weights determined before and after combustion. [...]
>
> Concerning the water which has been formed, it could neither be collected nor weighed, and I have elsewhere

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[11] "Mémoire sur la combinaison du principe oxygine avec l'esprit de vin, l'huile et différents corps combustibles" (Lavoisier, 1862, pp. 586-600).

> explained the reason for it; it is the same as for the spirit of wine; I have therefore determined it by calculation, always starting from the supposition that the weight of the materials is the same before and after the operation, which I regard as evident [...] (Lavoisier, 1862, p. 595)

In the specific case of Lavoisier, the law of conservation of weight was neither proved nor derived from experiments, and he did not care to test it. It is clear that he did not regard it as an empirical law nor as something that could be checked. It seemed an *a priori* truth, quite obvious for him.

The reader of this paper might be perplexed: if the equality of the weight of the materials before and after chemical reaction is *evident*, why wasn't it used before Lavoisier?

As a matter of fact, it was much used before the French chemist.

> Most students of chemistry have diligently learned that it was Lavoisier who introduced the concept of the conservation of matter into chemistry, using this principle to dismiss the imaginary element of phlogiston.
>
> Nevertheless, the conservation of matter is a basic assumption that underlies ancient physics. A great majority of Greek philosophers and early modern scientists considered matter to be eternal and indestructible without having any experimental evidence for it. The conservation of matter is so deeply embedded in western science that the philosopher Émile Meyerson considered it as an *a priori* metaphysical assumption and the necessary foundation for all scientific endeavors[12]. (Bensaude-Vincent & Simon, 2008, pp. 115-116)

Once more, it is necessary to remark that many teachers and students of chemistry are misled by the eponym, "Lavoisier's law". The naïve reader might reason thus: "If the law of

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[12] As will be seen later, this interpretation of Émile Meyerson's views is not correct.

conservation of matter is ascribed to someone called Lavoisier, that person must have been the *first* to prove, or to discover, or to present, or to use it… otherwise it would not have been named thus, isn't it?" No, that is not correct! Beware of eponyms! They are usually meaningless and they convey a misleading view of the history of science (Martins, 2015).

> The concept of conservation of mass was suggested in the section on vinous fermentation. Lavoisier recognized that sugar, which is composed of carbon, hydrogen, and oxygen, is converted by means of yeast to carbon dioxide and spirit of wine, for which he introduced the Arabic term alcohol. He took for granted that it should be possible to account for all the original matter in the final products. In other words, he reduced chemical change to a balance-sheet type of operation when he suggested that the weight of products should be equal to the weight of reactants, an idea utilized but not expressed earlier by Black and Cavendish. Even earlier, the Russian scientist Mikhail Vasilevich Lomonosov (1711-1765) had tacitly assumed that when one body gained weight some other body lost an equivalent amount of weight. (Ihde, 1984, pp. 79-80)

A short account of the relevant contributions of Joseph Black, Henry Cavendish, and Jean Rey, can be found in Robert Whitaker's paper (Whitaker, 1975). Aaron Ihde's account, quoted above, might suggest that other people before Lavoisier had only *implicitly* used the principle of mass conservation, but that they had not clearly or explicitly presented it. That is not true. We can find clear statements of the law before Lavoisier:

> […] we want to insist on the fact that Lavoisier did not invent either the idea of the conservation of mass in a chemical reaction or the use of the scales in chemistry. The idea that nothing can be created or destroyed can be found in the writings of the ancients, and many physicists and chemists had championed it as an axiom before Lavoisier. Van Helmont, for example, explicitly proposed that: "Nothing

> comes into being from nothing. The weight comes from another body weighing just as much." (Bensaude-Vincent & Simon, 2008, pp. 85-86)

> In quantitative experiments Lavoisier assumed the truth of the law of conservation of matter. He was not responsible for first stating this principle, which goes back to Classical antiquity […] and was often mentioned before his time. Chardenon in 1764 said: "it is a generally adopted principle, that the absolute weight of a body can be increased only by the addition of new parts of matter. The law of converses therefore points out that it cannot become lighter except by the subtraction of these same parts". (Partington, 1961-1970, vol. 3, p. 377)[13]

Émile Meyerson described one of the oldest uses of arguments similar to those of Lavoisier for finding the weight of something that was not weighed:

> […] one cannot doubt that it [the law of conservation of weight] had been understood and taught in some philosophical schools of Antiquity. A curious passage of a treatise attributed to Lucian [of Samosata] endorses this. "If I burn one thousand pounds of wood, Demonax, how many pounds will there be of smoke?" "Weigh the ashes", he says, "the rest is the smoke that is searched for". Demonax was a Cynic philosopher of the second century of our era. He appears to have been exclusively preoccupied with ethics, theology, and politics. Nothing indicates either that he professed atomistic opinions, nor that he had studied

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[13] Cited in French by Partington: "c'est un principe généralement adopté, que la pesanteur absolue d'un corps ne peut être augmentée que par l'addition de nouvelles parties de matiere. La loi des contraires indique donc qu'il ne peut devenir plus léger que par la soustraction de ces mêmes parties". Notice that Chardenon's paper containing this statement described his research on the calcination of metals and their changes of weight – a subject that was shortly afterwards studied by Lavoisier.

> scientific questions. The quotation is all the more significant, because it proves that this reasoning, analogous to those that we make because of the principle [of conservation of weight], had become current in the philosophical schools. (Meyerson, 1908, p. 137)

One of the few authors of the seventeenth century who stated and used the principle of conservation of weight was Jean Rey. Meyerson conjectured that his thought was influenced by the atomism of the ancient philosophers, since at that time the work of Lucretius (*De Rerum Natura*) had been published and was widely discussed. Rey accepted that all matter has weight, including air; and explained the increase of weight of calcined tin as due to the addition of air (Meyerson, 1908, p. 145).

The history of the law of conservation of matter was the subject of Émile Meyerson's earliest publication: "Jean Rey et la loi de la conservation de la matière" (Meyerson, 1884).

> This law is regarded, after the admirable Lessons on the Philosophy of Chemistry of Mr. Dumas, as Lavoisier's creation. It is undeniable that this was the principle that guided him in all his discoveries and that with its help he reformed Chemistry. However, that law was clearly stated, rigorously formulated and applied to the study of chemical phenomena almost one and a half century before him, by the physician Jean Rey. (Meyerson, 1884, p. 299)

Jean Rey's work was published as a pamphlet with the title "Essays […] on the search for the cause of the increase of weight of tin and lead when they are calcined", in 1630. In this book, Jean Rey first argues that all the four material elements that were accepted in Antiquity – earth, water, air, and fire – are heavy; that all of them have a tendency to fall; and that there exists no absolute lightness of matter – a tendency to recede from the Earth or from the center of the universe, as taught by Aristotle. Then, Jean Rey presents his principle of conservation of weight:

> ESSAY VI. *Heaviness is so closely united to the primary matter of the elements, that when these are changed one into the other they always retain the same weight.*
>
> My chief care hitherto has been to impress on the minds of all the persuasion that air is heavy, inasmuch as from it I propose to derive the increase in weight of tin and lead when they are calcined. But before showing how that comes to pass, I must make this observation that the weight of a thing may be examined in two ways, viz. by the aid of reason, or with the balance. It is reason which has led me to discover weight in all the elements, and it is reason which now leads me to give a flat denial to that erroneous maxim which has been current since the birth of Philosophy—that the elements mutually undergoing change, one into the other, lose or gain weight, according as in changing they become rarefied or condensed. With the arms of reason I boldly enter the lists to combat this error, and to sustain that weight is so closely united to the primary matter of the elements that they can never be deprived of it. The weight with which each portion of matter was endued at the cradle, will be carried by it to the grave. In whatever place, in whatever form, to whatever volume it may be reduced, the same weight always persists. (Rey, 1904, p. 14)

This is a clear and explicit exposition of the principle of conservation of weight.

