

***The Mixed Blessings of Pragmatism. Jean-Baptiste Dumas
and the (Al)chemical Quest for Metallic Transmutation***

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Received: Jun 01, 2023 **Revised:** Jun 26, 2023 **Just Accepted Online:** Jul 03, 2023 **Published:** Xxx

This article has been accepted for publication and undergone full peer review but has not been through the copyediting, typesetting, pagination and proofreading process, which may lead to differences between this version and the Version of Record.

Please cite this article as:

L. Anatrini (2023) The Mixed Blessings of Pragmatism. Jean-Baptiste Dumas and the (Al)chemical Quest for Metallic Transmutation. **Substantia**. *Just Accepted*. DOI: 10.36253/Substantia-2169

Abstract - There were at least three prerequisites for the transmutability of metals to become once again a scientifically acceptable subject of research from the 1810s: new hypotheses concerning the mutual reducibility of certain elements, such as those of integer multiples and protyle put forward by the British chemist and physician William Prout; the experimental confirmation that chemical compounds with the same percentage composition could be substances with very different properties, i.e. the discovery of isomerism and allotropy; the comparison between metals and compound radicals of organic chemistry. This paper aims at illustrating how these premises were exploited by Jean-Baptiste Dumas, one of the leading French chemists of the 19th century, to reintroduce in the chemical discourse the alchemical topic of transmutation.

Keywords – Transmutation, Jean-Baptiste Dumas, Ammonium, Jöns Jacob Berzelius, Cyprien-Théodore Tiffereau

Introduction

The experimentation conducted towards the end of the 18th century, which helped to describe the regularity and reproducibility that characterised a wide range of chemical compositions and led, for example, to the enunciation of the law of definite proportions by Joseph-Louis Proust (1754 - 1826), were propaedeutic to the spread of a new idea of atomism. After all, with a simple logical step, it was possible to deduce, from the regularities observed in chemical reactions, a

necessary regularity in the composition of matter of a corpuscular nature. The atom originally theorised in Democritus' time (c. 460 - c. 370 BC), however, was the minimal entity of a uniform and continuous matter understood ontologically and not instrumentally, whose essentially identical parts differed only in size, form and motion. In such a system, chemical change was generated by alterations - occurring in a vacuum - in the ordering of atoms into *molecular* structures. Nevertheless, everything since the results produced by the chemical reform of Georg Ernst Stahl (1659 - 1734), suggested the existence of a whole range of elements characterised by exclusive qualities.

Stemming from the desire and need to quantify the ultimate units of matter for calculative purposes, aiming at a consequent mathematisation of chemistry, the atomic theory of John Dalton (1766 - 1844) was born. With it, the English scientist united quantitative speculations inspired by Democritus with a qualitatively categorised matter, overcoming the very limits beyond which Antoine Lavoisier (1743 - 1794) had relegated a purely philosophical investigation of Empedoclean descent. In an effort of hopeful pragmatism, Dalton's atom was no longer the omnipresent manifestation of matter, becoming instead the physical unit of measurement of Lavoisier's *substances simples*¹.

Although the acceptance of the physical reality of the Daltonian atom in the first half of the 19th century was certainly not extraordinarily widespread in France, the theorisations of the English scientist had emphasised the importance of the quantification guaranteed by the system of atomic weights in the elaboration of the categorisations and classifications necessary to establish the foundations of a science of chemical relationships, a prodrome to structural chemistry. And it was precisely in this field that in 1826 one of the fathers of organic chemistry, Jean-Baptiste Dumas (1800 - 1884), brought his research to the attention of the international scientific community². By experimentally applying the hypothesis of Amedeo Avogadro (1776 - 1856)³ - which he knew and studied through André-Marie Ampère (1775 - 1836)⁴ -, he proposed new methods for determining the molecular weight of gases, obtained through volume density, succeeding in optimising and even correcting the data of Jöns Jacob Berzelius (1779 - 1848), who, with his calculation of the atomic weights of 45 different elements and the centesimal composition of some 2000 chemical compounds, is remembered as the greatest experimenter of the early 19th century⁵. In 1818, he was able to theorise a first set of atomic weights based on entirely experimental data⁶, disproving the possibility of a total generalisation of the hypothesis developed in 1815 by the physician and chemist William Prout (1785 - 1850) aimed at illustrating the atomic weights of the elements as integer multiples of that of hydrogen⁷. Then, from 1826 onwards, Berzelius refounded his investigation - and in this context, the desire for revenge provoked by Dumas' essay must have played no small part - through the instrumental adoption of two innovations of particular importance for chemical research.

In 1819, chemist Pierre Louis Dulong (1785 - 1838) and physicist Alexis Thérèse Petit (1791 - 1820), succeeded in calculating the specific heat of 13 different elements (11 metals, tellurium and sulphur) and discovered their similar heat capacity (between 0.3675 and 0.3830, for O = 1 and H₂O having $c = 1$), i.e. the constant describing the product between the relative atomic weight of an element and its specific heat⁸. In the same year, the German Eilhard Mitscherlich (1794 - 1863), in the course of lengthy crystallographic experiments, put forward a hypothesis concerning the possibility that substances with similar chemical properties and crystalline form, called isomorphic by Berzelius himself, might have similar formulae⁹.

Believing is seeing: the convictions of a scientist

Dumas desired to succeed in obtaining stable and experimentally consistent principles, and the opportunity that Avogadro's hypothesis offered was unrepeatable. However, from the time of his first major publication - as is also evident from his 1832 doctoral thesis - the young chemist had been grappling with a problem difficult to surmount, found in the measurement of the molecular volumes of the phosphorus contained in his trichloride (PCl₃) and the sulphur contained in hydrogen sulphide (H₂S)¹⁰.

In the particular case of phosphorus and chlorine, by reacting one volume of the former with three volumes of the latter, Dumas could not explain how it was possible that, if Avogadro and Ampère had been right, not one but two volumes of phosphorus trichloride would be generated¹¹. The cause of the problem lay in the widespread terminological confusion linked to the atomistic lexicon, which Dumas had declined from Ampère's, even going so far as to hypothesise the divisibility of the elementary molecules, although he did not realise the tetratomicity of liquid phosphorus and the diatomicity of chlorine in the gaseous state (and, more generally, not imagining that elements placed in reaction could give rise to *variations in atomicity*), finally resorting to expressions that were variously criticised, when not entirely oxymoronic, such as that of 'half-atom'¹². Moreover, the values he calculated were based on Berzelius' 1818 atomic weights, which for phosphorus and chlorine were twice as high as they should have been. Thus, he would have expected a synthesis reaction of the type $P + 3Cl \rightarrow PCl_3$, whereas what he obtained was $P_4 + 6Cl_2 \rightarrow 4PCl_3$ ¹³.

From 1828 onwards, Dumas endeavoured to adopt the Avogadrian criterion of the distinction between the physical particle and the chemical particle, constantly emphasising the material reality of the former and the purely instrumental dimension of the latter, with a methodology partly borrowed from the thought of William Hyde Wollaston (1766 - 1828)¹⁴. This latter, in fact, decided to address the shortcomings caused by an ontologically understood atomism from a decidedly more pragmatic point of view, emphasising the priority of practical effects and purposes of chemical research. In a

celebrated 1814 essay entitled *A Synoptic Scale of Chemical Equivalents*, he introduced a stable categorisation to define the minimum quantities required for elements to enter combinations forming compounds¹⁵.

Dumas, however, was not satisfied with a utilitarian implementation of Wollaston's equivalents¹⁶. In fact, by the time his colleague Marc Antoine Gaudin (1804 - 1880), with his *Recherche sur la structure intime des corps inorganiques* (1833), had brought to the attention of the scientific community the importance of a stable nomenclature that provided a clear distinction between concepts such as 'molecule' and 'atom', especially in virtue of the increasingly encouraging results provided by research into atomic weights (regarding which Gaudin was the first to hypothesise the polyatomicity of certain elements), Dumas had become definitively convinced that the results of his experiments offered a clear refutation of Avogadro's hypothesis¹⁷.

As has been persuasively illustrated by some scholars, at the basis of Dumas' rejection was surely the perception that atomism was little more than a faith and that, in the end, no experimentation would be able to account for the existence of ultimate physical entities¹⁸. Therefore, faced with the results of his experiments, rather than questioning the ontological value of what he probably considered to be non-essential abstractions, he chose, while recognising the instrumental usefulness of the system developed by Dalton, not to engage in theoretical elaborations concerning the physical dimension of ultimate entities. Conversely, he showed an increasing interest in another hypothesis, based on experimental data which, although variously manipulated and aiming at an even more general theorisation, were plausibly preferred as they could be used to investigate the relationships between the various elements without necessarily delving into lucubrations on the nature of matter. The hypothesis in question is that of the integer multiples of hydrogen elaborated by Prout, which Dumas no doubt became acquainted with thanks to the French edition of *An Attempt to Establish the First Principles of Chemistry by Experiment* (1825), by Dalton's pupil and first biographer, Thomas Thomson (1773 - 1852)¹⁹. Added to this, there were two other factors which plausibly, at a time before proper research on valence and stereochemistry, led Dumas to increasingly doubt the value of atomic theory.

