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Published in:
Substantia

DOI:
10.13128/Substantia-740

Publication date:
2019

Document version
Publisher's PDF, also known as Version of record

Citation for published version (APA):
Kragh, H. S. (2019). Controversial elements: Priority disputes and the discovery of chemical elements. *Substantia*, *3*(2), 79-90. https://doi.org/10.13128/Substantia-740

Substantia
An International Journal of the History of Chemistry

Firenze University Press
www.fupress.com/substantia

Citation: H. Kragh (2019) Controversial Elements: Priority Disputes and the Discovery of Chemical Elements. *Substantia* 3(2) Suppl. 5: 79-90. doi: 10.13128/Substantia-740

Data Availability Statement: All relevant data are within the paper and its Supporting Information files.

Competing Interests: The Author(s) declare(s) no conflict of interest.

Controversial Elements: Priority Disputes and the Discovery of Chemical Elements

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Abstract. There are only a limited number of chemical elements and to be credited with the discovery of a new one is therefore considered of great importance. Adding to the honour and fame is that traditionally the discoverer has the right to name the element in question. For these and other reasons, element discoveries are often followed by controversies regarding priority. While some of these are contemporary with the discovery process, others occur much later and are attempts to rewrite history. But what is a scientific discovery, more precisely, and why does it sometimes become controversial? From a scientific point of view, does it really matter who is recognised for the discovery of a new element? These are some of the questions considered in the paper, together with a few concrete cases from the history of chemistry. As shown by the recent disputes concerning the discoveries of synthetic elements at the end of the periodic system, modern priority disputes differ in some ways from the disputes of the past. On the other hand, there are also significant similarities.

Keywords. Discovery, chemical elements, controversy, priority, periodic system.

1. INTRODUCTION

The subject of this paper is priority controversies related to the discoveries of chemical elements. In order to discuss the subject rationally it will be useful to introduce and clarify the meaning of two key terms, namely "discovery" and "priority controversy," in a general way that does not necessarily relate to the chemists' elements. These are concepts that are often taken for granted, but scientists, philosophers and historians actually use them with different meanings, such as will be discussed in the following two sections. Nor is the meaning of "element" self-evident as it has changed through different phases of history to be explicated in Section 4, where a historical classification of element discoveries is suggested. In the last sections I look at aspects of three discovery cases from different periods and with different characteristics. The chosen cases are aluminium from the pre-Mendeleev era, lutetium from the early twentieth century, and nobelium from the transuranic age.

2. WHAT IS A SCIENTIFIC DISCOVERY?

Much of the discussion about scientific discoveries can be traced back to different conceptions of what constitutes a discovery.[1] It is generally assumed that a scientist (or a group of scientists) has discovered X if he or she has convincingly established that X exists or is the case. X may be an object, a phenomenon, or a significant relation between empirical data. In the latter case it may consist of a structural organisation of data, where all or some of the data may be known in advance. An example from the history of chemistry is the Dulong-Petit law of 1819 correlating the specific heats of elements and their atomic weights. Another and more important example is Mendeleev's periodic system fifty years later. Although Mendeleev's system did not originally rely on new discoveries of objects and their properties, he discovered the system in the constructive sense that he organised known data into a new conceptual framework.

It may seem obvious that only objects which really exist can be discovered. When we say that William Ramsay and Lord Rayleigh discovered argon in 1894, it implies that argon really exists as a component of the atmosphere. As the philosopher Peter Achinstein has argued, truth and discovery go together: "Discovering something requires the existence of what is discovered. You cannot discover what doesn't exist."[2] Of course, one can *claim* to have discovered a non-existing object, and the claim may even be broadly accepted for a period of time, but in that case the claim does not count as a proper discovery.

Although Achinstein's view may seem to be common sense, from a historical perspective it is problematic to reserve the category of discovery for what is presently accepted as true. From this perspective one may legitimately speak of the discovery of non-existing objects or phenomena, namely if the discovery claim received wide recognition at the time it was announced. Phlogiston does not exist and yet the substance was believed to exist for half a century or so. It makes sense to say that Georg E. Stahl discovered phlogiston in about 1720 and also that Joseph Black discovered the heat substance called caloric in the 1730s. These non-existing entities were discovered and later de-discovered. There is another reason why Achinstein's claim is problematic, namely that it seems to presuppose that objects exist in nature prior to their discovery. But there are objects, such as the artificially produced superheavy elements, that only come into existence *with* their discovery. The so far heaviest known element, oganesson with atomic number 118, was not discovered because it existed. It exists because it was discovered.

A chemical element is not a specific and localisable object of the same kind as, say, a planet. The chemist cannot point to a piece of sodium and claim that "*this* is sodium" in the same sense as the astronomer can point to a planet and claim that "*this* is Neptune." On the other hand, to say that something is or contains the element sodium involves the *concept* of an element, just like the identification of Neptune as a planet involves the concept of a planet. The discovery of a new element is thus to demonstrate convincingly the elemental nature of some substance, which is a conceptual discovery, and also to find this substance in nature – or perhaps to synthesize it in the laboratory. The latter is an empirical discovery.