Jean Rey did not attempt to prove it by experiments, since he regarded it as a product of reason. He tried to demonstrate it by an aprioristic argument, but his reasoning was unconvincing and it will not be reproduced here. In the same work, Jean Rey used the principle to elucidate the increase of weight of calcined tin and lead, explaining it as due to the combination of the metals with air (oxygen was unknown, at that time). Nowadays, his contribution is regarded as a very important scientific contribution. At that time, however, Jean Rey's ideas had no impact.

In the late seventeenth century, Christiaan Huygens stated that weight was the measurement of quantity of matter

(Meyerson, 1908, p. 147); and Isaac Newton introduced the concept of mass as equivalent to quantity of matter. They did not, however, deal with mass (or weight) conservation. Robert Boyle, their contemporaneous chemist, made implicit use of the principle, although he did not express it (Meyerson, 1908, p. 148). In the early eighteenth century, several researchers, including Samuel Cottereau Duclos, Wilhelm Homberg, Pieter van Musschenbroek and George Berkeley, have also made implicit use of the principle (Meyerson, 1908, p. 149). However, belief in the conservation of weight was not consensual and even scientist who made extensive use of the balance, such as Henry Cavendish, denied the principle (Meyerson, 1908, p. 149).

Neither Lavoisier, nor the other scientists or philosophers who accepted the principle of conservation of mass (or weight) before him, attempted to prove it by experiment. They admitted it as an evident or obvious *a priori* truth.

7. ÉMILE MEYERSON ON THE LAWS OF CONSERVATION AND THE PRINCIPLE OF CAUSALITY

Although several authors had accepted and used the principle of conservation of weight before Lavoisier, one should not think that *everyone* agreed to it. Although Lavoisier compared it to a mathematical truth (the whole is equal to the sum of its parts), it did not command a general acceptance, as arithmetic or geometry did. To be sure, there were many relevant authors from Antiquity to the eighteenth century who did not accept it – including Aristotle and Descartes (Meyerson, 1908, chapter IV). Were they unable to understand such an obvious truth?

> But generations of scholars and philosophers have advanced opinions clearly implying the negation of the principle. Can we say that the idea never suggested itself to them? It is in itself of great simplicity and can be deduced so directly from the principle of causality that it presents itself, so to speak, invincibly to our mind. (Meyerson, 1908, p. 162)

Émile Meyerson presented for the first time a deep analysis of this historical problem in his book *Identité et Réalité* (1908). His main thesis is that there is a *philosophical* aprioristic principle that was always accepted – the principle of causality – which is ample and vague, impossible to submit to any empirical test because it provides no clear observational prediction. On the other hand, there are several *scientific* principles of conservation that are similar, in form, to the principle of causality; they can be submitted to experimental tests; but they usually seem *a priori* truths because of their resemblance to the principle of causality.

The main subject of Meyerson's book *Identité et Réalité* is the law of causality. When he wrote his work, philosophy of science was strongly influenced by positivistic views that stated that the only aim of science was to *describe* the world, by means of laws; *understanding* the world seemed impossible and/or unnecessary. Meyerson's most general claim was that the search for lawfulness is not sufficient: "science also seeks to *explain* the phenomena, and that explanation lies in the identification of the antecedent and the consequent" (Meyerson, 1908, p. vi). In some sense, everyone agrees that science looks for explanations; however, Meyerson's peculiar understanding of explanation and causality was not the traditional one – it was highly peculiar.

What did Meyerson understand as the principle of causality? He turned his attention to a specific meaning of cause, broadly corresponding to Aristotle's material cause (Drouin 1964) – that is, something constant underlying the phenomena (Follon 1988). Meyerson emphasized the special interpretation of causality that presupposes equality between cause and effect, being equivalent "to the well-known formula of the scholastics, *causa aequat effectum*" (Meyerson 1908, 16). The world is full of changing phenomena; since Antiquity, philosophers (and, later on, scientists) have been searching for something constant behind the mutable events, something that is the same before and after changes occur. That was the motivation of the early

Greek philosophers in the search of the material invisible substratum of nature – and, more specifically, of the atomistic theory (Meyerson, 1908, chapter 2). It is also the motivation behind every law of conservation that has been proposed in science, because they state that something is constant in time, although observable changes are occurring.

> It is from that second principle, the principle of *scientific causality*, that come the atomic theories (chapter II). It also interferes in the laws of science, creating the principles of conservation (chapters III, IV and V) […] Those conclusions are not a scientific result, they are the outcome of the aprioristic elements that it [science] contains [….] (Meyerson, 1908, p. vi)

According to Meyerson, the search for constancy behind the phenomena is not a result of empirical research – it is a demand of our reason; and it can never be completely fulfilled, otherwise there could be no change in the world.

The principle of causality urges us to search for entities that do not change, in phenomena. The principle of causality is not a description of the world, it is a demand of our reason. In each particular case, we may find – or not – the specific unchangeable entity (or magnitude). However, the world could be built without anything constant or permanent.

> The way followed by our understanding to apply the principle of identity clarifies that it is liable to mistakes, in this matter. There were principles of conservation that have been proposed and that science had to abandon completely, afterwards; or it was necessary to severely transform its content, changing the expression of what is conserved. Along our work we have found examples of both cases. Black's principle of conservation of caloric belongs to the first category: that proposition seems nowadays clearly wrong and, in addition, as contradicted by facts of vulgar experience, such as the heat produced by friction. However, it was for a

> long time accepted as securely founded, as one of the most solid grounds of physics. (Meyerson, 1908, p. 370)

When the principle of causality, thus understood, is applied to the concept of matter, it generates the principle of conservation of matter. In its more general form, it states that, although visible matter changes, there is some primordial matter that does not change and is constant throughout all phenomena. The primordial matter of Aristotle was different from the primordial matter of the Greek atomists; but the general principle was the same. During the nineteenth century, some chemists (such as John Dalton) accepted the existence of immutable atoms that remained constant in chemical change; others (such as Wilhelm Ostwald) did not believe that atoms did exist, but nevertheless they admitted chemical unchanging elements – so, the principle of conservation of matter can be satisfied in many different and incompatible forms.

