ALCHEMY *and* CHEMISTRY

BREAKING UP AND MAKING UP (AGAIN AND AGAIN)

LAWRENCE M. PRINCIPE

Dibner Library Lecture

DECEMBER 11, 2014

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Smithsonian Libraries

PUBLISHED BY
Smithsonian Libraries
P.O. Box 37012, MRC 154
Washington, D.C. 20013-7012
library.si.edu

EDITED BY
Kara Mason

DESIGNED BY
Fuszion

PRODUCED BY
Elizabeth O'Brien and Lilla Vekerdy

Library of Congress Control Number:
2017940463

ISBN (electronic) 978-0-692-88221-4
ISBN (pbk) 978-0-692-87991-7

Printed in the United States of America

About the Author

Lawrence M. Principe is the Drew Professor of Humanities at Johns Hopkins University in the Department of History of Science and Technology and the Department of Chemistry. He is also Director of the Singleton Center for the Study of Premodern Europe. He earned a Ph.D. in Organic Chemistry from Indiana University, and a Ph.D. in History of Science from Johns Hopkins University.

Prof. Principe's research focuses on the early modern period, with particular emphasis on the history of alchemy/chemistry. His books include *The Aspiring Adept: Robert Boyle and His Alchemical Quest* (1998) and *Alchemy Tried in the Fire: Starkey, Boyle, and the Fate of Helmontian Chymistry* (co-authored with William R. Newman; Chicago, 2004), winner of the 2005 Pfizer Prize. He has authored two books for general readership: *The Scientific Revolution: A Very Short Introduction* (2011) and *The Secrets of Alchemy* (2013).

His research has been supported by the National Science Foundation, the National Endowment for the Humanities, and a Guggenheim Fellowship. He is the inaugural recipient of the Francis Bacon Medal (2005) for significant contributions to the history of science. He is currently completing a study of the transformations of chemistry at the Parisian Académie Royale des Sciences, 1666–1730.

Acknowledgements

Part of the research presented in this publication was supported by National Science Foundation grant #9984106, "The Formation and Support of Chemists and Chemistry," and by a 2015–16 John Simon Guggenheim Memorial Fellowship. I would like to thank Isabelle Maurin-Joffre and her helpful and generous team at the archives of the Académie des Sciences for their assistance and for permission to publish photographs of Tiffereau's materials. I also thank Lilla Vekerdy for guiding this publication into print and express my gratitude to her and to the Smithsonian Libraries' Dibner Library of the History of Science and Technology for the invitation to deliver this lecture.

Foreword

"Alchemy." To most people, the word stirs up a vision of medieval conjurors experimenting with ways of turning base metals into gold. Nothing could be further from the truth, says Lawrence M. Principe, the 21st annual lecturer in the series hosted by the Dibner Library of the History of Science and Technology of the Smithsonian Libraries. In this highly readable essay expanded from his lecture, Dr. Principe traces the fascination of practitioners, theorists, and scientists with the idea of transmutation of base metals from earliest times, invigorated again in the 19th and early 20th centuries. He highlights "the successive making-up and breaking-up of alchemy and chemistry," since both were stimulated by "the desire to understand and to control matter and its transformations."

In his extensive research on alchemy and chemistry in France, Dr. Principe features the work of Cyprien-Théodore Tiffereau, whom he calls "the alchemist of the nineteenth-century." In the process, he recounts for the first time a discovery in the Archives of the Académie des Sciences that is a researcher's dream— opening a paper packet that had resided there, unopened, for over 160 years and discovering real treasure. You'll have to read the essay to find out what it was!

The subject of alchemy has fascinated historians of science for many years. In 1990, the Smithsonian Libraries published a 1988 lecture by Betty Jo Teeter Dobbs, a historian specializing in Isaac Newton occult studies, called *Alchemical Death and Resurrection: the Significance of Alchemy in the Age of Newton*. The Dibner Library holds at least 42 original published works and manuscripts of the Early Modern Period on alchemy, and there are many more facsimiles and reprints, plus secondary resources, scattered throughout the Libraries system. The Dibner Library's collection is one of the top, nationally known libraries to foster research in the history of science and technology from the Renaissance and Early Modern Period through the 19th century. The Library's 35,000 rare books and 2,000 manuscript groups cover mathematics, engineering, transportation, chemistry, physics, electricity, astronomy, and much besides. The Libraries is making many of these works available digitally; they can be accessed through http://library.si.edu/libraries/dibner-library-history-science-and-technology. We are grateful for a special project funded by Mark Dibner and the Argus Fund, which is making many of the valuable manuscripts widely available for the first time.

Bern Dibner (1897–1988), for whom the Dibner Library is named, donated his magnificent collection of rare books, manuscripts, portraits, and medals in the history of science and technology to the Smithsonian on the occasion of the U.S. Bicentennial in 1976. Trained in electrical engineering at the Polytechnic Institute of Brooklyn, Dibner built a considerable fortune through his company, the Burndy Corporation, and created the Burndy Library on its grounds. The balance of the Burndy Library's collections reside in the Huntington Library in Pasadena, California. Mr. Dibner's family has endowed and continued to support the Dibner Library, for which we remain ever grateful.

The Smithsonian Libraries' network of 21 specialized libraries are spread among the museums and research centers of the Smithsonian, from Washington, DC to the Republic of Panama to New York City, and to Edgewater and Suitland, Md. Open to the public and readily available to researchers, these libraries serve the Smithsonian's research and education enterprise and present their collections and expertise broadly through the Internet and through public lectures, exhibitions, and social media. For more information about the Smithsonian Libraries, visit library.si.edu.

Nancy E. Gwinn, Ph.D.
Director, Smithsonian Libraries
March 23, 2017

Until quite recently, history—or more accurately, those who write history—have not been terribly kind to alchemy. The prevailing image of alchemy was that of an archaic, perhaps superstitious or simply fraudulent, and at best senseless pursuit of making gold that contributed little to human knowledge. Historical figures such as Robert Boyle (1627–1691) were praised specifically for rejecting alchemy and replacing it with scientific methods of experimentation and observation that comprise the foundations of modern chemistry. In the nineteenth and early twentieth century, some radical reinterpretations of alchemy treated the subject more positively, but only by presenting it as a repository of grand esoteric or occult wisdom, or as a self-transformative spiritual or psychological endeavor, thereby simultaneously reinforcing its supposed essential distinctness from chemistry.

Scholarly efforts over the past generation have greatly modified such views. Alchemy is now recognized as a key part of the history of science.[1] Many alchemists, centuries before the emergence of a "modern" chemistry, developed and deployed sophisticated experimental techniques and based their work and ideas on logical theories of matter and its transformations derived from observation and practical experience. Alchemy, perhaps more than any other natural philosophical practice, instilled a culture of practical experimentation and observation into early modern Europe as a means of exploring nature. Figures like Boyle did not, in fact, reject traditional alchemy but instead learned from it, often continuing its long-standing aims such as the search for the philosophers' stone and the transmutation of base metals into gold.[2] Alchemy has now been revealed as something indistinguishable from chemistry in the early modern period; indeed, the two words were used largely interchangeably until about 1700. For this reason, William Newman and I proposed some years ago to use the word *chymistry* to refer to the undifferentiated premodern whole, thereby avoiding the modern connotations of the words *alchemy* and *chemistry*.[3]

While the word *chymistry* rightly signifies the original undifferentiated domain of alchemy and chemistry, it remains undeniable that during the early eighteenth century the two words acquired distinct meanings, connotations, and evaluations. Chemistry has since that time generally designated a scientific and useful enterprise practiced by learned and professionalized researchers, and a subject taught in schools and colleges. Alchemy on the other hand has generally signified a prescientific (at best "protoscientific," at worst "pseudoscientific") activity aimed predominantly at making gold that was practiced by, as George Sarton put it in his unsubtle fashion, "fools or knaves, or more often a combination of both in various proportions." Unlike chemistry, alchemy was excluded from the academy and widely scorned by learned people, save occasionally as a historical phenomenon.[4] Where, when, how, and why did this division come to be?

The ready answer to the question is that sometime around 1700, increasing scientific knowledge ruled out the possibility of transmutation. Like most ready answers to complex questions, this one is wrong. In the first place, it reads history backward from our current state of knowledge, which, it is assumed, *should* have been obvious to earlier generations. In the second, it presumes that transmutation had either no, or only weak, theoretical and observational foundations, and thus was easy to refute scientifically. But in fact transmutation was the logical consequence of a coherent theory of matter, in particular the idea that the metals are not elements (as we think of them) but rather *compounds* of the same two (or more) ingredients combined in different proportions and degrees of purity.[5] Hence, changing one metal into another should be, in principle, a straightforward matter of changing the relative proportions of their common ingredients. The only problem for chrysopoetic (gold-making) chymists was finding effective ways to carry out this transformation in practice. Their enterprise was nonetheless encouraged by the observation that such metallic transformations appeared to happen naturally underground, as witnessed by the well-known fact that lead ores almost always contain some silver, and silver ores almost always contain some gold, as if the less perfect, less well-concocted metals were slowly being transformed into better ones. Thus the quest for metallic transmutation was a well-grounded and reasonable pursuit based on the best scientific knowledge available at the time (fig. 1).

FIGURE 1

St. Thomas Aquinas points to the vapors of Sulphur (left) and Mercury (right) rising within the caverns in the earth where they combine to form the metals. A refiner is shown at the top of the mountain, smelting the resultant metal from its ore. St. Thomas was thought to be the author of an alchemical treatise titled *Aurora consurgens*; in fact, the text is not by him. From *Musaeum hermeticum* (Frankfurt, 1678); Roy G. Neville Chemical Historical Library, Chemical Heritage Foundation, Philadelphia. Originally published in Michael Maier, *Symbolum aureae mensae duodecim nationum* (Frankfurt, 1617).

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So why then did transmutation disappear from the customary interests and pursuits of chymists sometime in the early eighteenth century, at the same time that the word *alchemy* began to refer exclusively, and generally pejoratively, to this endeavor? I do not yet have a complete answer. The disappearance of transmutation was complex, and it occurred at different times in different places—in the 1720s in France and England, not until the 1740s or 1750s in Germany and Sweden—and resulted from a variety of local factors, many of which still need to be better explored.[6] For now, I must restrict myself predominantly to one locale, France, and an examination of its official scientific body, the Parisian Académie Royale des Sciences. And here, in what is arguably the most important locus for the development of chemistry during the eighteenth and early nineteenth century, some surprising discoveries can be made.

Alchemy at the Académie Royale des Sciences in the Eighteenth Century

BETWEEN THE ACADÉMIE'S FOUNDING IN 1666 AND THE YEAR 1700, the study of metallic transmutation was explicitly forbidden to the Académie's chymists on at least three separate occasions. Strikingly, *all* such restrictions came not from the chymists or other academicians basing themselves on scientific evidence but rather from *political* figures and administrators—first from Louis XIV's minister Jean-Baptiste Colbert (1619–1683) in 1666, then again in 1685 from Colbert's successor as governmental administrator of the Académie, François-Michel le Tellier, Marquis de Louvois (1641–1691), and finally from Louis XIV himself in the early 1690s. In the last case, the reason was given explicitly, namely that "the king does not wish it to be thought that his money is produced by goldmaking [*per aurifactionem*]"—a purely political and economic motivation, not a scientific one.[7] The Académie's perpetual secretary and official public spokesman, Bernard le Bovier de Fontenelle (1657–1757), continued and intensified the rejection of transmutation—particularly by categorizing it as simply futile and dishonest—through the first decades of the eighteenth century, and by the early 1720s he had begun using the words *chimie* and *alchimie* in very nearly their modern connotations.[8]

The Académie's chymists, however, acted the way all academics should toward administrators. They ignored them. The three most prominent chymists of the Académie's first fifty years, Samuel Cottereau Duclos (1598–1685), Wilhelm

Homberg (1653–1715), and Étienne-François Geoffroy (1672–1731), all worked openly and unapologetically on transmutation (figs. 2A and 2B).[9] No theory- or evidence-based refutation of transmutation ever appeared in the Académie's records. But when administrative decrees were replaced with bold rhetoric insisting on the purely *fraudulent and inherently deceptive* nature of claims of transmutation, transmutation finally disappeared from view within the Académie in the 1720s. This rhetoric was crafted primarily by Fontenelle, rather than by the chymists.[10] Geoffroy finally assisted in this endeavor in 1722 by giving a public description of frauds practiced by those claiming successful transmutation—although he never went so far as to say that transmutation was impossible, and borrowed his examples from a book published a hundred years earlier by an alchemical practitioner teaching fellow alchemists how to distinguish frauds from true transmutations.[11] In short, it was the linking of transmutation with socially unacceptable and immoral behavior that caused transmutation's retreat, not scientific argument or evidence. Thus alchemy (gold-making) and chemistry (a modern science) seem to have parted company definitively in France in the 1720s in an unfriendly divorce.

Over the past decade, however, rummaging through French archives has turned up many previously unknown private papers of the Académie's chemists. These tell a different story. The pursuit of alchemical transmutation did *not* in fact die out; it merely went more or less underground. Geoffroy, for example, continued his earlier interests in transmutation even after his 1722 paper. More strikingly, virtually *every* Académie chemist either pursued or defended transmutation until the 1770s—some fifty years later than previously thought.[12] Perhaps most significantly for our understanding of alchemy, these eighteenth-century academicians did *not* simultaneously cling to outdated ideas and explanations with a sort of scientific contrarianism

FIGURE 2A

The three successive chief chemists of the Académie Royale des Sciences—Samuel Cottereau Duclos (above; 1598–1685), Wilhelm Homberg (not pictured; 1653–1715), and Étienne-François Geoffroy (opposite; 1672–1731)—all worked seriously on metallic transmutation.

FIGURE 2B
Étienne-François Geoffroy (1672–1731)

Images courtesy of the History of Medicine Division at the National Library of Medicine. No portrait of Homberg has yet come to light.

or cognitive dissonance. Their "alchemy" was not a fossilized, superannuated, or reactionary endeavor. Instead they brought the newest chemical ideas and discoveries—many of which they helped to develop and explore—to bear on the old problem of transmutation. Far from stamping out the pursuit of transmutation, virtually every new chemical theory and discovery of the period was able to perpetuate it.