For something to be a discovery it is normally assumed that it must be a novelty, and for this reason it cannot be made twice at different times. On the other hand, there are many examples in the history of science of so-called rediscoveries, a concept which typically refers to insights that originally attracted very little attention and at a later time were unknowingly duplicated.[3] The rediscovery will almost always be in a different form than the original discovery. An example from the history of chemistry is provided by the discovery of vanadium, which was first isolated by Andrés Manuel del Río in 1801 and rediscovered by Nils G. Sefström thirty years later.[4]

If a discovery is little known and exerts almost no impact on the scientific community, the rediscovery is more effective than the one with which the discovery is often associated. The useful concept of the "effective discovery of an element" was introduced by the Danish chemist Edmond Rancke-Madsen, who referred to the cases of hydrogen (Henry Cavendish, 1766), oxygen (Joseph Priestley, 1775) and chlorine (Carl W. Scheele, 1774) as examples.[5] According to Rancke-Madsen, for a scientist to be the effective discoverer of an element, he (or she) must have observed the existence of a new substance "which is different from earlier described substances, and this new substance is recognized by him *or later by scientists* as being elemental" (emphasis added). Moreover, the discovery of the new substance must have been announced publicly and attracted attention among contemporary scientists. Notice that according to this view, the effective discoverer does not need to have recognised the substance as an element; what matters is only that it was granted this status by later scientists and that this is still its status today.

The notion of an effective discovery underlines that a discovery cannot be a private matter or limited to just a few persons. Not only must the discovery claim be publicly available, it must also be known and accepted by at least a substantial part of the relevant scientific community. It must be communicated, usually in a journal article although it can also be in the form of a well-publicized lecture or a press conference. The Swedish chemist and historian of chemistry Jan Trofast offers the following definition of the discovery of a chemical element:

> *A discovery is established when the scientist has shown new properties of the new element in form of e.g. a number of salts and clearly and unambiguously shown that it is a new element. ... Further the time of discovery is said to be when the first publication (could be in the form of a letter to a colleague) is available and not when the first observation is made or when the first suspicion was aroused in the laboratory.*[6]

However, to include a private letter under the label "publication" is too wide an interpretation of the term. Communication by letter does not secure dissemination to the scientific community but at most to a few members of it. Only in exceptional cases, namely if the letter is copied or its content otherwise circulated to a large number of scientists, can this form of communication be of a public or semi-public nature. The scientist who makes an observation of something new, but reports it only in his diary or in a letter, has not made a discovery and that even though he may have recognised the novelty and significance of what he has observed. According to Alan Gross, "There is no such thing as a private discovery… A scientific discovery, then, is the public attribution of novelty to a claim regarded by a relevant scientific community as possible and as the consequence of following appropriate methods."[7]

Consider the case of plutonium which was identified in nuclear reactions by Glenn Seaborg and his team in late 1940 (Pu-238) and early 1941 (Pu-239). As a result of the unusual political circumstances of World War II the discovery paper submitted on 7 March 1941 to *Physical Review*, only appeared in print five years later.[8] Although plutonium thus became publicly discovered only in 1946, it is customary and reasonable to date the discovery to the year 1941. Incidentally, in this case there was no priority controversy as the discovery was unanimously assigned to Seaborg and his collaborators.

According to the individualist or "heroic" model of discovery widely favoured by scientists and journalists, one can identify the moment a discovery occurred and also the individual who should be credited. However, historical studies demonstrate that in many cases this is not possible and, generally, that the model is inadequate. Rather than focusing on the discovery itself some historians and sociologists of science argue that what matters is not so much the discovery's intellectual history as its social history. How and why does a discovery claim become accepted as a *bona fide* discovery by the scientific community? According to this view discoveries are retrospective judgments which are socially defined and constructed. They are labels attributed *post hoc* to some discovery claims but not to others. As one author puts it, "Discoveries do not simply 'occur' or 'happen' naturalistically, but are socially defined and recognized productions."[9]

While the importance of the social history is beyond doubt in discovery studies, it does not follow that it offers a sufficient account of discoveries and their receptions. Moreover, the social analysis is not incompatible with a more traditional, intellectual analysis. The two approaches are supplementary and none of them is sufficient alone.

Discoveries are often thought to be purely empirical, meaning that the first observation of an object or phenomenon X constitutes the discovery of X. However, philosophers have long pointed out that this is too simplistic a view and that a discovery involves an active mental process as it relies on theoretical preconditions. A scientist may observe or perceive X without identifying it *as* X. Or to put it differently, there is a crucial difference between "seeing that" and "seeing as."[10] For example, in experiments with iron and dilute strong acids Robert Boyle and other seventeenth-century chemists observed an "air" without recognising it to be new or elemental. They observed what became known as hydrogen, but they did not discover hydrogen. This view is contrary to the one of Rancke-Madsen as cited above.