In Antiquity, the Greek and Roman atomists accepted the principle of causality, stating that nothing can be created from nothing, and that nothing can be annihilated. They applied the principle to atoms, which were supposed to be eternal, uncreated, indestructible (Meyerson, 1908, p. 137). Their atoms had weight, and we may suppose that the atomists accepted that weight is conserved, although neither that statement nor its implicit use can be found in their extant writings.

Other Greek philosophers, including Plato and Aristotle, could not accept the conservation of weight. They believed that the four material elements (earth, water, air and fire) can transform in one another; water is heavy and falls to the ground, and it can become air (vapor) that is light is recedes from the ground. In many cases such as this, it was supposed that weight had disappeared or changed. For many philosophers, weight was an accidental quality of matter, as color or shape – and none of those qualities is preserved in the transformations of matter (Meyerson, 1908, p. 139). That way of thinking was dominant during the Middle Ages. The principle of causality was always accepted, and philosophers

> [...] were certainly convinced that something essential in matter, its *substance*, was maintained during its modification. However, since the concept of matter was justifiably separated from weight, it became too difficult to provide a quantitative basis to matter. (Meyerson, 1908, p. 142)

> We find this principle [of conservation of matter] expressed in several quite different ways. Let us discard from the star the formulation "nothing is created, nothing is destroyed" that is still sometimes associated to it and that is obviously too broad: it could be applied also to the conservation of velocity and that of energy – and that is not surprising, because this statement is, as we have seen, just one of the statements of the principle of causality. At least one should say: matter is not created nor destroyed. However, even this statement lacks precision. (Meyerson, 1908, p. 136)

Conservation of matter can be regarded as a *qualitative* principle; or it may be assumed as a *quantitative* principle. In the second case, it becomes the principle of conservation of the quantity of matter. But how should we understand the "quantity of matter" that remains the same before and after the visible transformations of matter? Is it the mass of substances, or some other quantity such as their volume or the number of atoms? The principle of causality is unable to provide any unambiguous interpretation.

A material object has many different properties, such as its size, volume, weight, color, etc. Do all those properties remain the same? Of course not – if *everything* remained unchanged, there could not happen any phenomenon. The principle of conservation of matter cannot state that nothing changes, because that is obviously false. It must indicate that *something* in matter is conserved; it cannot be created or destroyed. Is it the *weight* of matter? Weight is the gravitational force acting upon a body. We know that the weight of an object on the Moon is different from its weight on the Earth; and that its weight on the Earth changes, according to the altitude of the place where it is placed. Therefore, the weight of a material object is not conserved, it may change. We nowadays accept that the *mass* of material bodies is constant. Hence, in a more precise way, the law of conservation of matter is understood as the law of conservation of mass (Meyerson, 1908, p. 137).

The concept of physical mass was only introduced in the seventeenth century. It is not a straightforward concept; and both before and after him, the most common way of describing the principle of conservation of matter is referring to the conservation of weight. Hence, in this paper, we will use "conservation of mass" and "conservation of weight" as equivalent.

> Like everything else we desire mass to be constant; but we desire it more strongly because it is capable of appearing as the essence of matter. (Meyerson 1930, 182)

There is a gap between the principle of causality and any specific law of conservation that cannot be filled by reason alone. The principle of causality is *a priori* and unquestionable. Each particular law of conservation that has ever been proposed in science can be submitted to tests and does not contain the same degree of certainty as the principle of causality.

Let us explain Meyerson's conception of conservation laws in a schematic way. The principle of causality, as understood by him, states: "In every transformation, *something* is permanent or constant." Philosophy can only go as far as that. Science, however, needs more: *what* remains constant in the transformations? How can I detect or measure this "something"? Reason cannot offer the solution. Only empirical research can provide a satisfactory answer. Therefore, every principle of conservation contains two components: the *a priori* structure of the principle, that comes from the principle of causality; and the *a posteriori* interpretation or scientific content, coming from experience.

> Therefore, what is really aprioristic in science is the starting series of postulates that we need for an empirical science, that is, for being able to articulate this proposition: nature is well-ordered, and we can know its route. However, this strictly empirical science is an artificial fabrication and *science* is not rigorously empirical; it is also the application to nature, by successive phases, of the principle of identity, the essence of our understanding. However, from that principle we cannot deduce any precise proposition: and for that reason, a *pure* science cannot exist, counter to what Kant supposed. When trying to explain the phenomena, we attempt to adjust them to what this principle postulates, and for that reason its intervention in science manifests itself as a tendency, the causal penchant. (Meyerson, 1908, p. 369)

8. HANS LANDOLT AND THE CONSERVATION OF MASS

Let us return to the principle of conservation of mass. Neither Lavoisier nor his predecessors have attempted to test it. What happened afterwards?

Although Lavoisier did not care to test the principle of conservation of mass, it could have occurred that later researchers did test and confirm it. It seems that most chemists, teachers and students, believe that this did happen. Meyerson, however, denied it:

> What is the current situation regarding this question? Since Lavoisier, the balance has become the preferred instrument of the chemist; and one could say – this is, for instance, Mr. Ostwald's opinion – that in some sense every quantitative analysis undertook by a chemist amounts to a verification of the conservation of matter. However, we must not try to prove too much. Those analyses, in general, agree but *grosso modo*; it is unusual that in any somewhat complex series of operations one does not observe deviations too great to be

> attributed to measuring instruments, as Mr. Ostwald is obliged to admit. (Meyerson, 1908, p. 157)

Most chemists, after Lavoisier, simply accepted the principle of mass conservation, without worrying to submit it to a trial. The situation changed, however, in the late nineteenth century.

> Where an effort has been made to verify directly and with precision the conservation of weight in chemical phenomena, one has not always been successful in obtaining results absolutely confirming this principle. It is well known that anomalies have been noticed quite recently by Mr. Landolt. The results of the German chemist[14], although sometimes questioned, seem to have been received in general with but little skepticism by the scientific world. (Meyerson, 1908, 158)

Hans Landolt's researches on the conservation of mass made a strong impression among chemists and physicists, in the late nineteenth and early twentieth centuries. Nowadays, it is ignored not only by scientists, but also by historians of science. I have presented a detailed description and analysis of this episode in another paper (Martins, 2019b; see also Martins, 1993; Cerruti, 1996). For this reason, it will be briefly presented here.

The motivation for Landolt's experiments was the hypothesis of William Prout (1785-1850) concerning the composition of atoms. According to Prout's proposal, hydrogen should be regarded as the first matter (πρώτη ὑλη) of which the other elements are made (Brock, 1969). Each atom should be composed of an integer number of atoms of hydrogen and, therefore, the atomic mass of any chemical element should be a multiple of the atomic mass of hydrogen.

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[14] As a matter of fact, Landolt was a Swiss chemist, although he worked in Germany.