In 1828, the German chemist Friedrich Wöhler (1800 - 1882) documented the first case of isomerism, unintentionally demonstrating the convertibility between organic and inorganic compounds. Trying to obtain ammonium cyanate ($\text{CH}_4\text{N}_2\text{O}$ - an inorganic salt), he succeeded instead in synthesising urea ($\text{CO}(\text{NH}_2)_2$ - an organic compound contained in the urine of almost all tetrapod organisms) by reacting purely inorganic substances such as silver cyanide (AgCN) and ammonium chloride (NH_4Cl). This experience led to the formulation of the principle of isomerism, whereby substances with very different physical properties and chemical behaviour can have the same

molecular mass and percentage composition²⁰. Something similar was observed for elementary substances (at the time mainly in carbon and sulphur), thanks to the polymorphic nature of certain *corps simples*, describing phenomena to which Berzelius would give the name allotropy in 1841²¹.

Convinced of the absolute precedence of experimental data in the elaboration of hypotheses otherwise judged arbitrary and aprioristic, Dumas, perhaps due to a lack of imagination and an excess of faith in the mathematisation of scientific research, abandoned the prophetic intuitions of Avogadro and Ampère in favour of data useful to quantify an illusion.

At this point, it would be as easy as it would be wrong to make Dumas a follower also of Prout's other famous hypothesis, enunciated for the first time one year after the hypothesis of integer multiples of hydrogen, as distinct from (though superimposable to) it, defining the hypothetical unity of matter as originating from a mysterious primordial element called protyle²². As already mentioned, the former became acquainted with the latter's work through the mediation of Thomson, who was as enchanted by the hypothesis of integer multiples as he was certainly annoyed - especially in virtue of the degree of probability he attributed to the Daltonian theory - by the possibility of discussing the unity of matter once again.

Dumas had shrewdly foreseen how the explanation of isomerism and the various phenomena of polymorphism, such as allotropy, passed through the investigation not only of the percentage composition of bodies but also of the structural arrangement of their constituents. However, in the absence of stable terminology and a clear distinction between atom and molecule, the constraint indicating the element as the limit of qualitative decomposability was lost. Thus, faced with the apparent superimposability and proportionality between the atomic weights of inorganic elements, it must have seemed natural to Dumas to ask himself whether it was not indeed possible to convert one species into another, to transmute matter. As if this were not enough, there was a whole tradition of studies revolving around the concept of transmutation of metals, which, within the elite of French scientific research came back to make its authority felt, as witnessed by one of Dumas' most famous works, the *Leçons sur la philosophie chimique*, published in 1837. Particularly interesting, and useful in clarifying how Dumas' alchemical knowledge came from a thoughtful as well as partial study of primary sources, is the attempted description in *chemical terms* of a supposed procedure for making the philosopher's stone, extracted from a work by the 15th-century English alchemist George Ripley (c. 1415 - 1490). Dumas adopted a hermeneutic oblivious of the philosophical and symbolic values of his chosen writing, interpreting it in the light of his own chemical knowledge and using the visual and thermal variations described as his only compass, decoding the various entities as the signifiers of an allegory and coming to the conclusion that Ripley's philosopher's stone was nothing more than acetone (C₃H₆O), obtained by repeated distillations of lead diacetate - Pb(CH₃COO)₂²³.

It is curious to note, in such an attitude, the adoption of a mirror-image approach to written testimony by the scientists and those who were to become, in the second half of the century, the *new* alchemists. For if the latter could be accused of anti-scientific behaviour in their constant substitution of the experimental method for textual authority, to which they generally attributed far greater value, the former resorted to anti-historical methods, carefully selecting the only data useful for the elaboration of a tradition no less unreal than that on which the hermeticists based their hermeneutics.

Considering the inescapable precedence that Dumas attached to experimental data, it is difficult to imagine that he could have devoted himself to such a felt study of alchemical texts before his calculations of atomic weights aroused probabilistic doubts in him. The fact remains that the motivations that led him to establish the possibility of some link between the superposition and proportionality of the atomic weights of many metals and the concept of transmutation stemmed from his inability to interpret the data of his experiments while preserving Avogadro's hypothesis. To seek an answer in the millenary tradition that glorified a process that could perhaps resolve his doubts, represented the crystallisation of a human limit, which concealed an insatiable desire for knowledge and not necessarily adherence to any form of esoteric thought. And Dumas certainly believed he could overcome the incompatibility between chemistry and alchemy by quantifying the study of the latter, so as to extract data useful for his research. However, this was a dangerous process, especially from a value-based point of view. Alchemy had been brought into play in order to reach a truth that chemistry, although hypothetically, was not even managing to describe comprehensively. The implicit risk was that of attributing a similar if not overlapping gnoseological value to the two disciplines, placing alchemy, which had already long since arrived at the concept of transmutation, in a privileged position, thus establishing an epistemologically null but logically consistent and easily misunderstandable parallel between textual authority and scientific authoritativeness that were no longer clearly distinct. Not to mention that every alchemical theory rested on concepts of unity of matter that, in the face of possible scientific confirmation of transmutation, would have offered an opportunity for generalisation that would have been difficult to avoid.

In spite of his ideals, Dumas chose to believe in order to see. Nonetheless, he probably never fully realised how contingent the value of the data from his experimental research was on his personal interpretation of them. Furthermore, as mentioned, there was a third factor that led him to question the plausibility of the elemental nature of many of the simple substances in mineral chemistry, namely the possibility to put in relation metals and compound radicals of organic chemistry. This factor arose from the field of investigation in which he recorded some of his greatest successes, that of chemical substitutions.

By 1833, Dumas and his students had embarked on an intensive experimental campaign concerning the chlorination processes of organic compounds and by the following year, he had collected sufficient data concerning the action of chlorine on turpentine essence and ethanol (C₂H₅OH) to affirm the ability of the said element to replace hydrogen ‘atom by atom’ in these compounds, which nevertheless had almost identical chemical properties²⁴. In doing so, he reinforced his belief that these properties depended more on the arrangement than on the nature of the particles.

In the eighth of the *Leçons*, Dumas’ observations on the hypothetical transmutability of elements were based on three different principles: isomerism, allotropy and supposed direct proportionality observable according to the atomic weight of different elements (16 metals, tellurium and sulphur)²⁵. These formulations and observations could describe the change of species produced by a transmutation but were not proof of the composite nature of metals. Hence the circumspection in the choice of words with which Dumas described the possibility and not the probability of a feat of alchemical memory. There was, however, at least one *sensata esperienza* that could have been used as evidence. Nevertheless, such evidence was carefully omitted in the *Leçons* (only a veiled allusion is made)²⁶ by virtue of the interpretability of the phenomenon on which this datum was based and the fact that, as the only useful experimental evidence, it certainly did not meet the criteria of exhaustiveness and rigour on which a theory should be based.

In 1808, Berzelius, together with his friend and future court physician Magnus Martin de Pontin (1781 - 1858), replicated the experiments that the previous year had led Humphry Davy (1778 - 1829) to the discovery of sodium and potassium, obtained from their hydroxides²⁷. The two Swedes extended Davy’s research to another substance that resembled the so-called caustic alkalis in chemical properties, i.e. ammonia (NH₃). By subjecting a negatively electrified quantity of mercury to electrolysis and placing it in contact with an aqueous ammonia solution, they produced a substance that had the appearance of an amalgam²⁸. In this regard, the sentence at the end of the first part of Berzelius and de Pontin’s account of their experiments is particularly interesting, in which the two scientists, apologising for the ‘almost alchemical’ tone of their lucubrations, observe how the phenomenon they investigated could have led to the ‘decomposition of metals’ and the discovery of the processes necessary to perform chrysopoeia, as already suspected by ‘many chemists’:

«And even if these discoveries do not bring us any closer to the goal [i.e. the transmutation of metals] so unsuccessfully pursued for so many centuries, they do at least give us a clearer idea of the decomposability of metals, making the possibility somewhat intelligible. We may be forgiven this almost alchemical argument; however, many chemists had already predicted [...] that one day we would discover the composition of gold and devise the means to assemble its components»²⁹.

So, how to explain such a phenomenon, considering that the composite nature of ammonia was well known?

When confronted with the work of his colleagues, Davy was enthusiastic and inclined to devise a classification of metals - described as hydrogenated compounds - at the basis of which, given its instability and discernible composite nature, was the hypothetical metallic element that together with mercury formed the amalgam, called *ammonium*:

«The more the properties of the amalgam obtained from ammonia are considered, the more extraordinary do they appear.

Mercury by combination with about 1/12000 part of its weight of new matter, is rendered a solid, yet has its specific gravity diminished from 13.5 to less than 3, and it retains all its metallic characters; its colour, lustre, opacity, and conducting powers remaining unimpaired.

It is scarcely possible to conceive that a substance which forms with mercury so perfect an amalgam, should not be metallic in its own nature; and on this idea to assist the discussion concerning it, it may be conveniently termed ammonium»³⁰.

The Stahlian dream of a metallising phlogistic principle seemed within reach once again, especially in light of the fact that ammonia was composed of non-metallic elements³¹. From the study of the amalgam, the formula for *ammonium* was logically deduced, parallel to that of today's ammonium ion (although it should be specified that the amalgam is formed after the alkaline or electrolytic reduction of the cation NH_4^+ into the ammonium radical $\text{NH}_4\cdot$).

Thus Berzelius, in the first edition of the second volume of his *Lärbok i kemien* (1812), did not hesitate to include *ammonium* in the list of elements that could be obtained from earths and alkalis, being all metals³². Moreover, it is in this book that can be found the first clear conceptual overlap between the radicals of organic chemistry and metals, observable in the title of the relevant chapter: «Alkaliernas och jordarternas metalliska radicaler» (*Metallic radicals of alkalis and earths*)³³.