The idea that discoveries involve changes in the theoretical or conceptual framework was a leading theme in Thomas Kuhn's influential essay dating from 1962. Using the discovery history of oxygen as a lead example Kuhn argued that "Observation and conceptualization, fact and assimilation of fact to theory, are inseparably linked in the discovery of scientific novelty."[11] Moreover, he distinguished between two classes of discovery, namely those which could be predicted from accepted theory in advance and those which could not. Kuhn found the second class – "discovery by accident" – to be more interesting, as this kind of discovery would typically force scientists to organise known data into a new conceptual framework. As a result an existing paradigm might be challenged and give rise to a revolutionary phase in science.

With respect to discoveries of the first class, such as the elements predicted or anticipated from the periodic system, Kuhn wrote that "There have been few priority debates … and only paucity of data can prevent the historian from ascribing them to a particular time and place." However, this is definitely a misconception. After the acceptance of the periodic system the frequency of priority controversies did not diminish nor did they become less serious. On the contrary, conflicts of this kind rather increased in number and intensity.

3. CONTROVERSIES OVER PRIORITY

In his *History and Present State of Electricity* dating from 1767, Priestley suggested that "mistakes, misapprehensions, and altercations" should have no place in the annals of science. According to him,

> *All the disputes which have no way contributed to the discovery of truth, I would gladly consign to eternal oblivion. Did it depend upon me, it should never be known to posterity, that there had ever been any such thing as envy, jealousy, or cavilling among the admirers of my favourite study.*[12]

Yet it is all too clear that controversies of various kinds do play an important and often fruitful role in science and have always done so. They are sometimes instrumental in defining the disciplinary boundaries related to a new subfield, such as exemplified by the emergence of physical chemistry in the late nineteenth century.[13]

As Robert Merton pointed out in a pioneering paper of 1957, not only are controversies abundant they also contribute – contrary to what Priestley thought – to scientific progress.[14] To be involved in a scientific controversy whether as a winner or loser, may cause a loss in reputation, but this is not generally the case. Priority controversies, in particular, may have the effect of increasing the competitive pressure and forcing the participants to study the subject in question more extensively and in greater depth than if the controversy had not existed. This kind of controversy goes far back in time, certainly to the age of Galileo if not earlier. One reason why the assignment of credit is important is that it helps in understanding the historical dynamics in the discovery process. Assigning the wrong credit for a discovery may distort the picture of how and why the discovery occurred.

There is no generally accepted definition of a scientific controversy, but it is useful to distinguish the concept from other forms of communicative disagreement, such as debate, discussion, polemics and dispute.[15] First, for a disagreement to qualify as a *scientific* controversy, evidently it should centre on a scientific issue and involve scientists as key participants. While some controversies are "pure," meaning that they are concerned almost exclusively with scientific questions, others are "mixed." The latter category refers to cases where political, environmental and ethical concerns enter significantly, such as the use of flame retardant chemicals.[16]

Whether belonging to one class or the other, typically a controversy is of some duration, it is expressed in public, and it takes place by means of arguments and counterarguments. Contrary to what is the case in a debate or discussion, the parties involved in a controversy must be committed to one of the opposing views. Being more than a quarrel between two individual scientists a controversy involves the relevant scientific community, and it is only if the community considers the disagreement worth taking seriously that it will develop into a proper controversy. In some if not all cases major parts of the scientific community will be engaged on both sides of the disagreement, although often disproportionally.

There are different kinds of controversies. Following a proposal of Ernan McMullin, one may distinguish between controversies of fact, of theory, and of principle.[17] In the present context dealing with element discoveries the first category is the most important. Here scientists disagree on whether a claimed entity or property actually exists. Does the substance claimed to be a new chemical element really have the status of an element? The two other categories relate to different theoretical views and methodological principles, respectively. The three categories are not mutually exclusive and may in some cases appear together, such as was the case with the much-discussed discovery history of oxygen.[18]

Disputes over priority mostly concern either factual or theoretical disagreements as in the discovery of objects or theories. They may also be about names which, in the case of new elements, have often provoked controversy if of a different kind. The accepted name of an element may directly or indirectly refer to the discoverer and thus suggest which scientist is to be credited with the discovery. Consider a scientist X who proposes the name A for a new element he claims to have found, while scientist Y independently finds what he names B and believes is the same element. In this case a dispute about the name reflects a controversy about discovery (see Section 6). Naming controversies have been common for the transuranic elements and in particular for those named after a scientist. The most controversial of the names was the one of element 106, seaborgium, but there were others as well.[19]

As controversies appear in different forms, so they terminate in different ways. A controversy may be resolved, meaning that the two parties come to agree, by means of scientific arguments, that one of the competing claims is after all superior to the other. The Irish chemist Richard Kirwan had for long defended the phlogiston theory and criticized Lavoisier's oxygen alternative, but in 1792 he gave in. "I lay down my arms and abandon the cause of phlogiston," he wrote.[20] A controversy may also terminate by withering away, perhaps by lack of interest or simply because the protagonists of one of the competing views disappear from the scene. Finally historians and sociologists speak about termination by closure if political or other non-scientific factors force the controversy to end.