The hypothesis was compatible with data available when Prout proposed it, but soon new estimates of the relative atomic weights, by Jons Jacob Berzelius (1779-1848), challenged the conjecture. However, in 1840 Jean Baptiste Dumas (1800-1884) and his student Jean Servais Stas (1813-1891) disputed Berzelius' measurements, providing new evidence that the atomic weight of carbon was almost exactly six times that of hydrogen. However, some atomic weights were clearly fractional, such as those of chlorine and copper. In 1857-1859 Dumas was obliged to introduce a new *ad hoc* hypothesis: that the weight of the fundamental particle was half or one quarter that of the atom of hydrogen (Farrar, 1965, pp. 302-305; Smith,1976, p. 61; Hamerla, 2003, p. 359).

Stas, who initially supported Prout's conjecture, published in 1860 a new determination of eight atomic weights that provided evidence *against* the hypothesis of multiple atomic weights. Jean Charles Galissard de Marignac (1817-1894) immediately defended Prout's hypothesis, claiming that the observed deviations could be due to violations of the law of constant composition, or to the breakdown of the law of conservation of weight in the formation of complex atoms (Farrar, 1965, pp. 306-308; Smith, 1976, pp. 62-63; Hamerla, 2003, pp. 359-360).

Julius Lothar Meyer (1830-1895) claimed that there should be some kind of primordial matter making up the several elements (Farrar 1965, 309; Smith 1976, 54). He conjectured that the atoms of the several elements were built up of hydrogen atoms *together with an ether condensation*. The formation of complex atoms could entail weight variation because of the addition of ether in their synthesis (Smith, 1976, p. 57; Kragh, 1989, pp. 59-60).

Besides Lothar Meyer's conjecture, there is another atomistic theory of the ether of the late nineteenth century that is especially relevant for understanding Landolt's experiments: that of the botanist Carl Wilhelm von Nägeli (1817-1891). In an appendix to his famous book on the theory of evolution, he proposed that the ether was composed of indivisible particles called *Ameren* (in German). According to him, the atoms of each element are built of millions of *Ameren* and, therefore, there is no reason why the law of multiple atomic weights should be valid.

Hans Landolt began his experiments on weight conservation in chemical reactions in 1890, motivated by the discussion on Prout's hypothesis. In his first paper on this subject (Landolt, 1893) he referred to Prout's hypothesis, to Marignac's conjectures, to Lothar Meyer's speculations and to Nägeli's ether theory. The rationale of his experiments can be reconstructed thus: *if* there is any change of weight related to the production of the atoms of the several elements, *then* there could also exist some change of weight in chemical reactions that do not involve the production of atoms, because of the possible change of ether condensations in the formation or decomposition of compounds.

In his first experiments, Landolt studied four different chemical reactions:

$$Ag_2SO_4 + 2FeSO_4 = 2Ag + Fe_2(SO_4)_3$$

$$KIO_3 + 6H_2SO_4 + 5KI = 3I_2 + 6KHSO_4 + 3H_2O$$

$$I_2 + 2Na_2S_2O_3 = 2NaI + Na_2S_4O_6$$

$$Cl_3C\text{-}CH(OH)_2 + KOH = Cl_3CH + HCO_2K + H_2O$$

The chemical reagents were enclosed in special hermetically sealed glass recipients. For each experiment, Landolt used two similar systems, with the same set of reagents. Their weights were closely similar, with a difference of just a few milligrams – for a total weight of about one kilogram.

To understand his procedure, let us call the two systems A and B. Before the chemical reaction had occurred in any of them, the weight difference was measured several times, in the course of a few days. Then, Landolt produced the chemical reaction in A. After the reaction was complete, he compared again the weights of A and B. Then, system B was manipulated and the chemical reaction occurred in the second device. After the reaction was complete, the weights of A and B were compared again. Landolt made a detailed study of systematic influences, taking into account changes of the moisture at the surface of the glass flasks, their changes of temperature, etc.

In 1890 he studied the first reaction (reduction of silver sulphate). In each set of measurements, the standard deviation was of the order of 0.01 mg. After the chemical reaction had occurred in the apparatus A, its weight was observed to be *smaller* than before, with a reduction of 0.167 mg. After the chemical reaction in B, its weight also decreased, by 0.131 mg. The changes were much larger than the random errors, and therefore they seemed significant. A repetition of this experiment produced similar results. Therefore, in the case of the first reaction, there was an apparent reduction of about 1/800,000 of the weight of the chemical substances, or 1/6,000,000 of the weight of the devices. For the other three reactions he studied, the observed changes were smaller and somewhat irregular.

Landolt was a highly respected chemist and his research seemed careful enough to deserve attention. His article was soon reproduced in two other scientific journals. There was immediate reaction –papers presenting new experiments with positive or negative results, and theoretical discussions.

In 1906 Landolt published his second paper on the apparent changes of weight in chemical reactions. Prompted by criticism and suggestions made by several authors, he obtained a new balance, introduced changes in the apparatus and took new precautions. In the new series of experiments, Landolt also observed significant reductions of weight in the reduction of silver sulfate or nitrate by iron sulfate. The mean change was a weight reduction of 0.29 mg for each 100 g of silver, and the change seemed proportional to the total weight of the chemicals.

In the case of other reactions, the observed effects were smaller and sometimes no change was noticed. He commented:

> The question now is how the weight loss can be explained. First of all, one can express the suspicion that there is still an external cause which has not yet been discovered, but taking into account the care with which all possible sources of error have been investigated, this view is unlikely to have any probable value. On the other hand, the fact that the change only occurs to a marked extent in certain reactions, such as the reduction of silver or iodine, and in others is slight or absent, strongly suggests a relation to the chemical process.
>
> Since the explanation must be such to account only for weight loss and never increase, no other hypothesis remains except the one already mentioned in the introduction, according to which the phenomenon is founded on the detachment of small mass particles from the chemical atoms. (Landolt, 1906, p. 619)

In principle, Landolt's hypothesis in 1892 was similar to that of 1906: in both articles, he supposed that some very small subatomic particles had escaped from the glass vessels. However, in his first paper he was thinking about *ether particles*, and in the second one he referred to the recently discovered *radioactive phenomena*. He conjectured that a small part of the atoms could split off, as the result of release of energy during chemical reactions. To account for the observed effect, the emitted particles should traverse the walls of the glass flasks. Therefore, the relevant problem was whether the glass vessels could be regarded as closed systems of not, regarding ponderable substances.

Notice that Landolt always accepted the strict conservation of weight in chemical reactions. He did state that the weight of the apparatus had changed, but he interpreted the change as due to something that had escaped from it – ether particles or subatomic particles. However, if those auxiliary hypotheses encountered severe difficulties and if there was no other alternative explanation for the change of weight during chemical reactions, then the experiment itself could have been faulty. That was Landolt's attitude in his last works on this subject, published in 1908 and 1910.

Although in his previous researches Landolt had paid much attention to all possible sources of systematic error, in his 1908 paper he studies again two of them: the variation of moisture on the outer walls of the glass vessels, due to the heat developed by the chemical reactions; and the change of volume of those containers. He concluded that the first effect did not introduce any relevant error; however, the second one did, because the thermal dilatation of the glass vessels was irreversible, contrary to his expectations. The increased volume produced an apparent decrease of the weight of the system, because it was weighed in the air – not in a vacuum.