Guillaume-François Rouelle (1703–1770) provides one clear example. Rouelle (fig. 3) kept a secret laboratory in Paris where he worked on metallic transmutation, and told his students in the 1760s (of whom Antoine-Laurent Lavoisier and Voltaire were two) that "the generality of natural philosophers doubt the truth of alchemy, but they are not able to be judges of a subject that is entirely unknown to them; the most knowledgeable chemists . . . do not call it into doubt." In regard to the philosophers' stone's ability to turn base metals into gold, Rouelle asserted that "the most sensible and knowledgeable chemists have believed it; the ignorant ones and uneducated people have denied it."[13] This continuing pursuit of transmutation, however, was built not upon ideas from alchemy's heyday a century or more earlier, but rather upon Rouelle's contemporaneous chemical system that was based on the ideas of Georg Ernst Stahl. Rouelle explicitly used Stahlian principles to explain the compound nature of the metals and their transmutation. In explaining the stone's action, he asserted that "the Philosophers' Stone is nothing other than the result of a fermentation of gold with mercury—not common mercury but a special mercury oversaturated with phlogiston," thus invoking the material and theoretical centerpiece of mid-eighteenth-century Stahlian chemistry.[14] In a similar fashion, when the mathematician Pierre-Louis Maupertuis (1698–1759) wrote sarcastically of the stone and transmutation, the prominent chemist Pierre-Joseph Macquer (1718–1784) penned an unpublished refutation (sometime between 1753 and 1766) that not only drew upon

FIGURE 3
Guillaume-François Rouelle (1703–1770), chemist of the Académie Royale des Sciences, reportedly maintained a secret laboratory in Paris for performing transmutational experiments. Courtesy of the Bibliothèque de l'Académie Nationale de Médecine, Paris.

FIGURE 4

Pierre-Joseph Macquer (1718–1784) defended the intellectual and scientific merit of seeking the secret of transmuting metals. Image courtesy of the History of Medicine Division at the National Library of Medicine.

contemporaneous chemical theory to defend transmutation, but also used that theory to suggest new avenues of practical experimentation to achieve it. Success in transmutation would, he wrote, "resolve one of the most beautiful and most difficult problems in natural philosophy."[15] Thus alchemy, now specifically in the narrowed sense of metallic transmutation, persisted in the Académie—the highest echelon of the French scientific establishment—far longer than previously imagined. Most significantly, rather than being a weakly founded pursuit ready to be toppled by scientific scrutiny, alchemy instead proved sufficiently flexible and dynamic to incorporate the newest chemical theories and benefitted from them at the hands of the most talented and respected chemists of the day.

It is only around 1780 that such continuing interest and belief seem to have come to an end. Possibly this is due to the work of Antoine-Laurent Lavoisier (1743–1794), whose interests and discoveries redirected attention toward the production of compounds from "simple bodies"—particularly the nonmetals (although he included the metals under the rubric of "simple bodies") rather than toward the composition of metals.[16] John Dalton's (1766–1844) atomic theory dating from the first years of the nineteenth century undoubtedly also contributed to a serious dampening of alchemical hopes and practices since it was based on the notion of immutable atoms. Did this period—the turn of the nineteenth century—then mark the definitive divorce between alchemy and chemistry? From the vantage point of about 1800 it might well have seemed so, although further research on this question is needed. Yet, transmutational endeavors had already proven their resilience, and this apparent parting of the ways proved to be only temporary; in the mid-nineteenth century, alchemy and chemistry would flirt with reunion.

Cyprien-Théodore Tiffereau: "The Alchemist of the Nineteenth-Century"

CYPRIEN-THÉODORE TIFFEREAU (1819–1909), LATER TO BE called "the alchemist of the nineteenth century," was born in the small village of Sainte-Radégonde-des-Noyers in the département of Vendée, just north of La Rochelle near the Atlantic coast of France (fig. 5).[17] Trained as a chemist, by 1840 he was the chemical demonstrator (*préparateur en chimie*) at the École Professionelle in Nantes. Having become deeply interested in metals and their nature, the young Tiffereau decided to travel to Mexico to make observations of the occurrence and extraction of precious metal ores firsthand, and so in December 1842, he sailed from Bordeaux for Vera Cruz. His passport states his reason for going was "to practice his profession," which in Mexico was the making of daguerreotypes, an invention then but a few years old (fig. 6).[18] The daguerreotypes he produced are now believed to be lost, but when they were exhibited in Paris they were praised for their technical innovation and composition.[19] Tiffereau later recounted that he used the practice of photography not only for financial support, but primarily as a cover for his real purposes, which related to the observation of metals and mining. Indeed, many of his Mexican photographs documented mining operations and geological formations associated with metallic ores. He traveled through Mexico for more than three years, observing geological conditions and carrying out chemical experiments. Many related to photography, which, in those extremely early days of the

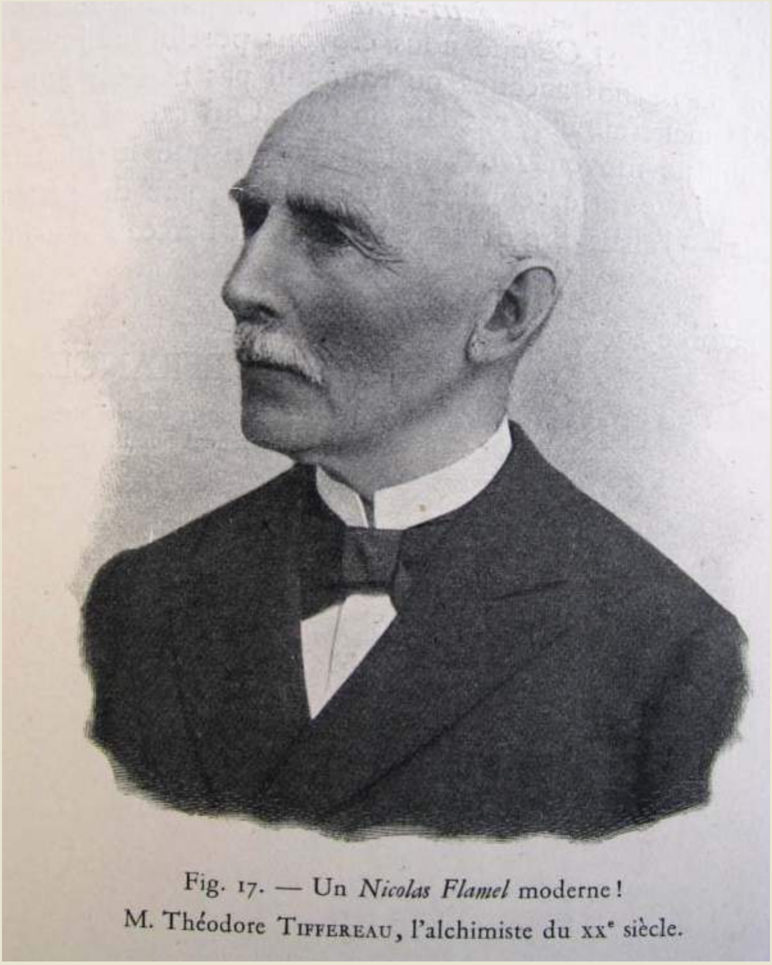
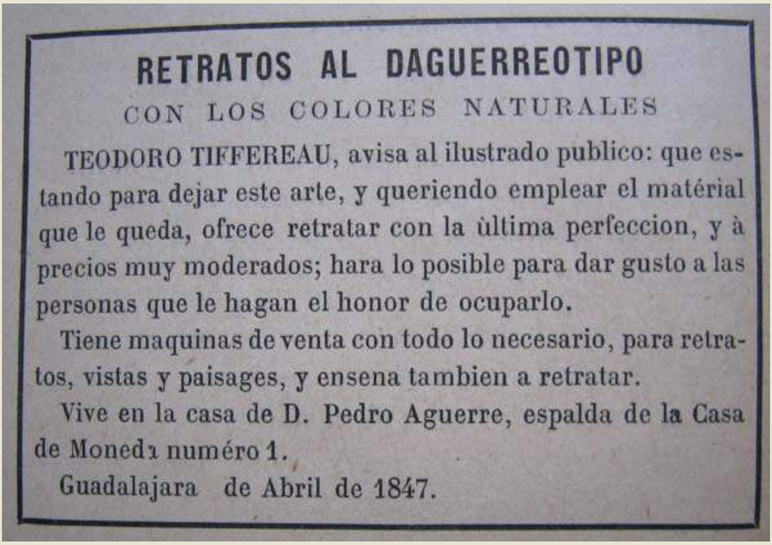


FIGURE 5

Cyprien-Théodore Tiffereau (1819–1909), ca. 1905, popularly called the "Alchemist of the Nineteenth-Century," has here been graduated to that of the twentieth. From Eugène Defrance,"Vieilles façades parisiennes," in *Les Parisiens de Paris* (Paris, 1905), vol. 5, 29. Author's collection.



RETRATOS AL DAGUERREOTIPO

CON LOS COLORES NATURALES

TEODORO TIFFEREAU, avisa al ilustrado publico: que estando para dejar este arte, y queriendo emplear el matérial que le queda, ofrece retratar con la ùltima perfeccion, y à precios muy moderados; hara lo posible para dar gusto a las personas que le hagan el honor de ocuparlo.

Tiene maquinas de venta con todo lo necesario, para retratos, vistas y paisages, y ensena tambien a retratar.

Vive en la casa de D. Pedro Aguerre, espalda de la Casa de Monedı numéro 1.

Guadalajara de Abril de 1847.

FIGURE 6

A typeset version of an advertisement Tiffereau used for his daguerreotyping business in Mexico. From *L'art de faire de l'or* (Paris, 1892). Author's collection.

technology, required substantial chemical manipulation, but many others were aimed at uncovering the nature of metals and exploring the possibility of their transmutation. After these years of traveling, observing, and experimenting, Tiffereau settled for a while in Guadalajara, where, in November 1846, he achieved a result that would inspire (or haunt) him for the rest of his days. After exposing nitric acid to strong sunlight for several days, he poured it over filings of a silver-copper alloy and left the mixture in the sun. A portion of the filings dissolved. He then boiled the mixture to dryness and added more acid. Upon repetitions of the process, the initially greenish-black residue grew increasingly lighter in color, and finally turned a brilliant metallic yellow. His tests (and those done by others later) showed the yellow material to be gold. He succeeded twice more—at Colima and again at Guadalajara in 1847—in the last case converting the entire quantity of the silver-copper alloy he employed into gold.

Tiffereau based his experiments upon his observations of metal deposits in Mexico, in particular the natural abundance of nitrates in their vicinity, and the testimony of Mexican miners that some gold nuggets they found were "ripe" and others "not yet." Owing to the turmoil caused by the Mexican-American War of 1846–48, Tiffereau was forced to close up shop and prepare to leave Mexico in late 1847. After much difficulty, he finally reached Tampico and sailed for France in early 1848, hoping to benefit himself and his native land with his discovery.[20] By the middle of 1850, he had settled in Paris on the rue de Vaugirard (no. 10), moving in early 1852 to the Rue du Théâtre on the southwestern outskirts of the metropole, where he would reside for more than fifty years.

At the end of 1850, Tiffereau initiated contact with the Académie des Sciences by sending them a *pacquet* (or *pli*) *cacheté*. Scientists and inventors used such *pacquets*—a short memoir in a sealed envelope—to mark priority before they were ready to make their discoveries public. A *pli* remained sealed until the depositor asked to have it either opened and published or returned.[21] Tiffereau sent a second *pli cacheté* in January 1851. This sealed memoir, titled "A New Point of View from which We ought to Envision the Metals," expresses his idea that the metals are actually compounds—not elements—produced from the union of "a hypothetical radical that we have not yet been able to isolate" with another substance, and are therefore potentially interconvertible. Tiffereau explicitly invokes Stahl's phlogiston theory as a precedent for thinking about metals as compounds, and notes that although phlogiston chemistry was undeniably refuted by Lavoisier's experiments, the subsequent assumption that metals are elemental "has been very harmful to the progress of this part of science by diverting the attentions of chemists away from this very important subject for so long." Although he links his "new view" to the "discussions that have recently been raised regarding the demonetization of gold," he does not mention his production of gold, alluding only to some unspecified and "not always fruitless" experiments that he has carried out.[22] The members of the Académie never saw this memoir, since Tiffereau never asked to have it opened.[23]

Tiffereau sent a much more significant *pli cacheté* to the Académie on May 31, 1852, one that he later revealed contained a sample of the gold he had made in Mexico. I examined this *pli* in June 2015, and found within the folds of the memoir a small paper packet (about 1 x 4 inches) still carefully closed with two green wax seals and labeled simply "Deposit of the gold (three centigrams)." When the *pli cacheté* was opened in November 1983, this smaller packet—enclosed within the sealed memoir—was left unopened (fig. 7).

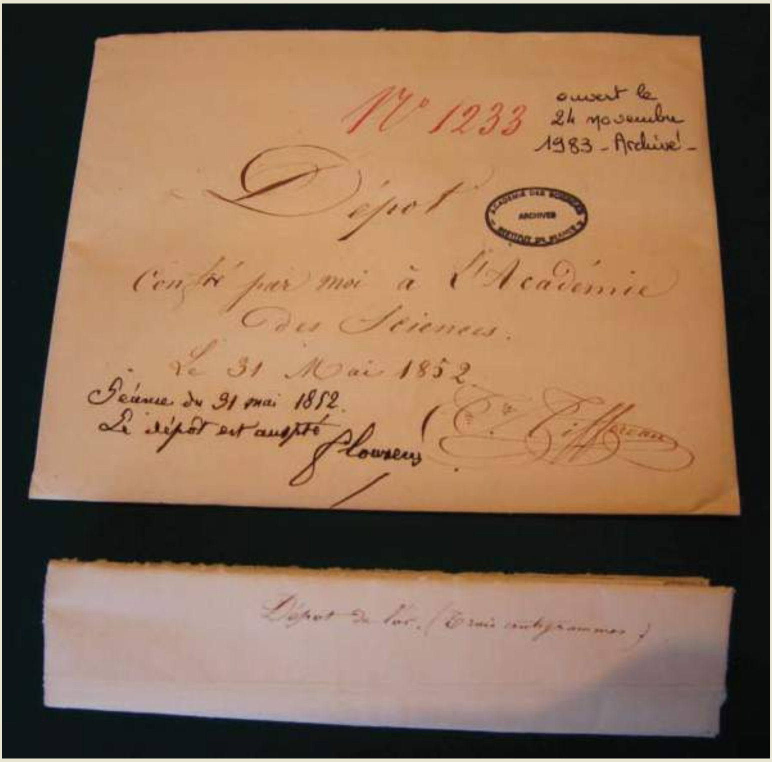


FIGURE 7

The *pli cacheté* that Tiffereau deposited at the Académie des Sciences in 1852; the packet containing the archived sample of his alchemical gold is shown below, photographed just after it was opened for the first time on June 22, 2015. Courtesy of the Archives de l'Académie des Sciences, Paris.

To my delight, the archivists permitted me to open it. Inside the sealed wrapper lay a tightly folded paper packet (fig. 8) bearing the inscription:

> Part of the artificial gold obtained for the first time in notable quantity (about three grams) in the city of Guadalajara (Mexico) the tenth of November eighteen-hundred-forty-six (10 November 1846): by me, Cyprien Théodore Tiffereau, of Puyravault, Vendée.