In most cases priority controversies take place simultaneously with the discovery claims and involve the competing scientists as the main contenders. But in other cases they emerge retrospectively many years after the contenders have passed away and the case apparently was closed. It may be that new data or historical sources come to the light of day, or that scientists re-examine the case and argue that X rather than Y should be credited with the discovery. As we shall see below, the discoveries of aluminium and lutetium are examples.

Another and more recently discussed case concerns element 75, rhenium, which is credited work by Ida Tacke (later Noddack), Walter Noddack and Otto Berg in 1925. However, many years earlier the Japanese chemist Masataka Ogawa believed to have found evidence for the element, which he called nipponium. By 1925 nipponium was long forgotten, but as late as 2004 it was argued that Ogawa had indeed discovered the element.[21] To the extent that one can speak of a priority conflict in this rather unconvincing attempt of rehabilitation, it was constructed much *post festum* (see also Section 4).

4. AN OVERVIEW OF ELEMENT DISCOVERIES

It is generally agreed that phosphorus is the first element with a known discovery history and discoverer. The earlier elements known to ancient cultures, such as sulphur, gold, silver and tin, were not discovered in any real sense (Figure 1).

When the Hamburg merchant and alchemist Hennig Brand in or about 1669 produced a white, waxy and luminous substance by distilling male urine and heating the remaining paste, he serendipitously discovered phosphorus in the form P_4 (Figure 2).[22] But he did not, strictly speaking, discover the chemical element phosphorus as neither he nor his contemporaries conceived the substance as an elementary body. Nor did Brand communicate his discovery publicly, in the form of a publication. Only in 1678 did the German chemist Johann Kunckel publish an account of the new substance and how to prepare it. Still a century after Brand's discovery phosphorus was thought to be a composite body, namely "a kind of sulphur composed of a particular acid united with phlogiston … [and which] resembles vitriolic sulphur also in this point that its phlogiston may be burnt, even with rapidity, without any decomposition of its acid."[23]

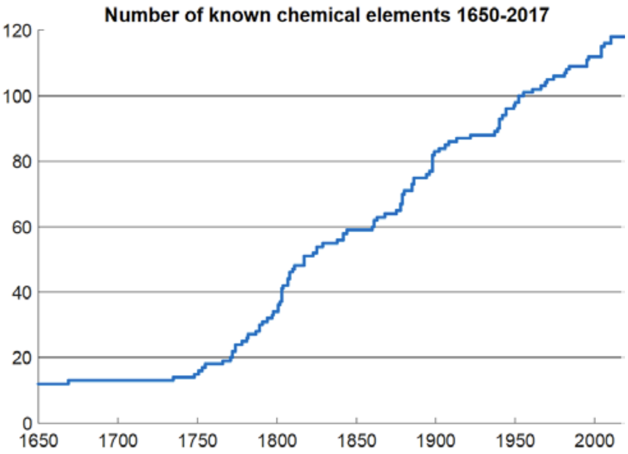

Figure 1. Discoveries of chemical elements since 1650. Source: https://commons.wikimedia.org/wiki/File:Known-elements-1650-present.png

Figure 2. Detail from Joseph Wright of Derby's painting of 1771 showing an alchemist discovering phosphorus. Source: https://resobscura.blogspot.com/2017/06/urine-phosphorus-and-philosophers-stone.html

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. Bâſe de la mofete.
	Hydrogène	Gaz inflammable. Baſe du gaz inflammable.
Subſtances ſimples non métalliques oxidables & acidifiables.	Soufre	Soufre.
	Phoſphore	Phoſphore.
	Carbone	Charbon pur.
	Radical muriatique.	Inconnu.
	Radical fluorique	Inconnu.
	Radical boracique	Inconnu.
Subſtances ſimples métalliques oxidables & acidifiables.	Antimoine	Antimoine.
	Argent	Argent.
	Arſenic	Arſenic.
	Biſmuth	Biſmuth.
	Cobolt	Cobolt.
	Cuivre	Cuivre.
	Etain	Etain.
	Fer	Fer.
	Manganèſe	Manganèſe.
	Mercure	Mercure.
	Molybdène	Molybdène.
	Nickel	Nickel.
	Or	Or.
	Platine	Platine.
	Plomb	Plomb.
	Tungſtène	Tungſtène.
	Zinc	Zinc.
Subſtances ſimples ſalifiables terreuſes.	Chaux	Terre calcaire, chaux.
	Magnéſie	Magnéſie, baſe du ſel d'Epſom.
	Baryte	Barote, terre peſante.
	Alumine	Argile, terre de l'alun, baſe de l'alun.
	Silice	Terre ſiliceuſe, terre vitrifiable.