Because of this possible source of error, Landolt repeated his experiments and he also computed corrections that should be applied to his former measurements. His final conclusion, taking into account 48 experiments concerning 15 different chemical reactions, was that there was no weight change larger than the estimated experimental error of 0.03 mg for a total weight of 400 g. If there was any real change of weight, it was smaller than one part in 10 million.

Landolt's last work on this subject was published posthumously (Landolt, 1910). It was a review of all his previous experiments, with a conclusion similar to that of the 1908 paper. Some later authors described Landolt's research as a *proof* of the law of conservation of matter (or weight) in chemical reactions (Laue, 1959, pp. 509-510). Of course, from a logical point of view, it is impossible to provide an experimental proof that the conservation of weight holds *exactly*, for *any* chemical reaction. All that can be safely admitted is that *for a specific set of chemical reactions*, studied under such and such conditions, no weight change *larger than the experimental error* was observed.

9. MEYERSON'S INTERPRETATION OF THE PRINCIPLE OF MASS CONSERVATION

After presenting an outline of the history of the principle of mass conservation, Meyerson discusses whether it is an empirical law (Meyerson, 1908, pp. 154-158) or an *a priori* principle (Meyerson, 1908, pp. 158-162), providing strong arguments against both alternatives; and then he explains his own view (Meyerson, 1908, pp. 162-168).

> Is this principle of empirical origin? This has often been affirmed, and John Stuart Mill especially formulated this thesis with much clearness. According to him, the conservation of matter is suggested to us from the very beginning of our observations, so to speak, by a large number of concordant phenomena, whereas others, on the contrary, seem to contradict it. The hypothesis was formulated that this principle was, not partially, but entirely, true, and it was verified afterwards. The verification having succeeded, the principle was established, exactly as any other experimental law. (Meyerson, 1908, p. 154)

This is, of course, the standard empiricist view of the founding of scientific laws. The principle of conservation of mass would have been obtained by experiments, that is, *a posteriori*. Historical information, however, disproves this interpretation. From the Greek atomists to Lavoisier, no researcher attempted to test the law.

> […] it is enough to peruse his [Lavoisier's] works to be convinced that this is not so; that, exactly as the ancients and as Jean Rey, he applies the principle with complete confidence, not doubting for a single moment that it should be confirmed by experience, and that every anomaly must be only apparent and should be explicable. (Meyerson, 1908, p. 156)

> It follows from the information that we have just briefly summarized that even at the present moment the certainty with which the principle of the conservation of matter appears invested is much higher than what is permitted by the experiments which are supposed to serve as its basis. (Meyerson, 1908, p. 158)

After Immanuel Kant's philosophical work, it had become common to distinguish scientific statements as *a priori* (that is, evident and produced by reason) or *a posteriori* (that is, not evident, not purely rational and depending on experience). Having shown that the principle of conservation of matter was not grounded on experiments, Meyerson discussed the second possibility:

> Is this principle therefore aprioristic? This seems to be the most widely diffused opinion in our time among scientists and philosophers, an opinion sometimes clearly stated, sometimes implicitly affirmed. Moreover, […] we shall not have to search long for the source of this opinion; all those who have endeavored to demonstrate *a priori* the conservation of matter have brought directly into play the principle of causality, to such a point that, as we have seen at the beginning of the chapter, the very statements of the two principles are sometimes muddled up. (Meyerson, 1908, pp. 158-159)

> Kant bothered himself with this question many times. […] He explained himself in a more detailed way in the *First Metaphysical Principles of Science and Nature*. There he expresses his "first theorem of mechanics" thus: "Through all the modifications of material nature, the total quantity of matter remains the same, without increase and without diminution." In the "demonstration" of this theorem, Kant makes use of the principle that he borrows from "general metaphysics," which states that in all the modifications of nature, there is neither creation nor destruction of substance. Afterwards he determines that substance, for matter, is its quantity […] (Meyerson, 1908, p. 159)

In the specific case of Kant, Meyerson shows that there was a gradual transformation of the general principle of causality to the specific statement of conservation of mass or weight. However, that transition requires some steps that cannot be justified by pure reason: the identification of "substance" with "quantity of matter" and then with weight of mass is unjustifiable *a priori*. Meyerson also discussed the *a priori* "proofs" of other authors (Arthur Schopenhauer, William Whewell and Herbert Spencer) and showed that they also mistook the empirical quantitative concepts of weight or mass for the philosophical concept of substance (Meyerson, 1908, pp. 160-161). Therefore, those aprioristic "proofs" are unacceptable. Meyerson also pointed out that many philosophers had clearly denied the principle; and that the circulation of atomistic ideas and, later, of Jean Rey's work did not lead to the acceptance of the principle of conservation of matter (Meyerson, 1908, p. 162). If that were really an aprioristic principle, why wasn't it accepted by everyone?

> The mystery disappears if we think what really is the aprioristic demonstration of the principle. It is completely based on causality. Now, what we call causality is only a tendency, the tendency to retain the identity of some things in time. At most one may say that the causal tendency makes us the hope that these things are such that we may, without too great a violence, regard them as essential. […]
>
> At the basis of the principle of conservation of matter there are three distinct notions: matter, weight, and mass. Matter is a common-sense notion, a complex one, which synthetizes an infinite number of properties. It is clearly contrary even to the most superficial experience to suppose the conservation of all those properties. Therefore, in sustaining the conservation of matter, one only postulates the permanence of some, among them; that is the reason why this statement cannot interest science as long as one does not elucidate precisely what must be conserved. (Meyerson, 1908, p. 162)

It is possible to measure many different properties of matter: its temperature, its size, its thermal capacity, etc. Which one is conserved, and why? This can only be found *a posteriori*, by experiments. There is no logical connection between mass (or weight) and the essential notion of matter. However, when one overlooks that gap, it seems that it is possible to derive the law of conservation of mass (or weight) from the *a priori* principle of causality and that no empirical assessment is needed.

> [...] any attempt to deduce *a priori* the principle of the conservation of weight will certainly be sterile. The opposite illusion can only have its source in the causal tendency; and it is this same tendency, which in ancient atomists, in Jean Rey, probably in Lavoisier himself, and surely in his contemporaries, has contributed to the rise of the principle, permitting its formulation, without proof, as a self-evident truth, ensuring its dominance.
>
> We desire mass to be constant, like everything else; but we desire it more strongly because it appears to us as the essence of matter. (Meyerson, 1908, p. 162)

> [..] the conservation of mass cannot be considered as an aprioristic truth; and we believe that the assembly of the considerations which we have brought out clearly predisposes to establish that it is a plausible proposition, just like the principle of inertia. (Meyerson, 1908, p. 163)

Meyerson's approach partially agrees with that of Kant:

> The true approach was pointed out by Kant: there is indeed a concord between our understanding and reality, but this agreement is partial […] Reality is partly intelligible and our scientific knowledge contains a mixture of aprioristic elements and other that are *a posteriori*. (Meyerson, 1908, p. 366)