Inside this second packet lay yet another, smaller, more tightly folded paper packet, which finally, when gently unfolded, revealed the glistening grains of Tiffereau's alchemical gold (fig. 9).[24] While other older samples of alchemical metals do exist in European repositories, Tiffereau's is undoubtedly the best attested and preserved sample, having been sealed up by the transmuter himself, guarded without interruption in the archives of the Académie des Sciences for over a century and a half, and opened only in 2015.[25]

The letter accompanying the gold sample notes the exact time, place, and witnesses of the transmutation. It also states that only Tiffereau knows the exact procedure used, although he explained it directly to none other than the president of France on June 23, 1851. The French president is undoubtedly the unnamed "homme haut placé" mentioned elsewhere who procured funds for Tiffereau to continue his research in 1851.[26] A public announcement occurred only in June 1853, when Tiffereau had an eight-page pamphlet printed with the lengthy descriptive title *The Metals are Not Simple Bodies, but Indeed Compound Bodies. The Artificial Production of Precious Metals is Possible: It is an Accomplished Fact* and sent a copy to the Académie asking for their opinion. Unfortunately, Tiffereau unwittingly ran afoul of the Académie's strict policy of not commenting on published materials. In any event, his thin pamphlet contained little to comment upon; it gave no experimental details, only an assertion that transmutation had taken place.[27]

This failure might well have been the end of it, but a remarkable thing happened next: the Académie des Sciences invited Tiffereau to present his work directly to one of their meetings. Thus on October 17, 1853, Tiffereau appeared before the official scientific body of France and read a paper about his successful transmutation of silver into gold. He showed the academicians a sample of his transmuted metal in the form of agglomerated filings, just as it was produced in his experiments, and another sample melted down into a small ingot. The Académie responded by forming an official commission of the three most distinguished chemists in France—Louis-Jacques Thénard (1777–1857), Michel-Eugene Chevreul (1786–1889), and Jean-Baptiste Dumas (1800–1884)—to examine Tiffereau's results (figs. 10–12).[28] This response should shock us; at an earlier period of the Académie, Tiffereau would never have gotten a foot in the door, much less an invitation to address the academicians directly and a commission formed to examine his claims. What had changed?

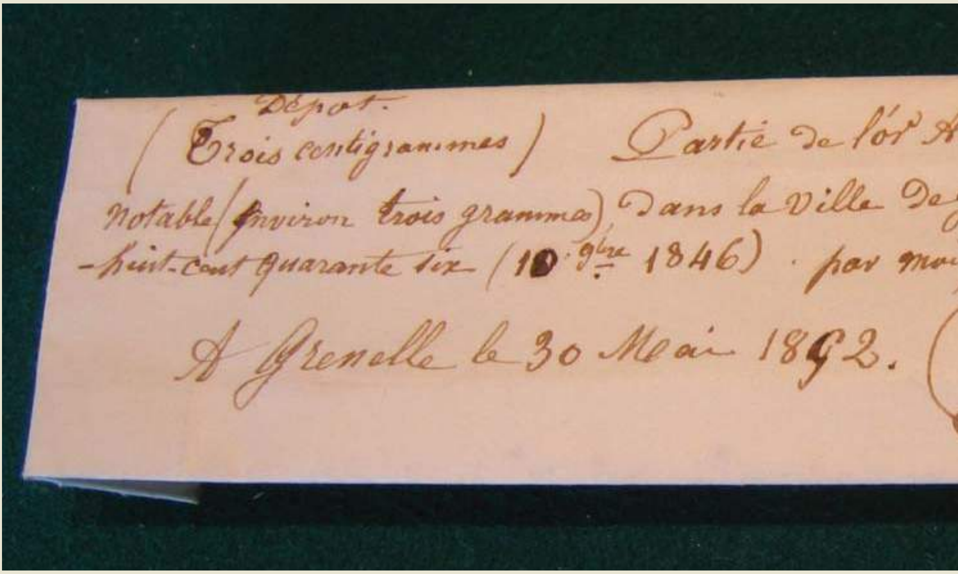


FIGURE 8

The inner wrapper found within the small sealed packet shown at the bottom of figure 7, inscribed with details of the transmutation and Tiffereau's signature. Courtesy of the Archives de l'Académie des Sciences, Paris.

FIGURE 9

Tiffereau's alchemical gold (opposite), 30 mg, as found inside the final wrapper originally enclosed within the packet shown in figure 8. The largest metal flakes are about the size of the head of a pin. Courtesy of the Archives de l'Académie des Sciences, Paris.

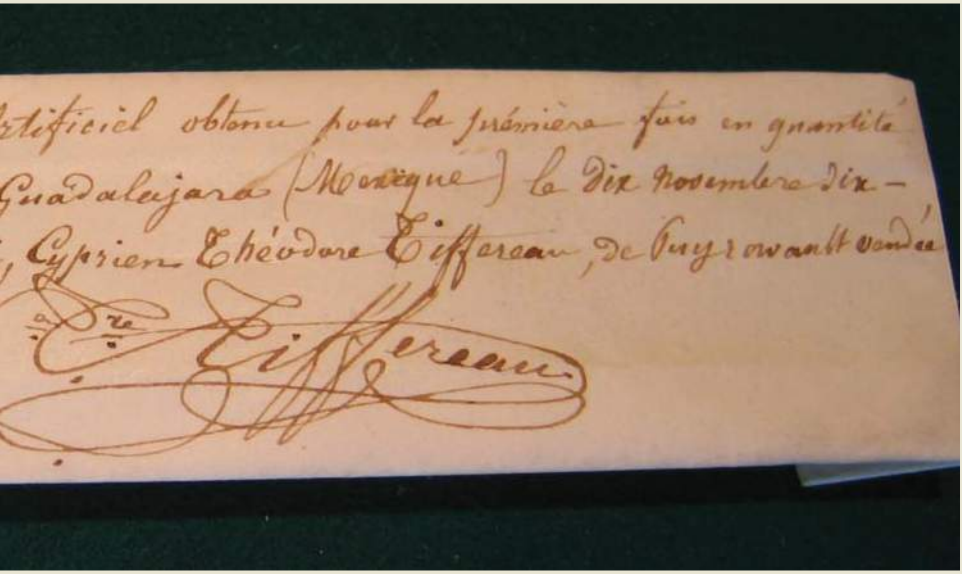

rtificiel obtenu pour la première fois en quantité
Guadalajara (Mexique) le dix novembre dix-
, Cyprien Théodore Tiffereau, de Puyrouvault Vendée
Tiffereau

FIGURE 10

Jean-Baptiste Dumas (1800–1884). Courtesy of Smithsonian Libraries, Galaxy of Images, SIL14-D5-08a.

FIGURE 11
Michel-Eugène Chevreul (1786–1889). Courtesy of Smithsonian Libraries, Galaxy of Images, SIL14-C3-10a.

FIGURE 12
Louis-Jacques Thénard (1777–1857). Courtesy of the Collections de la Bibliothèque Interuniversitaire de Santé, Medécine, Paris.

Chemistry and Alchemy Together Again?

BY THE MID-NINETEENTH CENTURY, CHEMISTRY'S NOTION OF immutable elements had been shaken by several new and puzzling discoveries.[29] The first followed upon the measurement of atomic weights—that is, the relative weight of an atom of one element compared to that of another. A remarkable observation had been made: the atomic weights of nearly every known element, about fifty at that time, turned out to be *integral* multiples of the weight of the lightest element, hydrogen. Carbon weighed exactly six hydrogens—oxygen eight, sulphur sixteen.[30] There was no reason to expect this striking regularity; on the contrary, it had to be explained. This strange outcome led William Prout (1785–1850) to propose in 1817 that all the known "elements" were actually condensations of hydrogen, such that hydrogen was the unique material building block of everything.[31] In so doing, Prout revived the notion of the "unity of matter," a monist position (i.e., that there is only one fundamental type of matter) that has had its ups and downs since Thales of Miletus opined that "everything is water" in the sixth century BC. Chemists lined up on opposite sides of Prout's hypothesis. The Swedish chemist Jakob Berzelius (1779–1848) denied the possibility, and spent years refining the measurement of atomic weights, showing that some elements—notably chlorine and copper—did not have the integral atomic weights necessary to fit the hypothesis.

Of equal stature on the other side of the argument was none other than Jean-Baptiste Dumas, the youngest member of the commission appointed to study Tiffereau's memoir. Throughout the 1840s, Dumas measured and remeasured atomic weights to confirm or refine Prout's hypothesis.

A second discovery that called the elemental nature of metals into question also involved Dumas: isomerism. It had been discovered that some organic compounds that were clearly different substances with very different properties were nevertheless composed of the same atoms. Isomerism implied that some unsuspected internal arrangement of their common components determined the properties of these substances, rather than merely the kind and number of atoms they contained. A similar dependence of properties on internal structure rather than on composition appeared in the phenomenon of polymorphism—the fact that the same substance could exist in multiple forms, each with its own unique properties. Finally, Dumas also famously extended the new "radical theory" of organic chemistry to the metals. Chemists had observed that some clusters of atoms generally survived intact through multiple reactions, as if they were especially tightly joined as a unit. One such cluster, or "radical," was ammonium, which had been shown by analysis to contain one nitrogen and four hydrogen atoms. Yet despite its demonstrably compound nature, ammonium not only transferred intact from one compound to another, but also easily and readily substituted in the place of a metal in salts—a type of substitution reaction that had long been recognized to occur among various metals, but *only* among metals. The implication was that ammonium was itself a metal composed of two nonmetals. Hence, there were ongoing attempts in midcentury, some seemingly successful, to isolate ammonium and certain other radicals as new and compound metals.[32]

These three observations—integral atomic weights, isomerism/polymorphism, and radicals—breathed new life into the old question of transmutation. If Prout was right, adding or subtracting hydrogens could interconvert all the substances thought to be elemental, including the metals. Isomerism meant that the distinct and varied properties of the known metals might not be evidence of distinct elemental natures; the various metals might instead simply be isomers (or polymorphs) composed of the same ingredients, but with unknown structural differences that produced their divergent properties. Radical theory suggested that metals could be compounds after all, just so tightly bound that the means of decomposing them had simply not yet been found. Dumas held forth at length at the 1851 meeting of the British Association for the Advancement of Science on the possibility that the metals were in fact radicals, and therefore might be interconvertible. On the same occasion, Michael Faraday (1791–1867), reacting to the increasing number of new metals then being discovered, urged chemists to look for ways to interconvert—that is, *transmute*—them one into another.[33] In light of these new ideas, Louis Figuier, when writing his 1854 history of alchemy, remarked that "chemistry today, after fifty years of having considered the elemental character of metals an unassailable principle, now leans towards abandoning it."[34] Strikingly, chemistry had returned in the middle of the nineteenth century to a view of the metals as compound bodies that was not very far from that of medieval alchemy.

This new rapprochement between the long-sundered topics of alchemy and chemistry was explicitly acknowledged by chemists of the day. Dumas noted that questions about the possible isomerism and polymorphism of substances considered elemental "touch upon the issue of the transmutation of the metals. Answered affirmatively, they would give chances of success to the search for the philosophers' stone."[35] Another chemist, Alexandre-Edouard Baudrimont (1806–1880), linked alchemical transmutation explicitly to Prout's hypothesis, and noted that while alchemists could not have made gold by combining various materials together, they might have done so by "impressing upon the nature of the bodies, under the influence of a catalytic agent, a modification akin to that which gives rise to isomerism." He likewise argued that "radicals are entirely comparable to metals . . . leading one to suspect that the metals are compounds." Nothing in modern chemistry, he went on, refutes alchemical claims, "on the contrary, as chemistry develops it seems to bring forth new facts on the order of those that formed the basis of alchemical theories."[36] Baudrimont in fact began collecting and studying alchemical documents, and some of his notes on them survive, including his endeavors to decipher allegorical seventeenth-century texts about making the philosophers' stone into workable chemical terms and practices.[37] Such speculations on the compound nature of metals were by no means restricted to French sources; they are found everywhere in midcentury chemical textbooks. For example, in discussing isomerism, a popular Anglo-American textbook of the period cites the simple mathematical relationships between the atomic weights of various metals, and asks, "May it not be possible that science shall hereafter find the metals so connected to be truly isomeric?"[38] Given the controverted state of these questions in 1853, Tiffereau's memoir titled "The Metals are Compound Bodies"—a notion that many chemists, notably Jean-Baptiste Dumas, were already seriously considering—and claiming experimental success in transforming one metal into another was sure to provoke interest, regardless of connections to an officially discredited alchemy.

Significantly, although Tiffereau linked his result to earlier alchemists, he never resorted to their theories. Instead, like Rouelle and Macquer before him, he made use of the latest chemical ideas and language to explain his results and to guide his research. He refers to isomerism and polymorphism much as Dumas did in his chemistry lectures, and to "the unknown radical that constitutes the metals. Everything," he says, "leads me to believe that this radical is hydrogen"—a clear link to Prout's hypothesis. This radical, he suggests, is modified by the amount of oxygen bound to it, a process linked to nitrogen acting as a catalyst or "ferment." The more oxygen, he hypothesizes, the more inert and denser the resultant metal. Hence, he proposes that his observed conversion of copper to silver and thence to gold involves the sequential addition of oxygen to the metal radical using nitric acid, a known oxidizer.[39] These are obviously all terms and entities that early modern alchemists never dreamed of.

Unfortunately, the Académie did not give Tiffereau the response he desired. Three weeks later, on November 7, 1853, the commission declined to make a report because Tiffereau still gave insufficient experimental details. Tiffereau asked for another audience, but was apparently told just to send more information, which he did over the succeeding months in the form of four more memoirs.[40] But before revealing anything further, he took a remarkable step: he obtained from the French government a fifteen-year *brevet d'invention*, something akin to a patent, "for the transmutation of metals one into another."[41] Clearly he wanted to safeguard his interests in the future manufacture of gold before revealing more details about the process. The *brevet* witnesses that Tiffereau was neither a joker nor a fraud, but was convinced of the reality of his process, and fully expected to put it into industrial production.[42]

Tiffereau's third memoir, presented in May 1854, describes openly the protocols of his Mexican experiments, and answers a nagging question: If he can make gold, why involve the Académie? The third memoir reports that although the process worked three times in Mexico, it now fails to work in France. Tiffereau's memoirs are thus appeals for the Académie to help him solve the problem. Tiffereau explained how upon his return to France he expected to put his gold-making quickly into production, but was confounded by the unexpected failure of his process.

> Upon arriving in Paris, I thought to follow the best route by devoting my finances to perfecting my discovery. I said to myself: When I no longer have the means of pursuing it with my own resources, I will share my work with the Académie, they will doubtless hasten to verify the facts. That by itself will suffice to make me find the means to pursue my researches. Today, circumstances *reduce me to making photographed portraits to survive*, while awaiting the report of the Commission designated to pronounce upon my discovery.[43]

To help make ends meet, Tiffereau manufactured and sold several inventions, such as new timekeeping devices and an improved gasometer for storing and measuring gases. He obtained *brevets d'invention* for these devices starting in 1850, and his first contacts with the Académie relate not to transmutation, but rather to these new inventions. He sent an improved *sablier* (hourglass) in July 1850 that was examined and well received; these *sabliers*, designed for timing photographic exposures, came to be widely known as *sabliers Tiffereau* and helped finance Tiffereau's experiments for many years. Tiffereau also sent the Académie a mechanism for regulating the flow rate of liquids from reservoirs.[44] He was clearly an adroit and clever mechanic, as well as a chemist and photographer.