Figure 3. Lavoisier's table of "simple substances" in his *Traité Élémentaire de Chimie* from 1789.

Although Brand's work of 1669 does not live up to current philosophical, non-anachronistic ideas of what constitute an element discovery, somehow it seems artificial to deprive him of the credit of having discovered phosphorus. There was at the time a kind of priority controversy even though it did not concern phosphorus as an element but only as a new and exciting substance. Within a decade or two Brand faded into obscurity, his priority defended only by Leibniz. By the turn of the century priority had effectively been conferred to either Kunckel or his compatriot Johan Daniel Krafft.

The point is that according to the early chemists phosphorus was not elemental. A concept of chemical elements roughly similar to the modern one only arose in the 1780s, perhaps first stated by the German chemist Johann Gmelin.[24] More famously and in greater detail it was stated by Antoine Lavoisier in his seminal treatise *Traité Élementaire de Chimie* published in 1789. According to Lavoisier, phosphorus was an element or "simple substance," meaning that it could not be decomposed – or had not yet been decomposed – into still simpler bodies (Figure 3).

The later history of element discoveries may conveniently be classified in four chronological phases, the first of which is associated with John Dalton's atomic theory. The immediate importance of Dalton's *New System of Chemical Philosophy* was not so much the atomic hypothesis as the idea to associate the relative weights of atoms with a measurable quantity, the atomic weight. As far as the concept of element was concerned, Dalton followed Lavoisier's operational formula: "By elementary principles or simple bodies we mean such as have not been decomposed, but are found to enter into combination with other bodies."[25] With Jöns Jacob Berzelius' staunch support of Dalton's theory the establishment of still more precise atomic weights became a matter of prime concern. In 1826 Berzelius published his final table of atomic weights. To him and many of his contemporaries the identification of new elements relied on determinations of their atomic weights. Often credited as the discoverer of five new elements (cerium, selenium, silicon, zirconium, and thorium), Berzelius was eminently successful and his successes depended to a large extent on his analytical skills in determining the elements' atomic weights.[26]

Dmitri Mendeleev's classification of elements in 1869, which I take to be the beginning of phase two, rested crucially on the postulate that an element was defined by its atomic weight. In his Faraday lecture of 1889 the Russian chemist pointed out that before the periodic system "there was no special reason to expect the discovery of new elements." It was only the gaps in the sequence of atomic weights as organised in the periodic system that "enabled us to perceive undiscovered elements at a distance which formerly was inaccessible to chemical vision, and long ere they were discovered."[27]

With the acceptance of the periodic system or table it came to define the possibility of new elements: If X has no place in the table, it cannot possibly be an element. The dogma was challenged with the discovery of argon and helium in the 1890s, but in this case order was reinstated by adding a new group of inert gases to the system. Despite the authority of Mendeleev's system, or the corresponding one of Lothar Meyer, chemists continued to suggest new elements. They sometimes hid them in the poorly understood group of rare earths and at other times they were just unconcerned with whether they fitted into the system or not. Random examples are nebulium, etherion and carolinium.[28] Characteristically these discovery claims were rarely taken seriously.

In the third phase, starting with the introduction of isotopy and the atomic number *Z* in about 1913 it turned out that the atomic weight was not after all the defining property of an element.[29] Yet the periodic system survived the redefinition of an element and the change of the elements' ordinal number from the atomic weight to the atomic number. The latter quantity, as given by the charge of the atomic nucleus, could be measured by means of the method of X-ray spectroscopy pioneered by Henry Moseley. However, it took until 1923 before the new definition of an element was sanctioned by IUPAC, the International Union of Pure and Applied Chemistry. Whereas the periodic system did not originally restrict the number of possible elements, with the introduction of the atomic number the existence of elements lighter than hydrogen was ruled out. What had formerly been possible, if unlikely, now became impossible. On the other hand, the new understanding of the periodic system did not preclude new elements heavier than uranium. In principle there might be any number of them.

The fourth and last phase in the history of element discoveries may be said to have started in 1937 with the manufacture and hence discovery of the first artificial element, soon to be followed by many transuranic elements. Technetium, the approved name of element 43, was discovered by the physicist Emilio Segré and the mineralogist Carlo Perrier by analysing a molybdenum target irradiated with deuterons and neutrons. There had earlier been several unconfirmed claims of having detected element 43 in nature, noticeably by the Noddack-Tacke-Berg team which in 1925 claimed to have found small amounts of the element. This evolved into a priority controversy between "masurium" and technetium which much later was re-opened by scientists in favour of the masurium claim.[30] The much delayed attempts to change the discovery history of element 43 were ignored by IUPAC and the large majority of chemists.