However, Meyerson criticized Kant for assigning an excessive weight to deduction:

> So, when he talks about conservation of matter, he says: "We borrow from general metaphysics this principle that we accept as basic: that in every change of nature, no substance is lost or created. Here one is just displaying what is substance, in matter." That is, indeed, as we have seen, the real foundation of that principle. However, for Kant, the last part of its propositions is likewise aprioristic: the concept of matter includes, in his opinion, not only that of mass, but also that of weight [...]
>
> Kant believes that science contains a *pure* component, that is, a purely rational one and, consequently, completely *a priori*. That part contains not only what we call cinematics […] but also part of mechanics. However, that is not the case. There is no pure mechanics, nor pure cinematics. (Meyerson, 1908, pp. 366-367)

There are scientific laws that can be submitted to experiments but that, nevertheless, are believed to be universal truths, because they seem *necessary*. Meyerson accepted William Whewell's interpretation of this apparent contradiction:

> "The solution of that paradox is this: those laws are interpretations of the axioms of causality. The axioms are universally and necessarily true, but the appropriate interpretation of the expressions they contain is taught by experience. Our idea of cause provides the *form* and experience the *matter* of those laws." (Whewell, *apud* Meyerson, 1908, pp. 367-368)

According to Meyerson, the scientific laws of conservation are neither *a priori* nor *a posteriori*, although they contain components of the two types. In the lack of a better terminology, he described them as *plausible* laws.

> […] taken literally, the principle of identity in time would signify: everything persists, an affirmation immediately denied by experience; […]. Hence the statement becomes: certain essential things persist. But this is an indefinite

> formula, for it does not tell us what are the things which persist and which, consequently, we ought to consider as essential. It is experience alone which can teach us that. But in this matter experience plays a peculiar role, in the sense that it is not free, for it obeys the principle of causality, which we may call more precisely the causal tendency because it manifests its action in commanding us to seek in the diversity of phenomena something which persists. The formula constitutes thus, according to Boutroux's admirable expression, not a law but a "template" of laws.
>
> From what we have just contended [...], we can draw this general conclusion: every proposition stipulating identity in time appears to us as invested *a priori* with a high degree of probability. It finds our minds prepared, it seduces them, and is immediately adopted, unless contradicted by very evident facts. Perhaps it would be wise to apply to statements of this category, intermediary between the *a priori* and the *a posteriori* a special term. We should propose, for lack of a better one, the term *plausible*. Therefore, every proposition stipulating identity in time, every law of conservation, is plausible. (Meyerson 1908, 133-134)

Meyerson used "plausible" for describing those statements that are neither entirely *a priori* nor totally *a posteriori*. That was not a wise choice, in my opinion, because "plausible" is usually understood as equivalent to probable. With the exception of this name, I agree with Meyerson's interpretation of the laws of conservation.

10. WHO WAS ÉMILE MEYERSON?

The interpretation used in this paper was heavily influenced by the work of Émile Meyerson. Since this philosopher is not well known nowadays, this section will present some biographical information about him.

Emil Azriel Meyerson (Fig. 6), better known by the French variant of his name (Émile Meyerson), was born in Lublin, Poland (then a Russian territory), in 1859, from Jew ancestors.[15] In 1870 he moved to Germany to complete his education, and he later attended the University of Heidelberg, where he studied chemistry with Robert Wilhelm Eberhard von Bunsen (1811-1899) and Hermann Franz Moritz Kopp (1817-1892), and afterwards with Carl Theodore Liebermann (1842-1914), at the University of Berlin (Telkes-Klein, 2007a).

Fig. 6 Émile Meyerson

Although his early involvement with chemistry was mostly scientific and technical, he soon became interested in the history

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[15] Émile Meyerson's life and the evolution of his thought have been recently elucidated thanks to the study of his correspondence and manuscripts kept at the Central Zionist Archives in Jerusalem. See, for instance, Bensaude-Vincent & Telkes-Klein (2016).

of that science. During his stay in Heidelberg, he came across the books on history of chemistry written by Kopp, and he was strongly influenced by them. From the history of science, he soon became involved in philosophy of science. His chemistry background clearly inspired much of his later work, as was also the case with other French historians and philosophers of science (Bensaude-Vincent, 2005; Brenner & Henn, 2013).

In 1882 Meyerson moved to Paris, where he was accepted as trainee of Paul Schützenberger (1829-1897) at the *Collège de France*, thanks to a recommendation letter written by Bunsen. Schützenberger was the professor of mineral chemistry at the *Collège de France* and the first director of the *École Supérieure de Physique et de Chimie Industrielles*, where Pierre Curie worked and Marie Curie later began her first researches on radioactivity. With Schützenberger, Meyerson acquired an expertise in industrial chemistry and the synthesis of dyes – one of the most profitable fields of chemistry, at that time. Parallel to his chemical studies, after his arrival to Paris, Meyerson frequently visited libraries to dedicate himself to the study of the history of chemistry. At this time, he became aware of the French version of the history of chemistry, sponsored by Jean-Baptiste Dumas (1800-1884) in his *Leçons sur la philosophie chimique*, which presented Lavoisier as the first chemist in history. He recalled Kopp's works that had shown the importance of pre-Lavoisier phlogiston theory, and began to study the role of some of the precursors of modern chemistry.

At the beginning of 1884, Meyerson visited Lublin and, during his stay there, he wrote his first article, a historical essay on Jean Rey and the conservation of matter (Meyerson, 1884).

Returning to France, he was hired by the chemical industry *Établissements A. Collineau et Cie*, in Argenteuil, where he worked on the production of dyes. After two years he was appointed as a chief chemist of the industry, with double salary; but in July 1886 he resigned this job. In January 1888 he registered a patent of a method for the synthetic production indigo, but the process was not successful on an industrial scale (Telkes-Klein, 2007a; Bensaude-Vincent, 2010a).

During the subsequent years he maintained some work on chemistry, both in France (analysis of wine and milk) and in Russia (the plan for a chemical industry in Saint Petersburg). Parallel to that work, he kept his scholar inquiry and published a few papers on the history of chemistry (Meyerson, 1888; Meyerson, 1891). Thanks to his knowledge of many languages (Polish, German, Russian, French, English, and Italian), in 1889 he became the foreign policy editor at the *Agence Havas*, one of the oldest news agency of the world. This job did not require a great effort from him – just a few hours a day – and gave him some financial safety in the next nine years, during which interval he dedicated himself deeply to his historical and philosophical research. He spent most of his free time at the *Bibliothèque Nationale*, reading and taking notes about philosophy, science, and the history of science. He was already working on his main philosophical project that would finally produce his masterpiece *Identité et réalité*, but he felt many difficulties and postponed it for years. During this period, besides reading about philosophy, he studied Latin (a language he did not know well) and he got a firm knowledge of mathematical physics. Meyerson's erudition was to become legendary (Telkes-Klein, 2010).