The problem also interested some of the leading French scientists of the time. First Claude-Louis Berthollet (1748 - 1822), then Ampère³⁴ examined the potential of Berzelius and de Pontin's discovery. Anyway, the scientific community, faced with the impossibility of finding a solution, and building on the results of an increasing number of studies based on better-established concepts of element, atom and molecule, although remembering the *ammonium* amalgam phenomenon, ended up shelving it almost completely until the mid-20th century³⁵. One of the few scientists who continued to take an interest in *ammonium* was exactly Dumas, who on three separate occasions while writing his *Traité de chimie* - one of the most important manuals dedicated to technical-industrial applications of chemical research of the period, the editing of which kept him busy for eighteen years, between 1828 and 1846 - dealt with Berzelius and de Pontin's discovery and what it might entail³⁶. Seeking

to establish a classification of metals on the basis of their respective chemical properties, after observing and describing the isomorphism of potassium, sodium, lithium, barium, strontium and calcium, he put forward a conjecture about the composite nature of metals (the highly speculative nature of which he himself affirmed), observing how, from the known data, it might be plausible to conclude that «ammonia is transformed into a metal when, to the three volumes of hydrogen it contains, a fourth is added»³⁷.

In the fifth volume (1835), on the other hand, building on the discoveries that led him to the substitution theory, attempting an initial classification of nitrogen and hydrogen compounds, Dumas identifies *ammonium* as a ‘metal-like body’, with a related discussion concerning the advantage that the identification of its oxide would represent. Firstly because, just as *ammonium* in the amalgam experiment was shown to be able to replace potassium, the hypothetical oxide would have confirmed the superposition of its chemical properties with those of sodium and potassium hydroxides. Second, because an entity such as *ammonium* oxide would have led to the assumption of «the existence of a large number of unknown combinations, which would replace all known hydrogenated compounds in the products they form by uniting with acids»³⁸. This statement is only apparently obscure since after a few lines it becomes clear what Dumas is aiming at, namely extending his theory of ethers to inorganic chemistry.

Between 1827 and 1828, together with his colleague and pharmacist Félix-Polydore Boullay (1806 - 1835), Dumas had arrived at the enunciation of the formula for the synthesis of ethers - $C_nH_{(2n+2)}O$ -, compounds formed, in the authors’ words, from ‘an acid combined with two volumes of ethylene - C_2H_4 , called *oleophilic gas* - and one volume of water vapour’; a description elaborated by generalising to an entire class of compounds the values describing the components of diethyl ether, at the time known as *sulphuric ether* ($C_4H_{10}O$)³⁹. But this generalisation, since supported by serious experimentation, had proved to be accurate, producing, as Charles Adolphe Wurtz (1817 - 1884) defined it forty years later in his history of chemistry, the first occasion on which:

«in organic chemistry a series of similar phenomena was grouped together by theory and [...] the facts relating to the formation, composition and metamorphoses of an entire class of bodies were given a simple interpretation, using atomic formulae and equations»⁴⁰.

In their account, Dumas and Boullay offer a term of comparison which, interpreted in the light of the mystery of *ammonium*, serves to clarify the curious digressions in the fifth volume of the *Traité*. The two had in fact described in an analogical key the function of ethylene in the formation of ethers with that of ammonia in the formation of ammonia salts, by virtue of the latter’s ability to decompose diethyl ether⁴¹.

The choice of comparative term stemmed from the observation of the properties of ammonia, which described capacities for the analysis of organic compounds and the synthesis of saline compounds in the presence of acids that overlapped and even exceeded those attributed to metal hydroxides used for the same purposes.

Years passed, and by the time Dumas wrote the next passage of the *Traité*, the constant comparison between *ammonium*, alkalis and earths had matured in him a definite conviction. Faced with the impossibility of isolating the fabled metal, obtaining its oxide would have provided solid proof not of the existence of a metallic phase of the radical NH_4^+ (a concept unknown to the chemistry of the time), but of the composite nature of metals, since the properties of the new *element* would have fallen squarely within the casuistry described by the alkaline and alkaline-earth metals (as we have already seen with Berzelius):

«Nevertheless, this is the place to bring out a theory already proposed by Ampère, on the occasion of the singular combinations that have been described under the names of ammoniacal hydrides of mercury or potassium and mercury. According to Ampère, these compounds, which have so often been compared to alloys, contain a kind of metal made up of 2 parts nitrogen to 8 parts hydrogen⁴². There is nothing to prevent us from classifying such a compound alongside the metals when we already classify cyanogen alongside chlorine and other similar non-metallic bodies.

If we assume this base, we would have the following series:

$\text{Az}^2 \text{H}^4$ a chlorine-like substance found in amines.

$\text{Az}^2 \text{H}^6$ ammonia.

$\text{Az}^2 \text{H}^8$ a metal-like substance. Ammonium.

$\text{Az}^2 \text{H}^8 \text{O}$ ammonium protoxide.

$\text{Az}^2 \text{H}^8, \text{Ch}^2$ ammonia hydrochlorate or rather ammonium chloride.

$\text{Az}^2 \text{H}^8 \text{O}, \text{S O}^3$ ammonia sulphate or rather ammonium protoxide sulphate,

and so on for the different ammonia salts known. With regard to the combination formed between anhydrous sulphuric acid, for example, and ammonia, it would necessarily be considered an amine.

Here are the main advantages of this theory, as far as I can appreciate them.

It explains the formation of the remarkable amalgams that first gave us the idea.

It eliminates hydrochlorates, hydriodates and other similar ammonia salts, whose existence embarrasses the theory of chlorides, iodides, etc.

It gives perfect simplicity to the formulae of double chlorides, double iodides and other similar compounds containing ammoniacal combinations, whereas in the other theory, these formulae are complicated and of an unusual form.

It gives a good idea of the basic role of ammonia since it is no longer ammonia that plays the role of base, but an oxide produced by the union of ammonia and water. This oxide is therefore completely comparable to potash or soda.

This better explains the isomorphism of ammoniacal salts with similar combinations of potassium or sodium; since, for example, ammonium replaces potassium everywhere, and ammonium oxide replaces potash.

Now here are its disadvantages:

It is based on the existence of a combination $Az^2 H^8$, which has not been isolated.

And on the existence of an ammonium oxide, $Az^2 H^8 O$, which is completely unknown to us, although ammonia and water can produce it by combining and these two bodies have been brought together in circumstances most favourable to combination.

It leads us to suppose the existence of a large number of unknown combinations, which would replace all the known hydrogenated compounds, in the products that these form by uniting with acids.

It therefore forces us to admit a large number of hypothetical hydrogen carbides, playing the role of metals, which is possible, but difficult to admit without proof.

Thus, as has already been pointed out, the theory of ethers and that of ammoniacal combinations are so closely linked that they will probably be decided by each other. Those who attribute the role of a base to the sulphuric ether will admit ammonium oxide; those who regard ammonia as a base must attribute the same role to carbonated hydrogen and its analogues. By showing that both theories are admissible, we have given a fair idea of the state of the question; by preferring the latter point of view, we have followed the general opinion.

Chemists who have turned their attention to the philosophy of science have all been struck by the difficulties that the history of ammonia has given rise to, and have long sought to discover some metallic radical in it, in order to bring this body back into the great family of oxides. After the useless attempts made by Davy and Berzelius twenty years ago, they returned to the original idea of considering ammonia as a base in itself. This discussion, almost forgotten, has been rejuvenated by its connection with the ether theory.

It is impossible to ignore what is broad and great in the point of view that would bring all these so diverse combinations back to the known laws that regulate those of metal oxides [...].

On the other hand, however, it is not an uncommon fact that ammonia is considered to be an alkaline base. It is a consequence of a principle that is no less extensive than the previous one, nor less worthy of attention. Is it not natural to admit, in fact, that hydrogen, by uniting with simple bodies, can sometimes constitute acids, sometimes bases, depending on whether its properties predominate or succumb in the presence of the antagonistic element?

If the ammonium theory had been generally accepted, sulphuric ether and its analogues would have been given the role of base. Of all the known phenomena, only those relating to the theory of substitutions can be explained by a single hypothesis, the one accepted in this book [...].

Leaving aside these hypotheses, we shall confine ourselves here to the pure and simple expression of facts [...]. We will therefore consider ammonia as a base in itself»⁴³.

Before asserting the plausibility of a hypothesis, Dumas considered it necessary to produce empirical proof. Otherwise, claiming its veracity on a logical-consequential basis would have led beyond the limits of a scientifically provable analogical correspondence, trespassing into the domain of personal convictions to which one could arbitrarily attribute the function of principles. So much so that the problem of *ammonium* in the *Leçons*, a work with historical ambitions but with a strongly

programmatic slant, is only hinted at transversally through recourse to the authority of certain 'illustrious chemists', Berzelius in the lead (and the only one to be cited by name), who had «put forward conjectures such as to make the composite nature of nitrogen conceivable»⁴⁴. Hence Dumas' progressive (and definitive) rejection of physical atomism, which he increasingly saw as a set of aleatory speculations about an invisible world, in favour of research with far greater classificatory potential, one that was devoted to the radicals of organic chemistry. On the strength of his successes in the elaboration of the substitution theory, in the same year of publication of the *Leçons*, he drafted, together with another great organic chemist of the time and long-time rival, Justus von Liebig (1803 - 1873), a programmatic article, almost a manifesto, with which the quest to finally bring order to the tumultuous sea of organic compounds was inaugurated, entitled *Note sur l'état actuel de la chimie organique*⁴⁵.