Between his return to France in 1848 and his appearance at the Académie in 1853, Tiffereau struggled to identify the problem in replicating his Mexican results. While confessing that he did not understand the causes, he suspected that the answer lay in "atmospheric factors" present in Mexico but absent in France. He cited possible differences in the electrical, magnetic, and meteorological environment, but focused in particular on the intense sunlight of Mexico to which he exposed his transmutational experiments. He showed special interest in the chemical effects of sunlight, observing for example the "sensitization" of iodized collodion photographic plates through preexposure to sunlight.[45] Some of his notable success with daguerreotyping in Mexico undoubtedly came from similar experiments, and certainly his photography and alchemy were connected: daguerreotypes are produced by the action of sunlight on silver, and his gold-making occurred using the action of sunlight (and nitric acid) on a silver alloy. He remarked, ostensibly in the context of photography, but surely also with reference to transmutation, that he had studied "for a long time the effects of this agent [sunlight] on bodies, and principally upon silver."[46] In Paris, he intensified his study of the chemical effects of sunlight, apparently to the detriment of his eyesight, and in 1854 sent the Académie a paper about the prolonged action of sunlight on a mixture of nitric acid and carbon disulfide. The Académie deemed this paper worthy of publication in their journal (Dumas was one of the *commissaires* that approved its publication), and it was shortly thereafter translated into German and republished in the *Journal für praktische Chemie*.[47]

Tiffereau also tried the effects of electricity and magnetic fields on his experiments; later visitors to his laboratory mention his electrical and magnetic apparatus.

While sending the Académie his successive memoirs about transmutations, he also had a related process examined (with ambiguous results) by the assayer of the Imperial Mint.[48] But he waited in vain for an official response—the "words of encouragement" and suggestions he sought—from the Académie. Losing patience with the delay, he took his case to the public, collecting his six memoirs into a slim volume, and very publicly asking the Académie's assistance. Here Tiffereau's experiments were clearly linked with historical alchemy, for his memoirs were followed by an essay on "Paracelsus and the alchemy of the sixteenth century" written by Adolph Franck (1810–1893), philosopher at the Sorbonne and member of the Académie des Sciences Morales et Politiques.[49] Tiffereau sent a copy to the Académie, again requesting a report, but received the reply that "they now cannot do so, according to the customs of the Académie concerning printed works published in France."[50] Tiffereau sent a fresh memoir later in 1855, and yet another—on the "artificial production of gold from the oxidation of sulfides" in 1858. Both were sent to a commission for examination, but no reports were issued.[51] Disappointed, Tiffereau fell silent at the end of 1858.

Was Tiffereau's engagement with the Académie fruitless? He was certainly disappointed by the lack of a formal report. But he may have stirred up less obvious responses. Beginning in 1857, Dumas presented a lengthy series of memoirs addressing the problem of elements, revisiting and revising Prout's hypothesis, and stating more strongly his conviction that the metals are actually compound bodies. Dumas was attacked by a fellow academician, the physicist

César-Mansuète Despretz, whose own paper actually cites Tiffereau's work alongside Dumas's.[52] It may be significant that Despretz's negative results in trying to decompose metals come from experiments similar to some that Tiffereau performed. Tiffereau, who clearly kept current with the scientific literature, noticed the critique and sent a letter stating that he found Despretz unconvincing—the Académie published a summary in their proceedings:

> Mr. Tiffereau, on the occasion of a recent work by Mr. Despretz about certain metals and gases, remarks that he has been led by his own researches to conclusions different from those that the learned academician deduces from his studies, and declares that he continues to consider the metals to be compound bodies.[53]

Dumas agreed. He sharply criticized Despretz, calling his experiments "useless and inadequate" for demonstrating the elemental nature of metals, and pointing out that his methods were so trivial that had such means been able to decompose the metals, chemists would have accomplished it long ago: "such experiments add nothing to our knowledge and teach nothing to anyone." Chevreul—another member of the commission appointed to examine Tiffereau's results—chimed in to support Dumas. Strikingly, Chevreul also promised to submit a further memoir reevaluating specifically the possibility of transmuting metals, perhaps a projected elaboration of something he had begun to draft for Tiffereau's commission.[54] Unfortunately, this follow-up paper never appeared. Thus not only did Tiffereau's claims connect smoothly with contemporaneous debates within chemistry, but they also seem to have stirred up fresh arguments within the Académie.

Tiffereau's influence may well have gone even further. In the same year, 1857, the chemist John William Draper (1811–1882), later to become the first president of the American Chemical Society, published a paper in which he "ingenuously confess[ed] that I have made several attempts at the transmutation of metals." Strikingly, Draper—both a chemist and a pioneer in photography like Tiffereau—conducted these experiments "by the aid of solar light," which Tiffereau had specified as the key factor in his own process. Draper's experiments also used silver and nitric acid, the same reagents Tiffereau used. Draper, who also doubted the elemental nature of metals, claimed that his experiments converted silver into a yellowish metal insoluble in nitric acid—that is, something with properties akin to those of gold.[55] While it is possible that Draper might have come up with the experiment independently, it also seems plausible, especially given the timing, that he was inspired by Tiffereau's publications. Draper had earlier theorized the existence of "chemical rays" carried with sunlight that caused chemical transformations. These chemical rays, or *tithonicity* in his terminology, were linked to the brilliance and wavelength of the light. Hence, he argued that the failure to replicate some of his results in England was due to the weakness of English sunlight relative to that of Virginia—a claim strikingly similar to, and perhaps a source for, Tiffereau's own suggestion about the difference between Mexican and French sunlight for his transmutations.[56] Whether or not the two are directly connected, Draper's work further witnesses the midcentury rapprochement between official chemistry and transmutational alchemy.

After his disappointment with the Académie, Tiffereau retired to private life—marrying, raising a family, and building up a very successful photography business (fig. 13).[57] No more was heard of his transmutations. He continued

FIGURE 13

Tiffereau's photography shop at 130 rue du Théâtre, Paris, ca. 1870, as shown on the reverse of one of his photographs. Author's collection.

selling and exhibiting his *sabliers* and *horologe hydraulique*, and won a bronze medal in 1860 from the Société d'Encouragement pour l'Industrie Nationale for his "very ingenious" gasometer.[58] In 1884, he sold his photography business to an assistant and reemerged to call attention once again to his transmutation and its benefits for France.[59] He wrote first to the chemist Marcellin Berthelot in March 1885, provoked by what the latter wrote about alchemy in his *Origines de l'alchimie*, published earlier that year. Berthelot emphatically asserted that no metallic transmutation had ever been accomplished, that such transformations were impossible, and remarked "I don't know if [the dream of the alchemists] still persists among certain minds."[60] These were words sure to elicit a response from Tiffereau. Unfortunately, Tiffereau's letter elicited no reply from Berthelot. Undaunted, Tiffereau wrote to the French Commission of the Budget and to the Senate in 1887 and to the Deputies of the Seine in 1888. He joined the Société chimique de France, sent them his memoirs, presented new experiments, and wrote to the Académie des Sciences again in June 1888 with a new *pli cacheté*. Again, he received no response, save from one deputy who inquired about the sum necessary to pursue his experiments; Tiffereau's estimate of fifty to one hundred thousand francs seems not to have garnered a response.[61]

Now, in the 1880s, the intellectual landscape had changed in several ways. Within chemistry, the question of the elemental nature of the metals, although not resolved definitively, was no longer a lively subject of debate. A lengthy section of Berthelot's *Origines* dismantled the relevance of the chemical phenomena—isomerism/polymorphism, atomic weights, and radical theory—that had earlier in the century encouraged the reunion of chemistry with transmutational alchemy, effectively closing the door on questions about the compound nature of metals.[62] The scientific establishment had meanwhile become more closed to nonprofessionals; while the Académie's *Comptes rendus* of the 1850s record numerous contributions from amateurs, such contributions decline rapidly in the succeeding years. Alchemy had also changed. The occultist revival and its radical reinterpretation of alchemy, which had spread to France from England starting in the 1860s, had now made the subject more distasteful, even embarrassing, to scientists. Many occultists set themselves in explicit opposition to the scientific establishment, decrying chemistry as mechanical and lifeless and chemists as blind, and asserting that chemistry and alchemy had little or nothing in common, with alchemy as the superior discipline.[63] In the context of the occult revival, it was now alchemy's turn to spurn chemistry.

Tiffereau held himself aloof from the occultist alchemical revival, although he benefitted from the new public popularity of alchemy by having his memoirs republished by the occultist publisher Chacornac as part of a series on the "sciences hermétiques," edited by Jules Lermina.[64] Instead, Tiffereau began holding public lectures in 1888, presenting over a dozen of these during the next fifteen years, and publishing pamphlet after pamphlet (fig. 14).[65] In August 1889, he presented a paper to the Association Française pour l'Avancement des Sciences with a formal analysis of his gold by an independent assayer, and also a sample of the gold. At the same time, he put some of his transmuted gold on exhibit at the Grande Exposition of 1889 (the event for which the Eiffel Tower was built).[66] The popular press interviewed him—mostly sympathetically—many times, while the scientific establishment remained silent.[67]

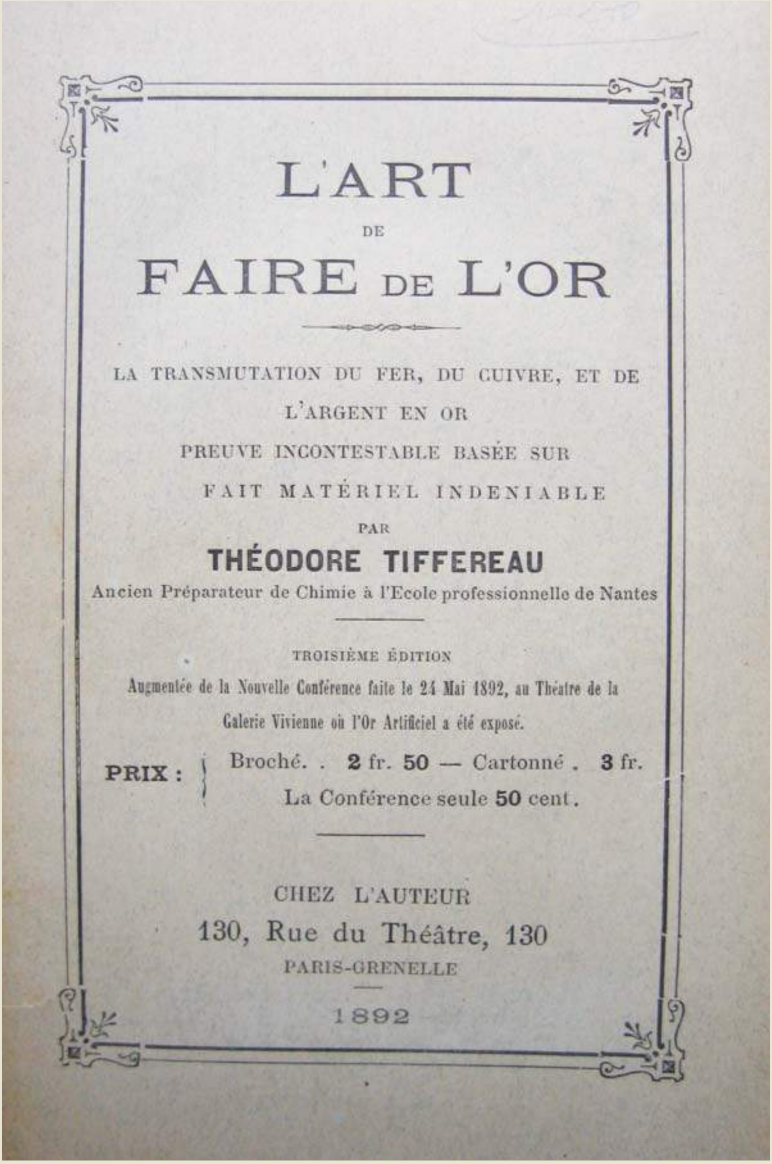

L'ART DE FAIRE DE L'OR
LA TRANSMUTATION DU FER, DU CUIVRE, ET DE L'ARGENT EN OR
PREUVE INCONTESTABLE BASÉE SUR FAIT MATÉRIEL INDENIABLE
PAR
THÉODORE TIFFEREAU
Ancien Préparateur de Chimie à l'Ecole professionnelle de Nantes
TROISIÈME ÉDITION
Augmentée de la Nouvelle Conférence faite le 24 Mai 1892, au Théatre de la Galerie Vivienne où l'Or Artificiel a été exposé.
PRIX : Broché. . 2 fr. 50 — Cartonné . 3 fr.
La Conférence seule 50 cent.
CHEZ L'AUTEUR
130, Rue du Théâtre, 130
PARIS-GRENELLE
1892

FIGURE 14
The Art of Making Gold (Paris, 1892), one of Tiffereau's numerous pamphlets presenting his ideas to the public and documenting his public lectures. Author's collection.