5. THE THIRD-MOST COMMON ELEMENT

Given that aluminium makes up 8.1% of the Earth's crust, it is remarkable that its discovery dates back less than 150 years. In the case of element 13 there was no major priority controversy, but there are other features in the history of the element that makes it instructive from a discovery perspective.[31] It is generally agreed that the German chemist Andreas Sigismund Marggraf was the first to realise, in 1754, that there is a separate "earth" (alumina) in alum different from the one in limestone.[32] The still unknown earth appeared as "argile" in Lavoisier's table of 1789, with the author suggesting that it might be a metallic element saturated with oxygen. Attempts to isolate the metal in alumina made by Humphry Davy and later by Berzelius failed, although Berzelius was able to determine its atomic weight to 27.4. The element was known to exist and Davy had even coined a name for it, alumium or what later became aluminium, but it had not yet been discovered.

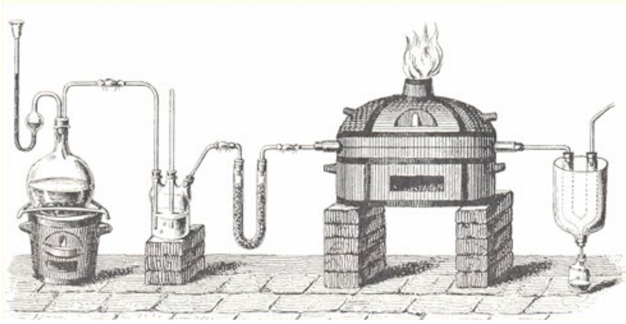

Figure 4. The first step in Ørsted's isolation of aluminium, the synthesis of $AlCl_3$. In modern nomenclature the process is $Al_2\,O_3+3C+3Cl_2 \rightarrow 3CO+2AlCl_3$. Reproduced from a Danish textbook of 1853 (H. Westergård, *Uorganisk Kemi*).

The main steps in the element's discovery process are well known and need only to be briefly recapitulated. In early 1825 H. C. Ørsted, the discoverer of electromagnetism, reported to the Royal Danish Academy of Science a new method in which he transformed alumina (Al_2O_3) to anhydrous aluminium chloride and subsequently reduced it by means of potassium amalgam (Figure 4). The result was what Ørsted described as "a lump of metal resembling tin in colour and lustre."[33] The brief Danish report was abstracted in Schweigger's *Journal für Physik und Chemie* and Poggendorff's *Annalen der Physik und Chemie*, and in 1827 it appeared in a German translation in Berzelius' *Jahresbericht über die Fortschritte der physischen Wissenschaften*.[34] Nonetheless, it failed to attract interest. Ørsted, who did not find the method and the new element to be very important, never returned to the subject. In September 1827 young Friedrich Wöhler visited Ørsted, who told him about the metal and encouraged him to take a closer look at it. Back in Germany Wöhler was unable to confirm that Ørsted's method yielded aluminium, but by using pure potassium as the reducing agent he produced the metal in the form of a grey powder. Wöhler came to believe that what Ørsted had thought was a lump of aluminium was instead a kind of alloy of aluminium and potassium.

In his discovery paper of 1827 Wöhler gave full credit to Ørsted for his discovery of aluminium chloride, carefully pointing out that he had no intention to exploit Ørsted's pioneering work of 1825 or being disloyal. He mentioned that the Danish scientist "has indirectly encouraged me to try to attain to further results myself." Ørsted seems not to have cared about priority, and there was no rivalry between him and Wöhler. In 1845 Wöhler was able to obtain aluminium in a compact metallic if still impure form and to determine, for the first time, the metal's principal properties. If Ørsted's work of 1825 was the first birth of aluminium, and Wöhler's of 1827 and 1845 marked the second birth, the third birth dates from 1854 when Henri Saint-Claire Deville found a new method to produce the metal in pure form. It was only with Deville's work that aluminium became a useful metal and not a mere chemical curiosity.

Deville never claimed to have discovered aluminium, an honour he fully ascribed to Wöhler, and yet in France he was widely and in part for national reasons considered the true discoverer.[35] As to Ørsted's role in the discovery process, Deville simply ignored it. In his influential book *De l'Aluminium* from 1859, the first comprehensive work on the history and use of the new metal, there is no mention of Ørsted. For most of a century Wöhler was recognised as the one and only discoverer of aluminium, whereas Ørsted's earlier synthesis was generally considered to be wrong or incomplete, perhaps an anticipation of aluminium but not an isolation of the metal.

However, on the occasion of the centenary of the discovery of electromagnetism Danish chemists reconsidered Ørsted's method and reconstructed the old experiments to establish whether or not aluminium had been obtained back in 1825. The result of this attempt to rewrite history was that in all likelihood Ørsted had isolated impure aluminium two years before Wöhler, a conclusion in which there clearly was an element of national pride. It is about time, wrote the distinguished chemist Niels Bjerrum, "to reinstate Ørsted as the first who obtained aluminium."[36] Contrary to the earlier mentioned case of element 75 (Section 3), in this case the attempt of rehabilitation succeeded to some extent. According to Harry Holmes, an American chemist, "It is now in order for the world to atone for the injustice by giving the Dane credit for the discovery."[37] Not all chemists and historians agree, but today it is not uncommon to name Ørsted as the discoverer of aluminium or to share the credit between him and Wöhler.