The historiography of chemistry has always emphasised how Dumas, at least from the early 1830s onwards, set atomism aside in favour of recourse to immediately measurable quantities such as equivalents; and that is true. However, the instrumental value of atomism was never denied by him, and in order to lend solid mathematical representability to his research, in the aftermath of the *Leçons* the scientist prepared an experimental investigation aimed once again at calculating the atomic weights of the principal agents of organic chemistry (carbon, hydrogen, oxygen and nitrogen). The results of this investigation, together with the theory of substitution and that of types, earned him the Copley Medal of the Royal Society in 1843. In the very definition of chemical type offered by Dumas, in fact, the recourse to atomistic concepts remained central, while to have been partially excluded was the concept of element, instrumentally necessary but *functionally replaced* by that of type⁴⁶.

As is well known, the idyll between Dumas and Liebig was very short-lived due to disagreements at a theoretical level⁴⁷. Ironically, it was Liebig's theory of compound radicals⁴⁸ that provided the rationale for hypothesising the analogy that Dumas, in the absence of *ammonium* oxide, had refused to enunciate openly out of methodological rigour. One of the fathers of structural chemistry, Alexandre-Édouard Baudrimont (1806 - 1880), who in turn adopted at least in part Prout's integer multiples hypothesis, was the one who performed the feat. Epitomising his words, in inorganic chemistry, oxygen, chlorine and sulphur could be combined with a metal to produce compounds in which the latter played the role of a radical, which, on the other hand, in organic chemistry never consisted of a single element. From the juxtaposition of the relevant data, Baudrimont concluded that metals not only could, but plausibly should, be *corps composés*:

«Just as a chemical element can only be analysed with its own matter, so it can only be produced with this same matter; thus, in the present state of chemistry, nothing other than gold can be found in gold, and gold can only be made with gold.

This is the limit of experience; the rest is mere conjecture or supposition. It is known, however, that alchemists claim to have made gold from bodies that were not gold, but these facts, although often presented with candour and with testimonies that leave little room for doubt, will only be accepted by science when they can be proved experimentally.

However, despite the deliberate obscurity that reigns in the writings of the alchemical philosophers, we can say with certainty that they did not produce gold by combinations, but by imprinting on the nature of the bodies a modification of the kind that gives rise to isomerism, under the influence of a catalytic agent. The bodies on which they worked were lead and mercury; their agent was the projection powder: this product they produced with such slow and arduous labour.

To the alchemists, we can add the theoretical ideas of Prout, who believes that all bodies are formed from one and the same material, the disposition of which alone causes the differences that we observe in bodies considered simple [...].

Liebig's theory, at least in most if not all cases, establishes radicals that are entirely comparable to metals. The theory I have defended leads us to suspect that metals are composed, and I believe this view to be as well-founded as that of Lavoisier, who thought that earths and alkalis could well be metallic oxides. Let us hope that a new Davy will resolve this question. Hydrogen would be the link that binds the constituent parts of metals together. If this bond could be broken, they could undoubtedly take on new arrangements, and metallic transmutation would take place»⁴⁹.

In a period during which Berzelius' electrochemical dualism was slowly being replaced by unitary theories also thanks to the discoveries of Michael Faraday (1791 - 1867) but immediately preceding the first studies on valence and the dawn of stereochemistry - yet still far from the refinement of nomenclature and atomistic conceptualisation brought about by Stanislao Cannizzaro (1826 - 1910) in the late 1850s -, the plausibility of hypotheses such as the transmutation of metals was hardly questionable.

Seeing is believing: the dreams of an inventor

During one of the sessions devoted to chemistry at the 21st annual conference of the *British Association for the Advancement of Science*, held in Ipswich in July 1851, Michael Faraday returned to emphasise how many scholars (including himself) expected future developments that would restore a simpler view of matter to physical and chemical research. This attitude implied a strong desire to curb that multiplicative drift characterised by an increasingly crowded pool of chemical elements, still interpreted by more than one scholar in the mid-century as an illogical break from an organisation of the physical world of (albeit now distant) Democritean and Empedoclean ancestry. Galvanised by Faraday's words, Jean-Baptiste Dumas took the opportunity to present his own convictions concerning the plausibility of transmutation to an audience of scholars. Carefully avoiding the edge

case of *ammonium*, Dumas resorted to the categorisation principles developed more than twenty years earlier by his German colleague Johann Wolfgang Döbereiner (1780 - 1849). This latter, after more than a decade of experimentation, had managed in 1829 to group fifteen elements into triads characterised by physical and chemical affinities, the middle term of which had an atomic weight equal to or close to the average of the sum of the atomic weights of the two extremes⁵⁰. Such research, further developed after the middle of the century, sanctioned a turning point that in the following decade culminated, thanks to the work of Dmitrij Ivanovič Mendeleev (1834 - 1907), in a stable theorisation of the periodicity of the elements⁵¹, while Dumas chose instead to resort to such organisational criteria for the purpose of revaluation. According to a rather obvious analogical process (and arguably influenced also by Baudrimont's lucubrations concerning the transmutability of matter), the French chemist saw in the triadic organisation of certain elements the possibility of deriving numerical ratios parallel to those of compound radicals. In this way, his observations became part of a possible confirmation of Prout's 'integer multiples' hypothesis, by which the elements that constituted triads in inorganic chemistry were characterised by an analogical relationship with the components of organic chemistry's homologous series⁵². Always well aware of the highly speculative nature of this kind of hypothesis, Dumas undertook further laboratory research before presenting them at the *Académie des Sciences* (where they ended up at the centre of a heated debate between 1858 and 1859)⁵³. Thus, on the occasion of the conference, he chose not to submit any paper, as indicated by the fact that the related *Report* contains no contribution of his own, nor any mention of the matter by Faraday. The only valuable, detailed account of the affair was published anonymously in the 12 July 1851 issue of *The Athenaeum*, the leading English generalist weekly magazine devoted to art and literature as well as scientific news and dissemination. Taking into account the technical knowledge required to produce such an account, we can attribute it with a fair degree of certainty to the scientist and politician Lyon Playfair (1818 - 1898), at the time co-chairman of the committee of the *British Association's* section dedicated to chemical research⁵⁴, as well as correspondent of *The Athenaeum*⁵⁵:

«[...] Dr. Faraday expressed an opinion that chemists had of late years viewed with regret the increase in the number of metals, and hoped that the day was not far distant when some of the metals would afford honour to chemists by new modes of investigation leading to their decomposition.

[...] Prof. Dumas gave many examples of groups of bodies, such as the alkalies, earths, &c., arranged in the order of their affinities. He called the attention in the Triad groups, to the intermediate body having most of its qualities intermediate with the properties of the extremes, and also that the atomic or combining number was also of the middle term, exactly half of the extremes added together; thus, sulphur 16, selenium 40, and tellurium 64. Half of the extremes give 40, the number for the middle term. Chlorine 35, bromine 80, and iodine 125. Or the alkalies, lithia, soda, and potassa, or earths, lime, strontia, and baryta, afford,

with many others, examples of this coincidence; hence the suggestion, that in a series of bodies, if the extremes were known by some law, intermediate bodies might be discovered; and in the spirit of these remarks, if bodies are to be transformed or decomposed into others the suggestion of suspicion is thrown upon the possibility of intermediate body being composed of the extremes of the series, and transmutable changes thus hoped for. Prof. Dumas then showed that in the metals similar properties are found to those of non-metallic bodies; alluding to the possibility that metals that were similar in their relations, and which may be substituted one for the other in certain compounds, might also be found *transmutable* the one into the other. He then took up the inorganic bodies where substitutions took place which he stated much resembled the metals. After discussing groups in triads, Prof. Dumas alluded to the ideas of the ancients of the transmutation of metals and their desire to change lead into silver and mercury into gold; but these metals do not appear to have the requisite similar relations to render these changes possible. He then passed to the changes of other bodies, such as the transmutation of diamonds into black lead under the voltaic arc. After elaborate reasoning and offering many analogies from the stores of chemical analysis, Prof. Dumas expressed the idea that the law of the substitution of one body for another in groups of compounds might lead to the transformation of one group into another at will; and should endeavour to devise means to divide the molecules of one body of one of these groups into two parts, and also of a third body, and then unite them, and probably the intermediate body might be the result. In this way, if bodies of similar properties and often associated together were transmutable one into the other, then by changes portions of one might often, if not always, be associated with the other [...].

Dr. Faraday expressed his hope that Prof. Dumas was setting chemists in the right path; and although conversationally acquainted with the subject, yet he had been by no means prepared for the multitude of analogies pointed out»⁵⁶.

At the time, Dumas could not have known that a young researcher, Cyprien-Théodore Tiffereau (1819 - 1909), had sent a memoir to the *Académie* in January of the same year entitled *Nouveau point de vue sous lequel nous devons envisager les métaux, basé sur un fait acquis à la science par l'expérimentation* (*A new way of looking at metals, based on a fact acquired by science through experimentation*), which stated that the theory of a metallising principle, openly borrowed from Stahl's thought, was one step away from experimental confirmation⁵⁷. That young man, who by mid-century was already making a name for himself as an inventor and photographer, is today mainly remembered for his dream of succeeding in transmuting metals, which accompanied and haunted him for 60 years⁵⁸.