In the decade of 1890 Émile Meyerson got gradually involved in Zionism (Telkes-Klein, 2015). In 1893 he became a member of the organization *'Hovevei Sion* (Lovers of Zion), which had the aim to promote Jewish immigration to Palestine, and advance Jewish settlement there. In 1894 he was the secretary of the *Conférence des Sociétés Palestiniennes*, and from 1894 to 1896 the secretary of the Central Committee of the *'Hovevei Sion* in Paris. In 1898 Meyerson got a job at Baron Maurice de Hirsch's *Jewish Colonization Association* that aimed at the creation of Jew colonies in several parts of the world. He also got involved with Baron Edmond Benjamin James de Rothschild's personal endeavor of establishing Jew colonies in Palestine. His job required him to travel to Russia, Poland and Palestine. He also published several works concerning the situation of Jews in Russia, and about the colonies established in Palestine. Meyerson became the director of one of its divisions, the *Jewish Colonization Association for Europe and Asia*.

A letter from Émile Meyerson to his sister shows that his decision to work at the *Jewish Colonization Association* was a financial choice (Telkes-Klein, 2010; Telkes-Klein, 2004, p. 206). His earnings at *Agence Havas* were low and he had debts. He was nearly 40 years old and had no hope of obtaining an academic position. He needed a more comfortable situation, although he knew in advance that he would not have so much free time in that new position as he had during the period of *Agence Havas*.

He never became a university professor. He was not, however, a secluded scholar. He involved himself in the *Société Française de Philosophie*, founded by Xavier Léon in 1901, and maintained a circle of friends with whom he kept a continuous discussion of philosophical ideas (Soulié, 2010). One of them was Lucien Lévy-Bruhl, who is nowadays better known for his anthropological and sociological studies, but who was at that time the professor of history of modern philosophy at the Sorbonne.

The first and most influential of Émile Meyerson's philosophical works was *Identité et réalité*, published in 1908 at the expenses of the author. The book received favorable reviews in several French and foreign journals. It established Meyerson as an outstanding philosopher and it immediately called the attention of Léon Brunschvicg, Henri Bergson, and Ernst Cassirer, to cite only a few remarkable names.

Meyerson became very prominent, not only through his publications but also because of an intellectual circle that gathered around him, including philosophers and scientists. After his retirement from the *Jewish Colonization Association*, in 1923, Meyerson promoted a weekly meeting at his home, every Thursday afternoon, regularly attended by young and older scholars, including Louis de Broglie, Léon Brunschvicg, Alexandre Koyré, André Lalande, Paul Langevin, Lucien Lévy-Bruhl, André George, Salomon Reinach, Henri Gouhier, André Metz, and Hélène Metzger (Telkes-Klein, 2007a; Telkes-Klein, 2007b).

Some of the participants of this circle can be called his disciples or *protégés*: Henri Gouhier, Alexandre Koyré, André Metz, Hélène Metzger, Henri Poirier, Désiré Roustan. Hélène Metzger was a niece of Lucien Lévy-Bruhl. She became a remarkable historian of chemistry and dedicated one of her books, *Newton, Stahl, Boerhaave et la doctrine chimique* (1930) to Meyerson (see, however, Chimisso & Freudenthal, 2003). Koyré referred to Meyerson as his teacher, and his first outstanding book on the history of science, *Études Galiléennes*, published in 1939, was dedicated to the memory of Émile Meyerson (Simons, 2017).

Besides the three editions of his first book, *Identité et réalité*, Meyerson published many articles and several books during his lifetime: *De l'explication dans les sciences*, 2 vols. (1921), *La déduction relativiste* (1925), and *Du cheminement de la pensée*, 3 vols. (1931). *La déduction relativiste* was written as a reaction to Albert Einstein's theory of general relativity, offering a philosophical interpretation of the role of space-time in that theory. In 1928, Einstein published a joint article with André Metz where he expressed approval and admiration for that work (Einstein & Metz, 1928). In 1930, Meyerson's *Identité et Réalité* had been translated to German, English, and Spanish, becoming available to many readers.

Shortly before his death, Meyerson wrote his last work, *Réel et déterminisme dans la physique quantique* (published in 1933), by request of his friend Louis de Broglie, who wrote the preface of the book. A posthumous volume, *Essais*, edited by Lucien Lévy-Bruhl in 1936, contains a selection of his articles.

Émile Meyerson's work was later strongly criticized by Gaston Bachelard (Lecourt, 2002, pp. 28, 35-39; Perraudin, 2008; Wetshingolo, 1996), maybe unfairly (Fruteau de Laclos, 2008; Bensaude-Vincent, 2010b), and this was possibly one reason why he was overlooked in the following decades in France. In other countries, the influence of Analytic Philosophy was certainly a motive for neglecting Meyerson's ideas (Biagoli, 1988, pp. 35-37). His importance was, however, vindicated by Thomas Kuhn, who referred to Meyerson's ideas as influential while developing the ideas for his main work *The Structure of Scientific Revolutions* (Kuhn, 2012, p. xl). In the recent decades, Meyerson's philosophy has received much more attention (Laugier, 2009; Brenner, 2010). Some of his letters and unpublished manuscripts have been recently published by Bernadette Bensaude-Vincent and Eva Telkes-Klein (Meyerson, 2009; Meyerson, 2011), and his books have received new translations.[16]

11. THE NATURE OF THE LAW OF MASS CONSERVATION AND SCIENTIFIC EDUCATION

The historical study of the law of conservation of mass provides a noteworthy instance of the influence of philosophical ideas in science. The philosophical principle of permanence of substance – Émile Meyerson's law of causality – is not a result of science; it is an *a priori* principle that has shaped the development of the laws of conservation.

This case study is relevant for the teaching of chemistry (and of science, in general), because it clearly shows, in a particular historical episode, the influence of philosophical principles on the development of science, thereby providing a nice example against the inductivist view of science.

There are, however, many difficulties in any attempt to convey this epistemological message in science teaching. Both teachers and students have their preconceptions about what is

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[16] There is a fairly complete bibliography on Meyerson, describing works published up to the beginning of the current century (Fruteau de Laclos, 2003).

science and about how scientists work, and most of them accept naïve inductivism. It is very hard to replace those preconceptions by a more adequate understanding of science – teachers and students usually react against any deep change of their views, or they simply disregard the new message and the arguments that are presented for it (in this case, historical information).

Any long-term educational change must involve both new educational materials and techniques, and the adequate training of teachers. The second point is probably the hardest challenge. University professors who instruct prospective science teachers usually have the same inadequate knowledge of history and philosophy of science that should be supplanted. All over the world, the number of historians and philosophers of science is negligible compared to that of scientists; their direct influence at the universities upon potential science teachers cannot be considerable. It would be possible, however, to strengthen their influence if they provided an adequate training to the future university professors who will instruct the potential new teachers.

Although digital media are increasingly used as educational materials, textbooks have not been superseded. Unfortunately, scientific textbooks show a strong apathy to the introduction of an adequate view on the nature of science, and they tend to perpetuate the old-fashioned views regretted by us – those who endorse the use of history and philosophy of science in education.