So, when was aluminium discovered and to whom should priority be allocated? As indicated in Section 2, the question is misguided as it presupposes an answer in terms of a definite year and a definite discoverer. A summary of the discovery process may provide the only appropriate answer: In 1754 Marggraf recognised a special "earth" in alum which subsequently became known as an element and prepared by Ørsted in 1825 in an impure form; two years later Wöhler produced aluminium as metallic powder and in 1845 he determined its density and some other properties; finally, in 1854 Deville created pure aluminium and laid the base for its industrial use.

6. LUTETIUM, A CONTROVERSIAL ELEMENT

Contrary to the case of aluminium, the discovery of the rare earth element 71, lutetium, involved a series of convoluted priority controversies concerning scientific as well as external issues. Although disputes about fact entered the controversy, it was basically about priority. For a long time the number of rare earths and their position in the periodic system was a matter of confusion and dispute, a situation which was only settled in the mid-1920s. The uncertainty resulted in several premature or wrong discovery claims of which "celtium" as a candidate for element 72 has received much attention by historians of science. As it turned out in 1923, element 72 (hafnium) is not a rare earth but a homologue to titanium and zirconium.[38] The case of element 71 is closely connected with the celtium-hafnium controversy but started earlier, at a time when a chemical element was still defined by its atomic weight. The controversy over this element took place in two separate phases, originally around 1908 and with a second round in 1923. It provides one more example of how later research may throw new light on the history of the discovery of elements.

The ytterbium earth isolated by Jean C. G. Marignac in 1878 was generally accepted as a chemical element for more than two decades, but in 1907 two chemists, Georges Urbain in France and Carl Auer von Welsbach in Austria, independently concluded that ytterbium contained a hitherto unknown element. Urbain reported his finding to the Paris Academy of Science on 4 November 1907, whereas Auer presented his full report to the Vienna Academy six weeks later (but had stated his claim in preliminary communications of 1905 and 1906; Figure 5).

While Urbain named the new element lutecium (Lu), and proposed neo-ytterbium (Ny) for the more dominant element corresponding to Marignac's ytterbium, Auer suggested the names cassiopeium (Cp) and aldebaranium (Ad). Both chemists claimed priority and immediately engaged in a heated controversy.[39] For example, at one stage Auer accused his French rival of foul play, to which Urbain indignantly responded: "[Auer] goes as far as accusing me of simply plagiarizing him. … It is disgraceful of Mr. Auer v. Welsbach to

Figure 5. Carl Auer von Welsbach. Source: https://commons.wikimedia.org/wiki/File:Carl_Auer_von_Welsbach_1910.jpg

make such accusations against his colleagues."[40]

Without going into further details, in 1909 the International Committee on Atomic Weights decided in favour of Urbain's priority, primarily because he was the first to publish an atomic weight for what now became lutecium and since 1949, lutetium. Several years later Auer unexpectedly got a second chance.

In the wake of the celtium-hafnium dispute scientists at Niels Bohr's institute in Copenhagen investigated anew Auer's cassiopeium by means of optical spectroscopy and compared the result with Urbain's spectrum of celtium from 1911. From this they concluded not only that celtium anno 1911 was nothing but element 71, but also that Urbain's original sample of lutetium contained much less of the element than Auer's cassiopeium. Consequently the Copenhagen scientists initiated a campaign to reinstate the Austrian chemist as the discoverer of element 71. The campaign succeeded in so far that the German Atomic Weight Commission gave full credit to Auer in 1923, but IUPAC maintained the name lutetium, or rather lutecium, and Urbain's priority.

Contrary to what is often stated in the chemical literature, Urbain's claim of having discovered lutetium in 1907 rested on a somewhat shaky foundation. Element 71 was undoubtedly discovered this year, but it might be just as reasonable to credit Auer with the discovery and relegate Urbain as an independent co-discoverer. The International Committee's decision to honour Urbain was based on incomplete information and an interpretation of available data favourable to Urbain's claim.[41] A contributing reason may have been that in 1909 Urbain served as chairman of the International Committee, the other members being Wilhelm Ostwald, Frank W. Clarke and Thomas Thorpe. Finally, the American chemist Charles James is sometimes mentioned as an independent discoverer or co-discoverer of lutetium, but since James did not publish his discovery and never pushed his own claim, this is unjustified.[42] He could have discovered the element, but did not.

7. NOBELIUM, ELEMENT 102

The manufacture of many of the transuranic elements, and especially those with atomic numbers between 100 and 113, has given rise to controversies regarding identification, name and priority. A noteworthy example is element 102, nobelium, which was first claimed discovered in 1957 but only received official recognition by IUPAC 35 years later.[43] The controversy, which was primarily concerned with whether priority belonged to teams of American or Russian scientists, has certain features in common with the earlier controversy over element 72. As the latter controversy was coloured by the international political climate in the early 1920s, so the controversy over element 102 included external factors reflecting the political atmosphere of the Cold War era.