Having completed a scientific education of which we know very little, Tiffereau, a native of a small village in the Vendée, after working as a chemical preparator at the *École Professionnelle* in Nantes, embarked for Mexico in 1842, eager to further his studies on the terrains in which precious metals are found and the technologies used for their mining. As can be deduced from the numerous biographical passages in the contributions he published after his return to France⁵⁹, during the period between 1843 and 1845 he travelled extensively, producing a considerable amount of photographic

evidence of his mining and chemical research, unfortunately lost today. It is, however, easy to see that his studies aimed at technological development in the field of photography were the occasion (if not even the pretext) to deepen chemical experimentation that was already tending towards the desire to confirm the transmutability of metals. In fact, it is impossible not to see clear points of contact between the methodologies employed at the time for the development of daguerreotypes and the particular transmuting procedures prepared by Tiffereau, which, by then, retained only a vague memory of the alchemical tradition.

In 1846, he decided to settle semi-permanently in Guadalajara, where he earned a living as a photographer (which also allowed him to subsidise his chemical research). It was at this juncture that Tiffereau became convinced, thanks to an experiment that was successfully replicated twice more over the next year, that he had achieved the synthesis of artificial gold. After subjecting nitric acid (HNO_3) to the direct action of sunlight for a few days, he added filings of a copper-silver alloy, leaving everything exposed to the sun again until the partial dissolution of said alloy. The next step, consisting almost of a trivial parody of the alchemical *solve et coagula*, involved *cooking* the metals until the solvent evaporated, which was again added and evaporated until the solid residue, initially blackish in colour, became progressively lighter. Once a bright yellow metallic hue was reached, the assay confirmed the successful transmutation into gold:

«I reduced 10 grams of silver alloyed with copper to filings and projected them into a flask 2/3 full of pure nitric acid at 36° [Bè]. At first, there was a lively release of nitrous gas; a little later, the attack having diminished in intensity, the release slowed down almost all of a sudden and remained barely noticeable, but nevertheless uniform, until the end of the operation; on the other hand, the unattacked filings seemed to increase a little in volume. After 3 weeks, I boiled the liquor in the sun on my terrace. The nitrous vapours ceased to be released and the boiling, which continued until it was dry, showed me a dull matter tending towards black, aggregated into a single whole: I noted no deposit of any saline part or any impurity.

I poured nitric acid at 36° [Bè] over the residue thus obtained. I boiled it and pushed the operation to complete dryness: I obtained, as before, an agglomerated whole but whose black colour took on a greenish appearance. Further attacks and successive boiling with concentrated acid gave me a residue that was still agglomerated but whose colour gradually changed from greenish to yellowish. Finally, during the last boiling to dryness, the dry matter, which had previously always been agglomerated, separated into a number of particles, making it perfectly clear that they consisted of filings that were easily crushed under the hammer. These various particles all had a clear golden yellow colour»⁶⁰.

This procedure, in all likelihood at least partly borrowed from alchemical readings that Tiffereau never explicitly quoted, was influenced by the traditional idea of accelerating the *ripening time* of metallic substances combined with a concept of photosensitivity attributed to metals and

acidic materials that was nevertheless foreign to traditional literature (which spoke at most of the astrological circumstances favourable or adverse to the Great Work). Tiffereau's experiments recalled instead the photographic impression procedures regulated by precise exposure times, considering how the development of daguerreotypes took place through the direct action of light on silver plates. The extent of the alchemical contribution to the genesis of Tiffereau's transmutational hypotheses remains unknown to this day, so to trace them back to the cross-reading of precise alchemical texts would represent mere conjecture.

Shortly after the outbreak of the Mexican-American War (1846-48), Tiffereau was forced to leave the New World. Already planning an industrialisation process to put his incredible discovery to good use, he sailed from Tampico to Paris in early 1848, but once back home, he came up against an obstacle he would never be able to overcome. For unknown reasons, it was impossible for him to successfully replicate his Mexican experiences. In the period immediately following his return, Tiffereau nevertheless managed to consolidate his position by establishing himself as a photographer and inventor. Over the next twenty years, several devices of his own creation, such as hourglasses for calculating the exposure times of photosensitive materials used in photography, laboratory gasometers and hydraulic clocks, became very popular and earned him academic prizes and awards. So much so that his first contact with the *Académie des Sciences* came by way of a *pli cacheté* dated November 1850 in which he discussed the possibility of using special aerostatic devices to irrigate cultivated fields⁶¹.

While Tiffereau was privately continuing his experiments in an attempt to carry out the transmutation once again, on 31 May 1852 he sent the *Académie* a sample of the artificial gold that had been transmuted in Guadalajara in 1846, together with a second missive, in which he stated that apart from himself, the only person made aware, on 23 June 1851, of the procedure successfully used in Mexico, was none other than Napoleon III (1808 - 1873), at the time *Président de la République*, who on that occasion granted the ambitious researcher financial support for his investigations⁶². When he felt ready to divulge his discovery, in June 1853 Tiffereau published a short memoir eloquently entitled *Les métaux ne sont pas des corps simples, mais bien des corps composés (Metals are not simple bodies, they are compound bodies)*, of which he sent a copy to the *Académie* clamouring for its judgement, plausibly ignoring the rule by which this institution refrained from commenting on scientific contributions already published in France. Surprisingly, it was nevertheless decided to summon him and so, on 17 October, Tiffereau presented the account of his Mexican experiences before the French scientific gotha, showing more samples of artificial gold. The aspiring transmuter must have realised early on that presenting a hypothesis partially based on the recovery of Stahl's phlogiston, considered for decades to be a pseudoscientific device, would not have been received as

a wise or inspired choice. Thus, he reshaped his ideas about transmutation on more recent concepts, considered at least probable and shared by more chemists, from the comparison between the compound radicals of organic chemistry and metals dear to Baudrimont and Dumas to Prout's integer multiples hypothesis. The attempt at theoretical generalisation, constructed using language more in keeping with mid-century chemical research, thus revolved around the possibility that metals, defined as isomer compounds (in deference to what Dumas had suspected since his *Leçons sur la philosophie chimique*), whose unknown radical would consist of one or even more allotropic states of hydrogen (in homage to Prout's integer multiples), could be transmuted through oxidation processes regulated by the catalytic and fermentative action of nitrogen (with indirect reference to the phenomenon of *ammonium*). Metals were thus presented as oxyhydrides, the more inert and dense the more the amount of oxygen present in them increased, thus justifying the use of nitric acid, a known oxidising agent.

Bearing in mind the decades-long debate about the actual 'simplicity' of metals, it should come as no surprise that the *Académie* responded by setting up a commission of enquiry⁶³, chaired by the leading expert on the subject, Jean-Baptiste Dumas; his mentor Louis-Jacques Thénard (1777 - 1857), since the 1810s accustomed to studying related topics, as we have seen in the case of *ammonium*⁶⁴; the most knowledgeable scholar of alchemical sources available to the institution, Michel-Eugène Chevreul (1786 - 1889), one of the founders of modern organic chemistry and a pioneer of fatty acid chemistry. On 7 November, the committee members informed Tiffereau that they would need more technical data to reach a judgement. In essence, they were asking him to reveal his experimental protocol. It took six months to formulate a response, for a rather obvious reason. Realising the magnitude of the economic and financial repercussions that would result from the confirmation of his hypothesis and, consequently, the achievement of the technical reproducibility of transmutation, Tiffereau wished to protect himself as the inventor of the procedure. To this end, on 22 December he applied for a fifteen-year patent - registered by imperial decree on 10 February 1855 - «for transmuting metals into one another»⁶⁵.

The new memoir, which reached the members of the commission in May 1854, finally explained the terms and methods of the experiments conducted in Mexico between 1846 and 1847, also asking for the scientific community's help in unravelling the mystery behind the impossibility of reproducing transmutations on French soil. However, Tiffereau received no further response. During the same year, he sent three more papers to the *Académie*, after which, having obtained the patent, he broke his silence and collected the communications sent to the institute in 1853-54 in a booklet with another self-explanatory title: *Les métaux sont des corps composés (Metals are compound bodies)*. The fifth of these communications (sent to the *Académie* on 16 October 1854) is

of particular interest, as it testifies to the repetition of Tiffereau's experiments at the Imperial Mint under the direction of the essayer and prominent metallurgist Alexandre Irénée François Levol (1808 - 1876)⁶⁶. The results, although ambiguous, were deemed conclusive and Tiffereau's request for further examinations was rejected⁶⁷. Between the end of 1855 and 1858, he produced two more memoirs, but, faced with the indifference of the experts, he temporarily shelved the enterprise and concentrated on his career. He married and had four children, and after he retired to private life in 1884 after selling the now-famous photographic atelier located at 130 rue du Théâtre to one of his employees, he resumed his research into transmutation, updating his hypotheses in the light of the latest chemical discoveries.

Conclusions

It could be argued that it was Dumas himself who indirectly took up Tiffereau's baton. The former, in fact, having long since abandoned all hope of confirmation of Avogadro's principle but constantly searching for criteria of categorisation and ordering useful in chemical research, between 1857 and 1859 produced a series of contributions of extreme interest in fully understanding the reasons for the survival of speculations concerning the transmutability of matter in the chemical field⁶⁸.

First with a *Mémoire sur les équivalents des corps simples* (1857) and then with a *Note* on the same subject published the following year, Dumas, building on the successes achieved through the application of his substitutions and types theories, made explicit his strategy aimed at achieving two co-implicating aims. One was the validation of Prout's hypothesis of integer multiples for as many *corps simples* as possible. The other, representing a crucial step towards analogical generalisations informed more by speculations typical of magical thinking than by scientifically plausible inductive inferences, involved the demonstration of the existence, between elements belonging to the same family, of relationships analogous to those of the homologous series of organic chemistry (just like speculated during the Ipswich conference of 1851)⁶⁹. Thus, Dumas implicitly repeated that comparison between radicals from organic chemistry and elements from inorganic chemistry that we found in Berzelius as the conceptual overlay that later evolved into a true hypothesis with Baudrimont.