In 1891, Tiffereau triumphantly announced "a second discovery that explains my first."[68] Having read recent scientific papers in microbiology, particularly the work of Sergei Vinogradskii (1856–1953) on the discovery and isolation of nitrogen-fixing bacteria, Tiffereau concluded that his Mexican success depended on the action of microbes.[69] These microbes, he surmised, were present naturally in metallic deposits, where they caused the slow conversion in the ground of lesser metals into gold (this is why, Tiffereau argued, gold is found predominantly near the surface, where the original silver from which it was produced would be more subject to microbial action and the necessary light and water). Indeed, Tiffereau concluded that his best success, in Guadalajara in 1847, was facilitated by the fact that his lodgings were adjacent to the Mint:

> Having exposed my silver filings and nitric acid [to sunlight] on the terrace of my apartment, one may suppose that the microbes on the ingots of gold and silver brought daily to the Mint chose to lodge in my flasks and efficiently aided the final transmutation.[70]

These "mineral microbes," he theorized, were able to decompose nitric acid into its constituents, including oxygen, which in its "nascent state" could combine with lesser metals like copper and silver to produce gold. The idea of a special reactivity of materials in their "nascent state" was itself drawn from contemporaneous chemistry, particularly electrochemistry.[71] The failure of Tiffereau's process in Paris was thus ascribable to the scarcity or absence of those particular microbes in France. Once again, a transmuter turned to the latest scientific ideas to assist in and explain his work—indeed, sections of his later pamphlets read like reviews of the scientific literature on microbiology, mineralogy, and chemistry. Following the methods of Vinogradskii and others, Tiffereau now tried to isolate, culture, and identify the responsible microbes. He collected rain and gutter water, hoping that a few of the correct "mineral microbes" would be present, and slowly increased the acidity of the medium to kill off the weaker microbes and isolate those able to survive in nitric acid.[72] (Ironically enough, in the 1980s it was discovered that a particular bacterium, *Bacillus cereus*, thrives specifically around gold veins, such that biological soil assays can be used to locate gold deposits.)[73] Although Tiffereau became increasingly annoyed by the lack of response from the scientific establishment—he expressed surprise and even bafflement at this continuing silence—he kept trying to engage with it. From 1896 through 1906 he sent the Académie des Sciences accounts of new experiments that seemed to produce carbon from aluminum (more evidence that the metals are compounds), further *pli cachetés*, and several pamphlets.[74]

Exasperated by the silence of official scientists and government officials, only toward the end of the 1890s did Tiffereau turn to the new audience of "neo-alchemists" who had emerged from alchemy's late nineteenth-century occult revival. He published pieces in their new journal *L'Hyperchimie*, established in 1896 by the Société Alchimique de France, a creation of François Jollivet-Castelot (1874–1937), with whom he began corresponding.[75] He also corresponded with Stephen Emmens, an American engineer who claimed to sell the United States Mint gold that had been made by pressure treating silver. Emmens's process was undoubtedly informed in part by Tiffereau's earlier work, insofar as it employed specifically Mexican silver and used nitric acid.[76] While Tiffereau was flattered by Emmens's support, and pleased that Emmens's claims brought fresh attention to his work, their direct interaction was short-lived, no doubt because the brash, irascible American entrepreneur was a poor match to the polite, soft-spoken Frenchman. Tiffereau also soon withdrew from the company of Jollivet-Castelot's circle (perhaps owing to their more grandiose and spiritualist claims about the nature of the universe and reality, not to mention a few quite fantastic notions that had no counterparts in Tiffereau's ideas), and then disappeared. Strangely, at about the same time, a newspaper article reported the sensational news in 1903 that "it is affirmed to us that there was seized from his home at Grenelle a notable quantity of gold, of a value that surpassed 300,000 francs."[77] A Parisian bookseller later recalled that Tiffereau simply "disappeared," noting that he saw him for the last time "a little before the war," yet "his health remained good despite his age, which made some of his friends say that he had found the elixir of long life."[78]

Newly discovered documents, however, indicate that Tiffereau left Paris at the end of 1905, and died in 1909.[79] The place of his death remains unknown; although it is reasonable to assume that he returned in his last years to his native Vendée, extensive investigations there have not found any traces of him.[80] Other items reveal that he kept mining the latest scientific literature for new insights and kept experimenting. His 1905 *La science en face de la transmutation des métaux* reviews new scientific developments regarding catalysis and catalyst poisons, colloidal states, and even the then newly demonstrated transmutation of radium into helium, and links these scientific advances to his Mexican results. Strikingly, the copy of his last pamphlet (1906) held in the library of the Conservatoire des Arts et Métiers in Paris preserves among its pages a few sheets of notes in Tiffereau's autograph.[81] One lists new scientific articles to consult, some dealing with radium, some dealing with the action of sunlight on precious metals. Other sheets, dated October 15 and 25, 1905, record continuing experiments to transmute silver. Interestingly, one employs "poudre de Californie"; perhaps Tiffereau obtained soil or ore from auriferous deposits in California hoping these would introduce the needed microbes or catalysts to effect transmutation. Clearly, Tiffereau never gave up hope of succeeding again with his transmutations, and never stopped updating his thinking with the latest scientific findings. His story provides the most striking and long-lived endeavor to reunite modern chemistry with transmutational alchemy by applying the latest scientific results to the ancient problem.

A New Century, a New Rapprochement

TIFFEREAU'S LAST YEARS COINCIDED WITH A NEW RAPPROCHEMENT between alchemy and chemistry, one that temporarily repaired the breach opened by the occultists' reinterpretation of alchemy and their (often strident) criticism of contemporaneous science. Members on both sides of the renewed alchemy-chemistry divide of the late nineteenth century were brought back together by a fresh discovery in chemistry, namely that of radioactive decay—a topic Tiffereau had himself begun to consider. On the alchemical side, a number of fin-de-siècle "neo-alchemists" who had emerged in the context of nineteenth-century occultist interpretations of alchemy—such as François Jollivet-Castelot—were energized by these discoveries, and began pursuing new laboratory procedures that carried them away (at least for a time) from more occultist, less experimentalist positions. Jollivet-Castelot envisioned the emergence of a new "hyperchemistry," a fruitful merger of nineteenth-century spiritual alchemy with traditional practical alchemy and modern chemistry, a new field that "ought to replace chemistry." Indeed, he saw his goal as "adapting the systems of the ancient masters of alchemy to contemporary theories," which he expected would "confirm the truths perceived" by them.[82]

On the chemical side, the chemists themselves embarked on a new and more fervent courtship with alchemy.[83] In 1901, Frederick Soddy (1877–1956) and Ernest Rutherford (1871–1937), both of whom would win Nobel Prizes in chemistry, discovered that the metal thorium decays spontaneously into other elements, producing an "emanation" (recognized by Soddy as a noble gas, and later named radon) that then transforms into yet other elements (figs. 15A and 15B). When Soddy first realized what was happening, he exclaimed to his collaborator that "this is transmutation!," causing Rutherford to snap back, "for Mike's sake, Soddy, don't call it transmutation. They'll have our heads off as alchemists. You know what they are!"[84] These experimental results changed not only Soddy's view of chemical elements, but also his evaluation of alchemists and alchemy. In 1900, just prior to this discovery, when Soddy taught a history of chemistry class at McGill, he stated that "it is impossible to regard the alchemistic period as a part of the normal development of chemistry," referring to alchemy as a "mental aberration." Yet in the aftermath of his experiments with thorium, he reversed his opinion, telling his next class that "alchemy must be regarded as the true beginning of the science of chemistry," and stated that transmutation "is, as it has always been, the real goal of the chemist."[85] New experimental results had brought alchemy and chemistry back together again.

Despite Rutherford's initial worries, over the next two decades chemists embraced the word *transmutation* and its implications as many of them embarked on a fervent pursuit to control transmutation. From 1901 through the 1920s, many chemists, encouraged by the natural transmutation observed during radioactive decay, endeavored to cause artificial transmutations by exposing an array of elements to the alpha-particle emissions of radium, as well as to X-rays, cathode rays, intense ultraviolet radiation, and

FIGURE 15A
Frederick Soddy (1877–1956). Courtesy of the Library of Congress, Prints & Photographs Division, George Grantham Bain Collection, LC-DIG-ggbain-35278.

FIGURE 15B

Ernest Rutherford (1871–1937). Courtesy of Smithsonian Libraries, Galaxy of Images, SIL14-R004-07a.

electric discharges. A surprising proportion of the claims of success dealt specifically with the transmutation of mercury into gold, the transformation so frequently cited within the literature of historical alchemy. In Japan, Hantaro Nagaoka (1865–1950), who was the first to propose a planetary model of the atom in 1904, announced in 1923 that he and coworkers had produced gold and silver from mercury by means of an electric discharge. At about the same time, the physicist Robert Millikan (1868–1953), at the California Institute of Technology, attempted other transmutations from aluminum, also using electric discharges. In the following year, two chemists in Germany claimed they had transmuted mercury into gold by the extended passage of electric current through a mercury vapor lamp. A chemist at the University of Amsterdam attempted a similar process on lead vapor, and reported the production of mercury and thallium (but not gold) therefrom.[86] Soddy, reacting to these reports, explained in *Nature* how electrical discharges could well cause the transmutations observed in Germany (by causing nuclear electron capture, thus lowering the atomic number by one, hence turning mercury into gold), and indeed once the correct voltage was found, such conversions seemed to him "inevitable."[87] Others opined that radium was itself the philosophers' stone, and that early alchemists must have isolated it centuries before Marie and Pierre Curie did so in 1898—this notion was expressed by both chemists and contemporaneous neo-alchemists.

It was Sir William Ramsay (1852–1916), another Nobelist chemist, who worked most assiduously on effecting transmutation, and believed that he had succeeded in producing an array of elements artificially (fig. 16). Following his collaboration with Soddy that demonstrated that the emanation from radium (i.e., radon) spontaneously transforms itself into helium, Ramsay focused his efforts

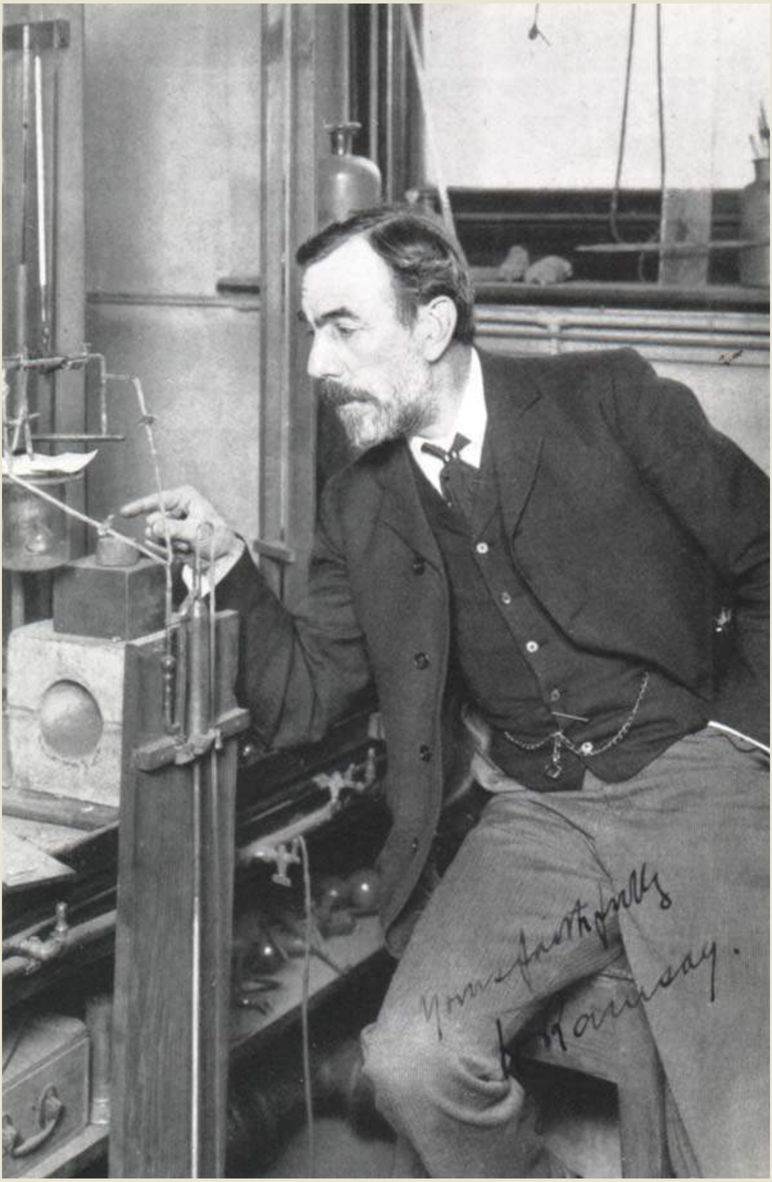


FIGURE 16

Sir William Ramsay (1852–1916). Courtesy of Smithsonian Libraries, Galaxy of Images, SIL14-R001-05a.

on this gaseous emanation. By combining it with other materials, Ramsay claimed to have produced various elements—as if radon were an agent of transmutation. When allowed to decay dry, the gas gave helium, but when mixed with water it produced neon. With aqueous copper salts, it gave argon instead, while apparently transmuting some of the copper into lithium and sodium. Thorium salts decaying in water produced carbon.[88] Such transmutatory abilities, combined with measurements of the astonishing amount of energy contained within this new substance, led some to equate it with "Bacon's Philosopher's Stone." And because the elements Ramsay claimed to have produced had atomic weights that were integral multiples of that of helium, it seemed to some observers that a new verification of a modified Prout's hypothesis, and support for the unity of matter—a doctrine constantly asserted by Jollivet-Castelot, his associates, and other neo-alchemists—had been found.[89]

Strikingly, Jollivet-Castelot carried out simultaneously in his own laboratory some of the same types of experiments, although more crudely performed, as the Nobelist chemists (fig. 17). In 1907, he purchased a milligram of radium bromide from the Curies and exposed copper and silver foils to its emissions for six months. Subsequent treatment suggested to him that minute quantities of copper and gold were produced in the silver as a result. In reporting this outcome, Jollivet-Castelot did not hesitate to compare his experiment "although incomplete and rudimentary" to Ramsay's nearly contemporaneous claim to have produced lithium from copper.[90] Thus the opening years of the twentieth century witnessed the remarkable situation that similar experiments were being carried out toward similar goals simultaneously by leading-edge scientists and by neo-alchemists. Jollivet-Castelot continued his practical experimentation into the 1930s, despite a disastrous fire that destroyed much of his residence at Douai in January 1925.[91]