But it started with a third group of contenders, namely an international team working at the Nobel Institute of Physics in Stockholm and consisting of four Swedes, two Britons and one American. In 1957 the team announced that it had detected element 102 by bombarding a sample of curium ($Z = 96$) with ions of carbon-13. As regards the name of the element the Stockholm scientists suggested to call it nobelium in honour of Alfred

Nobel. The discovery claim created much attention in Swedish and British news media, not least because it was the first transuranic element discovered in Europe. At the time nuclear syntheses of heavy elements was a monopoly of two research groups, one associated with the University of California at Berkeley, and the other with the Dubna nuclear research facility in Russia. None of the groups accepted the news from Stockholm and they were unable to reproduce the claimed results.

Even though the Swedish-led discovery claim turned out to be unfounded, this was only the beginning of a much longer priority controversy involving American and Russian scientists as competitors. The discovery story of nobelium has been called "the most convoluted and misunderstood of all [the discovery stories of] the transfermiums."[44] In short, the Berkeley team led by Albert Ghiorso first claimed to have produced the element in 1958, but the Dubna team vehemently denied the claim and argued that, "Element 102 was discovered at Dubna in studies carried out during 1963-1966. Those papers contain unambiguous and complete evidence for the synthesis of its nuclei."[45] Ghiorso and his collaborators (including the Nobel laureate Glenn Seaborg) responded by criticizing the Russian results and maintaining the validity of their own work.

Somewhat strangely, element 102 is still named nobelium and thus refers to a discovery claim that was known to be wrong or at least highly insufficient. Although neither the Americans nor the Russians accepted the claim, none of them suggested a different name. Nobelium had quickly entered textbooks and periodic tables, and in 1961 IUPAC approved the name and symbol without evaluating the validity of the Stockholm experiments. For a while the Russians used the name "joliotium," a reference to the French nuclear physicist Frédéric Joliot (or Joliot-Curie), but the name never caught on.[46] It is not irrelevant to mention that Joliot was a devoted communist and staunch supporter of the Soviet Union.

Figure 6. Russian stamp of 2013 dedicated to G. Flerov after whom element 114, flerovium, is named. Flerov was also head of the research team credited with the discovery of nobelium. Source: https://commons.wikimedia.org/wiki/File:RUSMARKA-1660.jpg

To take care of the many priority disputes IUPAC and IUPAP (International Union of Pure and Applied Physics) established in 1985 a joint Transfermium Working Group (TWG) consisting of nuclear physicists and chemists. After a review of all relevant papers on element 102, in a report of 1992 the TWG concluded in favour of the Dubna team whereas it found that the Berkeley experiments did not qualify as a discovery. The decision caused strong reactions from the Americans who not only charged that the TWG panel was incompetent but also that it was biased in favour of the Dubna claim. But IUPAC accepted the TWG report, meaning that the Russian nuclear physicist Georgii Flerov (or Georgy Flyorov) and his team were approved as discoverers of element 102 (Figure 6). This was not quite the end of it, though, for Ghiorso and Seaborg restated their case in "an appeal to the historians of science to reread the cited literature and perhaps, belatedly, to reassign credit." If they could not get full credit they would accept "in the spirit of glasnost" to share it with the Russians.[47] But the appeal was ineffective. The controversy terminated by a mixture of resolution and closure.

The TWG panel was acutely aware that an assignment of priority for having discovered an element cannot be separated from a definition of what constitutes a discovery. The chosen and agreed-upon definition was simply that the discovery of a chemical element is an experiment which convincingly demonstrates "the existence of a nuclide with an atomic number *Z* not identified before." The TWG panel further reflected on the historical importance of element discoveries:

> *The centuries-old history of the definition and discovery of chemical elements has a deep scientific and general fascination. … The problem is open although of final scope, unlike the number of continents upon the surface of the earth where we know with certainty that none still awaits discovery. These considerations give to the discovery of new elements an importance, an allure and a romance that does not attach to the discovery of, say, a new comet or a new beetle where many more such discoveries are to be anticipated in the future.*[48]

The TWG comment related to the synthetic elements produced at the end of the twentieth century, but it could as well have been written by chemists at the time of Mendeleev.

8. CONCLUSION

Although there are today formal criteria for the discovery of a new element, and for assigning priority to the discovery, these are not applicable to many discoveries in the past. The relevant criteria depend on the historical period and so do the accepted rules for priority. It seems hardly possible to come up with a fixed definition of element discovery which makes sense over the approximately 250 years during which chemists have searched for new elements. The search has often given rise to priority controversies, a phenomenon one can find throughout history and independent of whether or not the discovery was guided by theoretical expectations. To understand these and other controversies related to the discovery of new elements, one needs to adopt the norms and rules of the period in question and not those of a later time. In this essay I have also pointed out that accepted discovery histories may retrospectively be questioned and revised. At least in principle it is possible that a future list of element discoveries will differ significantly from the one accepted today.

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