Dumas, however, could not have known that in that same period his Italian colleague Stanislao Cannizzaro, with his *Sunto di un corso di filosofia chimica* (1858), was laying the foundations for one of the first turning points towards the acceptance of the physical reality of atoms. And it was Cannizzaro himself who emphasised first the methodological shortcomings of Dumas' hypotheses by reiterating what had been stated in his *Sunto*:

«Dumas has set out to resolve one of the most important and general questions of natural philosophy; but for this purpose, it seems to me that one should compare not the quantities of bodies that are substituted, but the weights of those last particles of theirs that always enter their molecules whole, and of their compounds, namely the atomic weights»⁷⁰.

Regardless of Cannizzaro's remarks, the following year Dumas came to a definitive systematisation of his research into the *équivalents des corps simples*. In a long essay, characterised by philosophical digressions and almost prophetic accents, the scientist's arguments culminate in the hope of finally achieving the 'decomposition of the radicals of inorganic chemistry':

«It is no more necessary to teach chemists that bodies they cannot decompose do not decompose than it would be to teach them that compound bodies decompose; these are two truths of the same order.

Chemists have taken their analysis as far as the power of the forces at their disposal or the energy of the reactions whose formulas they know.

They have done even better, for by this analysis they have reduced all the natural bodies to certain metallic or non-metallic bodies, showing by indisputable common characteristics and by an energetic mutual affinity that they are all radicals of the same order.

When, in this state of affairs, there appears to be a reason to doubt that these radicals are simple bodies and that chemistry has said its last word about them, is it necessary to repeat this series of perfectly established demonstrations which prove that it has not hitherto been possible to decompose them? I do not think so. The infinite manipulations of the laboratories of science and industry over the last century have left no clouds in people's minds on this subject. There is no question of going back to the past; what it has left us, everyone takes to be true and sufficiently proven. It is a question of looking to the future and seeing if we can go one step further. But it is a difficult step, the most difficult, in my opinion, that human science has ever attempted, and which requires something other than the use of heat or the application of ordinary electrical forces.

Chemistry may be a new science, but chemical phenomena are as old as the world itself, and the radicals of inorganic chemistry that are to be subjected to further decomposition have been known to mankind for a long time. Their existence is revealed from the earliest historical times when their immutability is also revealed in a way [...].

Decomposing the radicals of inorganic chemistry would therefore be a more difficult task than the one Lavoisier had the pleasure of undertaking and accomplishing. For it would mean revealing not only new and unknown beings, as we discover from time to time, but beings of a new and unknown nature whose appearances and properties our minds cannot by any analogy imagine. This would mean taking the analysis of matter to a point that neither the most energetic natural forces nor the combinations and processes of the most powerful science have ever reached. It would mean harnessing forces that we are unaware of, or reactions that no one has imagined.

It's one of those problems that human thought needs to ponder for centuries [...]]»⁷¹.

Nowadays, we can safely assume that this essay represents one of the last academic contributions to that chemical research on metallic transmutation influenced by concepts, images and speculations of alchemical derivation. Moreover, when in 1888 Tiffereau returned to the question of how to replicate the Mexican transmutations of over forty years earlier, his interlocutor would no longer be Academia, but the elite of the Second Generation of French occultism⁷². Within the *fin de siècle* occultist milieu, constantly seeking an epistemologically impossible synthesis between science and esoteric beliefs, speculations on the unity of matter would know a new phase, aiming at a representation and study of matter understood as an epiphenomenon of ether, described in turn as the material and vital principle of the entire phenomenical reality⁷³.

By contributing to the acceptance of an atomism physically intended - which, however, in France would prevail only in the early 20th century⁷⁴ - Cannizzaro's reform, crowned by the endorsement of the greater part of the chemistry community gathered in Karlsruhe for its first international conference (3-5 September 1860)⁷⁵, in all likelihood also contributed to the demise of theoretical elaborations such as those of Dumas.

Just as alchemy did not meet its end with the birth of modern chemistry during the second half of the 18th century, it did not continue to exist, from then on, solely as an esoteric discipline governed by gnoseological paradigms irreconcilable with any modern idea of science. In this respect, the enquiry into the relationships between chemistry and alchemy during the 19th century offers an excellent chance to investigate in a more in-depth and impartial fashion not only debates on the nature and behaviour of matter on the one hand and esoteric conceptualisations on the other. Such an enquiry grants also the opportunity to analyse the set of biases, convictions and personal beliefs (often part of unconscious cognitive processes) that characterised the history of both disciplines during a period of epochal transformations. The case of Dumas dealt with on this occasion has been chosen both for its relevance and to illustrate how it does not represent a rare exception, but rather part of a large and multi-faceted chapter in the history of science and ideas still largely to be written.

¹ W. H. Brock, *From Protyle to Proton. William Prout and the Nature of Matter, 1785-1985*, Adam Hilger Ltd., Bristol-Boston, **1985**, pp. 83-84; M. P. Banchetti-Robino, in *What Is a Chemical Element? A Collection of Essays by Chemists, Philosophers, Historians, and Educators* (Eds. E. Scerri, M. Ghibaudi), Oxford University Press, Oxford-New York, **2020**, pp. 87-108; B. Bensaude-Vincent, *ivi*, pp. 32-52.

² J.-B. Dumas, *Annales de chimie et de physique*, **1826**, *33*, 337-391; M. Chaigneau, *J.-B. Dumas, chimiste et homme politique. Sa vie, son oeuvre: 1800-1884*, Guy Le Prat, Paris, **1984**, pp. 81-88; M. Novitski, *Auguste Laurent and the Prehistory of Valence*, Harwood Academic Publishers, Chur [etc.], **1992**, pp. 15-16.

³ A. Avogadro, *Journal de physique, de chimie, d'histoire naturelle et des arts*, **1811**, *73*, 58-76.

⁴ A.-M. Ampère, *Annales de chimie, ou recueil de mémoires concernant la chimie et les arts qui en dépendent*, **1814**, *90*, 43-86.

⁵ J. I. Solov'ev, *Эволюция основных теоретических проблем химии*, Наука, Москва, **1971**, p. 128. Dumas certainly came to know the results of Berzelius' research on the calculation of atomic weights - published both in the third volume

of his *Lärbok i kemien* and in the fifth of the journal he founded together with Wilhelm Hisinger, 1766 - 1852 (*Afhandlingar i fysik, kemi och mineralogi*) - through their French edition, the publication of which was supervised by the author himself; J. J. Berzelius, *Lärbok i kemien*, Tryckt hos Direct. Henr. A. Nordström, Stockholm, **1808-18** (3 vols.), III (**1818**); Id., *Essai sur la théorie des proportions chimique et sur l'influence chimique de l'électricité* [...], chez Méquignon-Marvis, libraire pour la partie de Médecine, rue de l'École de Médecine, n. 3, près celle de la Harpe, Paris, **1819**.

⁶ See ref. 5 (Berzelius **1808-18**), III, pp. 104-130; E. M. Melhado, *Jacob Berzelius: The Emergence of his Chemical System*, Almqvist & Wiksell, Stockholm; University of Wisconsin Press, Madison (WI), **1981**, pp. 270-278; G. Eriksson in *Enlightenment Science in the Romantic Era: The Chemistry of Berzelius and its Cultural Setting* (Eds.: E. M. Melhado, T. L. Frängsmyr), Cambridge University Press, Cambridge, **1992**, pp. 56-84.

⁷ W. Prout, *Annals of Philosophy*, **1815**, 6, pp. 321-330; see ref. 1 (Brock), pp. 82-89.

⁸ P. L. Dulong, A. T. Petit, *Annales de chimie et de physique*, **1819**, 19, pp. 395-413; R. Fox, *The British Journal for the History of Science*, **1968**, 4,1, pp. 1-22. To appreciate the degree of accuracy achieved by the two scientists in calculating the constant (equal to 6.2 cal/°C), it is only necessary to multiply the average of the values describing the heat capacity of the elements by the atomic weight unit derived from Berzelius' data (O = 100) and divide it by the correct atomic weight of the same element (O = 15.9994). The result obtained is 6.004.

⁹ E. Mitscherlich, *Annales de chimie et de physique*, **1820**, 14, pp. 172-191; Id., *Kongliga vetenskaps akademiens handlingar*, **1821**, 9, pp. 4-79; E. M. Melhado, *Historical Studies in the Physical Sciences*, **1980**, 11,1, pp. 87-123; H.-W. Schütt, *Eilhard Mitscherlich: Baumeister am Fundament der Chemie*, Oldenbourg, München, **1992**, pp. 81-90.

On Berzelius' application of Dulong-Petit law and isomorphism to the calculation of atomic weights, see J. W. van Spronsen, *Chymia*, **1967**, 12, pp. 157-169; J. I. Solov'ev, V. I. Kurinnoi, *Якоб Берцелиус: Жизнь и деятельность*, Наука, Москва, **1980**², pp. 78-83; H.-W. Schütt in ref. 6 (E. M. Melhado, T. L. Frängsmyr), pp. 171-179. Berzelius' results were disseminated in France through a new translation of the *Lärbok* and a second edition of the 1819 monograph; cf. J. J. Berzelius, *Traité de chimie minérale, végétale et animale*, Firmin-Didot Frères, Libraires-Éditeurs, Paris, **1829-33** (8 vols.); Id., *Théorie des proportions chimiques, et table synoptique des poids atomiques des corps simples et de leurs combinaisons les plus importantes*, Firmin-Didot Frères, Libraires-Éditeurs, Paris, **1835**.