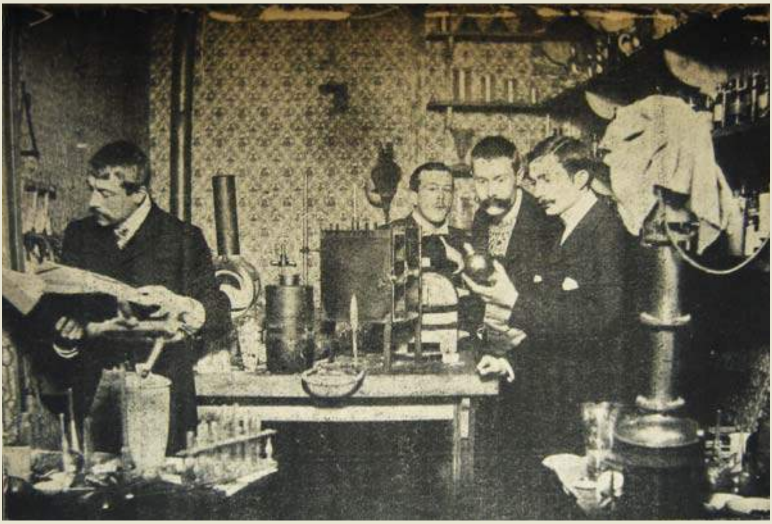
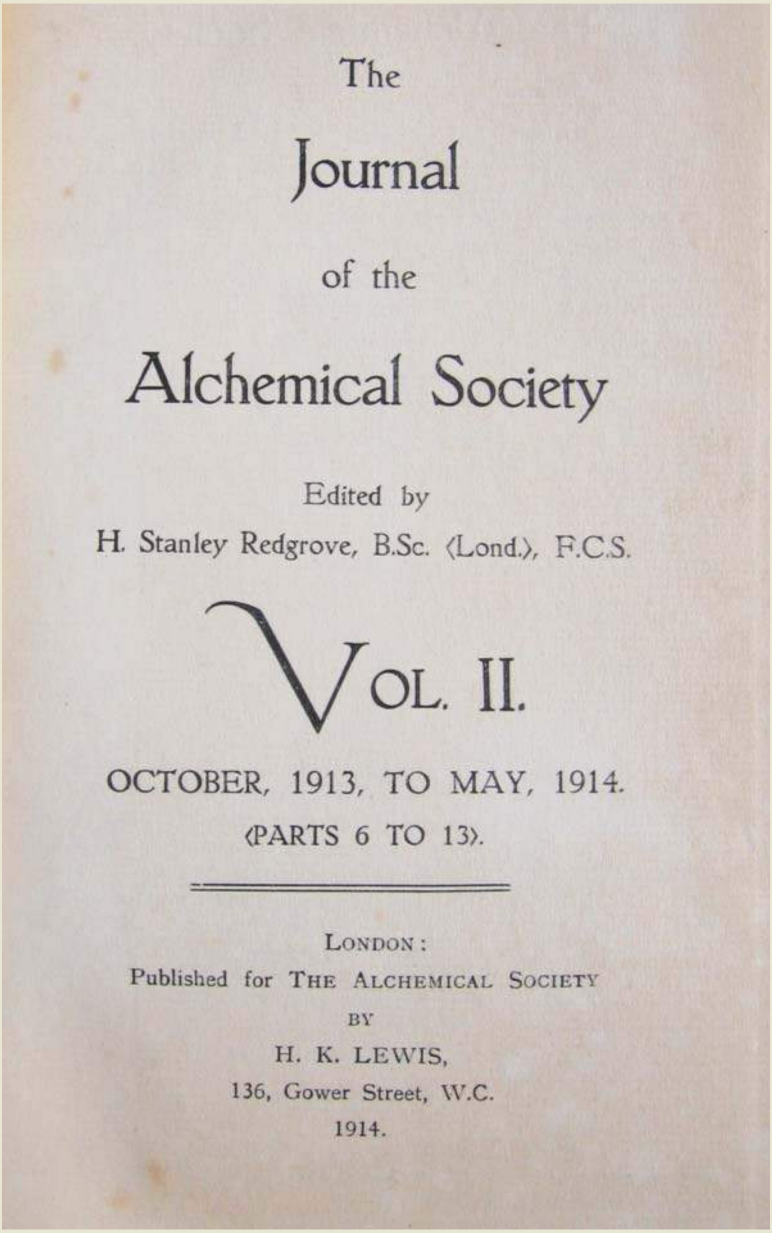


FIGURE 17

A laboratory of the Société Alchimique de France, ca. 1904. François Jollivet-Castelot is second from the right. From Jollivet-Castelot, *La science alchimique* (Paris, 1904).

An equally dramatic illustration of the new reunion of alchemy and chemistry is provided by the fact that in 1904, Ramsay contacted and then collaborated secretly for two years with a self-styled alchemist in Philadelphia named Robert Melville Hunter. Hunter claimed to be able to make gold from silver through a process of maturation—an idea not far removed from the ancient alchemical idea that metals mature naturally, one into the other, underground. Hunter even began planning to open a factory in Philadelphia to commercialize his process for gold-making.[92] Analogous, if less dramatic, contacts were made more openly in London with the foundation of the Alchemical Society, in 1912, by H. Stanley Redgrove (1887–1943), professor of chemistry at Glasgow and Fellow of the Chemical Society. A sister society to Jollivet-Castelot's Société Alchimique de France, the Alchemical Society's specific goal was to facilitate discussions and collaborations between chemists, alchemists, occultists, and historians, a charge it carried out surprisingly successfully for three years before the onset of World War I forced its closure. The Society published a monthly journal where contributions by chemists, historians, and occultists appeared side by side, and the membership lists reveal a correspondingly broad spread of participants (fig. 18). Advertisements and respectful reports about the Society and its journal appeared both in *Nature* and in occultist publications. Some renowned chemists even started exploring other concepts from traditional alchemy, such as the growth and maturation of metals in the earth—the new nuclear chemistry seemingly put everything up for grabs again. In fact, Soddy was sufficiently sure that the problem of making gold would soon be solved that he (and others) began to express concerns about the stability of the gold standard and the potential for catastrophic effects on the world economy once an efficient means of transmutation was developed, a worry that Tiffereau had often voiced years earlier.[93]



The

Journal

of the

Alchemical Society

Edited by

H. Stanley Redgrove, B.Sc. (Lond.), F.C.S.

VOL. II.

OCTOBER, 1913, TO MAY, 1914.

(PARTS 6 TO 13).

LONDON:

Published for THE ALCHEMICAL SOCIETY

BY

H. K. LEWIS,

136, Gower Street, W.C.

1914.

FIGURE 18

The title page from an issue of the London-based *Journal of the Alchemical Society*, active 1913–15. Author's collection.

Conclusion

IT IS THUS CLEAR THAT ALCHEMY AND CHEMISTRY HAVE BEEN considerably more intertwined, and their interactions more dynamic, over the past three hundred years than has generally been recognized. Far from suffering a definitive divorce in the early eighteenth century, chemistry and alchemy have only occasionally been far apart. Instead, they have repeatedly flirted with one another, and even contemplated "reunion" during the intervening years. The traditional alchemical goal of metallic transmutation was by no means banished from chemistry by experimental or theoretical considerations in the early eighteenth century, even though social and institutional pressures made its continued open pursuit discreditable, and retrospective histories of chemistry fixed on the putative final repudiation of transmutation as a measure of "progress." Instead, new experiments and theories repeatedly revived the hopes of successful transmutation. While some later alchemists continued to turn to the cryptic books of the premodern alchemical tradition, others turned to the newest concepts and discoveries of chemistry—phlogiston, Prout's hypothesis, isomerism, integral atomic weights, microbial action, radioactive decay—in their pursuit of long-standing goals. There likewise remain today many people, many of them serious and intelligent, at work on alchemical transmutations; as previously, some of them endeavor to interpret early texts, while others appeal to contemporary chemistry and the other sciences for assistance and inspiration. Thus the story recounted here could be carried down another hundred years to the present day, although perhaps not so many prominent chemists were involved in this more recent history as in the past.

It has been pointed out that the discovery of the subatomically composite nature of elements did, in a certain sense, restore the premodern alchemical notion that the metals are compounds, and thus able to be taken apart and recombined in different ways. Yet the gap between theoretical possibilities and successful experimental practices that so frustrated premodern alchemists widened rather than narrowed when the boundary was passed from the domain of chemistry and its sensible macroscopic manipulations of materials to the bizarre, intractable world of subatomic physics. The elements turned out to be compound materials after all, but the unexpected result was that their components—protons, electrons, and neutrons—were no longer chemical substances. Perhaps ironically, it was a group comprised predominantly of chemists that successfully transmuted bismuth into gold by atomic bombardment at Lawrence Berkeley National Laboratory in 1980, although in quantities too small for chemical methods to detect and at a cost too exorbitant to cheer the heart of any chrysopoeian.[94] An interesting story might well be written of the gradual shift of research on atomic structure and radioactivity from chemistry to physics during the twentieth century, and how this has affected our understanding both of the natural world and of the history of science.

The successive making-up and breaking-up of alchemy and chemistry underscores the commonality of goals and practices expressed by the word *chymistry* when speaking of the early modern period. The desire to understand and to control matter and its transformations lies at the heart of both alchemy and chemistry, even during those periods when these goals were expressed or pursued in different ways. The story also underscores how difficult it really has been (and remains) to understand the microstructure—indeed the very nature itself—of matter, a realm forever beyond the limits of human sense perception. The ancients, at least from the time of Thales, struggled with the nature of matter, medieval and early modern alchemists theorized and experimented to discover it, chemists devised and wore out system after system to explain it and to account for their observations, and modern physicists, in very different ways, continue to puzzle over it. The on-again, off-again relationship between transmutational alchemy and chemistry illuminates the curious ways in which science develops—neither by a linear movement from discovery to discovery, nor by the orderly accumulation of knowledge and experience, but rather by a tortuous, unpredictable path full of turnings and double-backs where ideas and knowledge are acquired, rejected, forgotten, revived, and revised, sometimes only to be forgotten or rejected again. It is part of the long and continuing history of the human desire to know, to do, and to create.

Notes

1 For reflections on the rehabilitation of alchemy within the history of science, see Lawrence M. Principe, "Alchemy Restored," *Isis* 102 (2011): 305–12. For an up-to-date survey of the history of alchemy, see Lawrence M. Principe, *The Secrets of Alchemy* (Chicago: University of Chicago Press, 2013).

2 Lawrence M. Principe, *The Aspiring Adept: Robert Boyle and His Alchemical Quest* (Princeton, NJ: Princeton University Press, 1998).

3 William R. Newman and Lawrence M. Principe, "Alchemy vs. Chemistry: The Etymological Origins of a Historiographical Mistake," *Early Science and Medicine* 3 (1998): 32–65; see also William R. Newman and Lawrence M. Principe, "Some Problems in the Historiography of Alchemy," in *Secrets of Nature: Astrology and Alchemy in Early Modern Europe*, ed. Newman and Anthony Grafton (Cambridge, MA: MIT Press, 2001), 385–431.

4 George Sarton, "Boyle and Bayle: The Sceptical Chemist and the Sceptical Historian," *Chymia* 3 (1950): 155–89; on 161–62, the paired term of "fools or knaves" is probably borrowed from David Brewster's characterization of one of Newton's favored alchemical authors as "a fool and a knave" in his *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton*, 2 vols. (Edinburgh, 1855), 2: 374–75.

5 For further treatment of this topic, see Principe, *Secrets of Alchemy*, 35–37, 56–58, 109–10, and passim.

6 For insights into related events in Sweden, see Hjalmar Fors, *The Limits of Matter: Chemistry, Mining, and Enlightenment* (Chicago: University of Chicago Press, 2015). For Germany, particularly in relation to Stahl, see Kevin (Ku-Ming) Chang, "Georg Ernst Stahl's Alchemical Publications: Anachronism, Reading Market, and a Scientific Lineage Redefined," in *New Narratives in Eighteenth-Century Chemistry* ed. Lawrence M. Principe (Dordrecht: Springer, 2007), 23–43.

7 Erich Odhelius to Urban Hjärne, June 6, 1692, in Carl Christoffer Gjörwell, *Det Swenska Biblioteket*, 2 vols. (Stockholm, 1757–62), 1: 337–39; for more details on these prohibitions, see Lawrence M. Principe, "The End of Alchemy? The Repudiation and Persistence of Chrysopoeia at the Académie Royale des Sciences in the Eighteenth Century," *Osiris* 29 (2014): 96–116.

8 Lawrence M. Principe, "A Revolution Nobody Noticed? Changes in Early Eighteenth Century Chymistry," in *New Narratives*, 1–22, esp. 9–13.

9 For these chymists' work in transmutation, see Bernard Joly, "Étienne-Francois Geoffroy: A Chemist on the Frontiers," *Osiris* 29 (2014): 117–31; for Duclos, see Alice Stroup, "Censure ou querelles savantes: L'Affaire Duclos (1666–1685)," in *Règlement, usages et science dans la France de l'absolutisme*, ed. Christiane Demeulenaere-Douyère and Éric Brian (Paris: Lavoisier Tec et Doc, 2002), 435–52; and Lawrence M. Principe, "Sir Kenelm Digby and His Alchemical Circle in 1650s Paris: Newly Discovered Manuscripts," *Ambix* 60 (2013): 3–24, esp. 17–19; for Homberg, Lawrence M. Principe, "Wilhelm Homberg: Chymical Corpuscularianism and Chrysopoeia in the Early Eighteenth Century," in *Late Medieval and Early Modern Corpuscular Matter Theories*, ed. C. Lüthy, J. E. Murdoch, and W. R. Newman (Leiden: Brill, 2001), 535–56; and "Wilhelm Homberg et la chimie de la lumière," *Methodos: Savoirs et textes* 8 (2008), http://methodos.revues.org/.

10 Nicolas Lemery had also condemned transmutation in the 1679 third edition of his *Cours de chymie* (57–58); although Lemery was an academician after 1699, his activity within the Académie was relatively minor and he acted primarily as an apothecary and physician rather than as a chymist, unlike Duclos, Homberg, and Geoffroy.

11 Étienne-François Geoffroy, "Des supercheries concernant la pierre philosophale," *Mémoires de l'Académie Royale des Sciences* 24 (1722): 61–70; on the context for this publication, see Principe, "End of Alchemy?," 105–06.

12 For details and documents revealing the survival of transmutational alchemy in the French Academy, see Principe, "The End of Alchemy?," 107–15.

13 Ibid., 113–15; Guillaume-François Rouelle, "Cours de Chimie," Clifton College MS, 894: "le commun des phisiciens doute de la verité de cette science; mais ils ne peuvent pas être juges dans une matiere qui leur est entierement inconnüe; les plus sçavants d'entre les Chimistes, ceux même qui n'ont pas possedé ces principes ne le revoquent pas en doute." Also, 898: "Les plus sensés et les plus sçavants chimistes l'ont crû, les ignorans et les gens peu instruits l'ont nié."

14 Ibid., 898: "M. Rouelle pense que la pierre philosophale, n'est autre chose que la résultat d'une fermentation de l'or, avec le mercure; non pas le mercure ordinaire, mais un mercure particulier surchargé de phlogistique."

15 Christine Lehman, "Alchemy Revisited by the Mid-Eighteenth Century Chemists in France: An Unpublished Manuscript by Pierre-Joseph Macquer," *Nuncius* 28 (2013): 165–216. This paper contains a full transcription of Macquer's manuscript (Bibliothèque Nationale, Paris, MS français 9132, fols. 98r–106v), 202–16. For more description of this manuscript, see William Smeaton, "Macquer on the Composition of Metals and the Artificial Production of Gold and Silver," *Chymia* 11 (1966): 81–88.

16 On Lavoisier and the marginalization of alchemy in the late 1780s, see Marco Beretta, "Transmutations and Frauds in Enlightened Paris: Lavoisier and Alchemy," in *Fakes!? Hoaxes, Counterfeits and Deception in Early Modern Science*, ed. Beretta and Maria Conforti (Sagamore Beach, MA: Science History Publications, 2014), 69–107.

17 Archives départmentales de la Vendée, Actes de naissance, mariages, et décès AD2E267/5: Sainte-Radégonde-des-Noyers, naissances, 1819, fol. 13v (June 24); born to Nicolas Tiffereau (age 35) and Rose Priouzeau. Tiffereau's biographical details have not previously been fully researched and few summaries exist (one, with some faulty information, is *Dictionnaire national des contemporains*, ed. C. E. Curinier [Paris, 1899] 1: 110–11); a good account was however recently published by Philippe Virat, "Cyprien-Théodore Tiffereau (1819–19..?): Chimiste, alchimiste et photographe," *Vaugirard-Grenelle: Bulletin de la Société Historique et Archéologique du XVème Arrondissement de Paris*, no. 45 (Spring 2015): 47–58. The fullest autobiographical details appear in Cyprien-Théodore Tiffereau, *Les métaux sont des corps composés: Production artificielle de l'or: Lettre à MM. les Membres de la Commission du Budget, à MM. les Sénateurs et à MM. les Députés, etc*. (Paris: Quelquejeu, 1888), 9–20, with some additional material in *La science en face de la transmutation des métaux* (Paris, 1906), 4–8.