There are some outstanding exceptions. In a well-known textbook written by two historians of physics[17] – Gerald Holton and Stephen Brush – we find a detailed and nice account of the history of the law of conservation of mass, including the

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[17] Holton and Brush's book was the third version of Holton's *Introduction to Concepts and Theories in Physical Science*, where we find a smaller version of the history, which does not mention Landolt (Holton, 1952, pp. 279-285).

contribution of Hans Landolt (Holton & Brush, 2001, chapter 15, pp. 203-208). Unfortunately, their careful historical approach is uncommon.

Historians have sometimes criticized the pseudo-historical lore appearing in textbooks, with little impact. Let us describe two examples concerning the specific case of the principle of conservation of mass.

Bernadette Bensaude-Vincent and Nicolas Journet, in a paper describing the contributions of Lavoisier, mentioned five French textbooks and pointed out some of their historical mistakes, such as ascribing to Lavoisier the "discovery" of the principle as a consequence of "delicate measurements"; the association between the principle and the atomic model (Lavoisier was not an atomist); and the supposed quantitative confirmation of the principle in Lavoisier's experiments on the composition of air (Bensaude-Vincent & Journet, 1993, p. 62).

At the end of his paper on the history of the principle of mass conservation, the historian of science Robert Siegfried commented that "The history of science can teach fundamental lessons about the nature of scientific thought itself" (Siegfried, 1989, p. 22), and then presented the flawed account he found in Ebbing's popular textbook on General Chemistry.

> Antoine Lavoisier (1743-1794), a French chemist, insisted on the use of the balance in chemical research. His experiments demonstrated the law of conservation of mass, *a principle that states that mass remains constant during a chemical change (chemical reaction)*. A flash bulb gives a convenient illustration of this law. (Ebbing, 1987, p. 3, *apud* Siegfried, 1989, p. 23)

Siegfried commented:

> The author completes his point by indicating that the flash bulb weighs the same before and after it is ignited. But what will the student learn from this passage? First, that Lavoisier demonstrated the law of conservation of mass or weight,

> presumably in a manner like that utilized in the flashbulb experiment. As we have seen, Lavoisier did no such thing, but took the principle as "an incontestable axiom" incapable of direct experimental demonstration.
>
> But the important point here is not the author's misrepresentation of Lavoisier's work (though that is lamentable enough), but that in so doing he misrepresents the manner by which such broad general principles are established in science. By implying that Lavoisier arrived at this principle by generalization from a large number of cases, presumably some 18th century equivalent of flash bulbs, the author is promoting the Baconian or inductive method, a view long recognized as inadequate and misleading. (Siegfried, 1989, p. 23)

Unfortunately, two decades after the publication of Siegfried's critical comment, the new editions of Ebbing's book still contain the same epistemological mistake:

> Antoine Lavoisier (1743-1794), a French chemist, was one of the first to insist on the use of the balance in chemical research. By weighing substances before and after chemical change, he demonstrated the *law of conservation of mass*, which states that *the total mass remains constant during a chemical change (chemical reaction)*. (Ebbing & Gammon, 2017, p. 6)

Unfortunately, misconceptions about the nature of science die hard.

Suppose that some teachers or students are trying to learn about the history of the principle of mass conservation. They would probably start by reading the versions contained in textbooks, popular works on history of science (especially biographical romances of "great scientists") and information available at the Internet. The historical and epistemological facets of those accounts will be probably flawed; but the readers will be usually unable to notice it. Of course, there are nice historical papers on the subject; however, as Gerald Holton remarked,

> Among the historians of science, of which there are only a few thousand professionals in the world, the writings in their professional journals are almost by definition of the kind that rarely would find their way into the hands of science educators. (Holton, 2003, p. 603)

It is frustrating to notice that academic work on history of science, intended to be used by educators, do seldom get noticed and used. Many years ago, I coauthored a paper, in Portuguese, on Lavoisier and the conservation of mass (Martins & Martins, 1993). The full paper is freely available at the Internet, from three different websites. Unfortunately, a fresh Google search provided only 37 web pages referring to that paper – including those pages that contain the paper itself. A slightly worse result was obtained searching for my paper on Hans Landolt's researchers on mass conservation (Martins, 1993).

Maybe the time has come when historians and philosophers of science who intend to produce any influence upon science teaching should devote themselves to the development of more effective strategies to disseminate a better view of the nature of science. It is not enough to produce adequate accounts: it is indispensable that they reach the students and teachers. A promising move would be contributing new information and correcting mistakes appearing in one of the most popular resources of the Internet: the Wikipedia.

In 2008, a graduate student called Sage Ross published an article where he called the attention of historians of science to the relevance of Wikipedia (Ross, 2008). As a rejoinder to this article, the historian of science Alan Rocke summoned up the members of the History of Science Society to review Wikipedia articles on historical material containing manifest blunders (Rocke, 2008).

What is the significance of Wikipedia's articles? Wikipedia is one of the ten top sites, worldwide. Links to Wikipedia pages usually appear at the top of Google searches, when one looks for scientific information. Updating Ross' data, we can remark that the English version of the article on Albert Einstein was retrieved about nine million times in 2017, a mean of about 26,000 times each day – therefore, it has a huge impact. The article on Lavoisier is not so popular, but there were about 450,000 page views in 2017, for the English version. Hence, a mean of over one thousand people find faulty historical information on Lavoisier, daily, when they consult the English version of Wikipedia.

Creating an independent web page on Lavoisier or publishing a paper on his contributions (such as the present paper) will have a much smaller impact than correcting the corresponding article on Wikipedia. Anyone can contribute to Wikipedia, either creating new articles or introducing changes in the available pages. If the modifications are accompanied by relevant scholarly bibliographic references, they will be accepted and preserved, as a rule. By allocating a few hours to improve any popular Wikipedia article such as that, a person can instantaneously reach a large readership and may help to improve the public understanding of the nature of science.

Besides the proposals and advices of Ross and Rocke on the subject, I would like to add another recommendation. I strongly suggest that any historian of science who is willing to contribute to Wikipedia should adopt the strategy of keeping (and criticizing) faulty views, instead of simply replacing them by better interpretations. Indeed, pseudohistory will not disappear only by presenting a sound historical version of the same episodes. Readers must be told about those faulty historical and epistemological versions, and they should also be informed about the reason why these should be rejected. After that, the more acceptable view should be introduced, also explaining why it is better than the other ones.

In my teaching practice, I have adopted a similar approach, and I can tell you that it does work (Martins, 2018). Of course, this is just one among many ways of improving the public understanding of the nature of science.

12. FINAL REMARKS

The aim of this paper was not to *defend* Émile Meyerson's interpretation of the laws of conservation (and, particularly, of the principle of conservation of mass). Meyerson's ideas do not need my support – they were well justified by their author. Unfortunately, Meyerson's prolix style did not help in the diffusion of his thought; and his work is not well known nowadays, either in France or abroad. I believe that his peculiar epistemological ideas, grounded upon sound historical research, deserve to be disseminated – and that they can help to prevent scientists, teachers and students from believing in the naïve empiricist interpretation of science.

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