¹⁰ J.-B. Dumas, *Dissertation sur la densité de la vapeur de quelques corps simples*, Imprimerie de M.^e V.^e Thuau, Paris, **1832**.

¹¹ See ref. 2 (Dumas), pp. 354-355.

¹² Ivi, p. 339; Id., *Traité de chimie, appliquée aux arts*, Chez Béchét jeune, Paris, **1828-46** (8 vols. and 1 atl.), I, pp. xxxviii-xxxix.

¹³ See ref. 5 (Berzelius **1808-18**), III, pp. 106-107. Cl = 221.325 (O = 100) → Cl = 69.164 instead of 35.453; weight of diatomic phosphorus contained in the molecule of phosphorus pentoxide (P₂O₅ but described as PO₅ by then) = 196.15 → P = 61.71 instead of 30.973.

¹⁴ M. G. Faershtein, *История учения о молекуле в химии (До 1860 г.)*, Наука, Москва, **1961**, pp. 70-88, 123-126.

¹⁵ W. H. Wollaston, *Philosophical Transactions of the Royal Society of London*, **1814**, 104, pp. 1-22; M. C. Usselman, *Pure Intelligence: The Life of William Hyde Wollaston*, The University of Chicago Press, Chicago, **2015**, pp. 190-239.

¹⁶ J. Petrel, *La négation de l'atome dans la chimie du XIX^{ème} siècle: cas de Jean-Baptiste Dumas*, Centre national de la recherche scientifique, Centre de documentation sciences humaines, Paris, **1979**, pp. 19-29.

¹⁷ M. A. Gaudin, *Annales de chimie et de physique*, **1833**, 52, pp. 113-133; T. M. Cole, *Isis*, **1975**, 66,3, pp. 334-360; see refs. 2 (Chaigneau), pp. 83-84, and 14 (Faershtein), pp. 88-98.

¹⁸ See ref. 16 (Petrel); L. J. Klosterman, *Annals of Science*, **1985**, 42,1, pp. 1-80: 73-80.

¹⁹ Thomas Thomson, *An Attempt to Establish the First Principles of Chemistry by Experiment*, Printed for Baldwin, Craddock, and Joy, London, **1825** (2 vols.); Id., *Principes de la chimie, établis par les expériences; ou essai sur les proportions définies dans la composition des corps* [...], Crevot, libraire-éditeur, rue de l'École de médecine, n. 3, près celle de la Harpe, Paris, **1825** (2 vols.); M. P. Crosland, in *John Dalton and the Progress of Science* (Ed.: D. S. L. Cardwell), Manchester University Press, Manchester, **1968**, pp. 274-287.

²⁰ R. Keen, *The Life and Work of Friedrich Wöhler (1800-1882)*, Traugott Bautz, Nordhausen, 2005, pp. 73-102; K. C. Nicolaou, *Angewandte Chemie International Edition*, **2013**, 52,1, pp. 131-146.

²¹ W. B. Jensen, *Journal of Chemical Education*, **2006**, 83,6, pp. 838-839.

²² W. Prout, *Annals of Philosophy*, **1816**, 7, pp. 111-113; see ref. 1 (Brock), pp. 97-108.

²³ J.-B. Dumas, *Leçons sur la philosophie chimique*, Bechet jeune, Paris, **1837**, pp. 30-32.

²⁴ J.-B. Dumas, *Annales de chimie et de physique*, **1834**, 56, pp. 113-154; A. Dumon, R. Luft, *Naissance de la chimie structurale*, EDP Sciences, Les Ulis, **2008**, pp. 31-38.

²⁵ See ref. 23 (Dumas), pp. 316-320.

²⁶ Ivi, p. 267.

²⁷ R. Siegfried, *Isis*, **1963**, 54,2, pp. 147-158.

²⁸ J. J. Berzelius, M. M de Pontin, *Economiska Annaler med Kongl. Maj:ts Nådigste Tillstånd utgisna af Kongl. Vetenskaps-Academien*, **1808**, 6 (May), pp. 110-130; 6 (June), pp. 113-118 (*maxime* pp. 122-130); see ref. 9 (Solov'ev, Kurinnoi), pp. 59-63; see ref. 6 (Melhado), pp. 203-210.

The curious amalgam was independently discovered and similarly described during the same year by Thomas Johann Seebeck (1770 - 1831) and Johann Friedrich August Götting (1753 - 1809); cf. T. J. Seebeck, *Journal der Chemie, Physik*

und Mineralogie, **1808**, 5, pp. 482-483; J. F. A. Göttling, *Elementarbuch der chemischen Experimentirkunst*, Bey H. Ch. W. Seidler, Jena, **1808-9** (2 vols.), I, pp. 247-249; J. R. Partington, *A History of Chemistry*, Macmillan, London, **1961-70** (4 vols.), IV, p. 48.

²⁹ See ref. 28 (Berzelius, de Pontin), p. 130.

³⁰ H. Davy, *The Collected Works of Sir Humphry Davy, Bart.* (Ed.: J. Davy), Smith, Elder and Co., Cornhill, London, **1839-40** (9 voll.), V, pp. 102-139 (*Electro-Chemical Researches on the Decomposition of the Earths* - 30 June **1808**): 130-131.

³¹ R. Siegfried, *Chymia*, **1964**, 9, pp. 117-124.

³² See ref. 5 (Berzelius **1808-18**), II, pp. 48-65.

³³ Ivi, pp. v, 45-68.

³⁴ C.-L. Berthollet, *Nouveau bulletin des sciences, par la Société Philomathique de Paris*, **1808**, 1, pp. 150-152; A.-M. Ampère, *Annales de chimie et de physique*, **1816**, 2, pp. 5-32: 16. Joseph Louis Gay-Lussac (1778 - 1850) and Louis Jacques Thénard - Dumas' mentor - for some time were even inclined to believe in the existence of a phlogistic principle whereby metals would be hydrogenated compounds: J. L. Lussac, L. J. Thénard, *Annales de chimie, ou recueil de mémoires concernant la chimie et les arts qui en dépendent*, **1808**, 65, pp. 325-326; Idd., ivi, **1808**, 66, pp. 205-217; Idd., ivi, **1810**, 73, pp. 197-214.

³⁵ M. Bernal, J. M. Harrie, S. W. Massey, *Monthly Notices of the Royal Astronomical Society*, **1954**, 114,2, pp. 172-179; D. J. Stevenson, *Nature*, **1975**, 258,5532, pp. 222-223; A. Baranski, W. Lu, *Electroanalytical Chemistry*, **1993**, 355,1/2, pp. 205-207. The solution to the problem required knowledge that would not be within the reach of physicochemical research for several decades yet. Moreover, the reasons behind the behaviour of ammonium remain largely to be clarified, after stabilisation of the metallic phase of the compound. So far, it has only been theorised and plausibly believed to exist under particular conditions of temperature and pressure, such as those hypothetically detectable in the cores of planets like Uranus and Neptune.

³⁶ See ref. 12 (Dumas), II, pp. 39-44; III, pp. 634-637; V, pp. 691-692.

³⁷ Ivi, II, p. 43.

³⁸ Ivi, V, p. 692.

³⁹ See ref. 2 (Chaigneau), pp. 96-102; J.-B. Dumas, F.-P. Boullay, *Annales de chimie et de physique*, **1827**, 36, pp. 294-310; Idd., ivi, **1828**, 37, pp. 15-53; U. Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century*, Stanford University Press, Stanford, **2003**, pp. 118-129, 133-137.

⁴⁰ C.-A. Wurtz, *Histoire des doctrines chimiques depuis Lavoisier jusqu'à nos jours*, Librairie de L. Hachette et C^{ie}, Boulevard Saint-Germain, n° 77, Paris, **1869**, p. 77.

⁴¹ See ref. 39 (Dumas, Boullay **1828**), pp. 36-37.

⁴² See ref. 34 (Ampère).

⁴³ See ref. 12 (Dumas), V, pp. 690-694.

⁴⁴ See ref. 23 (Dumas), p. 267. In chronological order, maybe the first of the 'illustrious chemists' Dumas was thinking of was Christoph Girtanner (1760 - 1800), who already prophesied an alchemical vengeance ushered by the decomposition of bodies deemed elementary, starting with nitrogen: "There is no refuted opinion to which we may not recur, and again examine. Philosophy acknowledges no authority which can proscribe it from admitting, or forbid it to examine. There are many other opinions, long ago refuted, to which we ought still to recur; for example, that of the transmutation of metals. What chemist at present will dare to deny the possibility of it? The change of one metal into another ought to appear less difficult than the conversion of the sweetest body (sugar) into the sourest (oxalic acid); than the change of the hardest body (the diamond) into the softest (carbonic acid gas); than the change of the most transparent (the diamond) into the most opaque (charcoal). In the 19th century the transmutation of metals will be generally known and practised"; C. Girtanner, *The Philosophical Magazine*, **1800**, 6, pp. 335-354: 353.

⁴⁵ J.-B. Dumas, J. von Liebig, *Comptes rendus de l'Académie des Sciences*, **1837**, 5, pp. 567-572; O. T. Benfey, *From Vital Force to Structural Formulas*, Houghton Mifflin Co., Boston, **1964**, pp. 36-37; see ref. 39 (Klein), pp. 161-163.