18 His passport, issued at Bordeaux on December 3, 1842, is at Archives Départementales de la Gironde, cote 4 M 722/670.

19 Olivier Debroise, *Mexican Suite: A History of Photography in Mexico* (Austin: University of Texas, 2001), 22–25; Rosa Casanova, Olivier Debroise, and Pablo Ortiz Monasterio, *Sobre la superficie bruñida de un espejo: Fotógrafos del siglo XIX* (Mexico City: Fondo de Cultura Económica, 1989), 28–29; and *La lumière* 4 (July 29, 1854): 119 and 5 (February 10, 1855): 23.

20 Cyprien-Théodore Tiffereau, *Les métaux sont des corps composés* (Vaugirard, 1855), 3, 22–23, and 27–29. Here it is stated that he left Tampico in May 1848, while *Les métaux* [1888], 12, gives February 1848.

21 On the Académie's *plis cachetés*, see Éric Brian and Christiane Demeulenaere-Douyère, *Histoire et mémoire de l'Académie des sciences: Guide des recherches* (Paris: Tec & Doc, 1996), 73–74.

22 *Comptes rendus hebdomadaires de l'Académie des Sciences* 32 (January 6, 1851): 64; Archives de l'Académie des Sciences (hereinafter AAS), Paris, *pli cacheté* #1070: "Nouveau point de veu sous lequel nous devons envisager les Métaux, basé sur un fait acquis à la Science par l'experimentation," now filed in the *pochette de séance* for January 13, 1851. In accordance with a policy initiated in the 1980s, the Archives began opening *plis cachetés* that have been on deposit for over one hundred years; #1070 was opened on June 9, 1983. Tiffereau's previous *pli*, #1052 (now filed with the *pochette* for November 25, 1850), deals with the use of balloons to transport water for irrigation—a topic he pursued alongside transmutation and for which he likewise made observations in Mexico. See Cyprien-Théodore Tiffereau, *Nouveaux procédés d'irrigation* (Paris, 1854).

23 My survey of the inventory of *plis* at the Académie indicates that very few were ever requested to be opened, undoubtedly because their use was primarily to decide cases of priority, and most authors later published some version of their contents without encountering such issues; thus the *plis* became no longer necessary and were forgotten.

24 The deposit is recorded in *Comptes rendus* 34 (May 31, 1852): 852; Tiffereau mentioned in his memoir of October 17, 1853 (Tiffereau, *Les métaux* [1855], 11) that this *pacquet* contained a sample of his artificial gold. AAS, *pli cacheté* #1233, May 31, 1852. I wish to express my profound thanks to the archivists who permitted me to open Tiffereau's sample.

25 Other samples include the numerous coins of alchemical metal produced in the sixteenth and seventeenth centuries. See Vladimir Karpenko, "Coins and Medals made of Alchemical Metal," *Ambix* 35 (1988): 65–76; "Alchemistische Münzen und Medaillen," *Anzeiger der Germanischen Nationalmuseums 2001*, 49–72; and *Alchemical Coins and Medals* (Glasgow: Adam MacLean, 1998), as well as the lumps of silver and gold produced by Johann Friedrich Böttger in 1713, preserved in the Staatliche Kunstsammlungen, Dresden (Porzellansammlung). Further analysis of Tiffereau's metal is planned; as of this publication, the author is awaiting the Académie's authorization to proceed.

26 Tiffereau, *Les métaux* [1888], 14.

27 Cyprien-Théodore Tiffereau, *Les métaux ne sont pas des corps simples, mais bien des corps composés. La production artificielle des métaux précieux est possible, est un fait avéré* (Paris: L. Martinet, 1853). The *Archiv der Pharmacie* 126 (1853): 76–80 contains a German translation by H. Wackenroder, an editor of the journal, along with a note explaining that he published it to show the continuation of alchemy; he had previously written a *Skizze der Geschichte der Alchemie*. *Comptes rendus* 36 (June 27, 1853): 1139 (citation of receipt of the pamphlet); *La lumière* 3 (July 16, 1853): 114 (explanation of why the Académie could not comment).

28 *Comptes rendus* 37 (October 18, 1853): 579; the autographed memoir is in the *pochette de séance* for October 17, 1853, and is published without the final paragraph requesting the Académie's attention and "encouragement moral," in Tiffereau, *Les métaux* (1855), "Deuxième mémoire," 9–16.

29 See W. V. Farrar, "Nineteenth-Century Speculations on the Complexity of Elements," *British Journal for the History of Science* 8 (1965): 297–323; and David M. Knight, *Atoms and Elements* (London: Hutchinson, 1967).

30 I here cite the atomic weight values accepted in the early nineteenth century rather than the modern values. Many of the early values were off by a factor of two since it was not recognized that the non-noble elemental gases, such as hydrogen, are diatomic.

31 William Prout, "On the Relation between the Specific Gravities of Bodies in their Gaseous State and the Weights of the Atoms," *Annals of Philosophy* 6 (1815): 321–30; "Correction of a Mistake in the Essay on the Relation Between Specific Gravities of Bodies in their Gaseous State and the Weights of their Atoms," *Annals of Philosophy* 7 (1817): 111–13: "We may almost consider the πρώτη ὕλη to be realised in hydrogen"; William H. Brock, *From Protyle to Proton: William Prout and the Nature of Matter, 1785–1985* (Bristol: Adam Hilger, 1985); and "Studies in the History of Prout's Hypothesis," *Annals of Science* 25 (1969): 49–80, 127–37.

32 The most striking of these experiments was the production of the ammonium amalgam by the electrolysis of ammonium salts using mercury as the anode to capture the liberated "ammonium metal." The fact that a shiny amalgam was formed in the process—something that otherwise occurred only with metals—gave impressive evidence of the metallic nature of the ammonium radical.

33 Farrar, "Speculations," 304.

34 Louis Figuier, *L'alchimie et les alchimistes* (Paris, 1854), 73.

35 Jean-Baptiste Dumas, *Leçons sur la philosophie chimique* (Paris, 1838), 318. A further investigation of Dumas's attitude toward transmutation and historical alchemy may be revealing, and perhaps elucidate his possible support of Tiffereau. An intriguing piece of evidence is Dumas's personal copy of Jean-Albert Belin's *Les aventures d'un philosophe inconnu* (1674 edition) interfoliated with his notes and annotations; this item recently appeared in a bookseller's catalogue (Catalogue #115 of L'intersigne, 25) but has now disappeared into private hands. On Belin's work, see Sylvain Matton's introduction to *Les aventures du philosophe inconnu* (Paris: Bibliotheca Hermetica, 1976).

36 Alexandre-Edouard Baudrimont, *Traité de chimie générale et expérimentale*, 2 vols. (Paris, 1844–46), 1: 68–69, 275.

37 Some of Baudrimont's alchemical manuscripts are preserved at the Université de Bordeaux Montaigne, fonds générale. MSS 1, 18, 19, and 24 bear his signature, and MSS 8, 13, 14, 17, 22, and 25–28 are likely to have been his as well. MS 19 contains Baudrimont's autograph notes on Basil Valentine's *Twelve Keys*, some dated 1862. Several other important alchemical manuscripts, presumably also Baudrimont's, were lost when a portion of the manuscript collection was transferred to the Bibliothèque Universitaire des Sciences et Techniques. In addition to twenty alchemical manuscripts, he also bequeathed the library one hundred printed alchemical works, and eighty books that belonged to Lavoisier. See Georges Rayet, "Histoire de la faculté des sciences de Bordeaux (1838–1894)," *Actes de l'Académie nationale des sciences, belles-lettres, et arts de Bordeaux*, third series, 59 (1897), esp. 73–74 and René Maury, "Les fonds patrimoniaux de la bibliothèque universitaire des sciences et techniques de Bordeaux," *Revue française d'histoire du livre* 88–89 (1995): 405–10.

38 Robert Kane, *Elements of Chemistry* (New York, 1842), 233.

39 Tiffereau, *Les métaux* (1855), 5–7, 23–24, 53–55; his citation of catalysis and the focus on the role of oxygen in the transformation may signal a theoretical debt to Baudrimont specifically, cf. his *Traité de chimie*, 1: 275, although Tiffereau also cites the Belgian chemist Jean-Baptiste Van Mons (1765–1842) in this regard.

40 *Comptes rendus* 38 (May 8, 1854): 832; 39 (August 21, 1854): 374; (October 16, 1854): 743; (December 26, 1854): 1205. All six memoirs are published in Tiffereau, *Les métaux* (1855).

41 The registration of this brevet on December 22, 1853, is published in *Bulletin des lois de la république française*, series 11, vol. 5 (Paris, 1855), 1178. His original application is preserved at the Institut National de la Propriété Industrielle, Paris, cote 1BB18349.

42 There is no evidence that the academicians considered Tiffereau dishonest, and virtually every contemporaneous account of him cites his sincerity and humility.

43 *La lumière* 4 (September 9, 1854): 142; cf. Tiffereau, *Les métaux* (1855), 20–21, 39.

44 *Comptes rendus* 31 (July 15, 1850): 62. For illustrations and descriptions of his devices, see Tiffereau, *Nouveaux procédés*, 23–33. For a recommendation of *sabliers Tiffereau* as essential equipment for photographers, see T. Robinson, *La photographie* (Paris, 1867), 22; see also *La lumière* 7 (January 24, 1857): 13. Tiffereau wrote the entry "sabliers" in the *Répertoire encyclopédique de photographie*, 2 vols. (Paris, 1862), 2: 340. For the *brevets*, see *Bulletin des lois de la république française*, series 10 (Paris, 1852), 8: 447: "une horologe hydraulique" (July 16, 1850); 9: 65: "perfectionnements au sablier commun" (August 10, 1850); series 11 (Paris 1854), 2: 812; the original applications are Institut National de la Propriété Industrielle, Paris, cotes 1BB10196, 1BB10327, 1BB13022, 1BB16905, and 1BB17825-6. For contact with the Académie regarding his hydraulic work, see *Comptes rendus* 32 (May 19, 1851): 775 (here he is erroneously listed as "Tissereau"); 34 (February 23, 1852): 290; and 34 (May 31, 1852): 847; for the *gazomètre*, 37 (July 11, 1853): 51. See also Tiffereau, *Les métaux* (1855), 20, for his "exploitation de quelques instruments relatifs aux arts physiques" to fund his research.

45 Tiffereau, *Les métaux* (1855), 19, 29, 36; *La lumière* 4 (October 21, 1854): 168; a letter from Tiffereau dated October 13, 1854; see also 5 (April 14, 1855) and (June 2, 1855): 88.

46 *La lumière* 4 (October 21, 1854): 168.

47 Cyprien-Théodore Tiffereau, "Acide nitrique et sulfure de carbone. Action directe des rayons solaires," *Comptes rendus* 39 (October 9, 1854): 692–94; *La lumière* 4 (October 21, 1854): 165 indicates that these experiments were carried out between July 1852 and September 1854. "Über die Einwirkung des directen Sonnenlichts auf ein Gemisch von Salpetersäure und Schwefelkohlenstoff," *Journal für praktische Chemie* 63 (1854): 307–09. Tiffereau returned to this process in the 1880s,

and later revealed that it was an attempt to produce diamonds. See Cyprien-Théodore Tiffereau, *L'art de faire de l'or* (Paris, 1894), 6–7. For his damaged eyesight, see Tiffereau, *Les métaux* (1855), 21.

48 Tiffereau, *Les métaux* (1855), 47–52. Many later writers fail to recognize that the process carried out at the mint was intended to be different from the one used in Mexico, and thus incorrectly conclude that the mint showed Tiffereau's Mexican process to be a failure. See, for example, Louis Figuier, *L'alchimie et les alchimistes*, 2nd ed. (Paris, 1856), 74n, 353n.

49 Tiffereau, *Les métaux* (1855). The entire text was reprinted as an appendix to Figuier, *L'alchimie*, 2nd ed., 391–406. An expanded edition appeared in 1857 (see note 49). A shorter version, containing only the first three memoirs (the first of which is titled simply "Vorwort") appeared in German in the same year, *Die Golderzeugung auf künstlichem Wege ist thatsächlich erwiesen: Die Metalle sind keine einfachen, sondern zusammengesetzte Körper*, (Berlin: S. Abelsdorff, 1855). There is no indication of who did the translation, but it is probably based on an intermediate French collection of the first three memoirs mentioned in *La lumière* 4 (June 3, 1854): 85–86 as "just published" and "on sale in the office of this journal"; I have not found any surviving copies of this pamphlet.

50 *Comptes rendus* 40 (June 18, 1855): 1317, 1319.

51 *Comptes rendus* 41 (October 22, 1855): 647; 42 (March 10, 1856): 475 and (March 17, 1856): 523; "Production artificielle d'or par l'oxydation des sulfures," *Comptes rendus* 46 (May 10, 1858): 896; and *La lumière* 8 (May 22, 1858): 82–83, (May 29, 1858): 87, and (July 3, 1858): 107. For the second of these memoirs, the commission again included Dumas and Chevreul, but with Théophile-Jules Pelouze and Jean-Baptiste-Joseph-Dieudonné Boussingault, replacing Thénard (who had died in 1857). Tiffereau published the first of these memoirs in the second edition of his *Les métaux sont des corps composés* (Vaugirard: Choisnet, 1857), and that memoir was joined by the 1858 paper in a reissue under the title *L'or et la transmutation des métaux: Mémoires et conferences* (Paris: Chacornac, 1889).

52 César-Mansuète Despretz, "Expériences sur quelques métaux et sur quelques gaz," *Comptes rendus* 47 (November 15, 1858): 746–63.

53 *Comptes rendus* 47 (December 27, 1858): 1077. A longer extract from the letter was printed in *L'ami des sciences* 5 (1859): 22–23.

54 Jean-Baptiste Dumas, "Question des corps simples," *Comptes rendus* 48 (January 17, 1859): 139–41; Michel-Eugene Chevreul, "Différence entre l'analyse immédiate des produits de l'organisation et l'analyse minérale," ibid., 142–44. Chevreul had already written on this subject in a lengthy review in *Journal des savants* (May 1851): 284–98, (June 1851): 337–52, (August 1851): 492–506, and (December 1851): 752–68.

55 John William Draper, "On the Influence of Light upon Chlorine, and some Remarks on Alchemy," *Philosophical Magazine*, fourth series, 14 (1857): 321–23. In his "Further Considerations on the Existence of a Fourth Imponderable," *Philosophical Magazine*, third series, 25 (1844): 103–16, Draper declared that "the forty different metals we are acquainted with are merely modifications of one or two more simple forms."

56 John William Draper, "On a New Imponderable Substance, and a New Class of Chemical Rays" *Philosophical Magazine*, third series, 21 (1842): 453–61 and Sarah Kate Gillespie, *The Early American Daguerreotype: Cross-Currents in Art and Technology* (Cambridge, MA: MIT Press, 2016), 114–33.

57 He married Marie-Rose Fleury (b. May 6, 1833) on June 20, 1861 (Archives de Paris, V4E 1819, fol. 120v, acte 237), and the couple had four children: Edmée-Marie-Julie, born April 9, 1862 (V4E 1825, fol. 73v, acte 525), Théodore-Lucien, born June 9, 1866 (V4E 1860, fol. 17r, acte 1107), Blanche-Adrienne-Gabrielle, born May 13, 1872 (V4E 4522, fol. 115r, acte 858), and Emile-Adrien, born 1874 (no record of birth found), died October 7, 1876, at age two (V4E 4569, fol 58v, acte 2513), as well as an unnamed male child, stillborn on June 8, 1863 (V4E 1839, fol. 18v, acte 1186). See also Virat, "Tiffereau," esp. 53–57, and the illustrations therein.

58 *Bulletin de la Société d'Encouragement pour l'Industrie Nationale* 59 (1860): 245; report on and description of the device at 58 (1859): 401–05.

59 Cyprien-Théodore Tiffereau, *L'or et la transmutation des métaux: mémoires et conferences*, (Paris: Chacornac, 1889), 156: "Parvenu après trente années du plus opiniâtre labeur à acquérir une modeste fortune, je résolu en 1884 de reprendre mon travail sur l'or et de le conduire à bonne fin"; *L'art de faire* (1892), 70. His photography business carried on under a successor whose family name was Désiré, as listed on photographs taken after this time; see Virat, "Tiffereau," 57.

60 Tiffereau, *Les métaux* (1888), i. Letter to Berthelot mentioned in Tiffereau, *L'or et la transmutation*, 156; Marcellin Berthelot, *Les origines de l'alchimie* (Paris, 1885), 285; a condensed version was published the year before: *Nouvelle revue* 26 (1884): 445–90, 668–707.

61 Tiffereau, *Les métaux* (1888), 4–25; *Bulletin de la Société chimique de France*, new series, 41 (1884): 321, 369, 593; 44 (1885): 50, 109–10; these experiments continued until at least 1888; see Tiffereau, *L'art de faire de l'or* (1894), 6–7. For contact with the Académie in the 1880s, see *L'or et la transmutation*, 157; AAS, *pochette de séance*, June 11, 1888 (pli #4288), and *Comptes rendus* 102 (June 21, 1886): 1509 (description of a gazomètre). After Tiffereau's 1858 withdrawal, there had been only one other report of transmutation sent to the Académie (by Pierre-Antoine Favre [1813–1880], a student of Dumas, professor at Marseille, and corresponding member of the Académie) although there seems to have been no follow-up to it, *Comptes rendus* 61 (December 18, 1865): 1130.

62 Berthelot, *Origines*, 288–319; Dumas, a major proponent of such ideas, died the year before Berthelot's book was published.

63 On the occult revival of alchemy, see Principe and Newman, "Some Problems in the Historiography of Alchemy," 388–400.

64 Tiffereau, *L'or et la transmutation des métaux* (1889). This publication contains new materials, including the text of a public lecture held March 16, 1889, and the assayer's report on the transmuted gold; it was reprinted in 1924.

65 The first conference seems to have occurred on December 26, 1888, at the Salle de l'Ermitage, on rue Jussieu (*La Justice*, December 28, 1888, 3). Another followed on February 16, 1889, at the salle Pétrelle (*Le Gaulois*, "La pierre philosophale," February 21, 1889, 2). A third was held on March 16, 1889, on the Boulevard des Capucins, much of the text of which is given in Tiffereau, *L'or et la transmutation*, 151–66; Tiffereau, *L'art de faire de l'or* (1892), 24 gives the erroneous date 1887 for this event.

66 Cyprien-Théodore Tiffereau, "Production des métaux précieux," *Association Française pour l'Avancement des Sciences: Comptes rendus*, 2 vols. (Paris, 1890), 279; Tiffereau, *L'art de faire de l'or* (1892), 14, 85.

67 A considerable bulk of *L'art de faire de l'or* (1892), 41–61 and (1894), 40–62, are devoted to reprinting and enumerating interviews and articles published in the popular press.

68 Cyprien-Théodore Tiffereau, *L'art de faire de l'or* (Paris: 1896), 4.

69 The first mention of microbial action was made at a public lecture on March 17, 1891. See Le Brun de Virloy and Cyprien-Théodore Tiffereau, *L'accroissement de la matière minérale et la transmutation des*

métaux (Paris, 1893), 30–31, and mentioned briefly in an interview in the *Le Figaro* (October 30, 1891), but the idea was not fully developed into his theory of metallic transmutation until his lecture on May 24, 1892, Tiffereau, *L'art de faire de l'or* (1892), 61–63, 65, and 97–103. Sergei Winogradsky, "Recherches sur les Organismes de la Nitrification," *Comptes rendus* 110 (1890): 1013–16 and Lloyd Ackert, *Sergei Vinogradskii and the Cycle of Life* (Dordrecht: Springer, 2013), 77–83.

70 Tiffereau, *L'art de faire de l'or* (1894), 14: "ayant moi-même exposé ma limaille d'argent et mon acide azotique sur la terrasse de mon appartement, il y a lieu de supposer que les microbes des lingots d'or et d'argent apportés journellement à la Monnaie ont fait élection de domicile dans mes fioles et ont aidé efficacement à la transmutation définitive."

71 See William B. Jensen, "Whatever Happened to the Nascent State?," *Bulletin for the History of Chemistry* 6 (1990): 26–36.

72 Tiffereau, *L'art de faire de l'or* (1896), 12, 15–17.

73 Nancy L. Parduhn and John R. Watterson, "Preliminary studies of Bacillus cereus distribution near a gold vein and a disseminated gold deposit," U.S. Department of the Interior, Geological Survey, Open-file Report 1984, 84–509; American Society for Microbiology, "Bacteria Point The Way To Gold Deposits," *Science Daily*, May 24, 2002, www.sciencedaily.com/releases/2002/05/020523075914.htm.

74 *Comptes rendus* 123 (December 14, 1896): 1097; 125 (October 18, 1897): 584; 127 (August 19, 1898): 403; the text of these papers was published in *Le Gaulois*, "Notes scientifiques," January 9, 1897, 3, and in "Un peu d'alchimie," *La science française* 8, (October 7, 1898): 166–67. The receipt of his *La transmutation des métaux* (Paris, 1900) was recorded in *Comptes rendus* 130 (April 30, 1900): 1219, as "hommage de l'auteur"; a copy went also to the Société Chimique de Paris, see their *Bulletin* 23 (1900): 418. Receipt of Tiffereau's *La science en face de la transmutation des métaux* (Paris: Billon, 1906) is recorded in *Comptes rendus* 143 (September 17, 1906): 439.

75 C. Théodore Tiffereau, "Un fait indéniable," *Hyperchimie* (1896) and "Les métaux et les métalloïdes sont des corps composés," *Hyperchimie* (January 1897). A July 24, 1898, letter to Jollivet-Castelot is published in Tiffereau, *Conférence sur la transmutation des métaux et la composition des gaz* (Paris, 1900), 16–20. Tiffereau also visited Jollivet-Castelot's associate, the Swedish playwright August Strindberg, who dabbled in transmutational experiments, in 1896–97 while Strindberg was resident in Paris; see François Jollivet-Castelot, *Bréviaire alchimique:*

Lettres d'August Strindberg à Jollivet Castelot (Paris: Durville, 1912), 21, 45; on Strindberg, see Alain Mercier, "August Strindberg et les alchimistes françaises: Hemel, Vial, Tiffereau, Jollivet-Castelot," *Revue de littérature comparée* 43 (1969): 23-46.

76 Tiffereau received letters from Emmens in 1897, as mentioned (and extracted) in J. Marcus de Vèze, *La transmutation des métaux* (Paris: Dorbon, 1902), 31–34, and also one dated September 7, 1902, which would represent the last known appearance of Emmens, on whom, see George B. Kauffman, "The Mystery of Stephen H. Emmens: Successful Alchemist or Ingenious Swindler?" *Ambix* 30 (1983): 65–88. Emmens's claims were widely covered in the French press, almost always accompanied by some mention of Tiffereau, e.g. the interview with Jollivet-Castelot in *Le Gaulois*, "Le premier lingot d'or artificiel" (June 13, 1897), 1.

77 *Le journal du dimanche* (March 1, 1903), 14.

78 *L'Ouest-Éclair* (February 15, 1926), 1; the source is an article about Jollivet-Castelot that appeared in *Paris-Midi*—I have not been able to trace the original.

79 This new information comes from a letter from Tiffereau's granddaughter, Lucie Bernard, written June 1, 1964, to the Secrétaire perpétuel of the Académie des Sciences, and filed in the *pochette de séance* for December 14, 1896. The letter concerns finding a depository for Tiffereau's manuscript papers and "a small flat mahogany box containing samples of artificial gold obtained in the course of experiments performed in Mexico around 1847 and continued in Paris until 1905." Most lamentably, the Académie declined to accept these precious materials; their present whereabouts, if they still survive, remain unknown.

80 Virat, "Tiffereau" and personal communication.

81 Although the 1905 (Paris, Quelquejeu) and 1906 (Paris, Billon) pamphlets share the title *La science en face de la transmutation des métaux*, their texts are completely different.

82 François Jollivet-Castelot, *Le grand-oeuvre alchimique* (Paris, 1901), 7, and *Devenir alchimiste*, 417. A very unfriendly assessment of Jollivet-Castelot (with unwarranted aspersions cast as well on Tiffereau) as part of a cabal of "educated charlatans" is given by H. Carrington Bolton, "The Revival of Alchemy in France," *Chemical News* 77 (1898): 69–70, 72–74. Bolton's more extensive remarks had been made to

a meeting of the New York section of the American Chemical Society in 1897 and fully published in *Science* 6 (1897): 853–63, and reprinted subsequently in several places. A stern rejoinder was made by Stephen Emmens, *Science* 7 (1898): 386–89.

83 This new rapprochement is excellently explored and documented in Mark S. Morrisson, *Modern Alchemy: Occultism and the Emergence of Atomic Theory* (Oxford: Oxford University Press, 2007).

84 Muriel Howorth, *Pioneer Research on the Atom* (London: New World Publications, 1958), quotation on 83–84. For more on these studies, see Morrisson, *Modern Alchemy*, 97–134; Thaddeus J. Trenn, *The Self-Splitting Atom: The History of the Rutherford-Soddy Collaboration* (London: Taylor & Francis, 1977), 42, 58–60, 111–17, and "The Justification of Transmutation: Speculations of Ramsay and Experiments of Rutherford," *Ambix* 21 (1974): 53–77. The transformation of the "emanation" into helium was demonstrated by Soddy in London and reported in William Ramsay and Frederick Soddy, "Experiments in Radioactivity and the Production of Helium from Radium," *Proceedings of the Royal Society of London* 72 (1903): 204–07; see Howorth, *Pioneer Research*, 98–101.

85 Trenn, *Self-Splitting Atom*, 26, 42; Howorth, *Pioneer Research*, 58. I rely on these secondary sources, not having examined the full originals of Soddy's unpublished lectures.

86 H. Nagaoka, Y. Sugiura, and T. Mishima, "The Fine Structure of Mercury Lines and the Isotopes," *Japanese Journal of Physics* 2 (1923): 121–62; "Preliminary Note on the Transmutation of Mercury into Gold," *Nature* 116 (1925): 95–96; "Die Umwandlung von Quecksilber in Gold," *Naturwissenschaften* 13 (1925): 682–84; Robert H. Kargon, "The Evolution of Matter: Nuclear Physics, Cosmic Rays, and Robert Millikan's Research Program," in William R Shea, *Otto Hahn and the Rise of Nuclear Physics* (Dordrecht: Reidel, 1983), 69–89, esp. 75–78; A. Miethe, "Der Zerfall des Quecksilberatoms," *Die Naturwissenschaften* 12 (1924): 597–98, "Gold aus Quecksilber," 13 (1925): 635–37; A. Miethe and H. Stammreich, "Bildung von Gold aus Quecksilber in abreissenden Lichtbögen," *Zeitschrift für anorganische Chemie* 150 (1926): 350–56; Miethe's autobiographical account of his transmutational experiments, and the impetus behind them, is available in Helmut Seibt, ed., *Adolf Miethe (1862–1927) Lebenserinnerungen* (Frankfurt: Verlag Harri Deutsch, 2012), 261–86. A. Smits, "The Transmutation of Elements," *Nature* 117 (1926): 13, 620; A. Smits and Karssen, *Zeitschrift für Elektrochemie und angewandte physikalische Chemie* 32 (1926): 577–86; A. C. Davies

and Frank Horton, "The Transmutation of Elements," *Nature* 117 (1926): 152. Interesting summaries and critiques of these experiments appear in "The Present Position of the Transmutation Controversy," *Nature* 117 (1926): 758–60, and F. Wolfers, *Transmutation des Éléments* (Paris: Société d'Éditions Scientifiques, 1929).

87 Frederick Soddy, "The Reported Transmutation of Mercury into Gold," *Nature* 114 (1924): 244–45.

88 Alexander Thomas Cameron and William Ramsay, "The Chemical Action of Radium Emanation: Part II," *Journal of the Chemical Society, Transactions* 91 (1907): 1593–606. The appearance of these other elements was later shown to be due to various contaminations.

89 Baskerville, "Recent Transmutations," 56–57.

90 F. Jollivet-Castelot, *La synthèse de l'or* (Paris: Daragon, 1909), 5–6. On Ramsay's work and publications, see Morrison, *Modern Alchemy*, 115–27.

91 Robert Duportail, "L'Alchimie devant la science," *La Pensée française* (January 12, 1925): 9–11. Jollivet-Castelot announced a successful production of gold in *La fabrication chimique de l'or* (Douai: Lefebvre Levêque, 1927), although by more traditionally chemical means than the employment of radium; see also the snarky review of the book in *Nature* 121 (June 23, 1928): 981. For more on Jollivet-Castelot, see Pierre Pelvet, *L'Alchimie en France dans la première moitié du XXème siècle* (PhD diss., Université Paris X - Nanterre, 1980).

92 Morrison, *Modern Alchemy*, 116–17; on Hunter, see Charles Baskerville, "Some Recent Transmutations," *Popular Science Monthly* 73 (1907): 46–51.

93 Morrison, *Modern Alchemy*, 135–60.

94 K. Aleklett, D. J. Morrissey, W. Loveland, P. L. McGaughey, and G. Seaborg, "Energy Dependence of Bi Fragmentation in Relativistic Nuclear Collisions," *Physical Review C* 23 (1981): 1044–46. Glenn Seaborg estimated the cost of production at one quadrillion dollars per ounce of gold.

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