⁴⁶ J.-B. Dumas, *Comptes rendus de l'Académie des Sciences*, **1840**, 10, pp. 149-178: 158; Id., *Annales de chimie et de physique*, **1840**, 73, pp. 73-100; Id., *Justus Liebigs Annalen der Chemie*, **1840**, 33, pp. 259-300; see ref. 2 (Chaigneau), pp. 117-131; C. Gérard, *L'actualité chimique*, **2002**, 254, pp. 38-46; see ref. 39 (Klein), pp. 188-202.

⁴⁷ See ref. 2 (Chaigneau), pp. 78-79; W. H. Brock, *Justus von Liebig: The Chemical Gatekeeper*, Cambridge University Press, Cambridge, **1995**, pp. 80-87; A. J. Rocke, *Nationalizing Science: Adolphe Wurtz and the Battle for French Chemistry*, The MIT Press, Cambridge (MA)-London, **2001**, pp. 94-95; J. Drulhon, *Jean-Baptiste Dumas (1800-1884). La vie d'un chimiste dans les allées de la science et du pouvoir*, Hermann, Paris, **2011**, pp. 84-87.

⁴⁸ E. I. Hjelt, *Sammlung chemischer und chemisch-technischer Vorträge*, **1908**, 12, pp. 447-482 (or. ed. Berzelius - Liebig - Dumas i deras förhållande till radikalteorin 1832-1840, Edlundska bokhandeln, Helsingfors, **1903**).

⁴⁹ A.-É. Baudrimont, *Traité de chimie générale et expérimentale, avec les applications aux Arts, à la Médecine et à la Pharmacie*, chez J.-B. Baillière, Paris, **1844-46** (2 vols.), I, pp. 68-69, 275.

⁵⁰ J. W. Döbereiner, *Annalen der Physik und Chemie*, **1829**, 15, pp. 301-307; E. Scerri, *The Periodic Table: Its Story and Its Significance*, Oxford University Press, Oxford-New York, **2020**², pp. 46-54.

⁵¹ Id., *Foundations of Chemistry*, **2010**, 12,1, pp. 69-83; see ref. 50 (Scerri), pp. 33-135.

⁵² L. Cerruti, in S. Cannizzaro, *Sunto di un corso di filosofia chimica* (Ed.: L. Cerruti), Sellerio, Palermo, **1991**, pp. 73-282: 185; L. Anatrini, M. Ciardi, *La scienza impossibile. Percorsi dell'alchimia in Francia tra Ottocento e Novecento*, Carocci, Roma, **2019**, p. 39-41, see ref. 50 (Scerri), p. 56.

⁵³ W. V. Farrar, *The British Journal for the History of Science*, **1965**, 2,4, pp. 297-323: 305-306; see ref. 52 (Cerruti), pp. 185-190.

⁵⁴ *Report of the Twenty-first Meeting of the British Association for the Advancement of Science; held at Ipswich in July 1851*, John Murray, Albemarle Street, London, **1852**, p. xxi.

⁵⁵ L. A. Marchand, *The Athenaeum: A Mirror of Victorian Culture*, The University of North Carolina Press, Chapel Hill (NC), **1941**, p. 226.

⁵⁶ [L. Playfair], *The Athenaeum: Journal of Literature, Science, and the Fine Arts*, 12 July **1851**, 1237, p. 750; see ref. 53 (Farrar), p. 304.

⁵⁷ This kind of papers, sent in the form of *pli cacheté* to be kept sealed, was used in disputes concerning discoveries and inventions, in order to establish paternity. The senders made use of them in particular, as in the case of Tiffereau, when they considered the results of their research not yet definitive, and therefore not disclosable. Thus, in most cases, as no dispute arose, these papers remained secret and their content known only to the authors. The *Académie des Sciences*, since the early 1980s, has adopted a new policy whereby papers dating back at least one hundred years can be opened; É. Brian, C. Demeulenaere-Douyère, *Histoire et mémoire de l'Académie des sciences: guide des recherches*, Tec & Doc, Paris, **1996**, pp. 73-74. The contents of the memoir sent by Tiffereau on 6 January 1851 only became known on 9 June 1983; Archives de l'Académie des Sciences, *pli cacheté* 1070.

⁵⁸ Tiffereau is one of the very few protagonists of 19th century alchemical research on whose work a dedicated study has been conducted: L. M. Principe, *Alchemy and Chemistry. Breaking Up and Making Up (Again and Again)*, Smithsonian Libraries, Washington D.C., **2017**, pp. 24-53. For a detailed biographical profile of Tiffereau, see P. Virat, *Vaugirard-Grenelle: Bulletin de la Société Historique et Archéologique du XV^e Arrondissement de Paris*, **2015**, 45, pp. 47-58.

⁵⁹ C.-T. Tiffereau, *Les métaux sont des corps composés. Production artificielle de l'or [...]*, Imprimerie A. Quelquejeu, rue Gerbert, 10, Paris, **1888**, pp. 9-20, Id., *La science en face de la transmutation des métaux. Production chimique de l'or*, Imprimerie Billon, rue du Commerce, 47, Paris, **1906**, pp. 4-8.

⁶⁰ Id., *La transmutation des métaux. Les métaux sont des corps composés ainsi que les gaz, preuves incontestables basées sur des faits indéniables*, chez l'Auteur, Vaugirard; Imprimerie A. Quelquejeu, rue Gerbert, 10, Paris, **1900**, pp. 4-5. Of the numerous accounts of the experiment provided by Tiffereau, this is arguably the most detailed one.

⁶¹ Archives de l'Académie des Sciences, *pli cacheté* 1052; C.-T. Tiffereau, *Nouveaux procédés d'irrigation, de dessèchement et de drainage spécialement applicables à la grande et à la petite industrie agricole [...]*, Imprimerie de L. Martinet, rue Mignon, 2, Paris, **1854**.

⁶² This other *pli* remained sealed until 1983, while the incredible annex containing the artificial gold sample was not opened until 2015 (related chemical analyses have yet to be conducted); see ref. 58 (Principe), pp. 28-33; Id., *L'actualité chimique*, **2017**, 424, pp. 68-71: 70-71.

⁶³ *Comptes rendus de l'Académie des Sciences*, **1853**, 37, p. 579.

⁶⁴ See ref. 34 (Gay-Lussac, Thénard).

⁶⁵ *Bulletin des lois de l'empire français. XI^e série*, Imprimerie impériale, Paris, **1853-69** (34 voll.), V (**1855**), pp. 1161, 1178.

⁶⁶ J. C. Poggendorff, *Biographisch-literarisches Handwörterbuch zur Geschichte der exacten Wissenschaften [...]*, Verlag von Johann Ambrosius Barth, Leipzig, **1863** (2 vols.), I, col. 1442; F. Szabadváry, *History of Analytical Chemistry*, Pergamon, Oxford, **1966**, p. 274.

⁶⁷ C.-T. Tiffereau, *Les métaux sont des corps composés [...]*, Imprimerie d'Alfred Choynet, rue de l'Église, 6 et 8, Vaugirard, **1855**, pp. 47-52. As carefully pointed out - see ref. 58 (Principe), p. 74 - the experimental protocol followed by Levol at the mint differed from the one originally developed by Tiffereau in Mexico. Unfortunately, the same goes with the only known reproduction conducted in more recent times; cf. A. Truman Schwartz, G. B. Kauffman, *Journal of Chemical Education*, **1976**, 53,3, pp. 136-138, 53,4, pp. 235-239: 238.

⁶⁸ J.-B. Dumas, *Comptes rendus de l'Académie des Sciences*, **1857**, 45, pp. 709-731; Id., *ivi*, **1858**, 46, pp. 951-953; 47, pp. 1026-1034; Id., *ivi*, **1859**, 48, pp. 139-142, 372-375; Id., *Annales de chimie et de physique*, **1859**, 55, pp. 129-210.

⁶⁹ See ref. 2 (Chaigneau), pp. 178-183.

⁷⁰ S. Cannizzaro, *Nuovo Cimento*, **1858**, 8, pp. 16-17: 16.

⁷¹ See refs. 68 (Dumas **1859 Annales**), pp. 207-209, 52 (Cerruti), pp. 188-189, and 52 (Anatrini, Ciardi), pp. 39-40.

⁷² See ref. 58 (Principe), p. 51.

⁷³ See ref. 52 (Anatrini, Ciardi), pp. 117-118, 160-161. On the role of ether physics in late 19th-century chemical research and esoteric discourse, see *Conceptions of Ether. Studies in the History of Ether Theories, 1740-1900* (Eds.: G. N. Cantor, M. J. S. Hodge), Cambridge University Press, Cambridge, **1981**; H. Kragh, *Ambix*, **1989**, 36,2, pp. 49-65; E. Aspren, *Aries. Journal for the Study of Western Esotericism*, **2011**, 11,2, pp. 129-165.

⁷⁴ P. Colmant, *Reuves des questions scientifiques*, **1972**, 33, pp. 493-519; A. J. Rocke, *Chemical Atomism in Nineteenth Century*, Ohio State University Press, Columbus (OH), **1984**, pp. 321-326.

⁷⁵ See refs. 14 (Faershtein), pp. 342-343, 5 (Solov'ev), p. 151, and 52 (Cerruti), pp. 207-214; H. Hartley in *Studies in the History of Chemistry*, Clarendon Press, Oxford, **1971**, pp. 185-194; M. Ciardi, *Reazioni tricolori. Aspetti della chimica italiana nell'età del Risorgimento*, Franco Angeli, Milano, **2010**, pp. 182-183.